Weather and our food supply

Center for Agricultural and Economic Development, Iowa State University

Cecil H. Wadleigh
United States Department of Agriculture

Robert H. Shaw
Iowa State University

Louis M. Thompson
Iowa State University

Robert F. Dale
Iowa State University

Follow this and additional works at: http://lib.dr.iastate.edu/card_reports

Part of the Agricultural and Resource Economics Commons, Agricultural Education Commons, Agricultural Science Commons, Climate Commons, Economics Commons, Meteorology Commons, and the Statistics and Probability Commons

Recommended Citation
http://lib.dr.iastate.edu/card_reports/20

This Book is brought to you for free and open access by the CARD Reports and Working Papers at Iowa State University Digital Repository. It has been accepted for inclusion in CARD Reports by an authorized administrator of Iowa State University Digital Repository. For more information, please contact digirep@iastate.edu.
WEATHER AND OUR FOOD SUPPLY

CAED Report 20

CENTER FOR AGRICULTURAL AND ECONOMIC DEVELOPMENT

IOWA STATE UNIVERSITY of Science and Technology

Ames, Iowa, 1964
The Center for Agricultural and Economic Development
Iowa State University of Science and Technology
1964

Work of the Center supported in part by a grant from the W. K. Kellogg Foundation
FOREWORD

The steep rate of increase in yield of grain crops in the United States since the mid-1950's has resulted in the use of the term "explosion in technology." Surplus grains piled up to such proportions after the 1960 harvest that acreage control appeared to be in order. But despite substantial reductions in acreages after 1960, the increased output per acre has just about compensated for acreage reductions. During this period of rapid increase in output per acre, there has been a growing tendency to believe that technology has reduced the influence of weather on grain production so that we no longer need to fear shortages due to unfavorable weather.

There is also a popular belief that acreage controls fail to achieve the objective of production control, and that public funds are being wasted in storing surplus grains which we don't need.

There is increasing evidence, however, that a period of favorable weather interacted with technology to produce our recent high yields, and that perhaps half of the increase in yield per acre since 1950 has been due to a change to more favorable weather for grain crops.

These findings have important implications in continued support for research in production technology and in the way in which we look at our surplus stocks of feed and food grains. If a period of favorable weather has been responsible for half of the increase in yields since 1950, then what can we expect if the weather trend reverses itself for a few years? Do we have periodicity in weather, and have we just passed through a run of favorable years that might be followed by a run of unfavorable years? Should we treat our surplus grains as reserves? How does our rate of growth in grain output compare with the needs of a growing world population? And of course, in the background of these questions is one big question -- how much of our recent high yields is really due to weather?

To answer these important questions, the Center for Agricultural and Economic Development invited outstanding authorities to present their ideas under three main headings: (1) Techniques for Evaluation of Weather Variables in Agricultural Production, (2) Periodicity in Weather Patterns: Implications in Agriculture, and (3) Weather Considerations in Agricultural Policy. The papers have been assembled in the order of their presentation under the general outline above.
The proceedings of the seminar are being made available at an early date due to the special efforts of Edwin O. Haroldsen, Editor for the Center for Agricultural and Economic Development, and his assistants, Mrs. Yvonne Dahlman and Mrs. Karla Reynolds. Also acknowledged are the contributions of seminar committee members, Earl O. Heady, John T. Pesek, Jr., Robert H. Shaw, Geoffrey S. Shepherd and J. William Uhrig, all of Iowa State University.

Louis M. Thompson, Chairman
Weather Seminar Committee
June 16, 1964
# TABLE OF CONTENTS

The Need to Evaluate Weather ........................................ Cecil Wadleigh 1

Grain Yields and Weather Fluctuations ..................... Robert H. Shaw 9
Louis M. Thompson

Changes in Moisture Stress Days
Since 1933 ............................................................... Robert F. Dale 23

The Contribution of Weather and Yield
Technology to Changes in U.S. Corn
Production 1939 to 1961 ........................................... Earl O. Heady 45

Multiple Regression Techniques in the
Evaluation of Weather and Technology
in Crop Production .................................................... Louis M. Thompson 75

The Weather Index Approach .................................... Lawrence H. Shaw
Donald D. Durost 93

Statistical Techniques Which Might Be
Useful in Further Research ..................................... Rex L. Hurst 103

Evidence of Solar-Climatic Relationships ................ Hurd C. Willett 123

The Predictability of Cycles, Trends and
Annual Fluctuations in Weather and
Crops ........................................................................ Louis H. Bean 153

Climatic Variability and Crop Production .............. Wayne C. Palmer 173

A Critical Appraisal of Periodicities
in Climate ............................................................... J. Murray Mitchell, Jr. 189
Forecasting Crop Yields . . . . . . . . . Bruce W. Kelly
John W. Kirkbride 229

Livestock Cycles and Their Relation to
Weather and Range Conditions . . . . . Harold F. Breimyer
Alan R. Thodey 241

The Size of Grain Stocks That Should
Be Maintained . . . . . . . . . Arthur T. Thompson 255

Consideration of Weather in Farm
Program Planning . . . . . . . . . M. L. Upchurch 263

World Food Problems . . . . . . . . . Mordecai Ezekiel 271

Panel Discussion of Implications of
Weather in Agricultural Policy
Planning . . . . . . . . . . . Karl Fox (moderator) 283
THE NEED TO EVALUATE WEATHER
Cecil H. Wadleigh

Evaluation of weather in relation to food production is more than a need; it is a must.

Thoughtful demographers are posing the harsh question: To what purpose do we use modern medicine to save an infant from lethal disease only to let it die of starvation a few years later? (Science 143:916)

Here in the United States the average man on the street would probably regard such a question as unduly farfetched or even nonsensical. His thoughts on food are likely to be confined to: (a) griping about the size of the tab (which covers many nonfood items) when checking out of the local supermarket, (b) developing strategems to avoid overeating and (c) decrying the cost to the taxpayer of government programs related to food surplus.

People view things in the light of their experience. And so we must recognize the general public empathy in the United States with relatively picayune food problems and widespread public apathy over the demands of burgeoning populations over the world for food.

Developing public awareness of the problems we are here discussing is certainly a need.

Most of you are acquainted with the excellent documentation that the Food and Agriculture Organization has brought together on the stark problems facing us if we are even to approach "freedom from hunger" in the years ahead. Some of you attended the World Food Congress in Washington, D. C., in 1963 and collected the vast array of manuscripts testifying to the urgency of meeting food production problems the world over. The formidable evidence makes one shudder.

I hesitate to bore you by repeating some of the statistics, but they ought to be in the record.

1/Director, Soil and Water Conservation Research Division, Agricultural Research Service, U. S. Department of Agriculture, Beltsville, Maryland.
The population of the world is expected to double and exceed six billion by the turn of the century.

World food production must be trebled by the turn of the century if the population is to have enough to eat.

One half or more of the present population of three billion suffer from hunger or malnutrition.

Two and one quarter billion people comprising the populations of Latin America, Africa, the Near East and the Far East receive only an average of 2,150 calories per day with about 80 percent of the diet made up of cereals, starchy roots and sugar. By contrast, the 875 million people in North America, Europe and Oceana receive 3,050 calories of food per day with a little over half coming from cereals, starchy roots and sugar.

In addition to attaining freedom from hunger there is pressing need now to upgrade diets in terms of the proportion of protein.

While recognizing the colossal task now before world leadership in meeting the enormous demands for food over coming decades, we also ought to keep in mind the eternal verity in the old cliche that "man cannot live by bread alone."

We are by nature wild animals "plus"; and the "plus" is the combined effects of civilization.

What is civilization?

Usually, we think of civilization as made up of artistic creations, mechanical devices, books and pictures, enlightened religious ideas, handsome buildings and superhighways, scientific accomplishments, social and philosophical knowledge, political institutions and ingenious ways of doing a wide variety of things. We think of man's abilities in creating these things as being due to his possession of a mind with the capacity to reason.

But what is the first requirement of civilization?

Let me quote a noted anthropologist, Prof. Braidwood of the University of Chicago (Agricultural History 28:41, 1954):

Historically oriented anthropologists agree that the one absolute necessity for the appearance of civilization (in a fully meaningful sense of the word) would
be full, efficient food production. Subsequent appearance of other attributes of civilization are contingent upon the original appearance of food production.

The history of ancient Sumeria provides eloquent testimony to support Prof. Braidwood's thesis. The fabulous cities with magnificent buildings that the Sumerians built in the third and second millennia B.C. were made possible by full and efficient food production from an irrigation agriculture based on a brilliantly engineered canal system. Basically, weather events destroyed this civilization. Flood waters coming down the Tigris and Euphrates eventually destroyed the canal system through a relentless deposition of sediment. In coping with the sediment Sumeria was largely dependent on the labor of captured slaves.

Some historians have indicated that the high level of civilization in ancient Mesopotamia was destroyed by the vicious raids of Mongols led by Hulagu Khan in 1258 A.D. But recent evidence by archeologists Jacobsen and Adams (Science 128:1251, 1958) indicates that Hulagu and his horsemen found nothing but a devastated scene when they invaded the region and ever since have been unjustly blamed for causing the devastation. These investigators found that by the 12th century, floods delivering sediment into the irrigation waters had caused far greater devastation to the irrigated land, and thus to the food supply of the cities, than any invading horde could have done.

This anthropological evidence drives home the point that in our consideration of weather and food we must go much beyond that needed merely to maintain man as a wild beast. We must provide for the "plus," the "plus" which is civilization.

We see from the foregoing that all the magnificent attributes of advanced civilization we have here in the United States rest solidly on the capacity of a relatively few American farmers to provide the basic requirements of full and efficient food production. One might even conclude that producing a bit of surplus food may not necessarily be an evil.

We now need to consider the potential supply of arable land that may be available for this full and efficient food production needed by rapidly expanding populations.

FAO reports that arable land now available for crop production over the earth is 3,500,000,000 acres. This acreage can be more effectively used by application of better technology.
Dr. Charles E. Kellogg, deputy administrator of the Soil Conservation Service, has recently completed a study of total potential land area available for crop production and finds this to be 6,589,000,000 acres. He emphasizes that this potential increase of 3,089,000,000 acres is made possible to a large extent by research findings on land reclamation and land use over the past 30 years.

Use of the potentially additional arable land over the earth will be especially susceptible to constraints imposed by weather, soil management practices, adapted varieties and pest control. More specifically, when we compare the weather-imposed problems on lands now used for food with comparable problems on lands that may be brought into production in the future, we can have full confidence that the tough ones remain to be solved. The apples on the low limbs are the easiest to pick.

If anyone doubts the profound influence of weather on the capacity of arable lands to produce food, it is suggested that they read the terse review by Dr. L. P. Smith on "Weather and Food" published by the World Meteorological Organization as Basic Study #1 in 1962. I am particularly delighted to note that Dr. Smith emphasizes the urgency of bringing new facts, new thinking and new knowledge into an evaluation of the whole array of weather factors that bear on crop production. Even so, we are not likely to eliminate the stricture so well emphasized by Maximov that water is the main limiting factor in crop production the world over.

Actually, our concern with weather in relation to food production would be largely abated should someone find the means to emasculate the "iffiness" that inordinately prevails. Let us consider an actual case.

When a wheat grower in Stanton County, Kansas, plants his grain in the fall, he can be quite certain of a bumper harvest the following summer:

If there is a good supply of available moisture in the subsoil at the end of the fallow period;

If rains occur in September and October to provide surface moisture essential for germination and seedling growth;

If leaf rust does not appear during the fall as a result of extensive dewfall or rainy weather with resultant weakening of the young plants;

If fall weather does not foster an infestation of "green bugs" which seriously weaken the young plants for tolerating winter cold;

If warm weather during the fall does not abet an infestation of mites that transmit the "mosaic" virus which can wipe out a crop;

If the plants are not winter-killed by sudden cold snaps following warm periods in the absence of snow cover;

If the plants are not blasted by soil blowing as a result of dry surface soil and high wind velocities during early spring;

If late spring frosts during anthesis do not bring about sterilization of the flowers;

If there is an adequate amount and distribution of late spring rains to provide necessary soil moisture to carry the grain through to maturity;

If heavy dews and rainy periods in the spring do not foster an infection of leaf and stem rust, which damage or even kill the plants;

If hot weather with desiccating winds does not occur during the time of filling resulting in low test weight of the grain;

If hot, dry weather does not foster a scourge of voracious grasshoppers to devour the crop;

If convective storms during late spring and early summer do not bring on a barrage of hail to destroy or severely damage the crops;

Finally, if the wheat grower could be liberated from all of the preceding "ifs," he would "have it made."

The foregoing list of suppositions does drive home the formidable array of imponderables that the wheat grower must face over the course of a season in his planning of operations. It emphasizes that this grower has little alternative than to base his decisions on a wealth of invalid assumptions and a dearth of accurately accrued information. In other words, the decisions the grower must make with respect to weather inputs essentially place him in a poker game with Mother Nature, in which she holds most of the aces and has a knack for drawing the one-eyed jacks.

Actually, the alternative decisions a farmer must make in the face of sequential weather events imply that he should be an authority on the theory of games if he expects to win, i.e., make a profit.

Thus, it is appropriate that we mention a few broad areas of information that are urgently needed to enable the food producer to more consistently win
while playing the game with Dame Nature. Thus, we need to be far more knowledgeable on:

1. The phenology of crop plants.
2. Weather probabilities.
3. Making alternative decisions in management practices in relation to sequential weather events.
4. Weather prediction.

**Phenology of Crop Plants**

The growth and development of any plant are determined by the environment in which it is grown within the limitations of its genetic potentials.

This nice terse statement would be much more useful if we could measure "environment" by some single-valued function such as we use pH to measure the acidity of soil. Unhappily, environment is the integrated resultant of a large number of widely fluctuating variables over time. Insofar as a given crop is concerned, plant performance is the only reliable integrator of this intricate complex, and each variety appears to have its own secret values for the parameters.

Crop environment involves isolation, day length, air temperature, humidity, carbon dioxide, air movement, soil moisture, soil temperature, soil aeration, soil fertility and soil toxins. We really know very little with respect to quantifying the interactive effects at varying levels of these entities and at different stages of crop growth.

Although some of these environmental factors such as soil fertility, carbon dioxide and soil toxins are outside the realm of plant phenology, they may be modified in their effects by climatic conditions. High rainfall may accentuate nitrogen inadequacy. Wind influences carbon dioxide availability around leaves during summer days. Low soil temperature depresses phosphorus availability.

Although the interrelationships affecting crop phenology are formidable in their complexity, we must have the information for the various stages of plant development, especially during germination and seedling growth, vegetative development, anthesis and maturation. We have information that prevailing weather during one stage of growth may physiologically precondition a plant with respect to responses at later stages of growth; but quantified evidence is meager.
We need handles with which to grasp this complex. The net curvilinear regression curves such as Louis Thompson finds in his analyses of the phenological effects on grains offer very convenient handles for the initial grasp. And we ought to keep in mind that genetics offers a powerful means of developing adaptation to phenological events.

Weather Probabilities

We are making progress in developing information on probabilities for certain weather events for specific land resource areas. Mr. Goren did not become an expert card player through ignorance of the odds. Likewise, the farmer should plan his operations with full knowledge of the odds when playing the game with Mother Nature.

Let us return to our Kansas wheat grower and the array of "ifs" with which he must cope. He should have information as to the number of years out of 10 a given iffy situation is of no consequence, the number of years in which it is limiting and the number in which it is critically adverse.

We also need to ascertain the degree of intercorrelation of probabilities with respect to sequential weather events. Are wet Septembers in western Kansas associated with wet Octobers? Are wet Mays associated with wet Junes?

Harold Crutcher of the Asheville Weather Center has made a comprehensive study of intercorrelation between sequential weather events over the United States and finds these relationships to be of low significance nationally. However, intercorrelation of these events might be more prevalent in local areas. At this point I would like to applaud the gold mine of data the Weather Bureau maintains at Asheville. We need the means to make better use of these basic data.

Management Decisions in Relation to Sequential Weather Events

As the farmer plans his operations over the course of a season in the face of changing weather conditions he usually must make a series of decisions among possible alternative practices. Ideally, he should have available the techniques of modern production economics that guide him to the alternatives that would tend to maximize profits, or at least minimize losses, over a period of years in relation to prevailing weather probabilities.

We are acquiring a mass of evidence that soil moisture reserves can be an important guide in the farmer's decisions among alternatives.

How much supplemental nitrogen should he apply to take full advantage of abundant soil moisture during wet years?

In prospective dry years, when soil moisture is limiting, should he spend money on herbicides to minimize competitive soil moisture losses by weeds, or should he avoid the expense of an input that might only add to his net loss?

As A. N. Duckham has pointed out in a recent paper (Reading Univ., United Kingdom, 1964), the modern farmer seeking to attain full and efficient production on an economic basis needs "to apply formal decision-making theory to 'weather chains.'"

Weather Prediction

On the evening of February 25, 1964, the Street Department in Washington sprinkled salt over key streets in anticipation of a 4- to 5-inch snowfall predicted by the Weather Bureau. No snow came.

On other occasions Washington has received a 7- to 9-inch snow when little or none was predicted.

These statements are in no sense an indictment of the Weather Bureau, but rather an illustration of the precarious state of the art. I doubt if we could find a more dedicated and conscientious group than those in the Weather Bureau, but the imponderables they must deal with are indeed frustrating.

On the other hand, a leading farm magazine publishes a chart indicating how much rain will fall and on what days it will come over a month in advance of publication. For example, the issue distributed in late September 1963 predicted that October rainfall in Maryland would be normal and that it would be well distributed over the month. In the Baltimore area, October 1963 was the first month on record in which no precipitation occurred.

To what purpose are farmers given the misinformation just cited?

Yet, one of the greatest needs in the realm of food production is that of the weather prediction. We urgently need reliable weather predictions over five-day periods, the coming month and the coming season. Research effort that would help attain such a goal is certainly of the highest priority.

In bringing together these few random thoughts on weather and food production, let no one doubt my deep conviction that progress in this field is of the greatest urgency for the future well-being of mankind. To those of you who can really do something about this need, I wish you Godspeed!
GRAIN YIELDS AND WEATHER FLUCTUATIONS

Robert H. Shaw and Louis M. Thompson

Within recent years there have been considerable questions raised regarding the relative effects of weather and technology upon our present high levels of production. When yield levels are extrapolated for five, 10 or more years into the future, a small error in the trend line can become greatly magnified. We must be sure what recent years represent before predictions for future years are made.

Because of the continually changing levels of technology determining the relationship between weather and yield under present-day agricultural conditions is not simple. In its simplest form, one could say that yields are a result of weather, of technology and of a weather technology interaction. Favorable weather alone will not produce high yields unless adequate technology is used. Improved technology alone will not produce high yields without adequate weather. Favorable weather conditions allow technology to express itself to its fullest potential. Probably much of the effect on yields is due to the interaction between technology and weather, which is difficult to evaluate under our continually changing level of technology. In many ways it is comparable to an experiment without a control or check plot.

In examining the relationship between grain yields and weather fluctuations in light of this conference, it seems that three questions are of considerable importance.

1. Are there periodic fluctuations in the weather, and are any of these predictable?

2. How closely is the weather at two locations related?

3. What effect has weather had upon our present high levels of grain production?

To gain some appreciation of variation in crop production as it might be related to weather let us first examine corn yields for Missouri shown in Figure 1. Severe drought conditions occurred in 1894 and again in 1901. For a period of several years after this, yields were generally above average. During the period 1910-1914, production declined. There were irregular fluctuations in yields to the mid-20's, then a general decline in yields until the mid-30's. The decline in yields from 1925 to 1935 was usually attributed to soil deterioration. The years 1934 and 1936 were also very severe drought years. Hybrid corn was introduced on a commercial scale in 1934 and was

1/Professor of Climatology and Associate Dean of Agriculture, respectively, Iowa State University.
rapidly adopted so that by 1945 about 90 percent of the corn in the Corn Belt was hybrid corn. Yields in the late 30's and early 40's showed a gradual increase. A slight decrease in yields occurred in the early 50's, which was due to cool and wet weather in 1950 and 1951 and dry years in the mid-50's. Recent years have shown high yields with an upward trend. A similar pattern is shown for Illinois and Iowa in Figure 1, with the general decline from 1925 to 1935 particularly noticeable.

**Periodicity of Weather Fluctuations**

Yields have shown certain patterns of periodic fluctuation. This raises the question of periodic fluctuations in weather. We use the term "periodic fluctuations" here rather than cycles because they do not have the rhythm associated with a true cycle. But if weather shows any degree of rhythmic pattern or just grouping into "good weather" and "bad weather" years, it is important to take this fact into consideration in agricultural planning. Any extrapolation of yields from the present situation must consider the effects of both weather and technology. If a physical explanation can be obtained for any grouping of the data or for any long-range forecast, even of a general nature, it would be extremely valuable. Even if all grouping of these years is due only to random factors, it cannot be completely ignored in agricultural planning. As will be brought out later we are enjoying a run of favorable years preceded by a run of unfavorable years. To project our recent trends of corn yields could lead to serious problems of food supply should we find ourselves with no reserve of feed grains at the end of favorable years and in a severe drought year.

Statistical techniques can be used to examine the periodicity and persistence of weather. One approach is to see if the runs of years depart from a random model. For example, let us use red, white and blue balls drawn from a fishbowl. The red can be dry, the white average, and the blue wet years, each occurring one third of the time. Probability theory can be used to show how often a dry year will follow another dry year, or any other choice you may select.

