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Revised Estimates of Halibut Abundance and the Thompson-Burkenroad Debate

by

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ABSTRACT

The effects of differences in hook-spacing and soak-time of dory gear and longline gear on catch per unit effort (CPUE) were reexamined. Contrary to earlier conclusions, these factors are of critical importance in the standardization of fishing effort. Estimates of the abundance of halibut from 1915 to 1930 have been recalculated to adjust for differences in the efficiency of dory and longline gear and for seasonal variation in availability. The results indicate that the original calculations by W. F. Thompson and his colleagues underestimated fishing effort and overestimated the catch per unit effort during the early years of the halibut fishery. Reanalysis of the data provides a more accurate estimate of stock abundance in the early years of the halibut fishery and shows that the decline in abundance prior to 1930 was not as precipitous as originally portrayed.

Burkenroad disagreed with Thompson's explanation that fishing mortality caused the decline in abundance, and he proposed natural changes as the cause for the decline. Although Burkenroad's objections to Thompson's interpretation of the original data are substantiated, the revised estimates of CPUE lend credibility to the thesis that the effect of fishing was the major cause of the decline. However, the importance of environmental changes cannot be dismissed, but the precise role of these factors cannot be defined until the estimates of the basic parameters and the interrelations of the stocks are more fully understood. Burkenroad also questioned Thompson's explanation of the increase in abundance after 1930. Economic conditions indicate that factors other than regulatory control, per se, were critical to the apparent recovery of the stocks. Apparently, remarkable increases in growth and flunctuations in year class strength also influenced the recovery, and the effect of these factors, or whether they are fishery induced or natural, has yet to be determined.

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INTRODUCTION

Management of the halibut *(Hippoglossus stenolepis)* fishery by the International Pacific Halibut Commission (IPHC) is cited frequently as one of the few examples of a successful scientific attempt to rehabilitate and maintain a fishery at the level of maximum sustained yield. Although biologists (Graham, 1935; Holt, 1951; Beverton and Holt, 1957; Ricker, 1958; and Fukuda, 1962) have cited shortcomings in IPHC's scientific analyses, few have questioned the basic conclusions and, in this regard, Burkenroad (1948, 1950, 1951, and 1953) stands alone in the depth and severity of his criticism. Specifically, Burkenroad claimed that the stock decline prior to 1930 could not be solely attributed to the effects of fishing as concluded by Thompson, Dunlop, and Bell (1931) and that the increase in abundance from 1930 to 1940 was not the result of management. Thompson (1950 and 1952) rejected Burkenroad's arguments and maintained that the fishery was the dominant factor determining the condition of the stocks. Huntsman (1953), Ketchen (1956), and Fukuda (1962) argued in support of Burkenroad's theory that the environment may have been a significant factor in the fluctuations of stock abundance, but Bell and Pruter (1958) discounted this explanation. For the most part, biologists have accepted Thompson's interpretation and rejected Burkenroad's thesis (Herrington, 1943; Nesbit, 1943; Kesteven, 1950; Holt, 1951; Schaefer, 1954; and Cushing, 1972).

The purposes of this paper are to reevaluate the estimates of stock abundance from 1915 to 1930 and to discuss the results as they concern the Thompson-Burkenroad dispute.* Changes in the fishery since 1955 biased the estimates of catch per unit effort (CPUE) and prompted the review of similar changes during the early years. An analysis of effort in the halibut longline fishery during the 1960's showed that estimates of stock abundance had been too high (Skud, 1972). The bias arose because fishermen had increased the spacing between hooks from 13-foot intervals to 18-, 21-, and 26-foot intervals and the change was not assessed correctly in terms of effective fishing effort. A similar change occurred between 1915 and 1930 when dories were replaced by longline vessels and fishermen increased the hook-spacing from 9-foot to 13-foot intervals. This gear change

* The Thompson-Burkenroad debate probably has a greater notoriety than any other dispute in fisheries science. In reopening the issue, I gave careful consideration to this background and to comments of Regier (1973) who remarked that **bona fide** public controversies in fisheries are sufficiently rare that one should not lightly ignore them. After rereading Beverton and Holt (1957), and noting that fully one-third of their five page introduction was devoted to Burkenroad's criticisms and suggestions, I concluded that the discussion of the debate was consistent with my objectives to provide better estimates of stock size during the early fishery and to determine the causes of fluctuations in abundance — then and now. Thompson's contributions to the "theory of fishing" is widely recognized and though my analyses show his assessment of the halibut fishery was in error, this does not invalidate his general thesis on the effects of fishing which remains an outstanding accomplishment in fishery science.

was also accompanied by a change in soak-time. The effects of these factors on CPUE are reevaluated in this study.

Estimates of CPUE also were influenced by the seasonal change in availability of halibut. This factor has been of singular importance in the fishery, because regulatory measures have significantly altered the fishing seasons. The most recent changes of this type have taken place in the Bering Sea; since 1965, fishing has been restricted to a few weeks in the spring and autumn. Because similar changes in fishing techniques and fishing seasons occurred before 1930, they are reexamined in this paper. The regulatory areas and the statistical areas used for the halibut fishery and referred to in the text are depicted in Figure 1.

SETLINE GEAR

A unit of setline gear or "skate" consists of a groundline, branch lines, and hooks. A number of lines (each 300 feet) spliced end to end form the groundline. In early years, the number of lines varied considerably, but the 6-line skate (1,800 feet) eventually was adopted by most of the fishermen. At regular intervals, loops of light twine (beckets) are attached to the groundline. Short branch lines (gangions) about 5 feet long are attached to the beckets and a hook is attached to the end of each gangion. The interval between hooks or "rig" of the gear has varied from 9 feet to as much as 42 feet. The most common rigs have been 9, 13, 18, 21, 24, and 26 feet, as these intervals facilitate baiting the hooks and coiling the lines.

Traditionally, a distinction has been made between setline gear fished from dories and that fished from larger vessels; the former known as dory gear, the latter as longline gear. The longline vessels carried more gear, used heavier twine, and generally used larger hooks, but the major differences between the two types of setline gear were the number of lines per skate and the spacing between hooks.

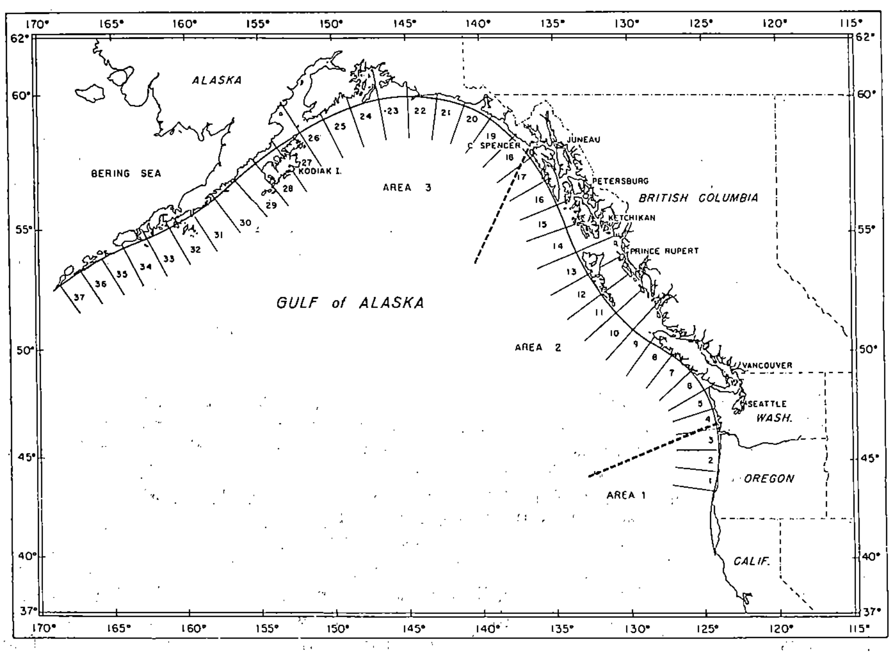

Figure 1. Regulatory areas (1-3) and statistical areas (1-37) of the halibut fishery.

Dory gear was rigged predominately with hooks at 9-foot intervals and the number of lines per skate varied from 6 to 10. Longline gear, which was introduced in 1915, was rigged with hooks at 13-foot intervals and seldom used more than 6 lines per skate. The 9-foot rig had space for 200 hooks on a standard 1,800-foot skate, whereas the 13-foot rig had space for 138 hooks. Wider hook-spacing, 18 to 26 feet, was not common until after 1955.

Another difference between dory and longline fishing was the "soak", the time that the gear remained on the bottom. Dory fishermen usually fished 2 to 6 skates and set and retrieved their gear twice during the daylight hours. Soak-time was usually less than 6 hours. Longline fishermen fished as many as 50 skates and the time between setting and retrieval often exceeded 16 hours. Different types of bait were also associated with the longer soak. The importance of these differences is discussed later in the report.

MEASURE OF EFFORT

In Thompson's (1916) early study of the halibut fishery, he examined a number of ways to assess the stock abundance of halibut. These methods were reviewed by Thompson et al (1931) and comparisons of CPUE were made with different measures of effort: number and length of trips, number of dories, number of men, and amount of gear. The results all showed a decline in stock abundance during the early 1900's. The acceptance of the skate as a standard measure of effort was thoroughly defended by the authors:

> "It at once occurs to the investigator to test carefully whether the skate of gear used by the halibut fleet is suitable for use as a statistical unit. To answer the requirements of an ideal unit of fishing effort, the skate should remain unchanged from year to year, or, if it changes, it should be capable of correction to make the results comparable. This is a matter of great importance if the measure of abundance is to be made an accurate one.
>
> That the skate of gear used by the halibut fleet has changed since the early years of the fishery was recognized in the earlier halibut investigation on this coast (Thompson, 1916, p. 72) and is well known to those engaged in or associated with the industry. Investigation shows, however, that correction can be made for the changes that have taken place in the skate and that the skate is entirely suitable for use as a unit of fishing effort for statistical purposes.
>
> The changes that have taken place in the skate and which might affect its efficiency have been of three kinds: First, a gradual reduction in the length of skate; second, an increase in the distance between hooks as the fleet shifted from dory fishing to long-line fishing; and third, the use in very recent years of lighter gear. These will be considered in turn to determine the effect of each."

