Comparing these curves with the results shown in Table V, it is apparent that the slope of such a curve is a definite index of the benefit to be obtained in a counter current operation. That is, the steeper the slope of the curve, the greater are the benefits to be derived.

An inspection of the slope of the curve after a little practice and experience will enable one to determine the proper amount of adsorbent required for a two stage operation.

TABLE V

| Adsorbent | Final Color Y. | Final Color R. | % Adsorbent Single Stage | % Adsorbent Two Stage Counter Current | % Saving In Adsorbent |
|---|---|---|---|---|---|
| Fullers Earth | 20 | 2.7 | 2.8 | 2.0 | 28% |
| Activated Carbon | 20 | 2.5 | 2.0 | 1.0 | 50% |
| Fullers Earth-Activated Carbon Mixture (10:1) | 20 | 1.8 | 3.3 | 2.2 | 33% |

# How Accurate Must An Official Fat Stability Test Be?

## Analysis of Three Year's Committee Studies of the Swift Fat Stability Test

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A tabulation summarizing the results of three years Committee testing of cooperative samples is discussed from the standpoint of the indicated precision, and it is shown that even with the liberal average tolerance of 17% of the value of the keeping times, the results of about one fourth of the laboratories reporting were outside of it. Throughout the series, four or about a third of the participating laboratories obtained consistently good results. These happened to be the laboratories which started using the method first. Of the others, two started getting good results after a bad first year, whereas two others failed to show agreement even after one or two year's previous experience. In the light of these apparent shortcomings of the method, it is discussed briefly from the standpoint of its suitability as an official method of the Society. This is suggested as being questionable, although the method in itself when used painstakingly and with especial regard to all necessary precautions has been shown to be inherently sound.

SIX years ago a paper (1) was read before this Society describing a method of estimating relative fat stabilities in which purified air was bubbled through the fat at an elevated temperature until the test fat became rancid to smell and had developed a peroxide number exceeding a certain threshhold value, which value depended on the type of fat being tested. The method possessed some obvious advantages over any previously known, and a committee was formed to try it on cooperative samples and determine its suitability as an official method of the Society. As the results of this investigation were not particularly good, the committee recommended further study. The next year the results were no better, but the committee recommended adoption as a tentative method and a continuation of the investigation. The Society, however, did not vote for tentative adoption, but for further work on the method. After the third year the committee again recommended its adoption as a tentative standard method, although the results were about the same as before. That year's report recapitulated the three years' work. Although any one laboratory might consistently obtain concordant results, the Uniform Methods and planning Committee disliked to recommend to the Society to adopt (even tentatively) as official, a method which had repeatedly shown certain shortcomings when the agreement between different laboratories on cooperative samples was so uncertain. Such recommendation was therefore again withheld but the committee was continued to perform further work.

Subsequent committees failed to produce any useful data and nothing further has been accomplished. After even a conscientious attempt at progress had thus died I subgested to the Uniform Methods and Planning Committee that it might reexamine its attitude on this test and determine if perhaps the interest of the Society might not best be served by having in its tentative and official methods, one, while far from ideal, is nevertheless considering many factors, still about the best that has been proposed. As a preliminary to this, I was asked to review the work of the former Stability Committees and present to the Society the situation as it now exists. I shall therefore give you an analysis that I have made of the Stability Committees' three seasons of productive work, together with their opinions and recommendations, and finally outline what appears to be a logical next step if the membership indicates that it considers an official fat stability test to be a desirable addition to its methods even if the test leaves much to be desired.

Table I reproduces the results of three years of cooperative testing, with the addition of my analysis of the indicated precision. The tolerances taken as the basis of the latter are entirely arbitrary and represent only what might be considered as reasonable on the basis of the cooperative data at hand. These tolerances were varied somewhat to fit the data on the individual samples. For example, remembering that the results are reported only to the nearest whole hour of indicated accelerated keeping time, if this time should average say 5½ hours on a certain sample (A), and if we take 15% of the average time as our reasonable allowance, we would have ±0.8 hour, or say 1 hour as the allowance. This would require that accepted values be either 5 hours or 6 hours. On the basis of the data reported,

this would be a rather severe requirement and would eliminate all but two results on this sample. The only alternative is to allow 1½ hours or 27%. By such similar adjustments the allowances on the other samples were chosen. This of course is rather on the liberal side.

Table II shows how the data would agree if the results had been reported only to the nearest multiple of five hours, a practice that would indicate the apparent degree of precision of the test, as between different laboratories.