During the mid-50's the climatology group at Iowa State had been frequently asked the question, "Does weather persist?" The climatologists were not considering day to day persistence but only long-range persistence of say, year to year, or growing season to growing season. A random study was made for Iowa, both for individual stations and for the state average for a long series of years, for the total rainfall for the growing season, April through September. The data for Iowa were not significantly different from a random model. Dry years followed dry years as frequently as the random data model predicted. This would indicate that no real weather persistence occurred in the total precipitation, April through September.
Figure 1. Fluctuations and trends in corn yields in Missouri, Illinois, and Iowa from 1890 to 1962.
The relationship between spring and fall weather was also examined, with no significant departure from the random model. The random model indicates that dry weather follows dry weather; in fact, it predicts it will occur with a certain probability. Once we have had a dry year (using the three groups above), there is a 1/3 chance of the next year being dry, and a 1/9 chance of the next two years being dry. When this technique was applied to Kansas data, dry years did seem to persist. More recently the question of randomness of weather has again been raised for Iowa conditions because of the occurrence of a run of favorable years from 1957 to 1963. The run of favorable years from 1937 to 1943 might have been regarded as a random occurrence, but to experience another run of favorable years with peak years nearly 20 years apart causes one to raise questions about randomness of weather in Iowa or in the midwest.

One additional point should be made here. There may be a difference between persistence in the weather and persistence in crop yields. In many areas producing grain, a low rainfall year may tend to create a "dry" crop year the next year, due to little carryover of soil moisture. Even with average rainfall the next year, it may seem like a dry crop year. In 1963 we had an excellent crop year in Iowa, partially because we had a good carryover of soil moisture from August, 1962. There definitely may be persistence from year to year in crop yields because of this carryover of soil moisture, which has helped carry the crop through dry periods and produce our high yields.

Figures 2 and 3 show relationships between July rainfall, July temperature and solar activity, where sunspots numbers have been shown with the minor maximum data plotted negatively. Only July rainfall and July temperature are shown, because they are so important in corn production and because they reflect the summer weather conditions. The year-to-year variation in July weather is rather striking in Figure 2. The three-year moving average shown in Figure 3 indicates some correlation with solar activity.

Willett (6) has stated that severe droughts in the midwest tend to occur during the period leading up to each major sunspot maximum. These major maxima have occurred in 1895, 1917, 1937 and 1957. Although a period may be very dry, a very wet year might occur within the dry period. The relations of this type, that is weather and solar activity, should only be used for general periods, not for individual years. But if any relationships can be found which allow us to predict that a period will be generally dry, or wet, this prediction could have important implications in farm program planning. In Figure 3 dry July's are shown near each of these years of sunspot maxima: 1891 and 1894, 1913 and 1916, 1934, 1935 and 1936, and 1954 and 1955. However, not all dry years were near the major maxima, and within some of these periods wet years occurred, for example 1896 and 1915.
Figure 2. Average July precipitation and July temperature for Kansas and yearly solar activity, 1892-1963.

Figure 3. 3-year averages of July precipitation and July temperature for Kansas and yearly solar activity, 1892-1963.
Figures 2 and 3 also show the strong negative relation frequently found between temperature and precipitation at a station. This is evident in several of the extreme years, but not always present. Figure 4 shows the fluctuations in summer weather in the six-state area including Nebraska, Kansas, Oklahoma, Iowa, Missouri and Arkansas. The correlation with solar activity is less apparent but the correlation with crop yield fluctuations shown in Figure 1 is rather striking. The low yields of 1894 and 1901 coincided with low summer precipitation. The 1910-1914 period showed decreasing summer rainfall; so did the early thirties, and generally favorable summer rainfall has occurred in recent years. And perhaps just as important is the fact that summer temperatures have been favorable in recent years.

The "areawise" relationship of weather is of importance in considering any farm program, particularly as it relates to the concentration of a crop within a smaller area. The more the acreage is concentrated the more susceptible the total production is to weather variability. If spread over a large area, good and bad weather may tend to average out in each year. When the acreage is concentrated, the entire area may be either "good" or "bad." This is something we know relatively little about at the present time--primarily because little effort has been expended on this problem. For example, is there an inverse relation between weather in the midwest and the east coast? The relationship between North Carolina and Nebraska is shown in Figure 5. An inverse relationship for July precipitation is indicated over much of the period. In the next figure, 6, the relationship appears quite evident when we look at cool season precipitation for 1963-1964. This shows considerable areas with the same pattern. This factor should definitely be considered in planning any concentration of the crop within an area if we want to stabilize production.

**Yields and Weather Fluctuations**

Now, the third question which was proposed, "What is the relationship between yields and weather fluctuations and what have crop yields been like in recent years?" Since we have looked at corn, let us examine soybeans and grain sorghums. (Figures 7 and 8.)

If we examine the soybean yields we see a frequent grouping of years above or below the trend line for average weather (5). One point of interest in this figure is that a line connecting the higher yields would be almost parallel to the trend line shown. Sorghum yields show a much more rapid increase due to newer hybrids introduced, greater use of fertilizers, and other factors along with a change to more favorable weather (4). Again there is a grouping of years above and below the trend line. A trend line greatly in error could easily have been projected from these data had the period 1934 to 1944 been used for projection. Certainly a trend line established for the period 1950 to 1960 would be just as misleading.
Figure 4. Average monthly precipitation and monthly temperature, June - August for 6 midwest states and yearly solar activity, 1892-1963.

Figure 5. Average July precipitation for North Carolina and Nebraska, 1892-1963.
Figure 6. Soil moisture accumulation in the U. S. (September 1963–March 1964) based on precipitation data.
Fig. 7. Actual and Calculated Yields of Soybeans in Illinois.
Fig. 8. Actual and Calculated Yields of Grain Sorghums for Kansas.
After looking at state and regional data it might be desirable to look at Story County, Iowa, which is just about in the center of the Corn Belt.

If we examine yields of corn in Story County in detail, several interesting facts appear. Figure 9 shows that through 1935 there appeared to be a gradual upward trend in yields. This was probably due to a number of miscellaneous management factors. The trend line shown for the period 1881 to 1945 was computed by Barger (1). In only two years of that period were yields over 10 bushels above the trend line. In six years yields were 10 bushels or more lower than the trend line. With the relatively low yields obtained, weather could fluctuate over a considerable range with relatively small effects on yield, although it is believed to have had a definite effect on yield patterns. With good weather, yields were limited by the relatively low level of technology. Extremely bad weather did depress yields. However, yields fluctuated around the trend line with little grouping of years—the longest consecutive period above or below normal was three years. Studies such as those made by Rose (3) showed low correlation between weather and corn yields in the heart of the Corn Belt, but much higher correlations in the climatically marginal areas of the Corn Belt. This was probably a very realistic picture for that period.

From 1936 to 1940 in Iowa the percent of hybrid corn grown increased from 10 percent to more than 90 percent. From 1937 through 1943 there were seven consecutive years with yields above this trend line. Is the trend line wrong? We don't think so. This series of favorable years is also shown by Dale (2) in a study of moisture stress days. The method for determining moisture stress days was developed on experimental plot yields where fertility—technology was held constant. The years 1937 and 1938 were not particularly favorable or unfavorable years. There may have been higher yields in 1937 because of a carryover effect of fertility from 1936, a very dry year. The years 1939 through 1943 were all increasingly better weather years, with a large number of non-stress days. Multiple regression studies for Iowa also confirm the idea of a run of favorable years from 1939 to 1943 (5). Ten of the next 13 years, from 1944 through 1956, were below a trend line extended at the same slope as that prior to 1935, but taking into account the effect of hybrid corn. The three high yielding years 1946, 1948 and 1952 all had a large number of non-stress days. For the other years with a large number of non-stress days certain weather factors depressed yields. In 1945, severe frost damage occurred; 1951 had a cool spring and early frost. The years 1944 and 1950 had low numbers of stress days, but both years had late cool springs, which were detrimental to corn yields. The years 1947, 1949, 1953, 1954, 1955 and 1956 all had large numbers of stress days.

In 1956, Story County yields for corn harvested were 43 bushels per acre. Actually, this is a biased yield. About 1/3 of the corn in the county went into the "Soil Bank" after the dry weather occurred—and nearly all of this was the poorest corn. If this would have averaged 10 bushels (and much of it would have produced nothing), the Story County yield would have been 32 bushels per acre.
Using Dale's stress day index, certain years are compared below.

<table>
<thead>
<tr>
<th>Year</th>
<th>Story County Yield</th>
<th>Non stress Days</th>
</tr>
</thead>
<tbody>
<tr>
<td>1934</td>
<td>22 bu/acre</td>
<td>10</td>
</tr>
<tr>
<td>1936</td>
<td>26</td>
<td>15</td>
</tr>
<tr>
<td>1947</td>
<td>30</td>
<td>25</td>
</tr>
<tr>
<td>1956</td>
<td>32 (as revised)</td>
<td>17</td>
</tr>
<tr>
<td>1942</td>
<td>65</td>
<td>55</td>
</tr>
<tr>
<td>1962</td>
<td>83</td>
<td>55</td>
</tr>
</tbody>
</table>

Low yields were associated with few non-stress days or a low weather index. In 1947, distribution of moisture was particularly bad—13.5 inches in June, then practically no rain during the remainder of the summer. In unfavorable weather years good management has not allowed us to produce much more than just ordinary management. In moderate weather years we may have done better, because of deeper rooting from fertilizers. However, if this moisture is removed from the lower subsoil one year, it may not be replenished by the beginning of the next growing season. In favorable weather years we have improved yields considerably by good management. Starting in 1957, all years have had a medium to high weather index and yields have been high.

Comparing 1942 with 1962 shows a yield increase in Story County of 18 bushels, an average increase of 0.90 bushels per year between these two favorable weather years. Thompson (5) has shown an average increase, using a linear trend adjusted for weather of 0.70 bushels per year for the state. This seems like very good agreement when one considers that Thompson's study was for the entire period from 1930 to 1962.

Figure 9 shows a trend line from 1941 to 1962 fitted by the least squares method. This line is too steep because of the fact that weather was generally more favorable in the last half than in the first half of the period. Nevertheless, it is significant that a line drawn from the peak yield of 1942 to the peak yield of 1962 would result in a line about parallel with the least squares trend line. Certainly a trend line fitted to the period 1950 to 1960 would result in a very steep slope because of the unfavorable years in the early and mid-50's followed by a run of favorable years after 1956.

In summary, the data presented in this paper show (1) great variation from year to year in weather and crop production (2) a close resemblance of crop and weather variation (3) a tendency for fluctuations to occur in such a manner as to cause alternation of groups of favorable and unfavorable years (4) a similarity of weather patterns in neighboring states of the midwest but dissimilarity of weather patterns when comparing the midwest to the Atlantic coastal area and (5) weather in the midwest which has been very favorable to crops since 1956, giving rise, therefore, to a steep trend in crop production from 1950 to 1960.
Bibliography


CHANGES IN MOISTURE STRESS DAYS SINCE 1933

Robert F. Dale

Introduction

Dr. Shaw mentioned a possible upward bias in the Story County average corn yield in 1956 stemming from the soil bank program. Besides any such farm program or assessment effects upon the county and state corn yield statistics, there are many other and probably more serious confounding effects when we use area average yields and weather variables to estimate the weather, technology and crop relations. While those empirical studies may be "in the right ball park" in estimating the relative effects of weather and technology, it is difficult to correctly evaluate or visualize the results of such studies. The yield series represents an areal average of all technological factors -- changes in residual soil fertility, differences in fertilizer use, hybrid varieties, crop densities, mechanization and perhaps even an increase in supplemental irrigation.

We also average the weather variables over time and space. Under the same crop densities, soil moisture is always higher on the low-lying or bottom lands than on the more droughty hills. As a general rule, night-time temperatures are lower in the valleys than on hills. The slope and aspect bring in important differences in micro-climate. Therefore, we cannot consider the average weather variables and their relation to crop yields as other than an index for the area. To infer from such an empirical study that "so many inches of rainfall will result in so many bushels of corn" is only a little more reasonable than claiming a man standing with one foot in boiling water and the other in ice-water is comfortable at an average water temperature of 50°C.

If we could obtain significant crop-weather relations on an experimental plot basis this would help efforts to isolate the weather and technology effects. We then could build these relations into a more meaningful area picture. Stallings (6) and Auer1 have already used the experimental

1/Central area climatologist, U. S. Weather Bureau, Iowa State University, Ames, Iowa.
2/See the following paper of this report, Ludwig Auer and Earl O. Heady, "The Contribution of Weather and Yield Technology to Changes in U.S. Corn Production, 1939 to 1961."
plot yields as a weather or phenological index in estimating area crop yields. A search for an appropriate weather variable to estimate experimental plot yields would carry this another step further.

This is not the only reason for seeking meaningful weather-experimental plot yield relations. The results of all agricultural research are conditioned by the weather regime under which the research was performed. Usually it is assumed that by replicating the experiment over a number of years and at selected field stations the environmental effects will average out, leaving the experimental results representative of the general area or soil unit. Unfortunately, average weather seldom occurs and the average experimental results are the integrated response to a wide range of conditions.

If we eliminate any economic considerations, we can consider the corn yield potential as being controlled by five factors: (1) weather, (2) soil fertility and physical conditions, (3) genetic differences between varieties, (4) population or geometry of planting and (5) miscellaneous biological or environmental occurrences. To evaluate the yield effect from any one of these five factors, the effects from the other four have to be considered. In the work reported in this paper (1), the plot corn yields from the Iowa State University Agronomy farm four-year rotation experiment were used to examine the effect of weather on corn yields by either removing or evaluating the effects from the three technological factors. The purpose of the four-year rotation experiment from the date of its establishment in 1917 has been to study the long-term effects of those rotation and fertility practices and corn varieties used by most Iowa farmers. Thus, we might consider that the technology on the four-year rotation experimental plots might be something near the average in the central Iowa crop reporting district with the important exception that there were no commercial fertilizer applications on the experimental plots used in this analysis.

**Experimental Procedure**

We attempted to hold the soil fertility effect (2) fairly constant by analyzing the weather effects on yields within the same treatment. In this paper we shall discuss the results from only one treatment, plot 01 which received 8 tons of manure once every four years. Plot 01 is 1/10 acre in size, and the treatment was not replicated. There was no change in the manure application over the period of record, although there may have been temporary or accumulated changes in residual soil fertility. The yield date (3) were adjusted for improvement in hybrids by means of overlapping varietal corn yield test comparisons, with all yields adjusted to the equivalent of those for Iowa hybrid 4570, the hybrid variety used last in the four-year rotation experiment. The effect of stand (4) was considered as an interaction by
including it with the weather variable in a multiple regression model. There is little we can do with the miscellaneous biological and environmental effects (5) until the first four effects are properly evaluated. This fifth factor was not considered and contributes to the variance from the regression yield estimates.

Should one wonder as to the need to spend this time "homogenizing" the experimental plot yield or dependent data series, the estimated differences in yields between hybrid varieties used over the 30-year period, 1933-1962, on plot 01 are shown in Table 1.

The different varieties used over the 30-year period of record are shown in the left-hand column, the period in which each was grown in the second column and the estimated yield increase over the previous variety grown in the third column. The accumulated correction which was added to the respective corn yields to adjust to those for Iowa Hybrid 4570 is shown in the right-hand column. These estimates were constructed from differences between varieties in randomized replicated corn yield tests in the same fields and years. To eliminate possible differences in varietal yields due to differences in stand, only those yield differences between corn varieties with stand percentages within 5 percent were used. These overlapping comparisons were necessary since there was no single variety grown throughout the period which could be used as a standard against which to compare yields.

The initial increase from the average of the open pollinated varieties to the first hybrid used, Iowa 942, was 8.2 bushels per acre. However, the yield increase due to improvement in hybrids was estimated to be about nine bushels per acre from Iowa 942 to Iowa 4570. This undoubtedly contributes toward a steeper technology trend since 1940 than before as indicated in the previous paper by Dr. Shaw. While there may be weather-varietal yield interactions, we believe most of the improvement in yields due to improvement in varieties has been removed in the adjusted data series.

A scatter diagram of plot 01 yields (adjusted) on stands, shown in Figure 1, indicates the great variability in the experimental plot stands from year-to-year. The stands on plot 01 ranged from less than 5,000 plants per acre in 1935 to more than 18,000 plants in 1961. Before 1953, stands averaged about 7,500 plants per acre, since then about 15,000. If we disregard the open circles, which represent years in which weather is believed to have been the primary limiting factor, the scatter diagram shows a general yield increase with stand. The effect of stand on yield depends on the weather, and stand was included with the weather variable in a multiple regression model.
**Figure 1.** Ames Agronomy Farm 4-year rotation plot 01 corn yield on stand, 1933-1962.
Table 1. **Corn varieties and years planted at the Ames Agronomy Farm** 
four-year rotation plots with estimated yield differences between 
indicated variety and Iowa 4570.

<table>
<thead>
<tr>
<th>Corn variety&lt;br&gt;planted</th>
<th>Period &lt;br&gt;(years)</th>
<th>Average increase&lt;br&gt;in yield between &lt;br&gt;listed hybrid over &lt;br&gt;next lower identified hybrid, bushels&lt;br&gt;per acre</th>
<th>Bushels to be added to yields in indicated periods for equivalent Iowa 4570 yield</th>
</tr>
</thead>
<tbody>
<tr>
<td>Iowa 4570</td>
<td>1957-62</td>
<td>+3.4</td>
<td>0</td>
</tr>
<tr>
<td>Iowa 4298</td>
<td>1951-56</td>
<td>+2.4</td>
<td>9.0</td>
</tr>
<tr>
<td>Iowa 306</td>
<td>1942-50</td>
<td>+1.2</td>
<td>5.8</td>
</tr>
<tr>
<td>Iowa 939</td>
<td>1940-41</td>
<td>+2.0 (over Iowa 942)</td>
<td>+7.0</td>
</tr>
<tr>
<td>&quot;Double-Double Hybrid&quot;</td>
<td>1939</td>
<td></td>
<td>+7.0</td>
</tr>
<tr>
<td>&quot;Double-Double Hybrid&quot;</td>
<td>1938</td>
<td></td>
<td>+7.0</td>
</tr>
<tr>
<td>Iowa 939 x US 13</td>
<td>1937</td>
<td></td>
<td>+7.0</td>
</tr>
<tr>
<td>Mixture 11 hybrids</td>
<td>1936</td>
<td></td>
<td>+9.0</td>
</tr>
<tr>
<td>Iowa 942</td>
<td>1935</td>
<td>+8.2</td>
<td>+9.0</td>
</tr>
<tr>
<td>(open pollinated)</td>
<td>1934</td>
<td></td>
<td>+17.2</td>
</tr>
<tr>
<td>Murphy strain (open pollinated)</td>
<td>1933</td>
<td></td>
<td>+17.2</td>
</tr>
</tbody>
</table>

*Pedigrees: Iowa 4570 (B14 x WP9) (187-2 x M14)  
Iowa 4298 (Os420 x 187-2) (WF-9 x M14)  
Iowa 306 (L289 x I205) (WF9 x Os426)  
Iowa 939 (L289 x I205) (Os420 x Os426)  
Iowa 942 (I234 x L289) (Os420 x Os426)*

If we want to study the effect of weather on corn we need to consider the weather with respect to the corn or phenological calendar. Dates of corn silking were not available for the experimental plots, but average silking dates for the county, district and state have been recorded by the U.S. Weather Bureau and State Department of Agriculture (Iowa) since 1926. Fortunately, there is very little variation in dates of silking over the state within years. The state average date was close to the central crop reporting district average and was considered the best estimate of the experimental plot silking date. In Figure 2 the average state planting dates are shown as the bottom curve, and the average date of 75 percent corn silked in the "main" fields in the top curve. There is considerable variation in silking dates between years, with a range of three weeks between the earliest date, July 22, 1939, and the latest date, August 12, 1945. The average planting dates show less variation. Since the weather
in the vicinity of silking is commonly considered to be most critical to the corn crop, the 75 percent silking date was used to "anchor" the phenological calendar, and the weather variables were integrated within various periods from date of silking.

**Moisture Stress Concept**

Using the phenological calendar and the adjusted experimental plot corn yields, we examined several different weather variables for their yield-estimating ability. We shall describe only one in this paper -- moisture stress day concept. This is illustrated in Figure 3, which is from the moisture stress experiment by Denmead and Shaw (3). Each of the points on the chart represents a different potometer in which four corn plants were growing at different soil moisture levels on three different atmospheric energy level days. The measured soil moisture content in percent of the soil volume is shown on the abscissa. The amount of transpiration or millimeters of soil water lost in 24 hours from each potometer is shown on the ordinate. The soil in the potometers had a field capacity near 36 percent soil moisture and a 15-atmosphere or permanent wilting point near 22 percent. August 5, or the lower curve, is an example of a low energy or low moisture demand day. The day was overcast and humid. The corn in all of the potometers transpired about 1.5 mm. that day regardless of the soil moisture content, which in the different potometers ranged from field capacity to 23 percent, almost to the wilting point. Where soil moisture was below 23 percent on that day there was no longer sufficient soil moisture to maintain transpiration even at the low energy level. On August 13, a partly cloudy day, transpiration was about 4.5 mm. in 24 hours in the potometers where soil moisture ranged from field capacity to about 28 percent. Where soil moisture was below 28 percent transpiration decreased rapidly with decreasing soil moisture. On July 30, a clear, dry sunny day, the transpiration was 6.5 mm. in 24 hours with soil moisture at field capacity, but transpiration was decreased where soil moisture was just slightly below field capacity. Denmead and Shaw called the "break" in the curve the "turgor loss point," where the plant cells lose turgor and the stomates begin to close. It is this point which has been used to separate moisture stress from non-stress days.

From such curves, Denmead and Shaw prepared a curve of the estimated turgor loss points. The curve is shown in Figure 4. The abscissa has been converted to inches of evapotranspiration at field capacity in 24 hours. The ordinate has been scaled to percent of available field capacity in the corn root zone i.e., 0 is the 15-atmosphere or permanent wilting point, and 100 percent is field capacity. If the soil moisture profile holds 80 percent of the available field capacity, the corn would not be under moisture stress if evapotranspiration at field capacity
Figure 3. Actual transpiration rate as a function of soil moisture content.
Figure 4. Estimated percent available field capacity in the corn root zone at the turgor loss point, $\theta_{TL}$, as a function of the evapotranspiration at field capacity. Solid curve is from Demmead and Shaw, and Dashed curve is adjusted to 5 mm aggregate Nicollet silt loam by soil moisture curve from Tamboli.
were less than .23 inch in 24 hours. But a potential evapotranspiration greater than .23 inch would indicate a stress day. Any combination of points falling below the curve would identify stress days, and points falling on or above the curve would identify non-stress days. Since cell turgidity is necessary for growth it was assumed that there would be little or no growth on a stress day, and that corn yields should be directly proportional to the number of non-stress days.