Thompson et al (1931) first addressed the relation of length of skate to catch and stressed the fact that the abundance of fish varied spatially: *"For this reason, fishing trials made at the same season and place had to be used for comparison . . . The number of skates obtained were, of course, always unequal and adjustment was necessary to determine the catch which would have been made had the numbers been equal to that of the gear with the lesser representation."* (My italics.) The authors compared catch data from 6- and 7-line dory gear and concluded "that the catch of skates, identical in every way except length, is proportional to length". Having established this premise, the authors reviewed early records of the fishery and found a gradual reduction in the length of skates. In the early 1900's, dory gear had 10 lines per skate (3,000 feet) but gradually decreased to 6 lines per skate (1,800 feet). Thompson et al (1931) estimated the number of skates and their average length each year and standardized effort by adjusting all gear to 6-line skates.

Effects of Hook-Spacing

Thompson et al (1931) considered the "relation of distance between hooks to catch" and compared the efficiency of dory gear (hooks spaced at 9-foot intervals) with that of longline gear (hooks at 13-foot intervals). To make this comparison, the authors tallied the number of skates and catch by dory and longline gear from statistical areas 24 through 30 during 1926. All of the longline gear had 6 lines per skate, whereas the dory gear had either 6 or 7 lines per skate. The average length of the dory gear was 6.54 lines per skate or 1.09 times that of longline gear. The catch per skate of dory gear was then adjusted (CPUE ÷ 1.09) so that both dory and longline gear were equivalent to 6-line skates. The results showed variations by month and area, but the average CPUE for the year was similar for the two types of gear. The comparison by Thompson et al (1931) is reproduced in Table 1. The authors concluded that the catch per skate . . .

Table 1. CPUE of dory and longline gear by month and area, 1926.*

(A)	Dory				Longline		
Month	Skate-sets	Pounds	Average Per Set	Corrected Average	Skate-sets	Pounds	Average Per Set
Feb.	2,013	348,200	173.0	158.7	6,412	1,012,945	158.0
Mar.	2,522	322,950	128.1	117.2	9,936	1,057,927	106.5
Apr.	3,540	329,800	93.2	85.5	12,446	1,241,600	100.0
May	3,882	319,100	82.2	75.4	9,846	888,275	90.2
June	5,288	553,600	104.7	96.1	8,333	867,384	104.1
July	2,819	339,000	120.3	110.4	7,341	689,135	93.9
Aug.	6,038	680,500	112.7	103.4	13,366	1,158,380	86.7
Sept.	3,894	358,600	92.1	84.5	11,695	1,079,380	92.3
Oct.	1,682	135,200	80.4	73.8	6,069	524,450	86.4
Nov.	119	14,700	123.5	113.3	1,283	122,400	95.4
Total	31,797	3,401,650	107.0	98.2	86,727	8,641,876	99.6
(B)	Dory				Longline		
Area	Skate-sets	Pounds	Average Per Set	Corrected Average	Skate-sets	Pounds	Average Per Set
24	1,831	176,800	96.6	88.6	7,326	638,609	87.2
25	4,953	564,650	114.0	104.6	20,107	2,037,403	101.3
26	10,908	1,276,500	117.0	107.3	18,525	2,048,940	110.6
27	2,438	278,900	114.4	105.0	11,000	996,025	90.5
28	4,551	350,500	77.0	70.0	11,522	995,700	86.4
29	5,501	582,800	105.9	97.2	11,896	1,268,585	106.6
30	1,615	171,500	106.2	97.4	6,351	657,034	103.5
Total	31,797	3,401,650	107.0	98.2	86,727	8,642,296	99.6

* From Thompson et al (1931).

". . . when corrected for the length of the skate, gave a catch of 98.2 pounds for a dory skate, as compared to 99.6 pounds for the long-line gear. *This shows the greater catch for the gear with the fewer hooks, the opposite of what might be expected.* But the slight difference shown is well within what would be expected through chance alone . . . It is difficult to make any analysis of why the catch does not differ more. It is certain, however, that if dory gear has an advantage in any place or season, due to the hooks being 9 instead of 13 feet apart, it is lost otherwise . . . To conclude, it is evident that the skate of halibut gear, if corrected for length, may be used as a unit of fishing effort without any consideration as to whether the hooks were 9 or 13 feet apart . . ." (My italics.)

Recent studies by Skud (1972) and by IPHC (1973) contradicted Thompson's conclusions and showed that catch per skate decreases as the interval between hooks increases (Figure 2). This difference prompted my reexamination of early log records and of the original comparison between dory gear and longline gear. First, I found that some of the dory gear had hooks spaced at 13-foot intervals, rather than 9-foot, and that some of the longline gear had hooks spaced at 9-foot intervals instead of 13. These data should have been excluded from Thompson's comparison. Second, I found that the ratio of the amount of 6- and 7-line dory gear varied considerably each month and that in certain months only 6-line gear was used. Furthermore, in 16 of the month-area combinations, only longline gear was fished, yet these data were used in the comparison with dory gear. Therefore, rather than correct the data by an annual average, adjustments of the CPUE of dory gear should have been made for each month and each area — and only when both dory and longline gear were in use. For example, in November all of the 119 dory skates were 6-line, yet Thompson et al (1931) corrected this figure by the annual ratio of 6- and 7-line gear, changing a catch per set of 123.5 pounds to 113.3 pounds (Table 1). Obviously, the annual correction distorted the monthly average. Thompson ignored his own admonition that comparisons be made at

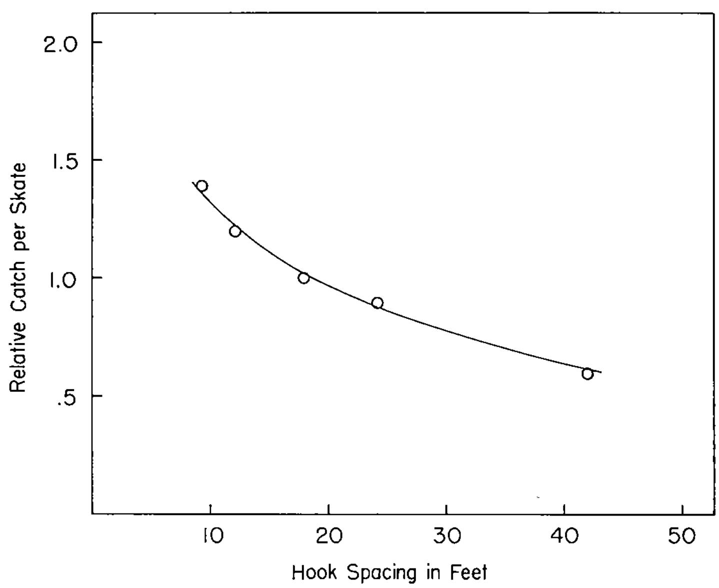

Figure 2. Relation of catch per skate to hook-spacing on longline gear.

the same time and place, his analyses included data that should have been omitted, and his conclusions concerning the "efficiency" of dory and longline gear were therefore incorrect.

Using the same basic data, I revised Thompson's analysis to correct for the above mentioned discrepancies, but, as noted later, the variation within months and within areas is so great that one cannot expect a high degree of reliability from this analysis. Instead of adjusting the CPUE of all dory gear by an annual correction, I tallied the actual number and catch of 6-line and 7-line skates of dory gear by month and by statistical area. I used only data that included all three types of gear in the same month and area, i.e., 6- and 7-line dory gear and longline gear. This approach reduced the number of skates for comparison but eliminated some of the errors in Thompson's original analysis. Rather than use the average length of the dory gear, I simply added the number of lines of 6- and 7-line gear and calculated the catch per line. Dory gear with hooks spaced at 13-foot intervals and longline gear spaced at 9-foot intervals were excluded. Exceptions by month and area occur as expected, but the averages for all months and areas show that the CPUE of dory gear with 9-foot hook-spacing was greater than the CPUE of longline gear with hooks spaced at 13-foot intervals (Table 2). I also compared (a) the catch per 6-line dory gear with longline gear, (b) the catch per 7-line dory gear with longline gear, and (c) the catch per line of 9-foot dory gear with 13-foot dory gear. In all of these comparisons, the average catch per line of 9-foot gear exceeded that of 13-foot gear. This difference between dory and longline gear was

Table 2. Revised CPUE of dory and longline gear by month and area, 1926.

	Dory Gear (9-foot spacing)					Longline Gear (13-foot spacing)		
	No. of Lines							
Month	6-line	7-line	Total Lines	Catch	Catch per Line	Number of Lines	Catch in Pounds	Catch per Line
Feb.	6,024	4,536	10,560	301,700	28.6	34,236	460,195	13.4
Mar.	7,668	5,523	13,191	288,000	21.8	58,908	1,050,227	17.8
Apr.	7,806	4,186	11,992	220,300	18.4	53,814	894,400	16.6
May	7,878	2,618	10,496	139,100	13.3	38,832	606,100	15.6
June	6,042	3,528	9,570	173,600	18.1	10,446	183,500	17.6
July	8,028	6,615	14,643	248,500	17.0	17,172	260,035	15.1
Aug.	4,572	9,576	14,148	188,000	13.3	26,970	429,750	15.9
Sept.	4,896	3,108	8,004	120,000	15.0	13,926	222,200	16.0
Oct.	618	1,470	2,088	18,500	8.9	8,472	130,433	15.4
Total	53,532	41,160	94,692	1,697,700	17.9	262,776	4,236,840	16.1
Area								
24	1,398	1,071	2,469	36,000	14.6	17,688	249,400	14.1
25	17,544	5,208	22,752	428,400	18.8	77,814	1,424,870	18.3
26	13,692	11,319	25,011	483,100	19.3	89,460	1,257,060	14.1
27	1,164	1,232	2,396	83,400	34.8	3,306	88,975	26.9
28	6,174	7,532	13,706	187,000	13.6	28,140	429,250	15.3
29	13,560	14,798	28,358	479,800	16.9	46,368	787,285	17.0
Total	53,532	41,160	94,692	1,697,700	17.9	262,776	4,236,840	16.1
Catch per 6-line Skate					107.4			96.6

in agreement with Skud's (1972) findings on hook-spacing that showed the catch per skate of wider-spaced gear (i.e., fewer hooks) was less productive.