Before discussing the data further let us examine the essential parts of the Committee reports:

1934: In view of the erratic results reported by two out of the six participating laboratories, the Committee stated, "there is nothing for the Committee to do but investigate the method further before it can make a final report." "The two laboratories reporting erratic results were those which had just installed the new stability apparatus and which had acquired very little experience with it." (2)

1935: "The laboratories which in agreement last year are, with the exception of a test on one sample, in agreement on these samples. The two laboratories whose data were out of line last year agree much more satisfactorily on the cooperative samples sent out this year. Of the three additional laboratories to which samples were sent, one of them submitted data which are in agreement with the data from the other cooperating laboratories. The other two laboratories reported that they were unable to get good checks on duplicate tests and the data they submitted to the committee and which are attached to this report clearly indicate this.

"Again the explanation for the lack of agreement of the data furnished by these two laboratories appears to lie in the fact that in both instances the laboratories had not had the apparatus very long and the kinks were not all ironed out of them, or in other words some undiscovered factor was responsible for the low results reported."

". . . . . The majority of this committee further agree that, although this test is not perfect, the fat and oil industry will be helped more than hindered if the terminology dealing with keeping quality can be clarified and made more consistent.

"We, therefore, recommend that the active oxygen or peroxide test for judging the relative stability of edible fats and oils be made a tentative method of the society and that the committee be continued to work on the test." (3)

1936: "With the exception of the discrepancies in the data from laboratories which reported the erratic results before, the data are in good agreement. From our general experience with the test and a study of the stability data reported on a total of twenty-two samples (cooperative) we believe that the picture will not be very greatly changed no matter how many more samples we may send to cooperating laboratories."

"Although the test is not perfect and has limitations we feel that since the method has so many uses and is the best accelerated procedure we know for judging the relative stability of fats and oils it should be written up as a method and made available for all skilled workers in the fat field. We believe that this can be done best by making it a tentative method of the Society."

"This report represents, as nearly as possible, the judgment of the majority of the members of the committee. It is not a unanimous opinion." (4)

Two significant statements in these reports require noting. In

TABLE I

LABORATORIES

| Code Designation Sample | | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | | Tolerance | | % of Labs. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Date | No. | Kind of fat | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Hrs. | Aver-ager‡ | Hrs. | % | Outside |
| 1934 | A | Lard | 7 | .... | .... | 7 | .... | 4 | 4 | 5 | .... | 6 | 6 | 5.3 | 1.5 | 27 | 0 |
| 1934 | B | Lard | 12 | .... | .... | 12 | .... | 6 | 5 | 8 | .... | 12 | 12 | 9.4 | | | |
| 1934 | C | Lard | 20 | .... | .... | 17 | .... | 15 | (9) | (14) | .... | 18 | (22) | 17.7 | 2.5 | 14 | 29 |
| 1934 | D | Hyd. cso. | 19 | .... | .... | 19 | .... | * | (16) | 18 | .... | 21 | (22) | 19.2 | 2.0 | 11 | 33 |
| 1934 | E | Hyd. cso. | 35 | .... | .... | 35 | .... | 40 | (24) | .... | .... | 34 | (42) | 37.2 | 4.0 | 11 | 33 |
| 1934 | F | Compound | 4 | .... | .... | 4 | .... | 4 | 3 | 5 | .... | 4 | 5 | 4.1 | 1.0 | 25 | 0 |
| 1934 | G | Compound | 10 | .... | .... | 9 | .... | 9 | (7) | 8 | .... | 9 | 11 | 9.5 | 1.5 | 16 | 14 |
| 1934 | H | Cso. salad | 10 | .... | .... | 11 | .... | (7) | 9 | .... | .... | 10 | 10 | 9.5 | 2.0 | 21 | 17 |
| 1934 | I | Corn salad | 8 | .... | .... | 8 | .... | (5) | 7 | .... | .... | 8 | (10) | 7.7 | 2.0 | 27 | 33 |
| | | | | | | | | | | | | | | | Av. | 19 | Av. 20 |
| | % | Outside | 0 | | | 0 | | 25 | 44 | 17 | | 0 | 44 | | | | |
| 1935 | 1 | Lard | 16 | 15 | .... | (19) | .... | 16 | (18) | 15 | (12) | (18) | (18) | 15.2 | 2.0 | 13 | 56 |
| 1935 | 2 | Compound | 9 | 9 | .... | 9 | .... | 9 | 9 | 8 | 11 | 11 | 10 | 9.5 | 1.5 | 16 | 0 |
| 1935 | 3 | Hyd. cso. | (33) | 30** | .... | (57) | .... | (31) | 49 | (†) | (36) | (58) | 44** | 44.0 | 7.0 | 16 | 84 |
| 1935 | 4 | Cso. salad | 12 | 11 | .... | 10 | .... | 9 | 10 | (†) | 12 | 10 | 11 | 10.6 | 1.5 | 14 | 0 |
| | | | | | | | | | | | | | | | Av. | 15 | Av. 35 |
| | % | Outside | 25 | 0 | | 50 | | 25 | 25 | 50 | 50 | 50 | 25 | | | | |
| | 1 | Lard | 9 | .... | 8 | 10 | 10 | (12) | 10 | .... | (7) | 10 | .... | 9.5 | 1.5 | 16 | 25 |
| 1935 | 2 | Lard | 9 | .... | 9 | 10 | 8 | (12) | 8 | .... | (7) | 9 | .... | 9.0 | 1.5 | 17 | 25 |
| | 3 | Lard | 17 | .... | (11) | 18 | 16 | (24) | 16 | .... | (13) | 18 | .... | 16.6 | 3.0 | 18 | 38 |
| and | 4 | Lard | 10 | 9 | 9 | 10 | 11 | 11 | 9 | .... | 8 | 9 | 8 | 9.4 | 1.5 | 16 | 0 |
| | 5 | Lard | 9 | 8 | 10 | 9 | 10 | (11) | 10?8? | .... | (7) | 9 | 9 | 9.1 | 1.0 | 10 | 20 |
| 1936 | 6 | Compound | 15 | 13 | 12 | 15 | 16 | 15 | 14 | .... | 12 | 13 | 14 | 13.9 | 2.0 | 14 | 0 |
| | 7 | Hyd. cso. | 45 | (180) | .... | 47 | 49 | .... | 51 | .... | (31) | 49 | 42 | 44.8 | 10.0 | 22 | 25 |
| | 8 | Hyd. cso. | 45 | (150) | .... | 47 | 49 | .... | 51 | .... | (32) | 49 | 42 | 45.0 | 10.0 | 22 | 25 |
| | 9 | Hyd. cso. | 47 | (125) | .... | 47 | 49 | .... | 51 | .... | (39) | 49 | 42 | 46.3 | 10.0 | 22 | 25 |
| | | | | | | | | | | | | | | | Av. | 17 | Av. 20 |
| | % | Outside | 0 | 50 | 17 | 0 | 0 | 67 | 0 | | 78 | 0 | 0 | | | | |
| | % | " (all groups) | 4 | 30 | 17 | 9 | 0 | 39 | 23 | 30 | 69 | 9 | 26 | | | 17 | 23 |