Two estimates were needed to classify a day as one with moisture stress or no stress: the moisture supply or soil moisture in the corn root zone, and the atmospheric demand for moisture or potential evapotranspiration for each day. The method of estimating these two variables will not be given here except to indicate that the potential ET was estimated from evaporation pan measurements (4), and the soil moisture was estimated as described by Shaw (5) (2).

The seasonal march of soil moisture is shown in Figure 5 for three years. Calendar date is on the abscissa and percent of available soil moisture in the corn root zone on the ordinate. The corn root zone includes only the top 6 inches at planting time and is gradually increased in depth to 5 feet by August 1. The lower curve is for 1954, a year in which several gravimetric measurements were available through the season to check the accuracy of the daily soil moisture estimates. The solid line represents the estimates, and the squares are the gravimetric measurements. The estimated and measured soil moisture shows good agreement. The open circles are the estimates of the percent available soil moisture needed in the corn root zone to prevent moisture stress in corn. These estimates were plotted only when they were above the available soil moisture curve, indicating moisture stress days. The top curve shows the soil moisture regime in 1958, the most favorable year "moisture stress-wise" of any in the last 30 years. Soil moisture remained near field capacity through July, and there were only two moisture stress days the entire season. The middle curve is that for 1956, the driest year on plot 01 since 1936, with moisture stress almost every day in July and August.

Moisture Stress and Experimental Plot Corn Yields

A scatter diagram of the adjusted plot 01 corn yields on the number of non-stress days in the period six weeks before silking to three weeks after silking is shown in Figure 6. The maximum number of non-stress days, or most favorable season moisture stress-wise, cannot exceed 63 days, the number of days in the nine-week period, six weeks before to three weeks after silking. We have indicated there were two general stand levels in the 30-year record studied. The open circles are the years
Figure 5. Estimates of daily percent available soil moisture in corn root zone for the seasons of 1954, 1956 and 1958 at Ames, Iowa, on well-drained soils having an available field capacity of 8.7 inches.
Figure 6. Agronomy Farm 4-year rotation plot of corn yield on number of non-stress days in 9-week period 6B-3A.
prior to 1953 with stands of less than 10,000 plants per acre. There are no open circles above 100 bushels per acre, even though there were some very favorable years moisture stress-wise. The solid circles indicate the years since 1953 with higher stands, at least 12,000 plants per acre. These higher stands allowed the plot 01 corn yields to exceed 100 bushels with favorable weather. In 1956, a year with higher stands, the plot corn yield was about the same as in 1934 and 1936, with approximately the same kind of weather, moisture stress-wise.

When there were less than 30 non-stress days, the moisture-stress effect seemed to exert the major control on yields; there was a linear relationship between the plot 01 corn yields and the number of non-stress days. Above 30 NSD, stand became of increasing importance, but up to about 40 NSD moisture stress still seemed to limit yields. For example, 1953, 1954 and 1955 were in the higher stand period, but the yields for these years were still below 100 bushels. Above 40 NSD there was increasing scatter, indicating that stand and possibly some weather effect other than moisture stress was the limiting factor. There is some evidence of a curvilinear effect which would indicate that some stress may be favorable, perhaps in encouraging deeper root penetration and greater proliferation to more fully exploit the available soil fertility.

The stress or non-stress day is a non-dimensional variable which only identifies whether or not the corn plant was under moisture stress. An entire season of cloudy, cool weather, even with relatively dry soil, might produce no moisture stress conditions, but there would be a lack of energy for crop growth. To correct for this deficiency, an energy index -- a summation of potential evapotranspiration on non-stress days -- was investigated. However, it was found associated with about the same amount of variation in the plot 01 corn yields as found with the non-stress day variable described here. In a solar radiation limiting area it would be expected to do better.

Several multiple regression models were used, but in this paper we shall discuss only one, using the 30-year period of record, 1933-1962. The variables used were yield on plot 01 in bushels per acre, the last two digits of year (to consider any linear trend in residual soil fertility), stand in number of corn plants per acre, stand$^2$, NSD or the number of non-stress days in the nine-week period, 6B - 3A, NSD$^2$, and the interaction stand $\times$ NSD. The regression equation:

$$Y_{01} = -4.3 - 1.32 \text{year} + 0.78 \text{stand} - 0.0041 \text{stand}^2 + 3.93 \text{NSD} - 0.0500 \text{NSD}^2 + 0.0125 \text{stand} \times \text{NSD}$$
was associated with 83 percent of the variance in the adjusted plot 01 yields. The F value of 18.95 was highly significant for 6 and 23 degrees of freedom. The partial regression coefficients for NSD and NSD$^2$ were significant to the .01 level. Trend and the stand X NSD interaction coefficients were significant at the .05 level. Stand and stand$^2$ coefficients were not significant. The negative trend coefficient of 1.32 bushels per acre per year estimates the average residual fertility decrease over the last 30 years under the plot 01 technology of 8 tons of manure every four years. A word of caution: the partial regression coefficient for trend, or the estimate of residual fertility loss, is only as good as the selection of the other variables in the equation. But some such multiple regression technique of considering the weather and other technological factors is necessary to evaluate this residual fertility trend.

The regression plot 01 yield estimates on NSD, evaluated for 1962 and three different stand levels are shown in Figure 7. As might be expected, the fitted multiple regression curves indicate the same pattern as the scatter diagram. The estimated yields for the three stand levels of 8, 12 and 16,000 plants per acre are much the same for nine-week seasons with less than 30 NSD. The corn yield differences between stand levels increase between 30 and 40 NSD, but below about 40 NSD there is still no difference between stands of 12,000 and 16,000. Here again, then, it appears that below 30 NSD under the plot 01 technology (as well as on plots with higher manure applications not shown here) it is the moisture stress variable which is limiting yields. Between 30 and 40 NSD the moisture stress becomes less important and stand becomes more important. Above about 40 NSD stand level is of major importance. The benefits of favorable weather cannot be realized unless stand (and other technological) levels are increased.

**Probabilities of Moisture Stress**

What are the probabilities of receiving less than 30 and 40 NSD in the nine-week period, 6B - 3A? An estimate of these probabilities at the Ames Agronomy Farm -- and probably in central Iowa on Nicollet or Clarion soils which hold about nine inches of available water in the top five feet of soil profile -- is shown in Figure 8. The number of non-stress days in the 63-day period, six weeks before silking to three weeks after silking, is shown on the abscissa. The percent chance of having the indicated number or less NSD for corn is shown on the ordinate, based on the 30-year period, 1933-1962. Assuming the past 30 years provides the best estimate of the weather regime over the next few years, the probability of having 30 or less NSD is about 28 percent, or about three in 10 years. The chance of having 40 or fewer NSD is 50 percent. Thus, moisture stress conditions would appear to be a limiting factor under the plot 01 technology in about half of the years.
Figure 7. Estimates Ames Agronomy Farm 4-year rotation plot 01 corn yields on number of non-stress days in 9-week period, 6B-3A, for stands of 8,000, 12,000 and 16,000 plants per acre evaluated for 1962.
Figure 8. Chance of having indicated number or less non-stress days for corn in the 9-week period, 6B-3A, on Nicollet silt loam with 9.0 inches available field capacity, Ames, Iowa based on 30-year record, 1933-1962.
Changes in Moisture Stress Conditions Since 1933

The actual (adjusted) plot 01 corn yields with the regression estimates are shown chronologically as the lower chart in Figure 9. Year is on the abscissa and the plot 01 corn yield in bushels per acre on the ordinate. The regression equation seems to fit the extremely low yields remarkably well. The yield trend is upward and is due to the resultant effect between stand increase and residual fertility decrease and the weather. We can see that it is primarily the 30's on the left end of the regression and the high yields since 1957 on the right end which probably provide the trend. The 20-year period from 1937 through 1956 would probably give us little trend in yields, if not even a negative trend.

In the top chart of Figure 9, the number of NSD's are shown for each year over the past 31 years. This chart shows roughly the same trend as the lower yield chart and is also primarily controlled by the dry 30's and favorable 1957-1962 period. Both curves show the estimate of trend depends upon the period selected, which I submit is due to weather "spells." We had some very favorable weather in the 40's, but the stands on plot 01 were not sufficient to realize the full benefit of this weather. The year 1963 has been included on the chart although it was not used in the regression. The important thing to note is that the last seven years have had above 40 NSD nine-week seasons. Thus, the lack of moisture-stress conditions was allowed the higher plot 01 stand levels their maximum effect these last seven years. The probability of getting another favorable year -- above 40 NSD -- is still 50-50, in fact, perhaps a little better now for 1964 with our full soil moisture profile. But one of these years we can expect a below 40 NSD season, which would not allow the higher stand levels to be fully effective.

While the precipitation variability over Iowa and time makes it extremely dangerous to compare moisture stress computations for one station, Ames, against the Iowa state average corn yield -- and we do not advocate its use -- a scatter diagram of the average Iowa corn yields on the estimated Ames NSD is shown in Figure 10. The state average corn yields are uncorrected for hybrids. The improvement in hybrids, increased use of fertilizer, increased plant populations and all other technological factors are included in the yield series. Yet, the scatter diagram shows much the same pattern as found in Figure 6, but of course, with less slope or increase in average state corn yield with NSD. If we disregard the state average corn yield in 1956 (although this was used in computed regression) or use Dr. Shaw's revised estimate of 32 bushels from Story County from the previous paper, the regression of yield on NSD appears almost linear up to about 40 NSD. Above 40 NSD
Figure 9. Lower solid line is estimated corn yield for Agronomy Farm 4-year rotation plot 01; open circles are the observed plot 01 corn yields, adjusted to hybrid Iowa 4570; dashed line is the linear yield trend. Upper solid line is the number of non-stress days in the 9-week period, 6B-3A, for each year, 1933-1962, at Ames, Iowa; dashed line is linear NSD trend.
Figure 10. Iowa state average corn yields on number of non-stress days in 9-week period 6B-3A at Ames, Iowa, 1933-1962.
there is increasing scatter, some of which is due to our single station estimate of the number of non-stress days in each season and some undoubtedly to an increase in technology. The regression equation shown in the figure was associated with 74 percent of the variance in the average state corn yield series from 1933-1962.

The estimate of the trend for technology of +0.72 bushels per acre per year was significant to the .01 level. This compares favorably with Dr. Thompson's estimated increase of +0.70 and Dr. Shaw's +0.86 figure provided in the previous paper. The two regressions drawn on Figure 10 are the evaluation of the regression equation for 1933 and 1962, the first and last years of the period of record used in computing the regression. Since there is no interaction term included in the regression model, the technology increase is averaged through all years, and I believe it incorrectly shows that the state average yields would be higher for low NSD in 1962 than 1933.

Thus, a total of more than 40 NSD (at Ames) also appears to be necessary to realize the benefits from the increase in technology on a state basis. Undoubtedly it is technology which has produced the steep upward trend in yields the last few years, but this increase due to technology is only possible because of the above 40 NSD weather we have enjoyed the last seven years. We cannot expect this favorable weather to continue indefinitely any more than we can expect to continue throwing heads on tossing a coin merely because we have just had a run of seven heads.
Bibliography


THE CONTRIBUTION OF WEATHER AND YIELD TECHNOLOGY TO CHANGES IN U.S. CORN PRODUCTION, 1939 TO 1961

Ludwig Auer¹ and Earl O. Heady²

This paper is part of a study initiated to impute changes in U.S. crop production to resource inputs and advances in technology. Weather is only one of many variables which relates to inter-year changes in production. However, it is a productive input which must be included in the analysis if the agricultural production function, or changes in it, is to be predicted. While the study deals with wheat, oats, barley, flax, soybeans, grain sorghum and corn, we shall report results for corn only. Space and time limitations prevent a more complete summary here, but methods outlined for corn also indicate the general procedures used for other crops.

Since the major purpose of the study was to impute changes in production over the last two decades, the concept of a time series production function was employed. Hence, from time series data we estimated production functions for each of the crops mentioned in each of the states which provide the major portion of the nation's supply of these crops. Ideally, the agricultural production function could be reviewed as in equation (1) where \( Y \) is output and

\[
Y = f(X_1, X_2, \ldots, X_g, X_{g+1}, \ldots, X_h, X_{h+1}, \ldots, X_n)
\]

\( X_1 \ldots X_h \ldots X_n \) are the inputs or variables which produce it. There are, of course, hundreds of specific input categories and may be represented as land of particular quality by \( X_1 \), labor in one month and of one quality by \( X_{10} \), crop variety by \( X_{15} \), etc.

In addition to these, there is a specific category denoted by \( X_{g+1} \) through \( X_h \) which may represent weather variables, e.g., temperature on June 1, rainfall on September 10 and humidity on July 4, etc. Similarly, variables \( X_{h+1} \) through \( X_n \) may represent other specific input categories. While this production function exists, data are not available for predicting it. We have, therefore, been forced into aggregating a large number of specific inputs or variables into "over-all" input classes. We use a

¹/Assistant agricultural economist at the University of Hawaii, formerly research associate at Iowa State University.

²/Professor of economics, "Charles F. Curtiss distinguished professor in agriculture" and executive director, Center for Agricultural and Economic Development, Iowa State University.
somewhat naive variable to represent weather, since emphasis on this source of inter-year change in production is less than for other variables considered. Our other variables on input categories also represent extreme aggregations, but are those which are consistent with the data and resources of the study which is yet in its first stages of implementation.

For example, we have not attempted to measure labor and machinery inputs or to predict their isolated effects on production. While timeliness of operations and cultural improvements represented by various machine-labor technologies do affect yield per acre, we handle them only in the time or trend variable which represents aggregation of a mass of effects. The other variables, employed here, grouped technologies into manageable categories. The procedures used for constructing these variables, while providing some actual basis in measurement, were difficult and need further refinement.

We use a simple algebraic form in estimation, a power function, because of its convenience and utility in application of our model. It is possible that other forms are equally or more appropriate as the study is extended and better measurements devised. The production function in all cases is of the form (2) where the variables, explained with reference to

$$ Y = bV^f W^w A^a T^t e $$

corn later, have the following meaning: $Y$ is yield per acre of each particular crop and state, $V$ is an index of crop varieties grown by farmers, $F$ is application rate of fertilizer, $A$ is an index of crop acreage, $W$ is a weather index, $T$ is a "catch all" variable to represent other aspects of technology and $e$ is an estimate of the error term.

In estimating this technological production function for the various crops and states, it became obvious that multicollinearity was great among certain variables. While singular moment matrices did not occur, regression coefficients estimated by least-squares methods were highly unstable in some instances. Given trends in technology over the last two decades, multicollinearity was especially high among the variety index, fertilization rates and time. Therefore, the effects (coefficients) of the variety and fertilizer variables were derived separately and then incorporated into the production function estimated by least-squares methods for acreage, weather and time variables. Methods of deriving the technological production functions by combining the estimated effects of variety improvement and fertilization rates with the predicted effects of the other variables will be explained after discussion of the yield variables.
Yield Variables

Time series observations for the three variables -- weather, variety index and fertilization rate -- are not available from any published source of annual statistics. Hence, they were estimated by the methods outlined below.

Corn hybrid index. Corn hybrid indices were developed to measure annual yield changes due to (a) hybrids versus open-pollinated varieties and (b) replacing older hybrids by higher yielding ones. They were estimated by the following procedure: Available experiment station test data were grouped by regions within states according to corn maturity groups or crop districts. Data sets of each region were summarized in terms of yield data of open-pollinated corn varieties, corn check hybrids and average yields of all hybrid entries. Test yields of check hybrids of earlier periods of testing were compared to open-pollinated varieties. For later periods, after discontinuation of open-pollinated varieties, yields of successive check hybrids were compared. Yield comparisons of check hybrids were then used to estimate the yield superiority of all hybrid entries relative to open-pollinated corn varieties. The degree of yield superiority was expressed in terms of relative corn hybrid test yields, a yield ratio which advanced over time due to gradual replacement of older hybrids by newer and higher yielding corn hybrids. Relative corn hybrid test yields were computed for each year and location. These yields were aggregated by relative corn acreages of the corn testing or crop reporting districts. Acreage weights rather than production weights were employed because they were rarely affected by yield variations and quite stable over time. State corn hybrid indices were then computed by combining relative corn hybrid test yields with adoption rates of hybrid corn.

Iowa state corn hybrid indices illustrate the estimation procedures. First, yield ratios between all corn hybrid entries and open-pollinated varieties were computed and aggregated over corn test districts. Then a time trend line was fitted to aggregated yield ratios so that annual variations were "smoothed out" for index computations. These relative corn test yields for Iowa are shown in Figure 1. Hybrids exceeded open-pollinated varieties by about 10 percent up to 1930 and by over 40 percent in 1960. The adoption curve of hybrids and the estimated Iowa corn hybrid index are shown in Figures 2 and 3. The Iowa corn hybrid index climbed most rapidly during the late thirties but continued to advance during the forties and fifties as newer hybrids were adopted.

Hybrid indices were estimated for 15 other states on the basis of over 50,000 corn yield tests. They differed between states depending on progress in hybridization and rates of adoption, as shown in Table 1. These
Estimated Yield Ratio of Hybrid Corn over Open-Pollinated Corn in IOWA

Figure 1. Relative corn test yields, Iowa, 1926-61.

Percentage of IOWA Corn Acreage Planted with Hybrid Seed Corn

Figure 2. Adoption of hybrid corn, Iowa, 1926-61.

Figure 3. Estimated corn hybrid index, Iowa, 1926-61.
Table 1. Estimated corn hybrid indices by states, 1939 to 1961.

<table>
<thead>
<tr>
<th>Year</th>
<th>Lake States</th>
<th>Corn Belt</th>
<th>Northern Plains</th>
<th>Southern Plains</th>
<th>Delta States</th>
</tr>
</thead>
<tbody>
<tr>
<td>1939</td>
<td>.85</td>
<td>.83</td>
<td>.80</td>
<td>.80</td>
<td>.88</td>
</tr>
<tr>
<td>1940</td>
<td>.88</td>
<td>.86</td>
<td>.82</td>
<td>.83</td>
<td>.92</td>
</tr>
<tr>
<td>1941</td>
<td>.91</td>
<td>.89</td>
<td>.87</td>
<td>.87</td>
<td>.94</td>
</tr>
<tr>
<td>1942</td>
<td>.94</td>
<td>.91</td>
<td>.90</td>
<td>.90</td>
<td>.96</td>
</tr>
<tr>
<td>1943</td>
<td>.95</td>
<td>.93</td>
<td>.92</td>
<td>.93</td>
<td>.96</td>
</tr>
<tr>
<td>1944</td>
<td>.96</td>
<td>.94</td>
<td>.94</td>
<td>.95</td>
<td>.97</td>
</tr>
<tr>
<td>1945</td>
<td>.97</td>
<td>.96</td>
<td>.96</td>
<td>.97</td>
<td>.98</td>
</tr>
<tr>
<td>1946</td>
<td>.98</td>
<td>.98</td>
<td>.98</td>
<td>.98</td>
<td>.98</td>
</tr>
<tr>
<td>1948</td>
<td>1.00</td>
<td>1.00</td>
<td>1.00</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>1949</td>
<td>1.01</td>
<td>1.01</td>
<td>1.01</td>
<td>1.01</td>
<td>1.01</td>
</tr>
<tr>
<td>1950</td>
<td>1.02</td>
<td>1.03</td>
<td>1.02</td>
<td>1.02</td>
<td>1.01</td>
</tr>
<tr>
<td>1951</td>
<td>1.03</td>
<td>1.04</td>
<td>1.03</td>
<td>1.02</td>
<td>1.02</td>
</tr>
<tr>
<td>1952</td>
<td>1.04</td>
<td>1.05</td>
<td>1.04</td>
<td>1.03</td>
<td>1.03</td>
</tr>
<tr>
<td>1953</td>
<td>1.05</td>
<td>1.06</td>
<td>1.05</td>
<td>1.03</td>
<td>1.04</td>
</tr>
<tr>
<td>1954</td>
<td>1.05</td>
<td>1.07</td>
<td>1.06</td>
<td>1.04</td>
<td>1.05</td>
</tr>
<tr>
<td>1955</td>
<td>1.06</td>
<td>1.08</td>
<td>1.07</td>
<td>1.05</td>
<td>1.06</td>
</tr>
<tr>
<td>1956</td>
<td>1.07</td>
<td>1.09</td>
<td>1.08</td>
<td>1.06</td>
<td>1.07</td>
</tr>
<tr>
<td>1957</td>
<td>1.08</td>
<td>1.10</td>
<td>1.09</td>
<td>1.06</td>
<td>1.08</td>
</tr>
<tr>
<td>1958</td>
<td>1.08</td>
<td>1.12</td>
<td>1.11</td>
<td>1.07</td>
<td>1.18</td>
</tr>
<tr>
<td>1959</td>
<td>1.09</td>
<td>1.13</td>
<td>1.11</td>
<td>1.07</td>
<td>1.09</td>
</tr>
<tr>
<td>1960</td>
<td>1.10</td>
<td>1.14</td>
<td>1.12</td>
<td>1.08</td>
<td>1.10</td>
</tr>
<tr>
<td>1961</td>
<td>1.10</td>
<td>1.15</td>
<td>1.13</td>
<td>1.08</td>
<td>1.11</td>
</tr>
</tbody>
</table>
indices, computed as for Iowa in Figure 3, were deflated by 1947-1949 base values.  

**Fertilizer.** United States fertilizer consumption has more than doubled since 1945; in terms of plant nutrients it increased from 2.6 million tons in 1945 to 7.4 million tons in 1960. The use of more fertilizer cannot be ignored in a study of corn yield technology. To estimate the impact of increased fertilizer application on state corn yields it was necessary to (a) quantify annual application rates of plant nutrients to corn by states, and (b) estimate the corresponding yield response.