To further test the relationship between dory and longline gear, I compared the CPUE data for 13-foot dory gear and 13-foot longline gear. The results, which are discussed in the next section, showed that 13-foot longline gear was more productive than 13-foot dory gear, indicating that differences other than hook-spacing must be considered in estimating CPUE.

As mentioned earlier, I repeated Thompson's analysis so the reader could readily follow the changes that I incorporated. Even though these comparisons show that the CPUE of dory gear is greater than that of longline gear, the results cannot be considered definitive and cannot alone be used to determine a correction factor for the two types of gear. Part of the problem relates directly to the limitations of the data. Except for the changes noted, my analysis used the same data as Thompson's and I assigned catch and effort data from the same vessels, designated as dory or longline, to the same areas and months. In subsequent rechecking of the original logs, however, questions arose as to the accuracy of these assignments and I found that the inclusion or exclusion of records from a single vessel could substantially alter the CPUE for a given month and area. Though the difference between dory and longline gear was usually maintained, the values of CPUE fluctuated considerably. Another problem was the variation in CPUE among vessels within a given month and area. The statistical areas are 60 miles wide and the CPUE of specific grounds differs with time. This variability was expected and was evident in the hook-spacing studies in which gear was set on the same day on the same grounds (Skud, 1972). Comparisons within a month and a 60-mile area must include a vast amount of data to provide reliable estimates of the differences in catch by gear.

Thompson's analysis did not adjust for the differences in catch between dory and longline gear, which he insisted was necessary in his comparison of 6- and 7-line dory gear, and his results were biased in favor of those months (or areas) with the greatest catch. For example, the highest catch by dory gear occurred in August and accounted for 20% of the total dory catch, whereas the catch by longline gear in August was only 13% of the longline total. The problem was compounded by the seasonal variation in CPUE that is discussed later in this report. Though weighting the CPUE by catch to obtain a representative measure for annual comparisons of CPUE is justified, the method can be questioned when the intent is to compare efficiencies of two types of gear. Because of the monthly differences in catch, one can argue that each observation (month-area) of catch and effort should be given equal weight in the calculation of mean CPUE. The two methods of calculation were compared, and, though the values of CPUE often showed substantial differences, the mean for dory gear was consistently higher.

Effects of Soak-Time

As explained in my earlier description of setline gear, dory gear usually is fished a shorter time than longline gear. This time from setting to retrieval of the gear is called the "soak" or "soak-time" and its effect on the estimation of CPUE must be assessed. Thompson et al (1931) made the following statement regarding soak-time:

> "There is, of course, a possibility that the unit of gear is allowed to lie on the bank for a longer or shorter time, thus varying what the fishermen call the 'soak'. This is a matter of individual judgement by the fisher-

> men. It is, and always has been, carefully adjusted to give maximum returns on the particular banks concerned. For this reason, the possibility is slight that the time of soak has ever been allowed to vary so greatly as to disturb our comparative results for any area. The error thus caused can be but very small relative to the great differences due to depletion, and no correction is necessary."

The assumption that the soak was adjusted to give maximum returns fails to distinguish between dory and longline gear. Thompson's conclusion about consistent soak-time may have been valid for dory fishing or for longline fishing, but as discussed below, was not valid when applied to both types of gear because the relative amount of effort for each gear was not constant. Dory gear was predominant before 1915 but was practically non-existent by 1930.

Many sources (Cobb, 1906; Marsh and Cobb, 1908; Alexander and Joyce, 1912; Bower and Fassett, 1914; etc.) as well as Thompson (1916), Thompson et al (1931), and Thompson and Freeman (1930) reported that dory gear was usually set twice a day. Hauling of the gear began within an hour or two after the gear was set and on occasion three sets were made in a day. The average soak per skate was probably no more than 4 hours. This schedule has also been confirmed in discussions with fishermen and others who fished dories or were familiar with fishing operations in the late 1920's and early 1930's (Arne Einmo, Harold Lokken, Mattias Madsen, Carl Serwold, and Dr. Richard Van Cleve).*

Because I was unable to locate a published account that described dory fishing in detail, I examined the early log records to obtain more precise information on soak-time and the number of sets. Most of the records only listed the location, date, number of skates, and catch. The log of the *Celestial Empire* was the only one that regularly included the time when dories were put overboard ("all-out") and retrieved ("all-in") and the general schedule conformed to that reported above. The *Celestial Empire* fished with as many as 12 dories in earlier years but in 1917 used 6, 7, or 8 dories. Each dory usually fished 3 skates of gear, though 2, 4, 5, and 6 skates were also fished. The dories usually fished twice each day, but at times only one set was made and on occasion three sets were made in a day. From April through June 1917, the vessel made six trips (two per month), and I calculated the elapsed time (all-out to all-in) for 90 dory sets during these months. The shortest period was 2 hours and 15 minutes, the longest 7 hours and 30 minutes. I compared the elapsed time with the number of skates fished and the averages were:

11 sets	2 skates	3.39 hours
56 sets	3 skates	4.25 hours
16 sets	4 skates	5.47 hours
6 sets	5 skates	6.67 hours
1 set	6 skates	6.75 hours

* My discussions with fishermen (Mattias Madsen and Carl Serwold) indicate that the longer soak of the longline gear was also accompanied by a change in bait. Apparently, dory fishermen mainly used herring as bait, whereas longline fishermen used cod and other species as well as herring, which deteriorates more rapidly. Though recent experiments by IPHC show that the catch does vary with the type of bait, no data were available from the early fishery to assess this difference. In my analyses, I have assumed that the advantage of longer-lasting bait is incorporated in the difference between the CPUE of dory and longline gear.

Part of the time was spent setting the gear and unloading the catch, so actual fishing time was less than indicated. The elapsed time and, therefore, the average soak per skate increased with the number of skates. (Other log records showed that elapsed time also increased with the number of dories.) Though the actual soak-time for each skate was different, if one assumes that the first skate soaked for 2 hours before being retrieved, the mid-point between 2 hours and the total elapsed time provides an estimate of the average soak per skate, i.e., a 4-hour average if the total time is 6 hours. Considering non-fishing time and the fact that most sets were completed in less than 6 hours, the average soak per skate would be less than 4 hours.

In contrast, longline gear was usually set before daylight (or even the previous night) and allowed to soak at least 4 to 6 hours before retrieval began. Since 30-50 skates were set, hauling usually continued well into the evening. Even though there was considerable variation in the fishing schedule by individual captains and on different fishing grounds, the average soak-time of longline gear was between 8 and 12 hours, two to three times longer than dory gear. The importance of soak-time is readily apparent in Figure 3. These data were collected by IPHC in the 1960's and were analyzed by Myhre (unpublished). The results were based on a large number of observations (100 to 300 per year) on different fishing grounds and, though variability was high, the increase in CPUE with soak-time was clearly evident. The curve may not be asymptotic for soaks longer than 15 hours, but these data were not pertinent to my comparison of dory and longline gear and were excluded from the figure.

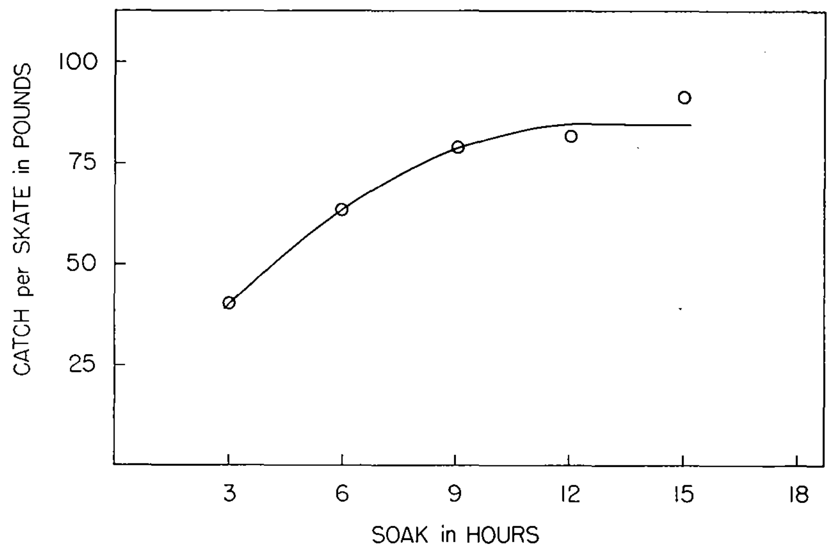

Figure 3. Relation of catch per skate to soak-time.

To determine whether the effect of soak-time on CPUE was apparent during the early years of the fishery, I compared the CPUE for 13-foot dory gear and 13-foot longline gear. These data were from the same months and areas used in Thompson's original analysis, but only 24 month-area combinations included both types of gear. The CPUE of 13-foot longline gear was higher than that of 13-foot dory gear and was indicative of an increased efficiency due to soak-time

(Table 3). From this evidence and the data on hook-spacing, one can conclude that: *when two types of gear had the same soak-time, the gear with the greater number of hooks (9-foot) produced the greater catch; whereas a longer soak-time was more productive when two types of gear had the same number of hooks.* When 9-foot dory gear (more hooks and shorter soak-time) and 13-foot longline gear (fewer hooks and longer soak-time) were compared, the effects, in part, were offsetting.

Thus, when longlining replaced dory fishing, two very significant changes occurred. First, the number of hooks per standard skate was reduced by 30% from approximately 200 to 138. Second, the soak-time was at least doubled. The increased soak-time partially compensated for the reduction in the number of hooks, and this fact apparently misled Thompson et al (1931) as to the importance of these changes and relative effectiveness of dory and longline gear.

Table 3. Catch per line of 13-foot dory and longline gear, Areas 24-30, 1926.