* Sample broken on arrival.
** Test was discontinued and never repeated.
† Couldn't get consistent results.
Data in parenthesis are those outside the tolerance given in the third-to-last column.

‡ In obtaining "% Outside Tolerance," the figures below were taken as the nearest whole number values, or the nearest 0.5 hr; i.e., 5.3 would be considered 5.5.

Table II

LABORATORIES

| Year | Sample | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | Agreement † |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | A Lard | 5 | | | 5 | | 5 | 5 | 5 | | 5 | 5 | Perfect |
| | B Lard | 10 | | | 10 | | 5 | 5 | 10 | | 10 | 10 | 2 - 5 |
| 9 | C Lard | 20 | | | 15 | | 15 | | 15 | | 20 | 20 | 3 - 3 |
| | D Hydro C.S.O. | 20 | | | 20 | | * | 15 | 20 | | 20 | 20 | 1 - 6 |
| 3 | E Hydro | 35 | | | 35 | | 40 | 7 | | | 35 | 40 | 2 - 3 |
| | F Compound | 5 | | | 5 | | 5 | 5 | 5 | | 5 | 5 | Perfect |
| 4 | G Compound | 10 | | | 10 | | 10 | 10 | 10 | | 10 | 10 | Perfect |
| | H C.S.O. Salad | 10 | | | 10 | | 5 | 10 | | | 10 | 10 | 1 - 5 |
| | I Corn Salad | 10 | | | 10 | | 5 | 5 | | | 10 | 10 | 2 - 4 |
| 1 | 1 Lard | 15 | 15 | | 20 | | 15 | 20 | 15 | 10 | 20 | 20 | 1 - 3 - 5 |
| 9 | 2 Compound | 10 | 10 | | 10 | | 10 | 10 | 10 | 10 | 10 | 10 | Perfect |
| 3 | 3 Hydro C.S.O. | 35 | ** | | 55 | | 30 | 50 | | 35 | 60 | ** | 1 - 2 - 1 - 1 |
| 5 | 4 C.S.O. Salad | 10 | 10 | | 10 | | 10 | 10 | | 10 | 10 | 10 | Perfect |
| 1 | 1 Lard | 10 | | 10 | 10 | 10 | 10 | 10 | | 5 | 10 | | 1 - 7 |
| 9 | 2 Lard | 10 | | 10 | 10 | 10 | 10 | 10 | | 5 | 10 | | 1 - 7 |
| 3 | 3 Lard | 15 | | 10 | 20 | 15 | 25 | 15 | | 15 | 20 | | 1 - 1 - 2 - 4 |
| 5 | 4 Lard | 10 | 10 | 10 | 10 | 10 | 10 | 10 | | 10 | 10 | 10 | Perfect |
| / | 5 Lard | 10 | 10 | 10 | 10 | 10 | 10 | 10 | | 5 | 10 | 10 | 1 - 9 |
| 1 | 6 Compound | 15 | 15 | 10 | 15 | 15 | 15 | 15 | | 10 | 15 | 15 | 2 - 8 |
| 9 | 7 Hydro C.S.O. | 45 | 180 | | 45 | 50 | | 50 | | 30 | 50 | 40 | 1 - 1 - 1 - 2 - 3 |
| 3 | 8 Hydro | 45 | 150 | | 45 | 50 | | 50 | | 30 | 50 | 40 | 1 - 1 - 1 - 2 - 3 |
| 6 | 9 Hydro | 45 | 125 | | 45 | 50 | | 50 | | 40 | 50 | 40 | 1 - 2 - 2 - 3 |