**Fertilizer application.** Estimation of annual application rates proceeded in two steps. First, survey estimates of application rates to individual crops were adjusted to conform to estimates of annual total state consumption of the same years. Secondly, fertilizer application rates were estimated for the intervening years in accordance with: (a) long run changes in application rates of each nutrient and each crop, (b) short run changes in annual acreage of each crop and (c) annual changes in total consumption of each nutrient. A summary of estimated nutrient application of corn is presented in Table 2 for 16 states of five corn producing regions.

**Fertilizer response.** Regression estimates of fertilizer response were computed on the basis of data collected by the National Soil and Fertilizer Research Committee and published by the U.S. Department of Agriculture in 1954 (10). This data collection consisted of a nationwide summary of state fertilizer response. All yield data were converted to relative yield response before regressions were fitted. Relative yield response is defined by equation (3) where crop yield $Y_i$ is functionally related to nutrient rate $X_i$, $Y_0$ is the base yield attained without fertilizer and $b$ is the exponent to be estimated.

$$Y_i/Y_0 = (X_i + 1.0)^b$$

Without fertilizer, crop yield $Y_i$ equals base yield $Y_0$ and the relative yield response $Y_i/Y_0$ equals one. A relative yield response of 1.10 implies that fertilizer application $X_i$, raises yield $Y_i$, 10 percent above base yield $Y_0$. To estimate $b$, logarithms of the $Y_i/Y_0$ ratios were regressed linearly on logarithms of the coded application rates ($X_i + 1.0$) by method of least squares.

**Indices of Corn Belt States** were quite uniform in 1939 with the exception of Missouri, where hybrids were adopted somewhat later. In other regions they started out at similar levels but North Dakota and Mississippi yields of hybrid corn barely exceeded yields of open-pollinated varieties in early years.

**Estimates of total fertilizer consumption have been published by the U.S. Department of Agriculture annually since 1930 in terms of principal nutrients, i.e., nitrogen, phosphoric oxide and potash, by states. Survey estimates of nutrient application to individual crops have been published at irregular intervals by the U.S. Department of Agriculture and the National Fertilizer Association in earlier years (See Bibliography reference Nos. 1 to 9 incl.)
Table 2. Estimated application rates of primary plant nutrients to corn in pounds per acre by states, 1939 to 1961.

<table>
<thead>
<tr>
<th>Year</th>
<th>Lake States</th>
<th>Corn Belt</th>
<th>Northern Plains</th>
<th>Southern Plains</th>
<th>Delta States</th>
</tr>
</thead>
<tbody>
<tr>
<td>1939</td>
<td>.6</td>
<td>5.3</td>
<td>6.3</td>
<td>1.1</td>
<td>.5</td>
</tr>
<tr>
<td>1940</td>
<td>.9</td>
<td>8.0</td>
<td>7.5</td>
<td>1.4</td>
<td>.6</td>
</tr>
<tr>
<td>1941</td>
<td>1.5</td>
<td>10.3</td>
<td>9.4</td>
<td>1.6</td>
<td>.9</td>
</tr>
<tr>
<td>1942</td>
<td>1.9</td>
<td>21.0</td>
<td>13.4</td>
<td>2.2</td>
<td>1.4</td>
</tr>
<tr>
<td>1943</td>
<td>2.0</td>
<td>13.9</td>
<td>12.4</td>
<td>2.6</td>
<td>1.7</td>
</tr>
<tr>
<td>1944</td>
<td>2.7</td>
<td>15.3</td>
<td>13.1</td>
<td>3.0</td>
<td>2.0</td>
</tr>
<tr>
<td>1945</td>
<td>3.5</td>
<td>18.1</td>
<td>13.8</td>
<td>3.9</td>
<td>3.3</td>
</tr>
<tr>
<td>1946</td>
<td>6.2</td>
<td>22.3</td>
<td>15.0</td>
<td>5.1</td>
<td>4.0</td>
</tr>
<tr>
<td>1947</td>
<td>6.1</td>
<td>26.5</td>
<td>15.7</td>
<td>7.9</td>
<td>5.0</td>
</tr>
<tr>
<td>1948</td>
<td>8.7</td>
<td>32.4</td>
<td>14.6</td>
<td>8.4</td>
<td>6.2</td>
</tr>
<tr>
<td>1949</td>
<td>10.6</td>
<td>30.9</td>
<td>16.1</td>
<td>10.5</td>
<td>7.7</td>
</tr>
<tr>
<td>1950</td>
<td>10.4</td>
<td>31.2</td>
<td>17.1</td>
<td>12.5</td>
<td>7.4</td>
</tr>
<tr>
<td>1951</td>
<td>11.3</td>
<td>40.9</td>
<td>26.4</td>
<td>28.5</td>
<td>11.1</td>
</tr>
<tr>
<td>1952</td>
<td>14.2</td>
<td>44.5</td>
<td>37.1</td>
<td>36.3</td>
<td>13.6</td>
</tr>
<tr>
<td>1953</td>
<td>17.7</td>
<td>49.7</td>
<td>47.7</td>
<td>45.4</td>
<td>19.9</td>
</tr>
<tr>
<td>1954</td>
<td>24.5</td>
<td>57.8</td>
<td>58.0</td>
<td>50.9</td>
<td>29.9</td>
</tr>
<tr>
<td>1955</td>
<td>31.2</td>
<td>59.2</td>
<td>67.7</td>
<td>47.6</td>
<td>30.3</td>
</tr>
<tr>
<td>1956</td>
<td>33.2</td>
<td>60.3</td>
<td>72.7</td>
<td>51.9</td>
<td>26.1</td>
</tr>
<tr>
<td>1957</td>
<td>40.2</td>
<td>65.9</td>
<td>82.7</td>
<td>63.3</td>
<td>28.2</td>
</tr>
<tr>
<td>1958</td>
<td>43.5</td>
<td>70.1</td>
<td>84.1</td>
<td>65.2</td>
<td>35.1</td>
</tr>
<tr>
<td>1959</td>
<td>47.7</td>
<td>74.9</td>
<td>95.7</td>
<td>70.4</td>
<td>40.1</td>
</tr>
<tr>
<td>1960</td>
<td>47.1</td>
<td>70.2</td>
<td>94.8</td>
<td>71.2</td>
<td>41.8</td>
</tr>
<tr>
<td>1961</td>
<td>56.5</td>
<td>79.5</td>
<td>111.0</td>
<td>94.0</td>
<td>57.5</td>
</tr>
</tbody>
</table>

-- Less than .05 pounds per acre.
The procedure of estimating fertilizer response coefficients may be illustrated by use of Indiana corn yield data. Data in Table 3 represent Indiana corn yields with varying rates of N-P-K application. The

Table 3. Corn yields with varying rates of N, P, and K application, Indiana, 1950.

<table>
<thead>
<tr>
<th>Yield bushels</th>
<th>N-rate pounds</th>
<th>Yield bushels</th>
<th>P-rate pounds</th>
<th>Yield bushels</th>
<th>K-rate pounds</th>
</tr>
</thead>
<tbody>
<tr>
<td>52.4</td>
<td>0</td>
<td>43.4</td>
<td>0</td>
<td>57.8</td>
<td>0</td>
</tr>
<tr>
<td>55.3</td>
<td>3.0</td>
<td>50.0</td>
<td>10.0</td>
<td>63.2</td>
<td>9.5</td>
</tr>
<tr>
<td>55.9</td>
<td>4.5</td>
<td>61.6</td>
<td>11.0</td>
<td>64.0</td>
<td>10.0</td>
</tr>
<tr>
<td>56.5</td>
<td>5.4</td>
<td>65.8</td>
<td>16.5</td>
<td>65.3</td>
<td>14.2</td>
</tr>
<tr>
<td>57.0</td>
<td>6.0</td>
<td>68.6</td>
<td>19.8</td>
<td>67.3</td>
<td>17.1</td>
</tr>
<tr>
<td>57.2</td>
<td>6.6</td>
<td>70.0</td>
<td>20.0</td>
<td>68.0</td>
<td>19.0</td>
</tr>
<tr>
<td>57.6</td>
<td>7.5</td>
<td>70.0</td>
<td>22.0</td>
<td>69.0</td>
<td>20.0</td>
</tr>
<tr>
<td>58.1</td>
<td>9.0</td>
<td>70.7</td>
<td>24.2</td>
<td>68.7</td>
<td>20.9</td>
</tr>
<tr>
<td>59.0</td>
<td>10.0</td>
<td>72.8</td>
<td>27.5</td>
<td>69.4</td>
<td>23.8</td>
</tr>
<tr>
<td>59.3</td>
<td>12.0</td>
<td>74.2</td>
<td>33.0</td>
<td>71.4</td>
<td>28.5</td>
</tr>
<tr>
<td>62.1</td>
<td>18.0</td>
<td>77.0</td>
<td>40.0</td>
<td>74.8</td>
<td>38.0</td>
</tr>
<tr>
<td>63.0</td>
<td>20.0</td>
<td>77.7</td>
<td>44.0</td>
<td>76.0</td>
<td>40.0</td>
</tr>
<tr>
<td>68.0</td>
<td>40.0</td>
<td>81.2</td>
<td>66.0</td>
<td>78.2</td>
<td>57.0</td>
</tr>
<tr>
<td>78.0</td>
<td>80.0</td>
<td>82.0</td>
<td>80.0</td>
<td>81.0</td>
<td>80.0</td>
</tr>
<tr>
<td>82.0</td>
<td>120.0</td>
<td>a</td>
<td>120.0</td>
<td>82.0</td>
<td>120.0</td>
</tr>
</tbody>
</table>

*aYield estimate not available. Source: (10, p. 32).

estimated single nutrient response functions are represented by equations (4), (5) and (6) where N, P and K refer to application of nitrogen, phosphoric oxide and potash; \( Y_n \), \( Y_p \) and \( Y_k \) denote the corresponding estimates of corn yields. These functions had \( r^2 \) values of .92, .99 and .97 respectively. Estimates for other states had \( r^2 \) values ranging from .80 to .99.

Single nutrient response functions required adjustments before they could be used as estimates of combined nutrient response. Exponents in functions (4), (5) and (6) were valid provided response to application of any one nutrient was not limited by lack of other nutrients. However, in practice farmers applied fertilizer mixtures containing two or three nutrients because response to any one nutrient was often limited by lack of others. Functions for combined nutrient response were computed according to (7)
where \( \frac{Y_{npk}}{Y_0} \) represents relative yield response to application of nutrient

\[
(7) \quad \frac{Y_{npk}}{Y_0} = (N + 1.0) \left(\frac{n^2}{n+p+k}\right) (P + 1.0) \left(\frac{p^2}{n+p+k}\right) (K + 1.0) \left(\frac{k^2}{n+p+k}\right)
\]

mix, N, P and K are application rates of nitrogen, phosphoric oxide and potash as before and \( n, p, k \) are the corresponding exponents, e.g., .068, .150 and .064 as in equations (4), (5) and (6) above. Adjustments of the exponents were made such that corn yield response to a nutrient mix could be neither smaller nor greater than response to any one nutrient. Adjusted response coefficients will be presented later together with other coefficients of state corn production functions.

**Corn weather index.** Annual corn weather indices, estimated here for the purpose of measuring weather inputs, were based on phenological data. They were not derived from climatic variables such as monthly rainfall and precipitation data of individual states but from corn test yields conducted by each state at a number of locations.

In computing state corn weather indices, annual average yields of hybrid tests were aggregated by weighting according to relative corn acreages of test districts. These weights were usually based on 10- to 20-year averages of corn acreages, but in some cases they were based on much shorter periods due to lack of data. Linear trend lines were fitted to aggregated corn test yields and annual average corn test yields were divided by estimated trend values. It was assumed that corn test yields advanced over time at a constant rate due to a gradual increase in fertilizer application, replacement of older corn hybrids by newly developed, higher yielding hybrids and improvements in other cultural practices. This assumption of a constant rate of yield change on test plots did not imply that corn yields of the state advanced at a constant rate. Using corn yield test plot data assured that yield effects of statewide changes in fertilization practices, in corn acreage and government programs were not confounded with state corn weather indices. Nevertheless, weather index computations could have benefited from further refinements, but lack of data and scope of study prevented it.

A summary of state corn weather indices is presented in Table 4 for 16 corn states. Due to insufficient data, weather indices for some states could not be estimated for earlier years. As an example, Iowa corn test yields and the trend line are shown in Figure 4. Extending over the years 1926 to 1961, these were based on annual average corn test yields of 12 test districts in Iowa. The corresponding annual Iowa corn weather index is shown in Figure 5. It was computed from ratios of annual corn test yields over trend line values. Evidently Iowa corn test yields fluctuated over time, but percentage deviations from the trend line have diminished, a characteristic that seems to be reflected all across the Corn Belt as illustrated by weather index values in Table 4. How much of the recent increase in yields and decrease in yield fluctuations was due to advances in crop yield technology can be measured only by estimating simultaneously the effects of all crop yield variables.
Figure 4. Corn test yields and trend yields, Iowa, 1926-61.

Figure 5. Estimated corn weather index, Iowa, 1926-61.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1939</td>
<td>1.39</td>
<td>1.19</td>
<td>1.21</td>
<td>1.08</td>
<td>1.23</td>
<td>1.13</td>
<td>1.25</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1940</td>
<td>1.09</td>
<td>1.04</td>
<td>1.06</td>
<td>1.08</td>
<td>1.07</td>
<td>1.03</td>
<td>1.04</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1941</td>
<td>1.11</td>
<td>1.05</td>
<td>1.13</td>
<td>1.16</td>
<td>1.12</td>
<td>1.21</td>
<td>1.16</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1942</td>
<td>1.06</td>
<td>1.11</td>
<td>1.08</td>
<td>1.16</td>
<td>1.15</td>
<td>1.14</td>
<td>1.17</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1943</td>
<td>1.05</td>
<td>1.04</td>
<td>1.10</td>
<td>1.09</td>
<td>1.12</td>
<td>1.12</td>
<td>1.17</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1944</td>
<td>1.15</td>
<td>1.10</td>
<td>1.14</td>
<td>1.16</td>
<td>1.17</td>
<td>1.18</td>
<td>1.18</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1945</td>
<td>1.05</td>
<td>0.89</td>
<td>1.06</td>
<td>1.08</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1946</td>
<td>1.05</td>
<td>0.84</td>
<td>1.06</td>
<td>1.08</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1947</td>
<td>0.81</td>
<td>0.89</td>
<td>1.08</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1948</td>
<td>0.73</td>
<td>0.92</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1949</td>
<td>0.76</td>
<td>0.94</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1950</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1951</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1952</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1953</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1954</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1955</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1956</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1957</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1958</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1959</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td>1960</td>
<td>0.76</td>
<td>1.05</td>
<td>1.12</td>
<td>1.23</td>
<td>1.21</td>
<td>1.21</td>
<td>1.21</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
</tbody>
</table>

--- Not available.  
* Iowa values.
Crop acreage index. To measure the effects of acreage on state corn yields, a state corn acreage index was devised. It measured annual corn acreage relative to the trend. Annual corn acreage indices were estimated by fitting linear time trend lines to harvested state corn acreages over the years 1939 to 1960. Acreage indices, expressing annual harvested acreages relative to trend acreages, may have been confounded with other variables. For example, abandonment of planted acreage is generally greater as weather is less favorable. Or if the price of the crop is high enough, farmers may expand acreage and apply more fertilizer at the same time. But, as in the case of many other time series data it was not possible to identify and isolate these factors.

Other crop yield variables. Aside from major forces like weather, hybridization, fertilizer application and perhaps annual acreage variations, there are other variables that affect state corn yields. Herbicides, better drainage, timeliness of operation, expansion of irrigated acreage and other improvements of cultural practices are examples. These were measured in their net effects only by including a time-trend variable (the last two digits of the year) in the production functions.

Production Function Analysis

Production functions have been estimated in many problem areas of agriculture, ranging from farms to crops and livestock (11). A basic difficulty in this study is: Important variables of corn yield technology are highly correlated over time. Variables relating to fertilizer, hybrid seeds, drainage and irrigation have all advanced simultaneously. Consequently the problem of multicollinearity in statistical estimation arises. The problem was overcome by selecting a production function model that permitted incorporating yield effects of crop varieties and fertilization estimated separately. Postulating the existence of such a functional form imposed undesirable rigidities on the analysis but was effective in eliminating certain problems of multicollinearity.

Algebraic form. To conform with previous models the functional form (8) was used where Y is annual state corn yield, b_0 is a constant, H is the corn hybrid index; N, P and K are rates of application of nitrogen, phosphoric oxide and potash, and n', p', k' are the corresponding exponents, while W, A and T denote respectively weather index, acreage index and net time trend variable. The reason for using this production function is as follows: If we assume that the hybrid index is at its 1947-49 level, that no fertilizer is

\[ Y = b_0 H^{1.0} (N+1.0)^{n'} (P+1.0)^{p'} (K+1.0)^{k'} W^w A^a T^t \]

The nutrient exponent n', p', k' are equivalent to exponents \( n^2/(n+p+k) \), \( p^2/(n+p+k) \), \( k^2/(n+p+k) \) in formula (7) above.
applied, and that weather and state corn acreage are "normal," then equation (8) above reduces to (9), where the constant term $b_0$ is multiplied by $T_0^t$, the product being $Y_0$. This value can be interpreted as the base yield of corn.

(9) $Y_o = b_0 T_0^t$

If fertilizer is applied, $Y_o$ becomes larger as $(N+1.0)^n'$, $(P+1.0)^p'$ and $(K+1.0)^k'$ change from unity to values greater than 1.0. A shift in the whole production function might be caused by changes in corn hybrid index, the acreage index or the time trend variable.

**Statistical estimation.** State corn production functions were estimated by least squares methods after modification of the state yield data. For the purpose of estimation, the state crop production function was re-arranged according to equation (10), which is identical to (8) above except for the error term $e$, and the fact that annual state corn yields $Y$, were

(10) $Y/(1.0(N+1.0)^n'(P+1.0)^p'(K+1.0)^k') = b_0 W^W A^a T^t e$

deflated by the annual crop variety index and the estimated yield response to application of plant nutrients. Without this modification, the regression estimates would have been unstable and caused distortions in the production functions. Variables of technological change were highly correlated, as illustrated by the frequency distribution for 13 states in Table 5.

**Table 5.** Frequency distribution of absolute values of simple correlation coefficients between corn yield variables of 13 states.

<table>
<thead>
<tr>
<th>Absolute values of correlation coefficients</th>
</tr>
</thead>
<tbody>
<tr>
<td>.00-</td>
</tr>
<tr>
<td>.09</td>
</tr>
</tbody>
</table>

- Weather index, Hybrid index
- Weather index, NPK application
- Weather index, Acreage index
- Weather index, Time trend
- Acreage index, Hybrid index
- Acreage index, NPK application
- Acreage index, Time trend

| **Frequency** | 44  | 29  | 10  | 5   | 2   | 1   | 0   | 0   | 10  | 29  |

** Tested statistically different from zero at the one percent level.
* Tested statistically different from zero at the five percent level.
Since the correlation between corn hybrid indices, rates of fertilizer application and the time trend variable were high compared to correlation among other variables, the adverse effects of multicollinearity on regression estimates were lessened by using estimating equation (10). The effects of corn hybrid improvement and N-P-K application were estimated separately before fitting the function to the data. By separating these two variables from the time trend variable the latter is used to measure the remaining time trend effects and is referred to as "net time trend" variable.

Estimated regression coefficients of 16 state corn production functions are shown in Table 6 by major agricultural production regions. The base yields, \( Y_0 \), are listed in the first column of this table. They represent the 1947-1949 yields excluding effects of fertilizer and assuming "normal" weather and acreage conditions. They are equivalent to \( Y_0 \) values in formula (9) above and suggest yield due to natural soil fertility. Nutrient response coefficients, estimated as explained above in (7), are shown in columns 2, 3 and 4 for nitrogen, phosphoric oxide and potash respectively. Exponents of weather variables in column 5 were all positive and tested statistically significant at the one percent level. They did, however, vary considerably, ranging from .340 to 1.203. Most of the exponents were smaller than 1.0 which implied, as expected, that yield variances on experimental station plots were larger than variances in state yields. Exponents of acreage indices in column 6 were not consistent in terms of sign. Ordinarily, it could be expected that there is a "normal acreage" best suited for a particular crop. As acreage is expanded beyond this acreage, yields should decline as crops are grown on less suited areas. Hence, a negative acreage exponent would be expected. Acreage exponents of Northern regions, e.g., Lake States and North Dakota, followed this pattern but exponents of other regions did not. Even though not statistically significant, they were mostly positive.

Other crop yield variables, measured aggregatively by the net time trend variable, appeared to exert a positive yield effect over time in most cases as indicated in column 7. Among negative coefficients (exponents) only the trend coefficient of North Dakota tested statistically significant at the five percent level. A negative coefficient meant a decline in yields under normal acreage and weather conditions after yield effects of fertilization and hybridization were taken into account. Was this a result of overestimating the impact of fertilization and corn hybrid improvement or did other variables really exert a negative yield effect? Two factors seem to indicate that there was in fact a negative net yield trend. First, the estimated yield effects of fertilizer and variety improvement were small compared to those of other states. Second, advance in corn hybrid indices was exceptionally slow. Consequently, overestimation of these variables was unlikely. Moreover, analysis of three other crops, i.e., oats, barley and flax, revealed the same negative trends.6

---

6/Details of results of other crops will be discussed in a forthcoming Iowa Experimental Station Bulletin.
Table 6. Characteristics of estimated state corn production functions by states.