	13-foot Dory			13-foot Longline		
Area - Month	Lines	Catch in Pounds	CPUE	Lines	Catch in Pounds	CPUE
24 Mar.	462	5,000	10.8	8,508	119,400	14.0
Oct.	2,496	47,600	19.1	3,012	30,834	10.2
Nov.	714	14,700	20.6	1,698	20,800	12.2
25 Feb.	1,484	28,500	19.2	14,244	314,070	22.0
June	525	3,750	7.1	2,082	30,000	14.4
26 June	525	3,750	7.1	3,990	75,000	18.8
27 Mar.	644	12,300	19.1	708	7,700	10.9
June	168	1,000	6.0	6,330	125,500	19.8
28* Feb.	294	2,000	6.8	732	7,900	10.8
Mar.	126	1,000	7.9	5,100	85,300	16.7
May	1,848	14,000	7.6	2,982	35,700	12.0
June	609	5,000	8.2	9,516	147,400	15.5
Aug.	336	3,000	8.9	15,060	236,450	15.7
Sept.	648	10,000	15.4	14,844	218,900	14.7
29 Apr.	2,149	23,500	10.9	9,234	214,300	23.2
May	870	8,000	9.2	10,950	199,300	18.2
June	1,568	21,500	13.7	10,446	183,500	17.6
Aug.	762	13,000	17.1	11,910	193,300	16.2
Sept.	1,800	37,000	20.6	13,812	237,000	17.2
30 Apr.	168	1,500	8.9	4,278	53,600	12.5
May	24	1,000	41.7	4,032	82,100	20.4
June	3,078	38,000	12.3	13,500	241,384	17.9
July	2,028	32,000	15.8	11,196	203,050	18.1
Aug.	2,394	39,000	16.3	3,924	47,900	12.2
Total	25,720	366,100	14.23	182,088	3,110,388	17.08
Mean of 24 entries			13.76			15.88
6-line skate			82.56			95.28

* Data from April in Area 28 was omitted because this observation seriously biased the results; the catch by dory gear was 30% of the dory catch from all other observations (2,940 dory lines; 109,000 pounds).

SEASONAL DIFFERENCES IN AVAILABILITY

Thompson et al (1931) examined the seasonal differences in CPUE by comparing data from March to June, July to October, and November to February. Generally, they found that CPUE was highest during the winter when halibut were more densely concentrated just before and during spawning.* The authors recognized that the amount of fishing during the winter affected the annual estimate of CPUE, but their main interest was to show that stock abundance declined from 1915 to 1930 and that the decline was evident in each season from year to year as well as in the annual estimates. They did not correct the annual CPUE for the seasonal differences even though some years included fishing during December and January and others did not. Then, too, a closed season from November 15 to February 15 was established in 1925. This closure reduced the fishing time by 25% (from 365 to 273 days) but, more importantly, eliminated much of the period which regularly had the highest CPUE. No adjustments were made for these changes; therefore, the annual comparisons of CPUE had to show an inordinate decline from the early years when fishing was year-round to the later years when the fishing season excluded the months when CPUE was highest. The problem is compounded by the fact that different grounds were fished in later years and this shift also could affect estimates of CPUE.

Gulland (1955 and 1964), Garrod (1964), and Zilstra and Boerma (1964) addressed the question of seasonal changes in the catchability coefficient and stressed the importance of including all months of the year in the calculation of CPUE, or at least all months when a constant catchability could be expected. Generally, these authors recommend the period of maximum density of fish if year-round measurements are not available. They concluded that annual comparisons of CPUE from the same months would have less bias than those based on different months.

I reexamined the seasonal differences in CPUE reported by Thompson et al (1931) and calculated CPUE for April through July, months that were regularly fished from 1915 to 1940, and in which catchability was relatively constant. A comparison of this seasonal estimate with the annual CPUE (Areas 2 and 3 combined) shows that the greatest difference occurred in the early years of the fishery when more fishing was done during the winter and before the closed season was established (Table 4). I also calculated CPUE for April through July from 1915 to 1940 for the traditional study areas — grounds south (Area 2) and west (Area 3) of Cape Spencer. Though occasional months are missing from the data, this analysis did correct for the seasonal bias of the annual estimates of CPUE and reduced the estimates of abundance in the early years. I then incorporated the adjustments for hook-spacing and soak-time for dory and longline gear that were outlined in the previous section; the results are presented in the next section.

* Evidence that the seasonal difference in availability still exists was obtained during research cruises in 1973. Longline gear was fished near Cape St. James, British Columbia and off Yakutat, Alaska during November and December. The CPUE from this experimental fishing averaged well over 200 pounds and on several days was between 300 and 400 pounds. From May through September 1973, the CPUE of the commercial fleet was less than 60 pounds and any daily CPUE in excess of 100 pounds was considered "good fishing". Similar evidence of the high availability during the winter also was obtained from research cruises during the late 1950's.

Table 4. **Comparison of April-July CPUE and annual CPUE, Areas 2 and 3.***

Year	April - May - June - July			Annual CPUE
	Skates	Pounds	CPUE	
1915	17,721	2,681,364	151	183
1916	11,119	1,612,037	145	165
1917	15,565	1,674,601	108	121
1918	13,206	1,374,899	104	109
1919	20,552	2,017,002	98	113
1920	18,275	1,974,910	108	120
1921	22,441	2,188,873	98	106
1922	24,802	2,263,345	91	96
1923	31,482	2,672,232	85	99
1924	45,854	3,693,600	81	89
1925	59,559	4,254,955	71	80
1926	166,941	11,769,398	71	74
1927	116,272	7,736,547	67	71
1928	182,133	10,826,592	59	62

* Data from Thompson et al (1931).

REVISED ESTIMATES OF ABUNDANCE

I recalculated CPUE from 1915 to 1930 to determine whether the adjustments for differences in hook-spacing, soak-time, and seasonal abundance significantly alter Thompson's estimates. Estimates of abundance from the early statistics, especially before 1920, are of low reliability because relatively few log records were available and data on the number of lines per skate were often lacking. My adjustments of these data do not eliminate the shortcomings inherent in the original data but do account for the differences in effectiveness of dory and long-line gear.

When Thompson (1916) first examined the longline data, he concluded that "... these vessels employ different units of gear than do the dory vessels and their results are not comparable ... " and he only used dory gear to estimate abundance. I examined the possibility of calculating CPUE from only one type of gear, but the data were so meager and the results so erratic in the early years that I abandoned this approach, as did Thompson et al (1931 and 1934) in later papers. I used both dory and longline gear and applied correction factors based on data from the commercial fishery and from the results of field experiments conducted in more recent years. I tallied the data from the original logs and only used dory data that included the number of lines per skate (either 6, 7, or 8 lines). Whereas Thompson assumed that longline gear consisted of 6-line skates for the entire period (1915-1930), I only made this assumption for data after 1920. Because so little longline data were available before 1921, it was necessary to use data with an unknown number of lines and to estimate the average length of the longline gear before 1920. Only 16 logs of longline vessels were available through 1920 and only 6 of these included data on the number of lines; 2 of these had 8 lines per skate, 3 had 7 lines, and only 1 had 6 lines. Therefore, I assumed, unless the logs specified otherwise, that all of the longline gear had 7-line skates before 1920 and 6-line skates thereafter. Though some 7-line gear was used after 1920, its proportion declined rapidly and does not seriously bias the results in later years. The assumption was in agreement with Thompson's annual estimate of the mean number of lines for all gear (6.9 lines in 1919). Further, this decision was supported by the following comparison of CPUE for longline and dory gear. When the 7-line average was used for longline gear before 1921, the average CPUE of dory gear was greater than that of longline gear. This difference was consistent with the results from the recent experimental data on hook-spacing and with data from the fishery after 1920. If a 6-line average was assumed before 1921, the CPUE of longline gear greatly exceeded that of dory gear, indicating a serious bias.

All effort, dory and longline, was recorded as the number of lines and dory effort was converted to equivalent units of longline gear to estimate total effort. The CPUE was calculated on the basis of 6-line skates to maintain comparability with Thompson's original presentation. Catch and effort statistics were recorded by the major regulatory areas: Area 2, waters south of Cape Spencer, Alaska and Area 3, waters west of Cape Spencer. To reduce the effect of seasonal changes in availability, only data from April 1 through July were used. To account for differences due to hook-spacing and soak-time, two methods of adjusting the dory gear were compared. In the first method, I calculated effort in terms of hooks and converted dory gear to longline equivalents. Skud (1972) has shown that the catch per skate is not an accurate measure of CPUE unless the number of hooks per skate and the hook-spacing are standardized. Though the number of hooks per skate for a given hook-spacing is variable, I divided the skate length (1,800 feet) by the hook-spacing to determine the average number of hooks, i.e., 1,800/9 = 200 and 1,800/13 = 138. (In practice, the average was probably closer to 180 hooks per skate for 9-foot gear and 120 hooks for 13-foot gear, but the ratio of number of hooks is similar and did not materially affect the results.) I converted the catch per skate of Thompson's analysis (as revised in Table 2) to catch per hook: 107.4/200 = .537 for dory gear and 96.6/138 = .700 for longline gear. The ratio of dory to longline gear was .537/.700 = .767. This value was similar to that obtained in the hook-spacing studies (unpublished) with 9- and 13-foot longline gear (.385/.478 = .805). To err on the conservative side, and because of the limitations of the early data from the fishery, I rounded the correction factor to .80 and used the following equation to convert 9-foot dory gear to equivalent units of 13-foot longline gear.

$$\frac{\text{Dory skates }(200)\ (.80)}{138} = \text{Longline skates,}$$

Where 200 is the number of hooks per 6-line dory skate,

138 is the number of hooks per 6-line longline skate,

and .80 is the correction factor.

These equivalent units were added to the regular longline gear and this estimate of total effort was then divided into the catch to obtain CPUE (Table 5).