* Sample broken on arrival ** Test discontinued and never repeated
† Number (not designation) of laboratories obtaining different results

The agreement obtained if the data of TABLE I had been expressed to the nearest 5-hour value.

1935, "the two laboratories whose data were out of line last year agree much more satisfactorily on the cooperative samples sent out this year": It is not known whether these laboratories had used the test all during the intervening year; so that we cannot conclude that a year's "induction" period is necessary for proficiency. Now in 1936: "With the exception of the discrepancies in the data from laboratories which reported the erratic results before, the data are in good agreement." In other words, the previous experience in this case was of no indicated benefit. Looking more closely at the data, we see that four laboratories had nearly perfect scores. One of these was the laboratory that developed the test, and there is reason to think that the other three had the benefit of advice and early experience associated with development. In the case of the other group of laboratories, Nos. 2 and 3 made good showings in their first participation. Nos. 7 and 11 showed poor agreement their first year but very good agreement the two following years, neither having any misses on nine samples the last year. The results from laboratories 6 and 9 show an apparent inability to learn, for their percentage of misses increased through the series.

There are various plausible and probably correct reasons we might advance to explain some of the poor agreement. Those laboratories that showed marked improvement through the series may have kept the same man on the test throughout; whereas those that continued to be in poor agreement may have used different chemists each year, none of whom had a chance to fully acquaint themselves with all the important precautions to be observed in its use. But we are not at the moment concerned with reasons. All of the laboratories participating stand high in the ranks of American fat testing laboratories. Certainly, we may assume that all tried conscientiously to make good showings; for we believe it to be a general rule that our members who engage in collaborative testing are inclined to give, if anything, greater attention to such work than to their ordinary routine. So in view of this thought, what manner of results might we expect if the method were employed indiscriminately in various laboratories, in some of which it might be used only occasionally in a referee status?

What are we to conclude from the decidedly indifferent results of these three years work, when viewed as a whole? It seems to me that we may take one of two attitudes: One is the negative view that the test has been given a series of fair and thorough trials in different laboratories and that cooperative results show it to be subject to accidental and uncontrolled factors which make it unreliable as a general instrument for evaluating fat stability where agreement between different laboratories is of first importance, and that in attempting to use it as an official method, we might expect more confusion and misunderstanding than benefits to the oil and fat trade. On the other hand, we may point out that the method has been used in a dozen or so laboratories in which the inherent difficulties have been overcome, that these obtain consistent and uniform results, and that where erratic results obtain they are simply the result of inadequate familiarization, or of using chemists lacking in sufficient training and manipulative skill. We may reason that any procedure designed to test keeping quality of fats must provide for the utmost cleanliness of apparatus, etc.; that is, the element of accidental contamination by oxidation catalysts and/or inhibitors, would be encountered in any other test. Furthermore, if we all recognize the extreme importance of cleanliness and the adherence to specified conditions for securing concordant results, then it is not the fault of the method if some chemists are not sufficiently careful in these respects. We may further point out that our Official Methods include others which require patience and above-average manipulative skill, such that if assigned to some of our younger and less experienced chemists, they might not yield acceptable results.