<table>
<thead>
<tr>
<th>Region and state</th>
<th>Years</th>
<th>Base yield $Y_0$</th>
<th>Plant nutrients $n'$, $p'$, $k'$</th>
<th>Weather $w$</th>
<th>Acreage $a$</th>
<th>Other $t$</th>
<th>$R^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Lake States</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Minnesota</td>
<td>37-61</td>
<td>43.4</td>
<td>.038</td>
<td>.012</td>
<td>.003</td>
<td>.484**</td>
<td>-.622</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>37-61</td>
<td>37.4</td>
<td>.029</td>
<td>.035</td>
<td>.051</td>
<td>.340**</td>
<td>-.365</td>
</tr>
<tr>
<td>Michigan</td>
<td>38-61</td>
<td>38.8</td>
<td>.014</td>
<td>.009</td>
<td>.013</td>
<td>.749**</td>
<td>-.622</td>
</tr>
<tr>
<td>Corn Belt</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Missouri</td>
<td>37-61</td>
<td>31.5</td>
<td>.070</td>
<td>.027</td>
<td>.004</td>
<td>1.203**</td>
<td>-.124</td>
</tr>
<tr>
<td>Iowa</td>
<td>26-61</td>
<td>51.1</td>
<td>.013</td>
<td>.007</td>
<td>.003</td>
<td>1.185**</td>
<td>.150</td>
</tr>
<tr>
<td>Illinois</td>
<td>34-61</td>
<td>47.7</td>
<td>.042</td>
<td>.014</td>
<td>.013</td>
<td>.870**</td>
<td>.260</td>
</tr>
<tr>
<td>Indiana</td>
<td>37-61</td>
<td>38.6</td>
<td>.016</td>
<td>.080</td>
<td>.015</td>
<td>.486**</td>
<td>-.346</td>
</tr>
<tr>
<td>Ohio</td>
<td>39-61</td>
<td>37.6</td>
<td>.037</td>
<td>.085</td>
<td>.007</td>
<td>.569**</td>
<td>.167</td>
</tr>
<tr>
<td>Northern Plains</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N. Dakota</td>
<td>44-61</td>
<td>21.4</td>
<td>.051</td>
<td>.009</td>
<td>.000</td>
<td>.895**</td>
<td>-.833</td>
</tr>
<tr>
<td>S. Dakota</td>
<td>37-61</td>
<td>24.4</td>
<td>.049</td>
<td>.022</td>
<td>.000</td>
<td>.695**</td>
<td>.907</td>
</tr>
<tr>
<td>Nebraska</td>
<td>37-61</td>
<td>26.8</td>
<td>.065</td>
<td>.000</td>
<td>.000</td>
<td>.849**</td>
<td>.185</td>
</tr>
<tr>
<td>Kansas</td>
<td>39-61</td>
<td>23.4</td>
<td>.118</td>
<td>.000</td>
<td>.005</td>
<td>.661**</td>
<td>.501</td>
</tr>
<tr>
<td>Southern Plains</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Oklahoma</td>
<td>43-61</td>
<td>18.1</td>
<td>.017</td>
<td>.012</td>
<td>.002</td>
<td>.350**</td>
<td>.123</td>
</tr>
<tr>
<td>Texas</td>
<td>41-61</td>
<td>16.3</td>
<td>.055</td>
<td>.014</td>
<td>.001</td>
<td>.482**</td>
<td>.288</td>
</tr>
<tr>
<td>Delta States</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Arkansas</td>
<td>42-61</td>
<td>14.7</td>
<td>.092</td>
<td>.007</td>
<td>.048</td>
<td>.505**</td>
<td>.317</td>
</tr>
<tr>
<td>Mississippi</td>
<td>39-61</td>
<td>12.1</td>
<td>.140</td>
<td>.004</td>
<td>.001</td>
<td>.482**</td>
<td>.133</td>
</tr>
</tbody>
</table>

a Coefficients predetermined, see text.
** Tested statistically significant at the one percent level.
* Tested statistically significant at the five percent level.
+ Tested statistically significant at the ten percent level.
Alternative estimates. Before the final set of crop yield variables was chosen, alternative forms were tested. In this context only the choice of weather variables is of further interest.

Two weather variables. Previously, it was illustrated how annual weather variations might cause shifts in corn production functions. Phenological state weather indices were derived from corn yield data for a number of test locations in each state. The estimated exponents of these indices were always positive and usually smaller than unity. This means the functional relation between weather index \( W \) and state corn yield \( Y \) was not straight linear but curvilinear and monotonically increasing. If variances in state yields were generally smaller than at test locations a modified function might have been more appropriate.

Weather index coefficients, allowing for less than proportionate response of state yields, were estimated by replacing the weather index variable \( W \), in equation (10) above by two indices, a "bad weather index" \( W_b \), and a "good weather index" \( W_g \), as in equation (10a). These indices

\[
Y(H^{1.0}(N + 1.0)^n (P + 1.0)^{p'}(K + 1.0)^{k'}) = b_0 A^a W_b W_g T e
\]

were derived from the original index values \( W \) by equality \( W_b = 1.0 + (1.0 - W) \) whenever \( W < 1.0 \), and by leaving \( W \) unchanged whenever \( W \geq 1.0 \). If yield response was less than proportionate the coefficients (exponents) of \( W_b \) had to be negative and the coefficients of \( W_g \) positive. The estimated coefficients are shown in Table 6a and most of the weather coefficients are of the expected magnitudes. It is noteworthy that all "bad weather coefficients" tested statistically significant at the one percent level while most "good weather coefficients" did not. Other coefficients were similar to previous estimates with negative acreage coefficients for the Lake States, Missouri, Indiana and North Dakota, and negative net time trend coefficients for Minnesota, Wisconsin, Indiana and North Dakota as before. Coefficients of nutrient response were identical. As a result, there was only a slight improvement in multiple correlation coefficients and therefore the earlier estimates were accepted. What must be remembered, however, is that yield response to more favorable weather conditions could not be clearly identified.

"Predetermined" weather indices. Our phenological weather indices were highly aggregative and not as detailed as Thompson's state weather analyses, (12). He employed as many as 17 weather variables and one time trend variable to estimate weather effects on state corn yields. While the same procedure could not be employed here,\(^7\) the relationship between Thompson's study of weather effects and this study of corn yield technology was tested. "Predetermined" weather indices, computed from Thompson's analysis by annual ratios of estimated over time trend yields, were inserted into equation (10) and regression estimates derived for Corn Belt states. The results of these computations were high multiple correlation coefficients, consistently negative coefficients of the net time trend variable, and

\(^7\) Selection of 17 weather variables and several variables of crop yield technology was not permissible here because of limited degrees of freedom.
Table 6a. Characteristics of estimated state corn production functions with two weather variables, by states.

<table>
<thead>
<tr>
<th>Region and State</th>
<th>Years</th>
<th>Weather</th>
<th>Acreage</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>$w_b$</td>
<td>$w_g$</td>
<td></td>
</tr>
<tr>
<td><strong>Lake States</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Minnesota</td>
<td>37-61</td>
<td>-.731**</td>
<td>.323</td>
<td>-.646**</td>
</tr>
<tr>
<td>Wisconsin</td>
<td>37-61</td>
<td>-.536**</td>
<td>.186</td>
<td>-.395</td>
</tr>
<tr>
<td>Michigan</td>
<td>38-61</td>
<td>-1.484**</td>
<td>-.042</td>
<td>-.211</td>
</tr>
<tr>
<td><strong>Corn Belt</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Missouri</td>
<td>37-61</td>
<td>-2.414**</td>
<td>.346</td>
<td>-.256</td>
</tr>
<tr>
<td>Iowa</td>
<td>26-61</td>
<td>-2.072**</td>
<td>.431</td>
<td>.140</td>
</tr>
<tr>
<td>Illinois</td>
<td>34-61</td>
<td>-1.304**</td>
<td>.643+</td>
<td>.220</td>
</tr>
<tr>
<td>Indiana</td>
<td>37-61</td>
<td>-.485**</td>
<td>.570**</td>
<td>-.330</td>
</tr>
<tr>
<td>Ohio</td>
<td>39-61</td>
<td>-.592**</td>
<td>.673**</td>
<td>.173</td>
</tr>
<tr>
<td><strong>Northern Plains</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N. Dakota</td>
<td>44-61</td>
<td>-.803**</td>
<td>1.231**</td>
<td>-.826**</td>
</tr>
<tr>
<td>S. Dakota</td>
<td>37-61</td>
<td>-1.411**</td>
<td>.290</td>
<td>.878</td>
</tr>
<tr>
<td>Nebraska</td>
<td>37-61</td>
<td>-1.267**</td>
<td>.743</td>
<td>.248</td>
</tr>
<tr>
<td>Kansas</td>
<td>39-61</td>
<td>-1.239**</td>
<td>.439</td>
<td>.424</td>
</tr>
<tr>
<td><strong>Delta States</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mississippi</td>
<td>43-61</td>
<td>-1.037**</td>
<td>.024</td>
<td>-.215</td>
</tr>
</tbody>
</table>

** Tested statistically significant at the one percent level.
* Tested statistically significant at the five percent level.
+ Tested statistically significant at the ten percent level.
coefficients (exponents) of weather indices close to unity, as shown in Table 6b. Correlation coefficients were higher than earlier values, an

Table 6b. Characteristics of estimated state corn production functions with "predetermined" weather variables, by States.

<table>
<thead>
<tr>
<th>Region and State</th>
<th>Years</th>
<th>Weather ( W_T )</th>
<th>Acreage ( a )</th>
<th>Other ( t )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Corn Belt</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Missouri</td>
<td>37-61</td>
<td>1.160**</td>
<td>-.169</td>
<td>-.640**</td>
</tr>
<tr>
<td>Iowa</td>
<td>30-61</td>
<td>1.021**</td>
<td>-.241</td>
<td>-.047</td>
</tr>
<tr>
<td>Illinois</td>
<td>34-61</td>
<td>1.003**</td>
<td>.145</td>
<td>-.086</td>
</tr>
<tr>
<td>Indiana</td>
<td>37-61</td>
<td>1.078**</td>
<td>.505**</td>
<td>-.189**</td>
</tr>
<tr>
<td>Ohio</td>
<td>39-61</td>
<td>.973**</td>
<td>-.016</td>
<td>-.058</td>
</tr>
</tbody>
</table>

** Tested statistically significant at the one percent level.

indication of the excellent fit of the Thompson functions. All net time trend variables were negative. This result could imply: (a) negative time trend yields caused by variables other than fertilization and hybridization, (b) overestimation of fertilizer response and hybridization in the present study, and/or (c) underestimation of yield technology in Thompson's study. It is unlikely that other variables caused a negative time trend because there is no evidence indicating existence of such trends. It is questionable that fertilizer response and hybridization were overestimated because there are indications of even greater yield effects, e.g., response to nitrogen application in Iowa. It is conceivable, however, that a single linear time trend variable was inadequate for estimating yield effects of crop yield technology. Whatever the implications, it was not permissible on conceptual grounds to derive a weather index from a secondary source, predicted on a linear time trend variable, and incorporate it in this analysis which assumed nonlinear yield effects. Therefore, all subsequent discussion is based on production functions presented earlier in Table 6.

Estimating the Contribution of Corn Yield Technology

The estimated state crop production functions provided the basis for measuring the contribution of corn yield technology to changes in state corn yields as well as aggregate corn production. In both cases the contribution of crop yield technology was measured on an annual basis as well as on a cumulative basis over the past two decades.
Yield technology and state corn production. In measuring the contribution of corn yield technology the analysis was directed at variables of corn hybridization, application rates of fertilizer and other variables affecting long-run corn yield trends. Short-run variations were considered later. By setting weather and acreage indices equal to 1.0 in (11), annual variations in both variables were ignored and corn yields estimated by this equation could be interpreted as "normal" state corn yields. Subscripts $j$ were added to denote annual values of crop yield variables. Equation (11) was simplified further by setting the three terms involving nutrients $N, P, K$ and exponents $n', p', k'$ equal to $F_j$ as in equations (12) and (13). Factor $F_j$ represented a ratio which measured nutrient response just like the corn hybrid index $V_j$ measured response to corn hybrid improvement.

(11) $Y_j = b_0 H_j \cdot (N_j + 1.0)^{n'} (P_j + 1.0)^{p'} (K_j + 1.0)^{k'} 1.0^{w} 1.0^{a} T_j^t$

(12) $F_j = (N_j + 1.0)^{n'} (P_j + 1.0)^{p'} (K_j + 1.0)^{k'}$

(13) $Y_j = b_0 H_j F_j T_j^t$

After this simplification the contribution of different corn technologies could be approximated by a first term Taylor expansion of equation (13), as in (14), where the annual change in corn yield from year $j$ to $j + 1$ was attributed to corn hybridization, change in fertilizer application and other crop yield technologists according to marginal productivities and the magnitude of change of each variable. Application of the Taylor expansion required that the function had finite and continuous partial derivatives of all orders and that the remainder term approached zero upon further expansion. In function (13) the exponents of $V_j$ and $F_j$ equalled 1.0. The first order (partial) derivatives were non-zero but derivatives of higher orders vanished. However, this did not impair approximation, because a first order expansion proved to be quite adequate. Annual changes in variables of crop yield technology were usually smaller than the original input level; hence the second condition could be met also. In order to change approximation (14) into an equality, values of individual terms were changed by the same percent, an adjustment that usually amounted to less than one percent of the annual change. Cumulative yields were computed by adding the annual changes of each variable to 1939 base yields.

Iowa corn yield data may serve as an empirical example of how the impact of crop yield technology was estimated for individual states. In Table 7 annual Iowa corn yields, annual corn hybrid indices and nutrient response values are shown in columns 1 to 3. Annual yield changes attributed to unspecified crop yield variables, corn hybridization and fertilizer use are shown in columns 4 to 6, and cumulative yields are listed in columns 7 to 9. All data were estimated by inserting annual values into equation (16),
<table>
<thead>
<tr>
<th>Year</th>
<th>Corn Yield (bu.)</th>
<th>Corn hybrid response index</th>
<th>Fertilizer (bu.)</th>
<th>Other corn technology (bu.)</th>
<th>Corn hybridization (bu.)</th>
<th>Fertilizer (bu.)</th>
<th>Other crop technology (bu.)</th>
<th>Corn hybridization (bu.)</th>
<th>Fertilizer (bu.)</th>
<th>Adjustment ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>1939</td>
<td>43.38</td>
<td>.88</td>
<td>1.00</td>
<td>.217</td>
<td>1.878</td>
<td>.006</td>
<td>37.59</td>
<td>43.29</td>
<td>43.38</td>
<td>1.00</td>
</tr>
<tr>
<td>1940</td>
<td>45.37</td>
<td>.92</td>
<td>1.00</td>
<td>.223</td>
<td>1.738</td>
<td>.028</td>
<td>37.81</td>
<td>45.25</td>
<td>45.37</td>
<td>1.00</td>
</tr>
<tr>
<td>1941</td>
<td>46.61</td>
<td>.94</td>
<td>1.00</td>
<td>.227</td>
<td>.949</td>
<td>.062</td>
<td>38.04</td>
<td>46.42</td>
<td>46.61</td>
<td>1.00</td>
</tr>
<tr>
<td>1942</td>
<td>47.50</td>
<td>.95</td>
<td>1.01</td>
<td>.227</td>
<td>.573</td>
<td>.094</td>
<td>38.26</td>
<td>47.22</td>
<td>47.50</td>
<td>1.00</td>
</tr>
<tr>
<td>1943</td>
<td>48.26</td>
<td>.96</td>
<td>1.01</td>
<td>.226</td>
<td>.499</td>
<td>.030</td>
<td>38.49</td>
<td>47.95</td>
<td>48.26</td>
<td>1.00</td>
</tr>
<tr>
<td>1944</td>
<td>48.98</td>
<td>.97</td>
<td>1.01</td>
<td>.224</td>
<td>.424</td>
<td>.071</td>
<td>38.71</td>
<td>48.60</td>
<td>48.98</td>
<td>1.00</td>
</tr>
<tr>
<td>1945</td>
<td>49.82</td>
<td>.98</td>
<td>1.01</td>
<td>.223</td>
<td>.467</td>
<td>.015</td>
<td>38.94</td>
<td>49.28</td>
<td>49.82</td>
<td>1.00</td>
</tr>
<tr>
<td>1946</td>
<td>50.61</td>
<td>.98</td>
<td>1.01</td>
<td>.221</td>
<td>.431</td>
<td>.140</td>
<td>39.16</td>
<td>49.94</td>
<td>50.61</td>
<td>1.00</td>
</tr>
<tr>
<td>1947</td>
<td>51.51</td>
<td>.99</td>
<td>1.02</td>
<td>.220</td>
<td>.434</td>
<td>.250</td>
<td>39.38</td>
<td>50.59</td>
<td>51.51</td>
<td>1.00</td>
</tr>
<tr>
<td>1948</td>
<td>52.33</td>
<td>1.00</td>
<td>1.02</td>
<td>.219</td>
<td>.398</td>
<td>.196</td>
<td>39.60</td>
<td>51.21</td>
<td>52.33</td>
<td>1.00</td>
</tr>
<tr>
<td>1949</td>
<td>53.19</td>
<td>1.01</td>
<td>1.03</td>
<td>.218</td>
<td>.442</td>
<td>.203</td>
<td>39.81</td>
<td>51.87</td>
<td>53.19</td>
<td>1.00</td>
</tr>
<tr>
<td>1950</td>
<td>53.80</td>
<td>1.02</td>
<td>1.03</td>
<td>.216</td>
<td>.443</td>
<td>-.051</td>
<td>40.03</td>
<td>52.53</td>
<td>53.80</td>
<td>1.00</td>
</tr>
<tr>
<td>1951</td>
<td>54.91</td>
<td>1.03</td>
<td>1.04</td>
<td>.216</td>
<td>.448</td>
<td>.450</td>
<td>40.25</td>
<td>53.19</td>
<td>54.91</td>
<td>1.01</td>
</tr>
<tr>
<td>1952</td>
<td>55.85</td>
<td>1.03</td>
<td>1.04</td>
<td>.216</td>
<td>.452</td>
<td>.266</td>
<td>40.46</td>
<td>53.86</td>
<td>55.85</td>
<td>1.00</td>
</tr>
<tr>
<td>1953</td>
<td>56.96</td>
<td>1.04</td>
<td>1.05</td>
<td>.215</td>
<td>.457</td>
<td>.443</td>
<td>40.68</td>
<td>54.53</td>
<td>56.96</td>
<td>1.00</td>
</tr>
<tr>
<td>1954</td>
<td>58.20</td>
<td>1.05</td>
<td>1.06</td>
<td>.215</td>
<td>.463</td>
<td>.559</td>
<td>40.89</td>
<td>55.21</td>
<td>58.20</td>
<td>1.00</td>
</tr>
<tr>
<td>1955</td>
<td>58.85</td>
<td>1.06</td>
<td>1.06</td>
<td>.215</td>
<td>.466</td>
<td>-.024</td>
<td>41.11</td>
<td>55.89</td>
<td>58.85</td>
<td>1.00</td>
</tr>
<tr>
<td>1956</td>
<td>59.26</td>
<td>1.07</td>
<td>1.05</td>
<td>.212</td>
<td>.464</td>
<td>-.275</td>
<td>41.32</td>
<td>56.57</td>
<td>59.26</td>
<td>.99</td>
</tr>
<tr>
<td>1957</td>
<td>60.06</td>
<td>1.08</td>
<td>1.06</td>
<td>.211</td>
<td>.468</td>
<td>.126</td>
<td>41.53</td>
<td>57.25</td>
<td>60.06</td>
<td>1.00</td>
</tr>
<tr>
<td>1958</td>
<td>61.07</td>
<td>1.08</td>
<td>1.06</td>
<td>.211</td>
<td>.472</td>
<td>.321</td>
<td>41.74</td>
<td>57.93</td>
<td>61.07</td>
<td>1.00</td>
</tr>
<tr>
<td>1959</td>
<td>61.95</td>
<td>1.09</td>
<td>1.06</td>
<td>.211</td>
<td>.475</td>
<td>.196</td>
<td>41.95</td>
<td>58.61</td>
<td>61.95</td>
<td>1.00</td>
</tr>
<tr>
<td>1960</td>
<td>62.72</td>
<td>1.10</td>
<td>1.07</td>
<td>.210</td>
<td>.478</td>
<td>.086</td>
<td>42.16</td>
<td>59.30</td>
<td>62.72</td>
<td>1.00</td>
</tr>
<tr>
<td>1961</td>
<td>63.91</td>
<td>1.11</td>
<td>1.07</td>
<td>.209</td>
<td>.482</td>
<td>.497</td>
<td>42.37</td>
<td>59.99</td>
<td>63.91</td>
<td>1.00</td>
</tr>
</tbody>
</table>
derived from its equivalent form (15). According to Table 7 (col. 1) "normal" Iowa corn yields increased from 43.39 bushels in 1939 to 63.91 bushels in 1960. Over the same time period corn hybrid indices advanced from .88 to 1.11 and fertilizer response values from 1.00 to 1.07 (col. 2 and

\[
(15) \quad Y_j = 23.63V_j \ (N+1.0)^{.013} \ (P+1.0)^{.007} \ (K+1.0)^{.003} \ A_j \ 1.185 \ T_{.199}
\]

\[
(16) \quad Y_j = 23.63V_jF_j \ T_j^{.199}
\]

col. 3). Annual corn hybrid indices increased year after year, particularly during the early period of adoption of hybrid corn.

Correspondingly the annual yield increase attributed to hybrid corn (col. 5) was high during earlier years, reaching almost 2.0 bushels in 1939. It declined later but still amounted to approximately .5 bushels per year. Yield changes caused by increased fertilizer use (col. 6) were exceptionally low during the 1940's but increased markedly in recent years. In three out of 23 years reductions in fertilizer use are estimated to have caused negative yield changes. These negative changes--in 1950, 1955 and 1956--were quite small except in 1956 when estimated change was .275 bushels per acre (almost three million bushels for the state).