The second method for adjusting dory gear was more direct and used the 6-line skate as the basic unit of effort, and the ratio of longline CPUE to dory CPUE was used as the correction factor. In the reexamination of the 1926 data (Table 2), this ratio was .90. In the commercial fishery from 1920 to 1930, the ratio averaged .85 for Areas 2 and 3 combined. In the recent hook-spacing studies, the CPUE ratio of 13-foot gear to 9-foot gear was .83, .87, .89, and .91 on four different cruises. On the basis of all these results, I accepted .90 as a conservative estimate of the correction factor, and all dory gear was adjusted accordingly. The adjusted dory skates were added to the longline effort, and CPUE was then

Table 5. Revised estimates of CPUE for dory and longline gear based on the hook correction.

Year	Adjusted Dory Skates	Longline Skates	Total Skates	Catch in Pounds	CPUE
			AREA 2		
1915	22,918	—	22,918	2,160,800	94.3
1916	14,067	805	14,872	1,403,067	94.3
1917	13,565	1,376	14,941	970,630	65.0
1918	2,732	3,749	6,481	404,240	62.4
1919	3,351	4,544	7,895	508,100	64.4
1920	2,837	5,342	8,179	515,600	63.0
1921	1,648	8,472	10,120	631,275	62.4
1922	2,032	11,127	13,159	717,950	54.6
1923	2,722	17,721	20,443	1,020,590	49.9
1924	4,310	18,837	23,147	1,172,380	50.6
1925	755	24,383	25,138	1,118,875	44.5
1926	7,485	91,121	98,606	5,018,461	50.9
1927	9,641	67,226	76,867	3,633,118	47.3
1928	11,306	86,007	97,313	4,514,777	46.4
1929	14,661	149,467	164,128	6,125,131	37.3
1930	7,893	214,195	222,088	7,293,276	32.8
			AREA 3		
1915	4,431	5,740	10,171	1,680,000	165.2
1916	867	7,109	7,976	1,097,500	137.6
1917	2,302	4,356	6,658	740,000	111.1
1918	6,485	5,657	12,142	1,134,300	93.4
1919	10,333	6,140	16,473	1,522,300	92.4
1920	3,663	6,305	9,968	1,151,000	115.5
1921	7,353	1,517	8,870	879,600	99.2
1922	9,447	2,530	11,977	1,321,100	110.3
1923	11,464	3,728	15,192	1,600,323	105.3
1924	12,783	12,142	24,925	2,282,700	91.6
1925	20,848	18,982	39,830	3,031,800	76.1
1926	27,277	65,236	92,513	8,045,975	87.0
1927	8,339	63,874	72,213	5,778,200	80.0
1928	9,010	92,176	101,186	6,875,297	67.9
1929	7,831	133,178	141,009	9,064,300	64.3
1930	11,255	165,063	176,318	10,358,800	58.8

calculated. The results differed by less than 5% from the estimate of CPUE made by the hook correction (Method 1) but were all higher; for example, in 1915 the CPUE was 167.3 compared to 164.1. Because of the limitations of the early statistics, these differences were not considered significant.

As the estimates of the two correction factors are not greatly different, I have used the results from the hook correction (Method 1) to compare with those of Thompson (Figure 4). The correction for gear and for seasonal availability had the same effect, i.e., both increased the estimate of total effort and decreased the estimate of CPUE. The revised estimates did not alter Thompson's conclusion of a stock decline but did show that the abundance (CPUE) was overestimated, particularly in the early years when dory gear and winter fishing predominated. The overall decline, 1915-1930, was less precipitous than Thompson surmised. His estimates showed an 83-pound reduction in Area 2 and 201 pounds in Area 3; whereas in the revised estimates, the reduction was 62 and 106 pounds, respectively. Based on the logarithmic transformation of CPUE in Area 3, the slope of the regression of CPUE on time for the revised data was 38% less than Thompson's estimates (—.05 vs. —.08). As shown in the next section, these differences in the estimates of CPUE are of particular importance to Burkenroad's (1948) arguments regarding estimates of stock size.

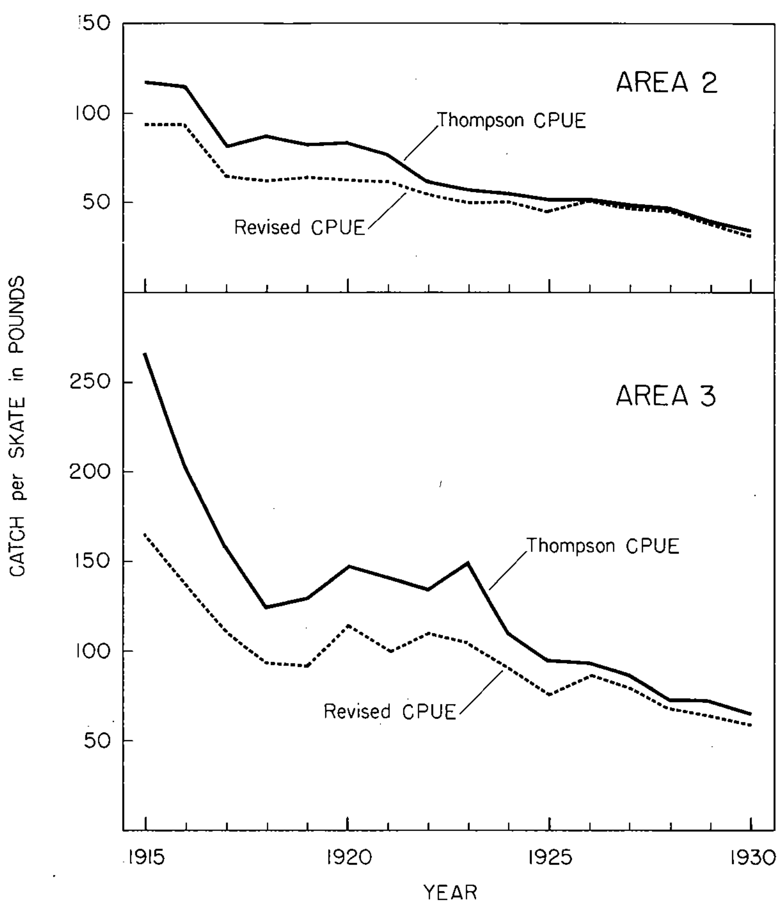

Figure 4. Comparison of Thompson's CPUE with the revised estimates, Areas 2 and 3.

To calculate total effort, I used the catch data as adjusted by Bell, Dunlop, and Freeman (1952). Both the revised CPUE and Thompson's CPUE were divided into the catch to compare the two estimates of fishing effort. The results showed that Thompson's effort was consistently low, that the greatest percentage difference occurred in the early years, and that the difference was greater in Area 3 than in Area 2 (Table 6). The significance of the higher estimates of fishing effort are apparent in the next section.

Table 6. Comparison of revised CPUE and effort with Thompson's estimates.

Year	Catch in Millions of Pounds	CPUE in Pounds per Skate		Effort in 000's of Skates	
		Revised	Thompson	Revised	Thompson
AREA 2					
1915	44.0	94.3	118.0	467	373
1916	30.3	94.3	114.6	321	264
1917	30.8	65.0	81.8	474	377
1918	26.3	62.4	87.5	421	301
1919	26.6	64.4	82.3	413	323
1920	32.4	63.0	84.1	514	385
1921	36.6	62.4	76.9	587	476
1922	30.5	54.6	62.6	559	487
1923	28.0	49.9	57.2	561	490
1924	26.2	50.6	55.8	516	468
1925	22.6	44.5	51.8	508	436
1926	24.7	50.9	52.2	485	473
1927	22.9	47.3	49.4	484	464
1928	25.4	46.4	47.8	547	531
1929	24.6	37.3	40.2	660	612
1930	21.4	32.8	35.1	652	610
AREA 3					
1915	24.5	165.2	266.1	148	92
1916	19.5	137.6	202.8	142	96
1917	17.8	111.1	157.9	160	113
1918	11.4	93.4	125.4	122	91
1919	13.5	92.4	129.9	146	104
1920	14.3	115.5	147.9	124	97
1921	15.5	99.2	141.4	156	110
1922	11.7	110.3	134.8	106	87
1923	22.2	105.3	150.3	211	148
1924	26.3	91.6	109.7	287	240
1925	26.8	76.1	95.2	352	282
1926	26.9	87.0	94.1	309	286
1927	30.8	80.0	86.9	385	354
1928	27.8	67.9	72.8	409	382
1929	31.1	64.3	72.6	484	428
1930	27.3	58.8	64.7	464	422

THE THOMPSON-BURKENROAD DEBATE

Early critics of Thompson's work were concerned mainly about methodology and the theoretical aspects of his findings. Michael Graham (1935) apparently was the first to comment — "... If one should criticize a paper which so notably advances the theory of fishing statistics, one would be inclined to say, that despite the agreement between theory and experience cited, the theory as given is still too far removed from reality to be very practical". Graham also criticized Thompson's statement regarding yield and the relationship between growth and natural mortality. Ricker (1948) recognized the pioneering effort of Thompson and his colleagues but questioned the assumption of uniform recruitment and called attention to systematic errors in the differences in the estimates of survival from age-distribution data and tag recoveries. Holt (1951) wrote about the correlation between catch and effort and about Thompson's "normal catch per set" and concluded that "... In fact the number of sets and the catch per set have not been estimated independently and the high value of the correlation coefficients that were found have no special significance; they do not demonstrate that fishing has played the *dominant* part in determining the size of the stocks". Beverton and Holt (1951) also cited shortcomings in Thompson's analyses, but it is unnecessary to review the extensive detail of their comments in this paper. Ricker (1958) noted the similarities between Thompson's (1950) concept of "normal catch" and Schaefer's (1954) "equilibrium catch" but considered the differences between the two approaches as being more important:

> "... whereas Thompson is impressed by the apparent constancy of the 'normal' catch over the indicated intervals of time, Schaefer joins with Baranov, Hjort and Graham in emphasizing that equilibrium catch must change as size of stock changes. Actually, of course, the normal levels used by Thompson did not remain normal in later years, and in addition some of the population statistics computed from them are seriously at odds with those derived from age structure of the population or from the results of tagging experiments."