All will agree, I think, that there is need for a good, reliable accelerated stability test with official standing. It is doubtful if we will ever find an ideal test. Many believe the test under consideration to be the best available; but whether it is good enough for official status, whether its repeatedly demonstrated faults more than outweigh such benefits to our mem-

bership and to the oil and fat producing and consuming trades that might follow its adoption in spite of its limitations — is a question which obliges us to inquire into the *raison d'etre* of a Scientific Society's collection of official methods. Is such a collection maintained principally as a source of approved ones to be drawn upon by one or more trade associations to be used in trade settlement and referee testing? Or may we take a disinterested and unworldly scientific attitude and hold that we are not concerned with the uses to which our standard methods may be applied — that they simply represent our selection of the best available so far as our committees have been able to determine, and their use is advocated generally in the interest of uniformity. If we take the first or utilitarian stand, then it is doubtful if the proposed stability test is adequate for our needs, provided we are to confine our judgment solely to the Committees' reported results. If the detached scientific view is held to be more proper for us, there would seem to be no serious objection to including this test in our Methods as a convenience to the many chemists who have come to value our collection; for many laboratories have found the test when properly conducted, to be simple and accurate and of considerable value.

It is hoped that the membership will make some definite expressions so that our incoming President may decide whether to form a new Stability Committee to rewrite the Swift test as a method for tentative adoption, or to await the development of a new and better test — one more suited to the Society's official needs.

**REFERENCES:**

(1) King, Roschen & Irwin, Oil and Soap, Volume 10, Page 105 (1933).
(2) Oil & Soap, August 1934 Issue.
(3) Oil & Soap, August 1935 Issue.
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# Cottonseed and the Southern Regional Research Laboratory*

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**Abstract**

Since many cotton farmers have borrowed to the limit on the lint portion of their crop, the cash income from the cottonseed is frequently the only money handled. Of vital interest to the cotton farmer and oil miller are efforts to increase the oil content of cottonseed, improve methods of extracting the oil in order to increase yields, and other research designed to improve the products from and raise the value of cottonseed. The commodities selected for initial study in the Southern Regional Research Laboratory are cotton, peanuts and sweetpotatoes, and in addition to research on cottonseed, such as enumerated above, studies will be carried out on cotton cellulose, the whole cotton plant, peanut oil and protein, sweetpotato starch and other products and by-products of the assigned commodities.

IN 1938, Congress directed the Secretary of Agriculture to establish four regional research laboratories, one in each of the major agricultural producing areas of the United States. An annual appropriation not to exceed a million dollars was made for each of these laboratories. The laboratories are "to conduct researches into and to develop new scientific, chemical and technical uses and new and extended markets and outlets for farm commodities and products and by-products thereof. Such research and development shall be devoted primarily to those farm commodities of which there are regular or seasonal surpluses, and their products and by-products."

I am aware of the fact that the major interest of the group in attendance here is in oils and fats. Our interests coincide on cottonseed and peanut oils. Naturally, the other products of cottonseed and peanuts — meal, hulls, etc., although of lesser importance as producers of revenue, also command our mutual attention. Before entering upon a particular discussion of cottonseed and peanuts, I should like to outline briefly the new regional research program which the Department of Agriculture is now undertaking.

The searching for new and wider industrial outlets and markets for farm products through research is just one of the several lines of attack on our national farm problem. Other attacks on this problem are being applied. One important line of attack which until now has received only minor attention from a monetary standpoint, is by means of research; not simply research on specific problems as they arise, as a sort of glorified trouble-shooting program, but rather a comprehensive, concerted, closely-knit, program of research — chemical, physical, biological, technological, and economic — all carried on with the specific aim of finding new and extended uses for farm commodities. We believe that research of this nature will pay, not immediately of course—that would be too much to hope for — but more and more with the passing of each year. We believe, moreover, that such a program is long overdue.

As a preliminary step to the setting up of these four Regional Research Laboratories, Congress provided for a survey to find out what research is now being carried on, to obtain suggestions for needed research, and to obtain information which would be helpful in fixing the scope of the laboratories. The members of this special survey staff visited every State of the Union, interviewing representatives of private and public research laboratories, educational institutions, and agricultural organizations. There was thus obtained a knowledge of the extent and nature of present research activities in the United States, and also many hundreds of suggestions regarding needed research on various farm commodities. Such information will be an invaluable aid in avoiding wasteful duplication of effort and as a guide in the selection of specific research projects.

In its survey of research and formulation of the objectives of its program, the Department has had the assistance and cooperation of a large number of those engaged in like work in Federal and State

* A talk delivered before the American Oil Chemists' Society, New Orleans, Louisiana, May 6.