The cumulative changes listed in columns 7, 8, 9 were computed by adding annual yield changes to the 1939 base yield of 43.29 bushels per acre, a level already raised from 37.56 bushels by hybrid corn. In 1961 the corresponding yield (without fertilizer application) was 59.99 bushels. Nearly 12 bushels \([(59.99-42.37) - (43.29-37.59) = 11.92]\) of this change was further attributed to hybrids. Another eight bushel yield increase was due to higher rates of fertilizer application. These and other variables of crop yield technology resulted in an estimated yield of almost 64 bushels, an increase of about 20 bushels per acre (63.91-43.39 = 20.53) between 1939 and 1961. Additional yield improvements in the more recent years 1958 to 1961, were largely attributed to favorable weather, a factor to be considered later. As mentioned earlier approximate values of annual yield change were changed by the same percent. The annual adjustment ratios are listed in column 10. Maximum adjustments amounted to plus or minus one percent but in 21 out of 23 years the adjustment was less than one percent of the annual yield change.

**Yield technology and aggregate production.** Estimation of the impact of yield technology on aggregate corn production necessitated explicit recognition of yield effects of regional specialization because corn yields differed between states and the pattern of state corn acreage changed significantly over the years.
Procedures for estimating the impact of regional specialization followed the principles of earlier analysis; only one additional variable, the relative state corn acreage, was required. This variable of regional specialization was incorporated in the analysis as follows: Aggregate corn yield $aY_j$ in year $j$ is defined by equation (17) as the sum of $m$ state crop yields $Y_{ij}$, each weighted by its relative state acreage $R_{ij}$. Relative state corn acreage is state corn acreage $A_{ij}$ divided by aggregate corn acreage, the sum of all $m$ state corn acreages in year $j$. The right hand side of equality (17) is equivalent to the sum of $m$ state crop production functions. Application of a first term Taylor expansion to (17), after the notation of change is simplified as in (18), yields an approximation of change in aggregate yield $\Delta aY_j$ as indicated by expression (19). Individual terms of summation (19) are made up of partial derivatives of aggregate corn yield, which is equal to partial derivatives of state corn yield multiplied by $R_{ij}$, as in (20). This is because aggregate corn yields are composed of state corn yields weighted by their relative state corn acreage $R_{ij}$ as shown earlier in (17). Thus annual aggregate corn yield change was attributed to four variables of yield technology: corn hybrid adoption and improvement, fertilizer use, other variables of corn yield technology and regional specialization. Algebraically the contribution of each was represented by the four members of summation (20). The last term quantified change in aggregate corn yield attributable to regional specialization. For each year approximate values of individual terms in (20) were changed by the same percent to make this approximation an equality. Estimated annual yield changes, attributed to various technologies and regional specialization, were summed over years. Effects of crop yield technology on total production could then be estimated by multiplying aggregate yield change by aggregate corn acreage.

\[
(17) \quad aY_j = \sum_{i=1}^{m} Y_{ij} R_{ij} = \sum_{i=1}^{m} b_i V_{ij} F_{ij} T_{ij} T_{ij} \quad \text{where} \quad R_{ij} = \frac{A_{ij}}{\sum_{i=1}^{m} A_{ij}}
\]

\[
(18) \Delta aY_j = aY_j - aY_{j-1}
\]

\[
\Delta V_{ij} = V_{ij} - V_i, \quad j-1
\]

\[
\Delta F_{ij} = F_{ij} - F_i, \quad j-1
\]

\[
\Delta T_{ij} = T_{ij} - T_i, \quad j-1
\]

\[
\Delta R_{ij} = R_{ij} - R_i, \quad j-1
\]

\[
(19) \quad \Delta aY_j \sim \sum_{i=1}^{m} \left\{ \frac{\partial aY_i}{\partial V_i} \Delta V_{ij} + \frac{\partial aY_i}{\partial F_i} \Delta F_{ij} + \frac{\partial aY_i}{\partial T_i} \Delta T_{ij} + \frac{\partial aY_i}{\partial R_i} \Delta R_{ij} \right\}
\]

\[
(20) \quad \sim \sum_{i=1}^{m} \left\{ \frac{\partial aY_i}{\partial V_i} \Delta V_{ij} R_{ij} + \left( \frac{\partial aY_i}{\partial F_i} \right) \Delta F_{ij} R_{ij} + \left( \frac{\partial aY_i}{\partial T_i} \right) \Delta T_{ij} R_{ij} + \left( \frac{\partial aY_i}{\partial R_i} \right) \Delta R_{ij} \right\}
\]
The estimated extent to which major crop yield technologies have changed regional yields is shown in Figure 6. Corn yields advanced strongly in all regions over the last two decades. The greatest percentage yield increase occurred in the Northern Plains and Delta States. However in terms of bushels the increase was greatest in the Corn Belt (from 39.6 in 1939 to 64.1 bushels in 1961), at a rate of 1.11 bushels per year assuming "normal" weather conditions. Hybridization of corn and higher rates of fertilizer application accounted for most of the yield change in all regions. Fertilization was the primary cause of higher yields in the Corn Belt and Delta States, hybridization in the Lake States and Southern Plains, and fertilization, hybrid improvement and other variables of corn yield technology in equal measure in the Northern Plains.

In the Southern Plains, unspecified crop yield variables, after correction for fertilization and hybrid improvement, reduced net yield by 1.1 bushels over the 22 year period. In all other regions yield effects of unspecified crop yield variables were positive and especially strong in the Northern Plains, where irrigated acreage nearly tripled between the years 1949 and 1959. Yield effects of regional specialization are not shown in Figure 6 because they were quite small. Cumulative yield effects of regional specialization over the two decades were estimated at .78 bushels for the Corn Belt, at .07 bushels for the Lake States, .17 bushels for the Northern Plains, -.18 for Southern Plains, and -.04 for the Delta States. These estimates indicate that regional specialization was not a strong contributor to yield change within major corn producing regions. On national basis yield effects of regional specialization were more pronounced because shifts in corn acreage between regions were more important than shifts within regions.

In order to obtain estimates of the total impact of crop yield technology corn production functions of 16 states were aggregated in accordance with formula (20), discussed earlier. Yield changes attributed to different technologies are illustrated by Figure 7 and Table 8. The 16 states include more than 80 percent of the nation's corn production. Aside from weather variations, aggregate corn yield advanced from an estimated 30.0 bushels in 1939 to 55.8 bushels in 1961. Of this 25.8 bushel yield increase, 8.1 bushels are attributed to higher rates of fertilization, 9.2 bushels to adoption and improvement of hybrid corn, 4.6 bushels to regional corn specialization and the remaining 3.9 bushels to other "long run" yield variables.

A summary of the estimated impact of advance in corn yield technology is presented in Table 9. Changes in U. S. production were computed by multiplying the annual cumulative yield change (column 1) by the U. S. corn acreage of 1960. Total change in U.S. production in 1960 attributed to these variables is 2.13 billion bushels as shown in column 2. According to these estimates, fertilization and corn hybridization added more than 700 million bushels each, and regional specialization and other yield variables added another 660 million bushels.
Figure 6. Changes in regional corn yields due to technology, 1939 to 1961.
Figure 7. Cumulative changes in aggregate corn yields attributed to different technologies, 1939 to 1961.

Figure 8. Annual changes in aggregate corn yields attributed to weather and yield technology, 1940 to 1961.
Table 8. Estimated contribution of corn hybrid improvement, fertilizer use, regional specialization and other crop yield technology to corn yields, 16 states, 1939 to 1961.

<table>
<thead>
<tr>
<th>Year</th>
<th>&quot;Normal&quot; corn yield (bu.)</th>
<th>Other corn technology (bu.)</th>
<th>Regional specialization (bu.)</th>
<th>Corn hybrid improvement (bu.)</th>
<th>Fertilizer use (bu.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1939</td>
<td>29.98</td>
<td>.000</td>
<td>.000</td>
<td>.000</td>
<td>.000</td>
</tr>
<tr>
<td>1940</td>
<td>31.11</td>
<td>.107</td>
<td>.143</td>
<td>.809</td>
<td>.282</td>
</tr>
<tr>
<td>1941</td>
<td>32.24</td>
<td>.088</td>
<td>.148</td>
<td>.818</td>
<td>.080</td>
</tr>
<tr>
<td>1942</td>
<td>33.16</td>
<td>.128</td>
<td>.151</td>
<td>.595</td>
<td>.302</td>
</tr>
<tr>
<td>1943</td>
<td>34.19</td>
<td>.282</td>
<td>.157</td>
<td>.448</td>
<td>.138</td>
</tr>
<tr>
<td>1944</td>
<td>35.44</td>
<td>.544</td>
<td>.164</td>
<td>.429</td>
<td>.111</td>
</tr>
<tr>
<td>1945</td>
<td>36.63</td>
<td>.251</td>
<td>.166</td>
<td>.477</td>
<td>.303</td>
</tr>
<tr>
<td>1946</td>
<td>38.05</td>
<td>.439</td>
<td>.173</td>
<td>.404</td>
<td>.399</td>
</tr>
<tr>
<td>1948</td>
<td>40.76</td>
<td>.647</td>
<td>.178</td>
<td>.353</td>
<td>.320</td>
</tr>
<tr>
<td>1949</td>
<td>41.54</td>
<td>.027</td>
<td>.078</td>
<td>.312</td>
<td>.265</td>
</tr>
<tr>
<td>1950</td>
<td>41.44</td>
<td>.847</td>
<td>.176</td>
<td>.312</td>
<td>.256</td>
</tr>
<tr>
<td>1951</td>
<td>43.52</td>
<td>.808</td>
<td>.178</td>
<td>.339</td>
<td>.757</td>
</tr>
<tr>
<td>1952</td>
<td>45.06</td>
<td>.364</td>
<td>.189</td>
<td>.384</td>
<td>.603</td>
</tr>
<tr>
<td>1953</td>
<td>46.89</td>
<td>.529</td>
<td>.192</td>
<td>.326</td>
<td>.781</td>
</tr>
<tr>
<td>1954</td>
<td>48.01</td>
<td>.023</td>
<td>.198</td>
<td>.314</td>
<td>.634</td>
</tr>
<tr>
<td>1955</td>
<td>49.05</td>
<td>.247</td>
<td>.194</td>
<td>.349</td>
<td>.253</td>
</tr>
<tr>
<td>1956</td>
<td>49.77</td>
<td>.125</td>
<td>.189</td>
<td>.344</td>
<td>.063</td>
</tr>
<tr>
<td>1957</td>
<td>50.85</td>
<td>.064</td>
<td>.181</td>
<td>.347</td>
<td>.485</td>
</tr>
<tr>
<td>1958</td>
<td>51.79</td>
<td>.046</td>
<td>.192</td>
<td>.379</td>
<td>.415</td>
</tr>
<tr>
<td>1959</td>
<td>53.37</td>
<td>.724</td>
<td>.201</td>
<td>.335</td>
<td>.323</td>
</tr>
<tr>
<td>1960</td>
<td>54.12</td>
<td>.194</td>
<td>.206</td>
<td>.366</td>
<td>.022</td>
</tr>
</tbody>
</table>
Table 9. Estimated annual change in U. S. corn production in 1960 due to advances in corn yield technology since 1939.

<table>
<thead>
<tr>
<th>Yield technology</th>
<th>Estimated changes</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Yield per acre</td>
<td>U. S. production^a</td>
</tr>
<tr>
<td></td>
<td>bushel (1)</td>
<td>million bushel (2)</td>
</tr>
<tr>
<td>Fertilization</td>
<td>8.11</td>
<td>754.2</td>
</tr>
<tr>
<td>Corn hybridization</td>
<td>9.16</td>
<td>715.2</td>
</tr>
<tr>
<td>Regional specialization</td>
<td>4.61</td>
<td>357.5</td>
</tr>
<tr>
<td>Other</td>
<td>3.94</td>
<td>303.1</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>25.82</strong></td>
<td><strong>2,130.0</strong></td>
</tr>
</tbody>
</table>

^a Estimated on the basis of 83.6 percent of U. S. corn production.

Weather and Acreage Effects

Annual effects of weather and acreage on state corn yields are estimated in Table 10. Normal yields in column (1) assume normal weather and acreage. Column (2) shows the estimated yield when acreage is considered while column (3) shows the total yield when actual (rather than normal) weather is considered. Acreage effects on yields for the 16 states are small, as is evidenced by comparison of columns (1) and (2). Deviations from normal yields are much greater when yearly values for the weather variable are used as in column (3).

The proportion of the yield variation attributable to weather is a question of particular interest for recent years. Yield effects of weather and yield technology were estimated simultaneously, the results being summarized in Figure 8. Yield changes due to weather are illustrated by the shaded areas and those due to technology by the unshaded areas in the center of the graph. Positive yield changes are marked off above the center line and negative yield changes below the center line.

In the year 1945 (i.e., between 1944 and 1945), for example, aggregate corn yields advanced from 35.4 to 36.6 bushels per acre if weather effects are not considered (column 1 of Table 10). This is a positive yield change of 1.2 bushels attributed to yield technology as indicated in Figure 8. In the same year weather effects caused a yield reduction of 2.0 bushels, reducing "normal" yields from 36.6 to 34.6 bushels. The 2.0 bushel reduction is depicted in Figure 8 by the shaded area below the center line in 1945. In that particular year, yield changes attributed to acre effects were zero. For most other years they were so small that they could not be shown in Figure 8 (exceptions were the years 1952, 1956 and 1957 when acreage caused yield changes of close to 0.5
Table 10. Aggregate corn yield estimates relating to weather and acreages for 16 states, 1939 to 1961.

<table>
<thead>
<tr>
<th>Year</th>
<th>Yield estimates excluding acreage and weather effects (bu.)</th>
<th>Yield estimates including only acreage effects (bu.)</th>
<th>Yield estimates including weather effects (bu.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>1939</td>
<td>30.0</td>
<td>29.8</td>
<td>31.8</td>
</tr>
<tr>
<td>1940</td>
<td>31.1</td>
<td>30.9</td>
<td>30.9</td>
</tr>
<tr>
<td>1941</td>
<td>32.2</td>
<td>32.1</td>
<td>31.6</td>
</tr>
<tr>
<td>1942</td>
<td>33.2</td>
<td>33.1</td>
<td>35.4</td>
</tr>
<tr>
<td>1943</td>
<td>34.2</td>
<td>34.4</td>
<td>34.6</td>
</tr>
<tr>
<td>1944</td>
<td>35.4</td>
<td>35.6</td>
<td>36.1</td>
</tr>
<tr>
<td>1945</td>
<td>36.6</td>
<td>36.6</td>
<td>34.6</td>
</tr>
<tr>
<td>1946</td>
<td>38.0</td>
<td>38.3</td>
<td>39.3</td>
</tr>
<tr>
<td>1947</td>
<td>39.3</td>
<td>39.4</td>
<td>30.2</td>
</tr>
<tr>
<td>1948</td>
<td>40.8</td>
<td>40.9</td>
<td>44.1</td>
</tr>
<tr>
<td>1949</td>
<td>41.5</td>
<td>41.7</td>
<td>39.8</td>
</tr>
<tr>
<td>1950</td>
<td>41.4</td>
<td>41.5</td>
<td>38.9</td>
</tr>
<tr>
<td>1951</td>
<td>43.5</td>
<td>43.7</td>
<td>39.7</td>
</tr>
<tr>
<td>1952</td>
<td>45.1</td>
<td>45.5</td>
<td>46.7</td>
</tr>
<tr>
<td>1953</td>
<td>46.9</td>
<td>47.2</td>
<td>46.1</td>
</tr>
<tr>
<td>1954</td>
<td>48.0</td>
<td>48.2</td>
<td>46.3</td>
</tr>
<tr>
<td>1955</td>
<td>49.0</td>
<td>48.9</td>
<td>45.6</td>
</tr>
<tr>
<td>1956</td>
<td>49.8</td>
<td>49.3</td>
<td>49.8</td>
</tr>
<tr>
<td>1957</td>
<td>50.8</td>
<td>50.4</td>
<td>52.3</td>
</tr>
<tr>
<td>1958</td>
<td>51.8</td>
<td>51.6</td>
<td>55.0</td>
</tr>
<tr>
<td>1959</td>
<td>53.4</td>
<td>53.3</td>
<td>54.8</td>
</tr>
<tr>
<td>1960</td>
<td>54.1</td>
<td>54.3</td>
<td>55.7</td>
</tr>
<tr>
<td>1961</td>
<td>55.8</td>
<td>55.6</td>
<td>59.7</td>
</tr>
</tbody>
</table>

Over the years yield technology contributed quite regularly to the over-all increase in corn yields.

An exceptional year was 1950, when yield change from technology caused by regional "de-specialization" was -0.1 bushels. However, in

8/ This yield change was composed of a positive yield change due to fertilization (+.256 bu.), hybridization (+.312 bu.), other yield technology (+.176 bu.) and a negative yield change (-.847 due to reduction in Corn Belt acreage.
1950 as in most other years, weather caused the biggest year-to-year changes in aggregate yields. Weather effects appear to have a non-random pattern in the distribution of yields over time. During the early 1950's weather effects were mostly negative. In more recent years, however, they were consistently positive and large. Hence, we also attribute a large part of the recent rise in yields to favorable weather conditions.

It has been necessary in this study to use highly aggregate variables and a production function that is quite inflexible. Results of this study indicate what proportion of yield increase in corn production are attributable to different technologies and weather but reliability of the estimates is probably stronger for below and average weather than good weather years. Further refinement of estimation techniques could be most useful for prediction of future production potentials of U. S. agriculture.
Bibliography


MULTIPLE REGRESSION TECHNIQUES IN
THE EVALUATION OF WEATHER AND TECHNOLOGY
IN CROP PRODUCTION

Louis M. Thompson

The yield per acre of the major grain crops in the central area of the United States has gained at a phenomenal rate since the mid-1930's. In the previous 30 year period (1905 to 1935) crop yields fluctuated about a near-level trend with some indication of a downward trend from about 1925 to 1935. During the mid-30's much attention in the press was drawn to soil deterioration as the main cause of the lag in agricultural productivity. Except for the drought years of 1934 and 1936 little attention was drawn to possible relation of climatic fluctuations to trends in crop yields. Furthermore, there has been little attention to the influence of weather on trends in crop yields since the drought years of the mid-30's. The rapid increase in yield per acre since the mid-30's has been generally credited to technology.

Since 1935 there have been several important advances in technology that have influenced crop yields per acre. The use of hybrid corn was a significant factor in the increase in corn yield per acre from 1935 to 1945. By 1945 nearly 90 percent of the corn acreage in the Corn Belt was planted to hybrid corn. As World War II ended in 1945, nitrogen production facilities developed for manufacture of explosives were made available to the fertilizer industry. There has been a fairly steady increase in use of fertilizers on grain crops since 1945. Improved farm machinery has contributed to higher crop yields by permitting more timely operations. There has also been some improvement of crop yields by land selection. For example, corn is now planted on about 60 million acres of land whereas this crop was planted on over 90 million acres in the early 30's. Wheat is now planted on about 50 million acres while in the early 30's wheat was planted on about 75 million acres.

Other practices such as control of weeds, diseases and insects, along with improved varieties of crops, have contributed to higher yields per acre.

Limitations of Simple Linear Regression

Despite the fairly steady adoption of technology and the improvement of management practices, it is significant that yields per acre fluctuate

1/Associate Dean of Agriculture, Iowa State University.
considerably from year to year and from one decade to another. This naturally leads to the question of the influence of weather on trends in crop yields. The difficulty in evaluating weather, however, arises because technology changes the way in which a given weather variable influences crop yields. For example, two inches of rain in July affects fertilized and unfertilized corn in a decidedly different manner. Yield data since 1930 plotted against a weather variable appears to be scattered over such a wide range as to defy analysis, but multiple regression and modern computing facilities enable us to sort out variables causing such scattered distribution.

If one could assume random distribution of weather variables it would be feasible to use a time trend to estimate the influence of technology and attribute the fluctuations in yields around the trend line to weather variables. For example, if technology has been adopted in a fairly steady and uniform rate then a simple linear equation of yield on years gives an estimate of the rate of adoption of technology.

If, however, weather for crop production has improved during the period of study then a time trend line measures both the improvement in weather and the adoption of technology and, consequently, the time trend overestimates the rate of adoption of technology.

Another problem arises where weather is assumed to be random and where the rate of technology appears to have been irregular over time. In such a situation the use of moving averages provides a better estimate of the rate of adoption of technology than a linear time trend. And again the deviations from the "technology line" represent the deviations due to weather. Such a system has been used recently by Shaw and Durost (6).

There is evidence, however, that weather has not been random but has improved for grain crops since the mid-30's in the central area of the United States. Certainly Figures 1, 2 and 3 leave little doubt about the trends that have occurred in July rainfall and July temperature from 1930 to 1963. There has been a significant trend upward in July rainfall and a significant trend downward in July temperature in Illinois, Iowa, Kansas and Nebraska since 1930 and particularly since the mid-30's. Numerous studies have shown that higher than average grain yields in these states are associated with higher than average rainfall and lower than average temperature in July (2, 3, 4, 5, 7, 8, 11, 13).
Fig. 1. Trend in July Temperature.
Fig. 2. Trend in July Rainfall.
Fig. 3. Trends in July Rainfall and Temperature and Corn Yields in Iowa.
In addition to a gradual improvement in July rainfall and July temperature (insofar as grain crops are concerned) there appears to have been unusually warm-dry periods in the mid-30's and in the mid-50's.

Because of the warm-dry periods in the mid-30's and mid-50's and because of the significant trend upward in July rainfall and significant trend downward in July temperature, it was believed that weather variables could not be treated as random variables. In other words, part of the increase in yield since the mid-30's has been due to improvement in weather and part of the increase in yield has been due to technology. The problem is to separate the variation due to technology from the variation due to weather.

A Multiple Regression Model

In attempting to solve this problem of separation of variations due to weather and technology it was decided to try multiple regression as a means of analysis. Table 1 shows two sets of hypothetical corn yield data with hypothetical July rainfall. In set "A" the rainfall was randomly distributed but with 1 bushel increase for each additional 0.2 inch of rain. The data also allowed 1 bushel increase in yield per acre per year for technology. Figure 4 shows two lines for technology. The dotted line is the simple regression line. The dash line was calculated for technology from the multiple regression equation.