Although Burkenroad (1948, 1950, and 1951) also commented on the theoretical aspects of Thompson's "normal catch" and the relation between catch and effort, he delved more deeply into the statistics and was concerned with specific aspects such as the magnitude of changes in abundance, mortality estimates, growth, and migration. Burkenroad disputed Thompson's interpretation of the data and claimed that fishing could not explain the early decline and that the restrictions on fishing could not have improved stock conditions so soon. Thompson (1950 and 1952) reiterated his earlier arguments and did not revise the original estimates of abundance. Other than a footnote to Burkenroad, Thompson did not specifically address the comments of his critics.

Accuracy of Statistics

Though Burkenroad (1948) questioned the reliability of Thompson's catch and effort data, he accepted and used the CPUE statistics to show that the catch in Area 3 "would have had to be 600 percent more than those reported, to match the decline in population". Burkenroad considered that several, relatively small errors in the catch and effort data could have such a large cumulative effect, but he dismissed this possibility as unlikely. (As shown in the previous section, both the adjustments for gear efficiency and seasonal availability did decrease the estimate of CPUE.)

Thompson's attitude about the statistics before 1925 changed over the years. In publications prior to 1950, he relied heavily on the data from 1913 to 1925 to support his conclusions of declining stock abundance. After Burkenroad (1948) voiced his disagreement, Thompson (1950) emphasized the limitations of the early data:

> "The logs collected for the earlier years were those kept and preserved without urging or compulsion. Of course they were relatively accurate, but they were necessarily those belonging to the more literate and intelligent of the fishermen. They were, thus, those for the better and more efficient boats, frequently so-called company boats. They covered a much smaller part of the total fleet activities than the records gathered in recent years and tended to describe fishing on the banks accessible mainly to large boats. *It is my opinion, from personal experience, that such records showed a higher catch per set than the present comprehensive method of collecting would have shown.* The catch per unit, as shown by these logs, was divided into the presumed total take from the grounds or each section of the grounds, to give as an indirect calculation the number of units fished. *Hence, if the total landed from the grounds was at all accurate, the amount of fishing—that is, the number of units of gear to take this total—must have been considerably understated. . . .* For these reasons, I have used only the period from 1925 on in this analysis." (My italics.)

When Thompson (1952) presented another review of his thesis, the explicit limitations of data prior to 1925 were not mentioned, and these earlier records were presented to support his arguments. He stated that recent data ". . . confirm the interpretation of the earlier history of the catch per set which has been advanced by the International Fisheries Commission and the writer".

Burkenroad (1948) pointed out a 20% difference in the 1928 catch and effort data presented by Thompson and Herrington (1930) and by Thompson and Bell (1934) and questioned the accuracy of these data. Evidence of the problem with the statistics was also discussed by Bell et al (1952), who revised the original catch data. I have examined the log records before 1925 and agree with Thompson's (1950) statement that effort may have been considerably underestimated. The number of logs available was small and probably not representative of the entire fishery. Beyond that, the differences between dory and longline gear (number of hooks and soak-time) and the methods of fishing were major problems. This variability was not emphasized in previous descriptions of the fishery. Though Thompson and his colleagues did a remarkable job in gathering statistics in these early years and deserve special commendation for their appreciation of accurate statistics, the data must be adjusted for differences in gear efficiency and, though useful in determining long-term trends, should not be used to interpret changes from one year to the next.

Decrease in Abundance Before 1930

The criteria required to use CPUE as an index of abundance have been discussed by many authors; for example, Ricker (1940 and 1958), Widrig (1954), Gulland (1955), and Beverton and Holt (1957). In the halibut fishery, CPUE has been used as a measure of abundance. However, as in other fisheries, few of the established criteria have been met. The amount of fishing effort, the effectiveness of the gear, and the length of the fishing seasons have been altered considerably. Thompson et al (1931) appreciated the need for standardization of effort and examined their records to determine whether the data conformed to these criteria.

However, the authors concluded that the observed deviations were of little consequence and analyzed the data without correcting for seasonal changes or for differences in hook-spacing and soak-time. Thompson (1950 and 1952) was emphatic about his results ". . . There can be absolutely no question as to which variable in the reciprocal relationship is the independent one — it is the number of sets of fishing gear". He stressed "the reciprocal relation between effort and CPUE" and explained "the fluctuations above the normal yield as over and under withdrawals from the accumulated stock". In this regard, both Burkenroad (1950) and Holt (1951) took Thompson to task concerning the high correlation of effort and CPUE which had not been determined independently.

Burkenroad (1948) compared the relative change in CPUE from 1915 to 1927 with the actual withdrawals from the fishery in Area 3. He used tagging data from Thompson and Herrington (1930) to estimate a stock size in 1927 of 300 million pounds (30 million pound catch and an estimated 10% fishing mortality). Thompson's CPUE in 1927 (87 pounds) was one-third of that in 1915 (266 pounds) so Burkenroad estimated the stock size in 1915 at 900 million pounds (Figure 5). The total catch during this period was 284 million pounds, only 47% of the 600 million pound decline in stock size. Certainly, this analysis was an oversimplification of the complex interactions of fishing and natural

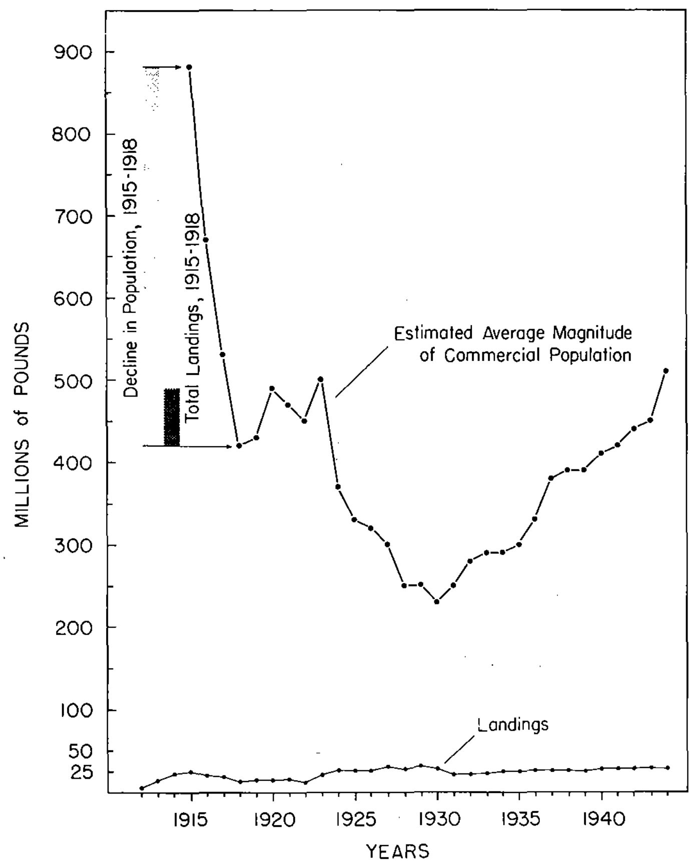

Figure 5. Estimated average commercial population of halibut on grounds west of Cape Spencer, 1915-1944; compared with landings; with histogram contrasting decline of population 1915-1918 with total landings during the same period (after Burkenroad, 1948).

mortality, emigration, immigration, growth, and recruitment. Burkenroad was not unaware of these complexities, however, and he concluded that the disparity, especially from 1915 to 1918, was far greater than might be explained by fishing mortality as Thompson professed. (Similarly, doubts can be raised about Burkenroad's explanation of an environmentally induced change; only a complete change in availability or a series of catastrophic events affecting recruitment could have reduced the population so severely.)

Thompson's (1950 and 1952) rebuttal was based on his estimate of CPUE (which I have shown exaggerated the rate of decline), and he claimed that the tag recovery rate was not representative of total stock in Area 3 but gave no data to support this conclusion. IPHC (1960) discounted the low estimates of fishing mortality based on tagging experiments in Area 3 and accepted estimates from age composition data and the virtual population method that were between .20 and .25. Chapman, Myhre, and Southward (1962) selected an intermediate value and estimated that 240,000 skates generated a fishing mortality of .15 during the 1950's; effort in the late 1920's was well over 350,000 skates and, based on the intermediate estimate, would have generated a fishing mortality greater than .20. Recent studies confirm that errors inherent in the tagging experiments (i.e., loss of tags, non-reporting, etc.) cause an underestimation of fishing mortality. If the fishing mortality was .20, twice the value reported by Thompson and Herrington (1930), Burkenroad's estimate of stock size would have been 150 million pounds in 1927 and 450 million in 1915. Hence, instead of a 600 million pound decline (900—300) from 1915 to 1927, the decline would have been 300 million (450—150) of which the catch would account for 95%. Burkenroad still could have questioned the 300 million decline which would not have accounted for growth and recruitment. My revised estimates showed a 50% reduction of CPUE (165 pounds in 1915 and 80 pounds in 1927) instead of the 67% reduction in Thompson's data (266 in 1915 to 87 in 1927). If my estimates of CPUE are used in Burkenroad's analysis along with a .20 fishing mortality, the total population would have been 300 million in 1915 and 150 million in 1927, a difference of 150 million pounds. This difference is 134 million pounds less than the total catch (284 million), suggesting that the reduction of the stock by fishing was greater than the replacement due to growth and recruitment less natural mortality. Applying a similar adjustment of CPUE and mortality to the 1915-1918 period also provided more credible estimates, but far less of the decline was accounted for by the catch.

Though I question the comparability of CPUE over a period of years, the crux of the above analysis is the estimate of fishing mortality. Examination of tagging data (Myhre, 1967) clearly shows that estimates of fishing mortality in Area 3 always have been low relative to those in Area 2. In the 19 experiments from Area 3 (1926-1955), the highest estimate of fishing mortality was .124 and the mean was .066; whereas in Area 2 during the same period, the highest estimate in 41 experiments was .647 and the mean was .250. This difference between areas has never been satisfactorily explained. Chapman, Myhre, and Southward (1962) noted the difference in the potential yield curves of the two areas, and Myhre (1967) concluded "... Since other analysis have indicated that the halibut of both areas are being fished at or near their maximum sustained yield level, it is concluded that either the fishing mortality estimates lack comparability or the halibut of the two areas have different levels of productivity or both".