The data of set "A" results in a slope for technology of 1 bushel per acre per year by use of either simple linear regression or multiple regression. Since there was virtually no improvement in weather over the entire period the simple linear regression equation would be adequate to describe the trend for technology for set "A".

In set "B" the rainfall-yield data were divided in two groups. All rainfall-yield data below average were randomly distributed for the first half of the period. Likewise all above average rainfall-yield data were randomly distributed for the last half the period. As in set "A," 1 bushel increase in yield was allowed for each additional 0.2 inches of rain, and 1 bushel increase in yield was allowed for increase in technology each year. Set "B" results in a steep slope for yield over time because of the combined improvement of rainfall and technology.

A simple linear regression of yield on years for set "B" results in a slope of 1.85 bushels per acre per year. But the coefficient for years from the multiple regression equation for set "B" is only 1 bushel per acre per year. The coefficient for years from the multiple regression equation for set "A" was likewise only 1 bushel per acre per year. Figure 5 shows two lines for technology. The dotted line is the simple regression line. The dash line shows the slope calculated from the multiple regression equation.
Fig. 4. Hypothetical Corn Yields with Weather Variable Randomly Distributed.
Fig. 5. Hypothetical Corn Yields with Improvement in Weather.
Table 1. Hypothetical Yield and Rainfall Data used to Illustrate Trends in Yields for Figures 4 and 5.

| Years | Set A | | | Set B | | |
|-------|-------| | |-------| | |
|       | July Rain | Corn Yield |       | July Rain | Corn Yield |       |
| 1930  | 5.0    | 40       | 1931  | 5.6    | 44       | 1932  | 1.4    | 24       | 1933  | 0.0    | 18       | 1934  | 1.8    | 28       | 1935  | 4.2    | 41       | 1936  | 6.2    | 52       | 1937  | 2.2    | 33       | 1938  | 3.4    | 40       | 1939  | 1.2    | 30       | 1940  | 0.2    | 26       | 1941  | 3.2    | 42       | 1942  | 3.0    | 42       | 1943  | 6.4    | 60       | 1944  | 2.0    | 39       | 1945  | 5.2    | 56       | 1946  | 5.8    | 60       | 1947  | 4.4    | 54       | 1948  | 2.6    | 46       | 1949  | 4.6    | 57       | 1950  | 1.0    | 40       | 1951  | 4.0    | 56       | 1952  | 5.4    | 66       | 1953  | 3.6    | 56       | 1954  | 2.4    | 51       | 1955  | 3.8    | 59       | 1956  | 0.4    | 43       | 1957  | 2.8    | 56       | 1958  | 0.8    | 47       | 1959  | 6.0    | 74       | 1960  | 1.6    | 53       | 1961  | 0.6    | 49       | 1962  | 4.8    | 71       |
Multiple regression is a technique that is particularly useful in distinguishing between the effects of two or more correlated independent variables such as improved weather and improved technology.

**Application of the Multiple Regression Technique**

Reference is now made to Figure 3. In this figure the simple linear regression equation results in a slope of 1.02 bushels of corn per acre per year but obviously after examining Figures 1, 2 and 3 one would have to conclude that this slope also includes the effects of improved weather.

Figure 6 shows the results of multiple regression analysis of corn yields in Iowa with several weather variables and years representing technology. The slope for technology is only 0.7, yet in Figure 3 the slope was 1.02. These results indicate that improved weather accounted for almost a third of the increase in yield from 1930 to 1962. Furthermore, Figure 6 indicates that nearly all of the deviations in yield from the trend line can be accounted for by weather variation.

Figure 6 was used from a previously published study (12). The weather variables include preseason rainfall (the total from September to May), June rainfall and June temperature, July rainfall and July temperature, August rainfall and August temperature, and interactions between rainfall and temperature for each summer month. Each weather variable was also treated as a curvilinear relationship to yield (a quadratic equation was used).

The analysis for corn production in Iowa indicates that groups of unfavorable years appear to alternate somewhat with groups of more favorable years. From 1930 to 1936 there was only one year with better than average weather conditions. During the ten year period from 1937 to 1946 there was only one year with yields below the trend line. During the 10-year period from 1947 to 1956 there were only two years with yields above the trend line, and since 1957 all years have been above average with respect to weather and corn yields.

An examination of the weather data for the period of study indicates hot-dry weather in 1934, 1936 and in the late summer of 1947. The summers of 1950 and 1951 were especially cool and wet. The summer weather of 1954, 1955 and 1956 was particularly warm with low rainfall in July in 1954 and low rainfall in August of 1955. The series of favorable years after 1956 have been associated with near optimum summer temperatures and near average or above average rainfall.
IOWA

$R^2 = .96$

$b = .70$

Fig. 6. The Relation of Weather and Technology to the Trend in Yield of Corn.
The Need for Curvilinear Regression

The first paper prepared on this general subject by the author in 1961 was sent to Mr. Louis Bean for review. The paper was based on multiple linear regression. Mr. Bean pointed out the difficulties involved with linear equations and suggested the use of curvilinear techniques. As long as linear equations were used there was a tendency to overestimate yields in the cool-wet years. In searching for an appropriate method of analysis the author found that Dr. Mordecai Ezekiel (1) used quadratic equations quite satisfactorily in evaluating weather variables. In general, one finds that by plotting yield (Y) data against temperature (X) one obtains a distribution of points that resemble a parabola, and the equation\[ Y = a + bX - cX^2 \]is usually applicable.

Figure 7 shows the temperature curves for corn in Iowa, Illinois and Indiana. These curves were prepared from the multiple regression equations used for the calculated yields in Figure 6. Calculations for each temperature variable were made by assuming no deviations from the mean for other variables in the multiple regression equation. In other words, the curves describe the relationship of temperature to yield only if rainfall is average.

The yield data on the Y axis were based on the average yields from 1930 to 1962. The curves indicate that high temperatures in August are more damaging to yield than the same temperature would be in July. In general, across the Corn Belt it is desirable to have warmer than average weather in June but cooler than average weather in July and August. It is of particular interest that high temperatures in August are more damaging to corn yields than the same temperature would be in July.

Rainfall and temperature are negatively correlated in each summer month. It is possible therefore to use only temperature or only rainfall for each summer month and obtain fairly high correlations between corn yield and weather variables. If one were interested in reducing the number of variables to the few most significant he should use June temperature, July rainfall and August temperature along with a time trend in evaluating weather and technology in the production of corn in the Corn Belt states.

In the early stages of this research program interactions were not used. It was observed that the regression equations underestimated corn yields in high yielding years and overestimated corn yields in the unfavorable years. Dr. George Snedecor* suggested the use of interactions to improve the prediction equations and the results were most gratifying. Figure 8 shows the curves calculated for corn yields at three different moisture levels with different levels of temperature. Lowest yields are associated with hot-dry

* Personal communication.
Fig. 7. The Relation of Yield of Corn to Average Monthly Temperature,
Fig. 8. The Relation of Corn Yield to July Temperature with Different Levels of Rainfall in Iowa.
conditions or very cool-wet conditions. In would appear that corn would tolerate temperature above a daily average of 80 degrees in July provided the rainfall were also high. Unfortunately, however, high temperatures are usually associated with low rainfall. One should be cautious in extrapolating such curves toward extremes, but the curves do indicate a tolerance for lower than normal rainfall if the temperature is also cooler than normal. Perhaps this is why corn yielded so well in Iowa in 1959 and 1960 when rainfall and temperature were both below average in July.

The Time Trend as a Measure of Technology

It is recognized that technology in corn production has not been introduced in a perfectly linear fashion since 1930. Perhaps the real trend is a slightly undulating sloping line that would average out as a linear trend. Several techniques were used to test the linear trend hypothesis. One was to use two time trend periods -- one from 1930 to 1945 and another from 1946 to 1962. The result was that the second period did not have a steeper slope for technology than the first period.

Another test was to use a time trend variable from 1946 to 1962 in addition to the time trend variable from 1930 to 1962. This technique did not indicate a steeper trend after 1945.

It appears likely that hybrid corn was the main factor in the trend for technology from 1930 to 1945 and that fertilizer (particularly nitrogen) was the main factor for technology after 1945. The gradual reduction in acreage planted to corn was probably a factor throughout the period of study from 1930 to 1962. Other factors such as improved mechanical operations have had some influence throughout the entire period of study. Collectively, these factors have resulted in a fairly steady trend upward for adoption of technology for corn production since about 1930.

The linear trend for technology was found to be adequate also for soybeans (11), but a curvilinear trend was necessary in the study of grain sorghums (10) and for wheat when the entire period from 1930 to 1962 was studied (12). Technology has been introduced at a much faster rate in the decade of the 50's than in the decade of the 30's in the Great Plains states, where wheat and grain sorghums are the most important grain crops.

General Discussion

During the past three years the author has studied the relation of weather to the trend in yields of crops in the 11-state area from North Dakota to Texas and from Iowa and Missouri across the Corn Belt to include Ohio. Four crops have been studied: corn, soybeans, wheat and grain
sorghums. Five states were studied for each crop. All of these studies have been published (9, 10, 11). As a general conclusion, about half the trend upward in yield since 1950 can be attributed to improvement in weather and about half the trend upward can be attributed to adoption of technology. The first half of the decade of the 50's was generally characterized by unfavorable weather for grain crops in the 11-state area. The years 1950 and 1951 were rather wet and cool in the Corn Belt and in the northern part of the Great Plains. The year 1952 was generally favorable, but the four-year period from 1953 through 1956 was a warm, relatively dry period over most of the 11-state area. The period from 1957 to 1962 was characterized by higher than average rainfall and cooler than average temperature during the summer months in this important grain producing area. The year 1958 was the peak year "weatherwise" for wheat but the years 1961, 1962 and 1963 were the most favorable "weather years" for corn and soybeans after 1950. The summer weather of the late 50's and early 60's was quite similar to that of the late 30's and early 40's in the Corn Belt.

To use the crop yield trend for the period from 1950 to 1960 as a basis for projection into the future one must assume that weather will continue to improve as it did over the decade of the 50's. Weather for corn did improve from 1960 through 1963 and since corn constitutes a high proportion of the crops produced in the United States one cannot help but be impressed by the success of the USDA policy makers in the use of the 1950 to 1960 decade as a basis for projecting crop yield trends. Their projections have been amazingly accurate and somewhat on the conservative side. But it would be folly to assume that weather will continue to improve. Weather fluctuates in such patterns as to suggest that we are now experiencing a favorable period not greatly different from that experienced after the turn of the century, 1902-1909 and again from 1918 to 1924 and from 1938 to 1944. In other words we should expect unfavorable periods of weather to occur in the future. It may be too ambitious at this stage to predict when the next unfavorable period will occur, but we would be rather naive to assume the unfavorable periods to be far enough in the future so as to permit us to do away with our surplus grains. The records of the past indicate that an unfavorable year might occur at any time. The grains we now have in reserve would be of tremendous value should 1964 and 1965 turn out to be years of unfavorable weather such as those experienced in the mid-50's or the mid-30's or the mid-teens.
Bibliography


THE WEATHER INDEX APPROACH

Lawrence H. Shaw¹ and Donald D. Durost²

Our assignment is to present our weather index. We propose to present (1) the reason for our interest in a weather measure, (2) how we constructed our weather index, (3) the results of our effort, (4) some of the strong and weak points of our method and (5) our proposed plans.

One of the primary aims of our research in the Farm Production Economics Division is to develop a clearer understanding of the factors which have been responsible for growth in the output and productivity of American agriculture.

The agricultural production process involves the use of certain environmental factors as essential inputs. These environmental factors, which we shall term weather, are neither under the control of the individual producer nor in constant supply over time. Consequently, yields fluctuate greatly from year to year. This fluctuation obscures systematic changes that take place in agricultural production and efficiency. These systematic changes are, in the long run, the result of improvements in farm practices and in the quantity and quality of the controllable inputs, and are important factors in determining the level of production and efficiency in agriculture.

Over the years, man has made significant progress in adapting himself to his environment. With modern machinery a farmer can take advantage of a short break in the weather to complete fully a field operation which may have required weeks using earlier kinds of machinery and power. Drought-resistant crop varieties, cultivation to maximize moisture availability, effective weed control, and proper placement of higher rates of improved fertilizer have done much to offset the adverse effects of unfavorable weather. Thus, today the same meteorological conditions do not affect crop production to the same extent that they did in 1930. In other words, weather and improved technology are not truly independent variables.

It is vital that we have a better understanding of the past patterns and causes of yield change (1) if we are to gain more knowledge about the

¹/Economist, Development and Trade Analysis Division; formerly economist, Farm Production Economics Division.
²/Agricultural Economist, Farm Production Economics Division.
The path of technological change, and (2) if we are to do a better job in projecting the future production potential and in providing analyses for agricultural policy decisions. Thus, our primary interest is in measuring the effects of technology; measuring weather impacts is of secondary interest. But to attain the former the latter measure must be developed.

Most of our analysis is at a macro rather than micro level. We believe that the study of functional relationships between individual meteorological factors and yields is in the realm of the agronomist and meteorologist. Since we are not directly concerned with the effects of the individual meteorological factors on aggregate yields, we have used what we call the "weather index" approach in our research. This general approach was used in the early 1950's by Glenn Johnson and Dale Hathaway, and later by James Stallings. We have used data on yields of corn for grain to develop our methodology. Thus, we are presenting a weather index for corn yields.

Methodology Used to Construct the Weather Index

The first requirement in constructing a weather index is to select appropriate yield data. The available data vary between two extremes: (1) check plot data where all practices have been held constant but usually are limited in the number of locations, and (2) Crop Reporting Board data on actual yields where all practices are free to vary over time. However, the Crop Reporting Board data have the advantage of a wide geographical coverage.

In the check plot data it can be assumed that the yield variation due to changes in soil is gradual and can be removed by a trend. Changes in yield due to weather can then be measured as deviations from the yield trend. The weather thus measured is relevant only to the level of technology of the experiment.

In the Crop Reporting Board data the weather which is being measured is relevant to the current level of technology. The problem in dealing with the data is the removal of technology or the controllable factors. It cannot be assumed that yield variation due to these controllable factors is gradual. An abrupt increase may be due to a sudden rise in the use of a yield-increasing input. Such abrupt increases would show in the index as weather variation.

We compromised between these two extremes by using the corn variety tests. These tests are, for the most part, conducted under actual farming conditions. The cooperating farmers prepare the plots and cultivate them in the same way that they treat the rest of their fields. Planting and harvesting are done by research workers. Plots are chosen to represent soil types in the area and a continuity in management is approximated. The tests provide a relatively wide geographical coverage of the area of production.

The use of the mean yields of the variety tests makes possible construction of a time series of yields at several locations with a state.\(^4\) For a given location, a trend is fitted to the yield series to represent the effects of non-weather factors. The observed mean yield each year is calculated as a percentage of the trend yield. The series of these percentages is termed a "weather index," with a value of 100 (the weather index for a year where observed and trend yields coincide) denoting a year of "normal" or "expected" weather.

The following specific steps were taken in developing a weather index for each location.

(1) A 9-year moving average of the mean yields of hybrid corn was computed. The moving average was a first approximation of the trend in yields due to factors which were not held constant. A period of 9 years is somewhat arbitrary; however, we considered it appropriate after testing other moving averages of 5, 7 and 11 years. The period needs to be long enough to average out the effect of extreme years, but not so long that it obscures changes in technology.

(2) The moving average was extrapolated forward and backward to the terminal years.

(3) Actual experimental yields for each year were divided by the corresponding moving average yield. Any year in which this percentage ranged from 85 to 115 was considered an "average-weather" year for the purpose of making a second approximation of the trend in yields. These steps were taken to eliminate the effects of the extreme yields on the trend, because of the limited number of observations.

(4) Yields in "average years" were used to compute the trend. We employed two methods to compute the yield trend. In our earlier study we filled in the yield gaps for the years with extreme weather by averaging the

---

yields at each end of the gap, and then interpolating. Finally, these average-weather yields were converted to a 5-year moving average, which was used as the final measure of trend in the yields from the variety tests. By using the 5-year moving average, errors due to changing cooperators and fluctuations because of the relatively wide range which was used to select average-weather years were reduced. Further analysis indicated that these trends were essentially linear in nature. In a recent effort we based our calculations of trends at each location on a linear regression of "average-weather" yields.

(5) The weather index for the location is the percentage that actual test yields are of trend test yields.

(6) Aggregate weather indexes are constructed by averaging weather index values for locations with a Crop Reporting District. The weather indexes for the Crop Reporting District are used to adjust District production in a simple deflation process. In effect, reported District yields are "deflated" by a weather index constructed from experimental yield data. Adjusted district production is summed to state adjusted production. State actual production divided by state adjusted production provides an implicitly weighted state index of the impact of weather on corn yields. Regional indexes for groups of states are constructed by using regional weights.

We used the weather index approach to construct a measure of the effects of weather variation on corn yields in the Corn Belt. The Corn Belt is composed of Ohio, Indiana, Illinois, Iowa and Missouri. Table 1 presents the weather index for corn yields. The index presents a measure of the annual effect on corn yields of environmental factors beyond the control of producers.

The weather index can be used to adjust the actual yield series in a simple deflation process. The actual and adjusted Corn Belt yields of corn for grain during the 1929-62 period are shown in Table 1 and are charted in Figure 1. The adjusted yield, being net of weather effects, shows more clearly the impact of changes in production practices. There is a certain amount of unexplained variation in the adjusted yield measure. Examples of years when adjusted yields are irregular are 1931, 1936 and 1941. Intuitively, it appears that the weather index overadjusted for the influence of weather during these years. At the regional level, as one would expect, the adjusted yield is smoother than at the state level. Even with the unexplained variation, the adjusted yield measure does indicate the direction and extent of technological change.

5/One of 10 major regions used by the Farm Production Economics Division, USDA, in reporting farm output statistics.
Table 1. Weather indexes for corn yields, actual and adjusted yields of corn for grain, Corn Belt farm production region, 1929-62.

<table>
<thead>
<tr>
<th>Year</th>
<th>Weather index</th>
<th>Actual yield</th>
<th>Adjusted yield</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Bushels</td>
<td>Bushels</td>
</tr>
<tr>
<td>1929</td>
<td>120</td>
<td>34.4</td>
<td>28.7</td>
</tr>
<tr>
<td>1930</td>
<td>96</td>
<td>27.5</td>
<td>28.7</td>
</tr>
<tr>
<td>1931</td>
<td>102</td>
<td>34.9</td>
<td>34.2</td>
</tr>
<tr>
<td>1932</td>
<td>120</td>
<td>39.1</td>
<td>32.5</td>
</tr>
<tr>
<td>1933</td>
<td>103</td>
<td>32.1</td>
<td>31.3</td>
</tr>
<tr>
<td>1934</td>
<td>77</td>
<td>25.4</td>
<td>33.1</td>
</tr>
<tr>
<td>1935</td>
<td>106</td>
<td>36.5</td>
<td>34.3</td>
</tr>
<tr>
<td>1936</td>
<td>54</td>
<td>23.0</td>
<td>42.4</td>
</tr>
<tr>
<td>1937</td>
<td>111</td>
<td>43.3</td>
<td>39.0</td>
</tr>
<tr>
<td>1938</td>
<td>102</td>
<td>42.1</td>
<td>41.4</td>
</tr>
<tr>
<td>1939</td>
<td>114</td>
<td>48.0</td>
<td>42.1</td>
</tr>
<tr>
<td>1940</td>
<td>97</td>
<td>43.0</td>
<td>44.1</td>
</tr>
<tr>
<td>1941</td>
<td>97</td>
<td>47.5</td>
<td>49.1</td>
</tr>
<tr>
<td>1942</td>
<td>112</td>
<td>53.6</td>
<td>47.9</td>
</tr>
<tr>
<td>1943</td>
<td>103</td>
<td>49.1</td>
<td>47.7</td>
</tr>
<tr>
<td>1944</td>
<td>92</td>
<td>44.7</td>
<td>48.5</td>
</tr>
<tr>
<td>1945</td>
<td>87</td>
<td>44.9</td>
<td>51.4</td>
</tr>
<tr>
<td>1946</td>
<td>100</td>
<td>52.4</td>
<td>52.4</td>
</tr>
<tr>
<td>1947</td>
<td>69</td>
<td>35.5</td>
<td>51.7</td>
</tr>
<tr>
<td>1948</td>
<td>106</td>
<td>57.9</td>
<td>54.8</td>
</tr>
<tr>
<td>1949</td>
<td>91</td>
<td>49.1</td>
<td>53.8</td>
</tr>
<tr>
<td>1950</td>
<td>88</td>
<td>49.0</td>
<td>55.8</td>
</tr>
<tr>
<td>1951</td>
<td>88</td>
<td>47.5</td>
<td>54.2</td>
</tr>
<tr>
<td>1952</td>
<td>101</td>
<td>55.7</td>
<td>55.3</td>
</tr>
<tr>
<td>1953</td>
<td>92</td>
<td>51.1</td>
<td>55.7</td>
</tr>
<tr>
<td>1954</td>
<td>94</td>
<td>51.1</td>
<td>54.7</td>
</tr>
<tr>
<td>1955</td>
<td>88</td>
<td>51.9</td>
<td>59.3</td>
</tr>
<tr>
<td>1956</td>
<td>104</td>
<td>59.0</td>
<td>56.5</td>
</tr>
<tr>
<td>1957</td>
<td>104</td>
<td>59.2</td>
<td>57.2</td>
</tr>
<tr>
<td>1958</td>
<td>105</td>
<td>64.5</td>
<td>61.4</td>
</tr>
<tr>
<td>1959</td>
<td>104</td>
<td>63.5</td>
<td>61.2</td>
</tr>
<tr>
<td>1960</td>
<td>105</td>
<td>64.6</td>
<td>61.3</td>
</tr>
<tr>
<td>1961</td>
<td>108</td>
<td>74.1</td>
<td>68.4</td>
</tr>
<tr>
<td>1962</td>
<td>106</td>
<td>77.2</td>
<td>72.8</td>
</tr>
</tbody>
</table>
ACTUAL AND ADJUSTED CORN YIELDS
Corn Belt Farm Production Region

U.S. DEPARTMENT OF AGRICULTURE
NEG. ERS 2816-64 (4) ECONOMIC RESEARCH SERVICE

Figure 1

* GRAIN PER HARVESTED ACRE.
Adjusted corn yields increased at a more or less constant rate up to 1948 and then the rate of increase slowed considerably. This initial period seems highly related to the adoption of hybrid seed. Adoption was 99 percent complete in the Corn Belt by 1948. The later period of yield increase seems highly related to stepped up fertilizer use.