In part, the rejection of the Area 3 tagging estimates of fishing mortality may have been influenced by the higher rates in Area 2 and the similarity of trends in the catch and CPUE. However, these similarities were misleading because far fewer but larger fish were taken per skate in Area 3 than in Area 2. Myhre (1967) showed that the square miles of fishing ground in Area 3 are more than double that in Area 2. Because fishing mortality is a function of catchability and fishing effort per unit area ($F = cf/A$), the lower estimates of fishing mortality in Area 3 may indeed be representative of differences in gear density, population density, or of fish distribution. Further, I question the adequacy of the analysis and the data which was available for Burkenroad's comparison of abundance between 1915-1927 — even though it was sufficient to reveal the fallacy of Thompson's CPUE estimates. Until these questions are resolved, the causes of the early decline in stock abundance cannot be precisely assigned to either the effects of fishing or to natural changes. *Nevertheless, Burkenroad's initial objections — on the basis of Thompson's CPUE data — were justified.*

In Area 2 (south of Cape Spencer), the questions regarding fishing mortality were reversed. Burkenroad argued that the 40% estimate of fishing mortality (Thompson and Herrington, 1931) was applicable only to those areas where tagging was conducted and not to Area 2 as a whole. Though Bell and Pruter (1958) claimed that concentrations of fishing effort distorted overall estimates of CPUE in Area 3, they denied this premise for Area 2. The authors multiplied the catch per skate for each statistical section in Area 2 by the area (square miles) within the 100-fathom contour of the section to obtain an estimate of the "probable size of stock" (Table 7). They concluded that statistical sections 10, 11, 13, and 15 (tagging areas) accounted for about 50% of the fishable stock in Area 2 and produced a similar percentage of the Area 2 catch. This correspondence of yield and their estimate of stock size was considered evidence to refute Burkenroad's (1951) argument that the estimate of fishing mortality (40%) from tagging may have been twice the level of mortality (20%) for Area 2 as a whole. If Bell and Pruter's analysis is carried further, however, the results actually support Burkenroad's contention. Their data show a *relative* fishing mortality (B/D in Table 7), of .023 in sections 10, 11, 13, and 15 and only .012 in the rest of Area 2, and .017 for Area 2 as a whole. Converting these relative values to equivalence with the 40% estimate from tagging studies ($.023/.40 = .017/X$), fishing mortality for all of Area 2 would be 30%. Contrary to Bell and Pruter's conclusions, the difference, though not as great as Burkenroad surmised, supports his contention that Thompson and Herrington's (1931) estimate of the fishing rate for Area 2 was too high.

Myhre (1967) analyzed the results of 41 tagging experiments in Area 2 (from 1925 to 1955) and reached the same conclusion as Burkenroad ". . . Since for most Area 2 experiments tagging and fishing tended to be concentrated on grounds where halibut are concentrated, the resulting estimates of catchability may be too high for Area 2 halibut in general". Further, Ricker (1948) and Beverton and Holt (1951) questioned Thompson and Herrington's (1930) estimates of fishing mortality because of errors in the method of back-extrapolation. Obviously, considerable care should be exercised in the application of estimates of fishing mortality from Area 2. Burkenroad (1951 and 1953) recognized the problem, but he did not contend that an adjusted estimate of mortality in Area 2 would prove his hypothesis that changes independent of the fishery had affected abundance. He thought his findings provided encouragement, not serious support, to this end.

Table 7. Approximate area of grounds within the 100-fathom line in each of 13 60-mile halibut statistical sections (Numbers 5-17 inclusive), and the catch and catch per skate in each for 1928 and 1929.*

A: Approximate determination of the area within the 100-fathom line from U.S. Coast and Geodetic Survey charts numbers 7002 and 8002.

B and C: Data from Table 5, page 51, Thompson and Herrington (1930).

D: Product of the catch per skate and area within the 100-fathom line.

E and F: Data from International Pacific Halibut Commission files.

		1928			1929		
	A	B	C	D	E	F	G
Statistical Section No.	Area	Catch	Catch per skate	Product A x C	Catch	Catch per skate	Product A x F
	sq. mi.	10^3 lb.	lb.		10^3 lb.	lb.	
5	1,380	242	43.1	59,487	298	26.1	36,018
6	1,980	605	34.3	67,914	722	28.5	56,430
7	1,800	312	34.3	61,740	335	31.3	56,340
8	660	390	34.3	22,638	510	31.0	20,460
9	1,800	551	42.9	77,220	980	30.8	55,440
10	2,630	3,060	43.3	113,879	2,736	36.7	96,521
11	2,970	2,345	51.7	153,549	2,775	41.8	124,146
12	3,330	2,030	44.5	148,185	1,476	36.5	121,545
13	3,240	5,188	51.1	165,564	4,748	38.8	125,712
14	460	785	46.8	21,528	1,655	41.1	18,906
15	1,350	919	53.1	71,685	2,502	47.1	63,585
16	1,210	1,283	54.0	65,340	3,266	44.0	53,240
17	910	586	52.0	47,320	1,182	39.8	36,218
Total of Sections 10, 11, 13, and 15	10,190	11,512	—	504,677	12,761	—	409,964
Total, all Sections	23,720	18,296	—	1,076,040	23,185	—	864,561
Sections 10, 11, 13, 15, as % of Total	43%	63%	—	47%	55%	—	47%

* After Bell and Pruter (1958).

Abundance Increase After 1930

Because Burkenroad also disagreed with Thompson's explanation about the increase in abundance after 1930, it is fitting to discuss the course of the fishery after that date. The full evaluation of the recovery through the 1940's merits a separate paper, but it may be useful to present my views that are based on the revisions of CPUE for earlier years and on general knowledge of the fishery throughout its existence.

As pointed out by Thompson and Bell (1934), the 1924 treaty allowed for a 3-month closed season, but this was ineffective in reducing total effort which increased steadily to 1930. When the treaty was revised, new regulations were promulgated in 1932 which included closed nursery areas and catch limitations (quotas). In 1934, the Commission heralded the success of its management program: ". . . These scientific results show the basis for the remarkable success of regulation to date, whereby the abundance of the stock has increased greatly" (Thompson and Bell, 1934). This initial pronouncement of success was based on the increase in CPUE and the decrease in effort from 1930 to 1933. In Area 2, CPUE increased 50% — from 35.1 pounds to 52.1 pounds. The change in Area 3 was 30% — from 64.7 pounds to 84.0 pounds. During the same period (1930-1933), fishing effort in Area 2 declined from 645,000 to 453,000 skates (30%) and in Area 3 from 410,000 to 267,000 skates (35%).

Though fishing effort was reduced between 1931 and 1932 when the regulations became effective, as great a reduction (or more) was experienced in 1931 before the regulations were introduced. The change is shown in the following table (using Thompson's original estimates of effort in thousands of skates):

	1930	1931	1932	1933
Area 2	645	548	457	453
Area 3	410	291	252	267

There was no appreciable change in effort in 1933. Hence, 50% of the reduction in Area 2 and 75% of the effort reduction in Area 3 occurred before the new regulations were effective. The catch was not appreciably reduced during this period, even though a quota was imposed. Burkenroad's (1950) supposition that the decline in effort at this time was caused by unfavorable economic conditions was confirmed by Bell et al (1952), in several newspaper articles, and by personal communication with Harold Lokken (Manager, Seattle Fishing Vessel Owners Association). Beyond that, I question whether the increase in CPUE was an accurate measure of abundance when effort changed 30% to 35%. Rounsefell and Everhart (1953) also discussed this change in effort and stressed the importance of gear competition. Finally and more important, so great a change in abundance could not be realized in 1 year as a result of reduced fishing effort because recruits are at least 4 years of age and the mean age of the catch is 8 years or more depending on the particular fishing grounds. Similarly, the increased "production of spawn" which was also credited to regulation (Thompson and Bell, 1934) could not be measured and would not affect recruitment for at least 4 years after the new regulations were introduced.

CPUE also increased in the late 1930's and the continued ascription of this increase to the management program led Burkenroad (1948) to question the Commission's interpretation of the changes in Area 3:

> "... although the average annual catch since 1932 (26 million lb., or 7% of the estimated average stock) has been lower than that during the 5 years 1927-1931 (27 million lb., or 9% of the average stock), it has been much greater than the average from 1915 through 1926 (18 million lb., or 4% of average stock). Thus, a stock of estimated average magnitude of only 375 million pounds in 1932-1944, subject to a fishing mortality averaging 7 per cent per year, nearly doubled during the period. In contrast, a stock averaging 488 million pounds in 1915-1926, and then subject to a fishing mortality averaging only 4 per cent per year, declined to one-third during the period."
>
> "*Comparison of Expected with Observed changes in Abundance.* It seems fairly clear from the foregoing various evidences that changes in fishing effort have not been proven sufficient to explain the recent increase in abundance of halibut on the western banks. Therefore, it is

possible that the correspondence shown by Thompson and Bell (1934-46: Fig. 17) between the changes in actual catch-per-unit-of-effort and those calculated from changes in fishing-effort (on the basis of an arbitrarily selected constant annual increment of young and assumed constant rates of natural mortality and of growth during the limited period 1920-1929) might represent a special result unobtainable (at the same constants) not only for the period of rapid decline before 1920 but for the period of increase since 1930 as well. That this is in fact the case is demonstrated in Fig. 5, which illustrates the results of calculations of expected catch-per-unit-of-effort for the period 1930-1944, using the same method and the same constants as were employed by Thompson and Bell (1934) for the period 1920-1929. . . . It will be seen that, on the basis of calculated stock in 1929 and observed extent of fishing-effort thereafter, the catch-per-unit-of-effort since 1930 should have risen only slightly from its dead low level in 1930. The actual course of events has been a rapid rise to a level greater than any since 1917."

Natural Fluctuations in Abundance

When Burkenroad (1948) concluded that Thompson et al's (1931) explanation for the changes in abundance in Area 3 could not account for the magnitude of the decline in the early years of the fishery, he proposed that the changes were environmentally induced. Burkenroad (1951) correlated the CPUE of Area 2 with that of Area 3 ($r = +0.88$), concluded that CPUE in both areas was governed by the same factor, and hypothesized that fluctuations were controlled by natural causes. (My revised estimates improve the correlation.) Burkenroad obviously accepted Thompson and Herrington's (1930) conclusion that the stocks in the two areas were separate and distinct. However, another explanation could account for the high correlation, i.e., that the stocks are not separate and that the effects of fishing in one area are also of consequence in the other. Results of tagging experiments conducted after 1930 (Myhre, 1967) and the decline in abundance since 1960 raised questions about the relationship of the stocks, but additional information is needed to resolve this matter.

Thompson's (1950 and 1952) arguments about the role of natural factors changed with time and he recognized the difficulty in identifying natural versus fishery induced changes. Whereas his initial thesis expressly ignored any role other than fishing mortality, he later considered a change in productivity — possibly due to environmental change — as an explanation for an increase in abundance during the late 1940's that did not correspond with the expected level of stock abundance. He did not, however, concede that fluctuations in abundance prior to 1940 could have been due to changes in productivity as was argued by Burkenroad (1948).

Bell and Pruter (1958) presented an extensive rebuttal to Burkenroad's (1948) and Ketchen's (1956) arguments and discounted the role of environmental factors in the fluctuations of halibut abundance, although other evidence (IPHC, 1948 and 1952; and Bell, 1970) show substantial variations in the strength of year classes. Bell and Pruter reviewed numerous studies that dealt with this environmental relationship and included a variety of species, pelagic and demersal, from North America and Europe. This compilation is a valuable reference, and one cannot argue with the author's admonition that extreme care be exercised in the selection and evaluation of data used to establish relations between environmental changes and stock productivity. However, the conclusions must be considered in the context of the author's introductory explanation ". . . It is inevitable that the reviews will be critical as we believe that many authors contributing to this subject have not closely examined the adequacy of presently available fishery and hydrographic or meteorological data". With this statement in their introduction and their pronounced conviction that natural fluctuations were of secondary importance in the halibut fishery, the conclusion was not unexpected ". . . The hypothesis that fishing, not natural forces, has been the major factor in affecting the stocks appears well founded". Bell and Pruter severely criticized the environmental and CPUE data of other authors and discounted apparent relations as fortuitous, whereas none of the imperfections of CPUE data for the halibut fishery were discussed. The reader was assured that ". . . the obvious changes in the efficiency of the unit of effort have been provided for". Dickie (1973) criticized the general conclusions of Bell and Pruter as being "deceptively simple" and argued that "improvements in the data appear to verify the reality of the relations which they questioned".

My revised estimates of CPUE show that the population change was not as great as Burkenroad (1948) had been led to believe, however, the role of natural factors, which he stressed, cannot be summarily rejected. Some of the problems in detecting environmentally induced changes in abundance of halibut merit attention. Year class strength has fluctuated significantly, yet the catch of recruits or "chicken halibut" (under 10 pounds) has declined steadily since 1930. One of the basic problems in assessing the effect of environmental conditions on year class strength was the fact that estimates of CPUE were based on weight and not numbers of fish, the importance of which was discussed by Murphy (1960). This difference was critical not only because the mean size of fish in Areas 2 and 3 was different but because the growth rate of halibut has increased continuously and was apparent as early as 1930 in areas where enough data were available. Burkenroad (1948), Fukuda (1962), Southward (1967), and Bell (1970) noted the importance of this change in growth in the assessment of recruitment, i.e., the rate of growth determines the age at which young halibut enter the fishery and the length of time they are in the chicken category. Furthermore, Ketchen (unpublished) has shown a close correlation between the age of entry and the price differential of chicken and larger halibut. Hence, the CPUE (by weight) or the age of entry of chicken halibut may not be reliable measures of recruitment. Similarly, the CPUE (by weight) of older fish can register an increase when the growth rate increases, whereas the number of fish may not change or may decrease. *Indeed, this phenomenon may explain much of the apparent improvement of stock condition from 1930 to 1940 and beyond.* Estimates of growth may also be affected by the change in hook-spacing and must be reexamined (Skud, 1972 and 1973). One cannot properly credit the change in abundance until questions about availability, growth, and recruitment are resolved.

Though Thompson was adamant about his analysis and interpretation of the data, his publications also include words of caution and repeated emphasis that the fishery is constantly changing and that all parameters must be monitored and reevaluated. The recent studies on hook-spacing support Thompson's encouragement for continued reevaluation; many facets of the fishery have yet to be fully understood. Differences in fishing techniques, types of bait and gear competition, the relation among stocks, growth rate, or environmental factors have not been adequately evaluated and must be studied to assess stock conditions properly. In short, the state of knowledge of the halibut fishery supports Dickie's (1973) criticism of the polarization of the scientific community into two camps and is consistent with his arguments that "the management system must necessarily consider environmental as well as fishery variables". Burkenroad (1953) voiced similar arguments, 20 years earlier, in his discussion of the difficulties in identifying the causes of change in fishery stocks and the need for an ecosystem approach to properly assess these changes.

EPILOGUE

Initially, I intended to incorporate the revised estimates of effort and CPUE into earlier analyses and to provide a reevaluation of the fluctuations in the abundance of halibut. I soon realized, however, that the revised estimates alone would not resolve the basic question. Throughout the paper, I have attempted to flag aspects that need to be clarified before a satisfactory resolution can be attained. The enigma of differences between Area 2 and Area 3 may well hold the key to solving many of the problems. I am confident that the differences in growth, mean age, recruitment, mortality, migration, etc. between areas can be rationally explained through an extensive reanalysis of existing data. The staff of IPHC has begun this reassessment, and results will be reported in other papers.

In retrospect, the repeated descriptions of the halibut fishery as a simple, easily defined and controlled entity (Thompson, 1950; Bell, 1970) were unfortunate. These circumstances, coupled with the attitude that the original analyses were beyond reproach, contributed to a complacency that may have discouraged or even repressed research that should have been undertaken. Criticism by other scientists, specifically Burkenroad (1948 and 1951), was considered a questioning of IPHC's integrity, whereas it should have stimulated a reexamination of such basic parameters as CPUE and growth. Though Beverton and Holt (1957) were unaware of the problems discussed in my paper, they recognized Burkenroad's contribution as well as the limitations of his conclusions; and I suggest that their remarks would have been a more fitting response for IPHC:

> ". . . Whilst applauding his (Burkenroad) vigorous attack on some loose and inconclusive thinking about this matter, and agreeing, reservedly, with his conclusion that regulations to conserve marine fisheries must be conceived in such a way that the results can be examined on a sound statistical basis, we nevertheless feel that he has over-emphasized the importance of the inductive method as compared with that of the deductive. This is perhaps because the over-fished species of which we have first-hand knowledge are in fact those for which, as Burkenroad concedes, the accidents of war provided conclusive proof of what he calls significant fishery-dependent changes. . . . nevertheless, in his later papers especially, Burkenroad is concerned rather with longer-term fishery-independent (i.e. natural) trends or oscillations. If the possibility of the occurrence of these be admitted, as we think it must be, it is equally true that great caution must be exercised in the interpretation of events during the experimental management phase; . . . Burkenroad's interesting suggestion that regulation should take the form of a controlled experiment with periodic relaxation of the restrictions cannot be dismissed lightly but the reservations referred to earlier are important . . . We have found, repeatedly, that with a knowledge of population statistics for two steady states much unravelling of the dynamic processes is possible and prediction of changes can profitably be attempted."

Though my reassessment of the early statistics has shown serious shortcomings in the original analyses, the work of Thompson and his colleagues will remain a monumental contribution to fishery science. Their appreciation of the need for accurate statistics and their basic understanding of population dynamics as well as of management concepts stand as a classic example of the scientific approach to fisheries regulation. These men helped establish a *modus operandi* that has become traditional throughout the fisheries field. More importantly, Thompson and his staff were able to communicate with fishermen, industry members, and government administrators alike, and they made a success of the first international body ever assigned the direct responsibility for fisheries management. Without this communication, the control necessary for management would never have been achieved; and whatever the ultimate assessment of the management program, their demonstration of practical methods will be of lasting value.

SUMMARY

Increased hook-spacing in the present-day fishery prompted a reevaluation of similar gear changes before 1930. Thompson and his colleagues had concluded that hook-spacing and hook number were unimportant and accepted a standard length of setline gear as the unit of fishing effort. Reexamination of the original analysis showed, in agreement with recent experimental data, that hook-spacing did affect the catch per skate and that the effective effort of dory gear (9-foot spacing) was greater than that of longline gear (13-foot spacing).

Thompson also concluded that the difference in soak-time between dory and longline gear was not significant. A review of the early literature and log records indicated that the soak-time of the two types of gear differed substantially. Reanalysis of the original data showed that soak-time did affect the catch rate and this, too, was supported by field experiments in recent years.

Thompson was aware of seasonal differences in availability, but he did not correct the annual estimates of catch per unit effort (CPUE) for seasonal changes in fishing effort. His estimates of abundance showed an inordinate decline from the early years when fishing was year-round to later years when fishing was prohibited during the months when CPUE was highest.

I corrected for the deficiencies in Thompson's initial work and revised the estimates of CPUE to adjust for changes in hook-spacing, soak-time, and seasonal availability. Though the early statistics of catch and effort are severely limited, the results of my analysis are in agreement with experimental data collected in recent years and provide a more accurate account of the effects of fishing in the early years of the fishery. Thompson's estimates of effort were low and his estimates of CPUE were high. The decline of stock abundance was not as precipitous as originally portrayed.

Burkenroad rightly questioned Thompson's interpretation of the early data. My revised estimates of abundance show that the original estimates of CPUE were in error and give credibility to the thesis that fishing mortality was the major cause of the decline in stock abundance. However, the exact role of the effects of fishing and environmental factors cannot be determined until there is a better understanding of the population parameters and the interrelation of the stocks. Burkenroad also questioned whether the recovery of stock abundance after 1930 could have been related to the IPHC regulations. Until unknowns, particularly about growth and recruitment, are determined, one cannot properly credit the increase in abundance to either the management program or to fishery induced changes or to environmental effects.

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