The Structure of Corn Yields in the Corn Belt, 1929–63

Our major aim in constructing the weather index was to facilitate more intensive analysis of the effects of improved technology on agricultural output and productivity. The weather index may be used as a variable in a supply equation, where the relationship between yield and various causal factors is estimated.

The weather index as constructed in this study is well suited to supply analyses of yields. As an indicator of the percentage effects on yields of weather factors, it is a better variable than one or a combination of the meteorological series, such as state average precipitation or temperature.

An equation was estimated for corn yields in the Corn Belt, 1929–62, with the following variables:

\[
X_1 \text{ -- Yield of corn for grain in bushels, from various published reports of the Crop Reporting Board, USDA. These are the latest official estimates as of December 1963.}
\]

\[
X_2 \text{ -- Weather index for corn yields.}
\]

\[
X_3 \text{ -- Relative adoption of hybrid seed in terms of percentage of total acres planted to hybrid seed.}
\]

\[
X_4 \text{ -- Use of nitrogen fertilizer on corn as a percentage of use in 1962.}
\]

\[
X_5 \text{ -- Plant population per acre derived from unpublished information as a percentage of 1962.}
\]

The estimated equation is as follows, with standard errors of the coefficients shown in parentheses:

\[
(1) \quad X_1 = -30.855 + .340X_2 + .178X_3 + .146X_4 + .367X_5
\]

\[
R^2 = .966 \quad (.034) \quad (.013) \quad (.035) \quad (.154)
\]

\[6/\] Numerical estimates for variables and a more detailed discussion are found in the authors' unpublished manuscript, "Sources of Yield Change -- The Roles of Weather and Technology in Rising Corn Yields."
Ninety-seven percent of the total variation in Corn Belt yields is accounted for by the equation. The regression coefficients \( b_1 \) and the coefficients of determination \( R^2 \) are highly significant at 1 percent levels.

The relationships estimated in the equation indicate that of the increase of approximately 40 bushels in corn yields since 1929, the use of improved varieties of hybrid seed accounted for 15 to 20 bushels. An additional 15 or 16 bushels can be accounted for by increased use of nitrogen fertilizer since 1947. The remainder is the result of higher planting rates and some other improved practices associated with plant populations.

The relationships described are very encouraging. Perhaps now we are able to isolate more readily the factors responsible for yield change. Regression studies of this nature, without the inclusion of the weather variable, have been very limited in their application. Rarely has more than 50 percent of total variation in yields been accounted for; further, the relationships estimated are quite illusory, as variation in the yield series due to various nonenvironmental factors is confounded by weather effects.

The same data used in fitting the above equation may be used to illustrate the illusory effects of weather variation. The equation was also fitted without the weather index resulting in the following coefficients:

\[
(2) \quad X_1 = -59.5 + 0.159X_3 + 0.095X_4 + 1.200X_5
\]

\[
R^2 = 0.601 \quad (.042) \quad (.099) \quad (.486)
\]

Comparing these coefficients with those estimated above shows how weather variation can affect the estimates of the role of technological variables. In particular, note the much smaller proportion of total variation accounted for by the equation. Also the sign and significance of the fertilizer coefficient have been altered. On the basis of equation (2), we would decide that because of the negative sign and because the value of the regression coefficient \( b_4 \) is not significantly different from zero at the 90 percent level of probability, fertilizer use is not a major variable in explaining increasing corn yields. This is quite the opposite from what we would conclude on the basis of equation (1).

We are the first to recognize that our approach has some weaknesses but we believe that the advantages outweigh the weaknesses. The weather index, being \textit{ex post} in its construction, is not useful in predicting yield or output on the basis of meteorological observations. It is not an automatic procedure which can be easily adapted to computer methods for rapid
expansion to other crops and regions of the country. A rather large body of experimental data must be collected and time will be required.

The variety test data used to construct our weather index is essentially based on the complete harvest of the total acreage planted. The Crop Reporting Board reports yields on a harvested acre basis, but harvested acreage may be considerably less than planted acreage. Therefore, our measure should include an allowance for abnormal acreage abandonment. In the case of corn, if part of the crop looks unfavorable a larger acreage may be harvested for silage. The failure to adjust our weather measure for acreage abandonment and diversion to silage may be part of the reason for the apparent overadjusting of yields in certain years.

The use of a linear trend to describe the path of technology in the experimental data may be a weakness. Some alternative approach might improve the estimates.

It is certainly encouraging that our supply equation, using only three variables along with the weather index, accounts for 97 percent of the yield variation over the study period. In using variations in experimental yields to indicate the net effect of the combined meteorological and associated variables, we have avoided a specification of complex cause-effect relationships. This approach avoids any special problems of aggregation. The same weights are used in aggregating both yield and weather measures from the crop reporting district to state or regional levels. This avoids the incongruities in weighting when aggregate meteorological variables and aggregate yield measures are related. Another advantage of the weather index approach is that, with the choice of appropriate yield data from which to measure the effect of weather, weather is measured relevant to existing levels of technology. Thus, the interaction between improved technology and the yield effect of meteorological variables is considered.

Conclusions

As we stated earlier, as economists we are primarily interested in describing the path of technology. We must develop a satisfactory measure of the effects of weather so that we can more effectively measure such things as production response.

We are not completely satisfied with our weather index because of the weaknesses mentioned earlier. We intend, however, to continue trying to develop a procedure that will overcome these handicaps. No matter what approach is used to measure weather, there are three important considerations we believe must be taken into account.
First, weather is relevant to a specific crop. We know, for example, that the meteorological conditions which are good for cotton are not good for oats. Thus, an over-all weather index must be developed for individual crops and then aggregated.

Second, weather is relevant to a specific area. Weather varies considerably from location to location, even within a state and certainly between states. For our purposes, we would hope to develop weather indexes for major crops by regions as well as for the United States as a whole.

Third, weather is relevant to the level of technology that exists at each point in time. As man makes technological advances, he is able to adapt to his environment better and use it more efficiently. A weather index must take account of this.

In the past, much work has been done to measure weather. Unfortunately, much of it has been of limited value for our research purposes. We hope the current revival of interest in weather will bear more fruit than in the past, and that we can develop a measure of the effects of weather that will more adequately meet our needs.
STATISTICAL TECHNIQUES WHICH MIGHT BE USEFUL IN FURTHER RESEARCH

Rex L. Hurst

The problems that I have encountered in the past which used mathematical models for the prediction of phenomena have had several phases in common. Two of these common phases are of particular importance: (1) the development of a proper mathematical model, and (2) the interpretation of the mathematical fitting process.

Most of the mathematical models currently being used to solve statistical problems are of the type we call multiple regression models. The remainder of this paper will be primarily devoted to discussing various aspects of the two cited common phases as they occur in the use of multiple regression models.

A. The Development of Models and Variables

1. Subjective procedures. I believe that too often both statisticians and research workers tacitly assume that all pertinent information can be extracted from a set of data by simple linear combinations of the variables. This assumption has been and is being concentrated mainly on making larger and larger models and developing more sophisticated computer techniques. The problem seems to be that the information recorded by the researchers' various instruments and measuring devices is quite likely not in a form that will allow a linear combination of the data to produce the desired results.

A number of research workers have acknowledged this possibility by turning to other than linear combinations of the data. A familiar example involves the use of degree days in studying the effect of temperature on growth.

I realize now that I unconsciously employed a type of nonlinear solution to a problem in my own thesis (7). I was trying to measure the effects of various soil and environmental factors on the growth and yield of corn. I had measured soil moisture through the growing season, soil density, various chemical components of the soil such as nitrogen, phosphorous and potassium, and organic matter. In spite of all these measurements, I did not feel that I had a good expression of the growth potential of the particular plots of ground that I was using. Therefore, I invited a soil specialist to come in

1/Head, Applied Statistics and Computer Service, Utah State University.
and make an individual assessment of the growth potential of each one of
the plots in the experiment and to place his assessment on a linear scale. At
the completion of the study I found that this soil classification factor contained
as much or more information than all of the recorded variables combined.

I recently participated in a review of an industrial process. In talking
with the research people about the variables they were using in their mathematical
model, they mentioned somewhat sheepishly that they were using a Brown factor.
When quizzed closely about this Brown factor, they admitted that it was an
assessment of the industrial process by a man named Brown.

Personally, I believe that we have not gone far enough in our use of
such Brown factors. The human brain can correlate information and develop
functions of that information that are more meaningful than those produced by
any of our measurement devices. In this particular area I would suggest that
the research workers be made conscious of this. They should routinely try to
determine if variables that they are not now measuring should be measured and
recorded, and if more esoteric combinations of the data than they are using
should be recorded as a new variable pertinent to the phenomena being studied.

As a case in point, we took this type of approach in a recent study and
had the research worker made a subjective evaluation of a given phenomenon
which in this case happened to be an evaluation of fluorine damage to a milk-
producing cow (11). We then used this as the dependent variable in studying
all the observed information that we had on the experimental animals. We
were trying to determine what combination of variables the research worker
had put together in his brain in his personal assessment of the total damage
sustained by each animal. In essence, we decomposed the knowledge of this
research worker into variables that could be measured and recorded. We also
were able to gain insight into the extent to which our present measurements
are unable to account for the professional experience.

Such an approach cannot be mathematically satisfying to the statistician,
because it is inherently subjective. I do not think, however, that anybody
should be discouraged from making such a subjective approach to a problem
in lieu of objective knowledge. Very often a subjective start will lead to an
objective solution.

2. **Objective procedures.** A number of more objective types of
procedures may be employed in attempts to develop better multiple regression
models.

(a) **Graphical procedures.** The first of these that we will consider
involves trying to obtain an impression of what the available data looks like
in multiple dimensions. If you have a multiple regression problem that
incorporates eight or 10 independent variables and you simply look at the
data as it is recorded on the data sheets, you will obtain very little impression
as to the model which should be used.
The beginning student in statistics is urged to make scatter diagrams of the data in his search for an answer to this particular type of problem. A scatter diagram technique has been developed further to the use of three-dimensional figures (2a). We have extended this graphical approach still further and have automated it (8). For example, if we have 10 independent variables and one dependent variable and we want to get some concept of the system, our approach would be roughly as follows: We would take the data and reduce it to punch cards. Then we would sort these data cards into sequence on independent variable one or \( x_1 \). Then we would take the lowest percentile group of these observations and recode in another area of the card a one punch for the lowest group, a two punch for the next highest group, three for the next highest, four for the next highest, etc. Then we would sort the cards on independent variable two and recode these into comparable percentile groups. This procedure would be completed for each of the independent variables. Next, we would use these recoded numbers in the cards to produce averages on the automatic computing equipment of first, main-effect cell averages for each of the independent variables, and then for all possible two-way classifications of the independent variables.

To illustrate this process let us take a look at Figure 1. In Figure 1 we have a scatter diagram of variable \( x_1 \) against \( Y \). By taking the range of observations in \( x_1 \) and breaking this into quartiles, we obtain cell means indicated by the crosses for all of those observations in each cell. Then, instead of having to plot all of the individual points to get a picture of the curve, we can simply plot the average value of the observations in each cell (or the crosses against coded \( x_1 \)) to get a two dimensional picture of the relationship between \( X_1 \) and \( Y \).

To continue this process further, consider Figure 2. Here we are looking down onto a scatter diagram such as in Figure 1, but now the diagram is the two-dimension scatter diagram that exists between variable one and variable two. If the observations are tallied according to the cell in which they occur in the two-way scatter diagram, averages can be obtained for each of the cells. In other words, if we obtain a cell average corresponding to a value for the center of the cell, we can plot these average values for each cell in terms of a three-dimensional picture. The heights in Figure 3 are the averages obtained from the cell averages of Figure 2. When the heights are connected with lines, we can visually get a picture of the three-dimensional relationship between \( X_1, X_2 \) and the dependent variable \( Y \). The number of observations in each cell gives us an idea as to the correlation among the X's.

By completing this process for every possible pair of the independent variables, we obtain a series of crude three-dimensional views of the multidimensional system. This is analogous to an analysis of variance situation in which we design a factorial experiment and analyze for main effects and two-way interactions.
Figure 1. The recoding of variables and graphing of cell means.

Figure 2. Scatter diagram of variable $X_1$ against $X_2$.

Figure 3. A possible three dimensional plotting of the cell means from Figure 2.
Significant high order interactions rarely occur in factorial analyses of variance involving biological phenomena. This should mean that it is fairly safe to extrapolate a series of three-dimensional figures as being representative of the multiple-dimensional system.

It is not difficult to look at these three-dimensional figures and evolve a much more appropriate linear mathematical model than could be derived by looking at the raw data. For example, from Figure 3, I would propose to use as terms in the multiple regression model, $X_1$, $X_1 X_2$, the product $X_1 X_2$, and the product $X_1 X_2^2$.

Anyone using this technique should realize that it is a very crude approach that may sometimes be misleading. The technique produces results quickly and relatively inexpensively and it does give the research worker a very valuable insight into the behavior of his system of variables. With a little experience, the actual graphing of the figures can usually be eliminated in favor of an examination of the means.

A word of caution: The research worker should not restrict himself to polynomial terms as I have done in the example above. He should use terms that will transform the figure he sees to a linear system. If a logarithm is indicated, it should be used. Likewise with any other transformation.

The use of the automated procedure requires a sizeable number of observations for best results, usually greater than 80. Small numbers of observations necessitate the construction of the three-dimensional figures on a peg board (9a).

(b) The use of patterns in model building. I believe that everyone who has worked with weather phenomena knows either intuitively or explicitly that it is not the temperature on a given day nor the soil moisture content during a given day or at a given time that defines the response of a plant to weather conditions. Rather, it is the pattern of the temperature and moisture effects through the entire growing season that constitutes the important information. In other words, a high moisture situation in the early part of the season followed by a very dry fall gives one type of a plant growth pattern. If we have extremely low soil moisture in the early part of the season, however, then we get another type of growth pattern. The concept of incorporating patterns into a mathematical model has been exploited a number of times already in dealing with weather phenomena. The early work of Fisher (3) on the effect of rainfall on the yields of wheat at Rothamstead Experimental Farm is a classic example of this approach. The thing that I am concerned about in using this approach is that the workers up to date have largely stressed or tried to determine the effect on the crop yield of an increasing amount of moisture or temperature through time. What should be stressed is that what they are really attempting to do is to capture the
information about a phenomena as it occurs through time in form suitable for incorporation into a mathematical model for predicting crop yields. They have taken the trend through time and transformed it by means of another mathematical function to a series of coefficients which express the pattern. In other words, they have reduced the pattern of an entire season to a series of coefficients which "hopefully" best express what happens through the entire season.

The major problem to be considered here is that most workers up to this time have used only two ways of capturing this pattern through time. The first employs orthogonal polynomials (6). A high degree polynomial function is used to express the precipitation pattern through time, and the coefficients of this function are then taken for inclusion in a mathematical model. The other way uses trigonometric functions to express patterns through time (1).

We should not limit ourselves just to the use of polynomial and trigonometric functions. Whatever mathematical function can best express the pattern of our phenomena through time, space, distance, or whatever should be chosen and fitted to the data (12, 10, 13). Then parameters of the mathematical function, which in essence captures all of the pertinent information concerning the pattern, should be used as independent variables in our multiple regression models.

(c) Construction of new variables by linear transformations. Some of the statistical techniques that have been developed in other fields of endeavor might provide useful ways to transform or manipulate our data in an attempt to create more meaningful variables.

Factor analysis (5) might be cited in this connection. In its simplest form, factor analysis consists of trying to capture the physical organization in space that our data points occupy. For example, consider a three dimensional system with variables $X_1$, $X_2$ and $X_3$. After obtaining a group of observations and recording the values for each variable, the points can be plotted as a three-dimensional figure. If we are dealing with a linear system the figure should be an elipsoid. We can make a linear transformation from our original variables to new variables which correspond with the axes of the ellipsoid. Many times all the information contained in several variables can be expressed in terms of a few new variables which account for the majority of the total variation in the system. In the case of weather phenomena, the use of factor analysis may permit extraction of meaningful directions of variation. These directions, in turn, when transformed into new variables, may capture pertinent information about our variable system which could be applied in a multiple regression problem. The reduction in the number of variables required to carry the information may be of particular importance.
As an illustration, consider the paper recently submitted for publication by M. J. Garber and others from the University of California at Riverside. These scientists applied factor analysis to the various measurements which would be concerned in a moisture stress analysis of growing oranges. New variables were extracted from among the various measured variables and used as independent variables in a multiple regression problem attempting to predict the daily growth rate of oranges. Details could be obtained by writing directly to Professor Garber.

The most pertinent illustration I can give of this from my own experience would be of a factor analysis that has been made of crop yields of alfalfa for a three-crop growing season, Table 1.

In factor analysis each of the variables is first standardized to a variable with zero mean and unit standard deviation. For example, the total variation in a three-variable problem is three units of variation. This is best expressed in the correlation matrix, which is also the starting point for factor analyses. Each of the factors that is extracted by factor analysis will express a certain proportion of this total variation.

The first variable in the alfalfa problem extracted a large proportion of the total of three units of variation and the second variable removed just about all the remainder. Essentially all of the variation among the three crop yields can be expressed in terms of two factors. Thus the first factor apparently expresses the characteristic of a high producing plant for the entire season, whereas the second factor seems to measure the disparity between early and late production.

In this case we are decomposing what might be considered dependent variables, but I think you realize that the same type of approach could be applied to a group of independent variables. The approach produces linear transformations of our variables that, hopefully, contain the information from the entire set of original variables in a small number of terms.

Another statistical technique that seems to hold some promise for further use is called canonical correlation (2). In multiple regression problems we try to relate a group of variables to a dependent variable. In canonical correlation we attempt to relate one group of variables to another group of variables. Such a solution would be particularly important when we are trying to predict a pattern of crop yields rather than the yield of a single crop. When the canonical correlation procedure is applied to a group of variables versus a single variable it becomes the multiple regression approach.

The only example I can give of the use of the canonical correlation technique is from data supplied by Eugene Peck of the U.S. Weather Bureau,
stationed at Salt Lake City. Mr. Peck is attempting to predict the patterns of precipitation associated with various types of storms as they come into the Wasatch Front region. The data were collected by means of weather observations taken at the middle of successive 12-hour periods during the pertinent season of the year and with recording instruments that operated at various elevations and locations and recorded the precipitation for the entire 12-hour period of record. These precipitation stations were located: (1) at the Salt Lake Airport, (2) at the foot of the Wasatch Front, (3) at the top of the Wasatch mountains, and (4) behind the Wasatch mountains.

Mr. Peck knew that various types of storms came into the region and that these storms tended to have different types of precipitation patterns with respect to elevation and position.

The application of the canonical correlation procedure, Figure 2, seemed to indicate, at least on the first set of data, that there were two principal types of storms moving into the area. One type would drop substantial moisture at the higher elevations, vector one, while the other type would be relatively effective at the low elevation, vector two. The canonical correlation technique in essence produces a personality profile of the two different types of storms.

A subsequent canonical correlation analysis done later on a new set of data produced essentially the same results.

Research workers who wish to use factor analysis and canonical correlation in an attempt to explain their phenomena should be aware that both of these techniques are essentially mathematical abstractions. The factors or patterns can not always be related to happenings in the real world, only in some cases will the mathematical abstractions lead to a suitable explanation of the system studied.

B. The Interpretation of the Mathematical Fitting Process

1. The use of Venn diagram. Many problems are associated with the interpretation of the mathematical fitting process. The one that seems to plague people the most is the interpretation of multiple regression results. I have found by experience that most users of multiple regression analysis do not have a complete understanding of what they are doing when they fit a multiple regression model and, more important, they do not seem to realize the full significance of the statistical tests employed.

   My own thinking along these lines has been greatly aided by the use of the Venn diagrams.
The prediction problem can be visualized as an attempt to develop a mathematical model which will give us perfect predictive power. In terms of multiple regression analysis, this is comparable to obtaining a multiple $R^2$ of 1. We can, therefore, think of the totality of information contained in a multiple regression problem as unity, and consider that the multiple $R^2$ produced in a particular case is the proportion of the total variation accounted for by our mathematical model. The totality of space inside the rectangle shown in Figure 4 is the total variation or unity. The circle inside the rectangle is the $R^2$ value of, let us say, $X_1$ against $Y$. To illustrate a point, we will give this a value of .45, which means we have .55 unit of unknown variation. This provides a very clear cut picture of exactly what the analysis gives us.

Now let us extend the situation to a problem having two independent variables, where $X_1$ and $X_2$ are both used in predicting $Y$ (Figure 5). The contents of circle A represent the predictive power provided by variable $X_1$. The contents of circle A and B represent the predictive power of variable $X_1$ and $X_2$ considered jointly. The proportion outside the circles but inside the rectangle is the unknown information.

If the two variables were completely independent, the information that we would have in our multiple regression system would be the summation of the contribution of the two variables or a multiple $R^2$ of .80. Yet, when we put these two variables into a multiple regression system we get $R^2_{X_1 X_2 Y} = .60$. Obviously part of the information that is contained in $X_1$ is also contained in $X_2$. The diagramatic approach allows us to partition this information picture into components, as illustrated in Figure 5. From this figure we see that the total $R^2_{X_1 X_2 Y} = .60$ can be decomposed into three different parts -- one that is unique to $X_1$, one that is unique to $X_2$, and one that is jointly obtainable from either variable. You will notice that the unknown information has dropped to .40.

The complicated distribution of the information contained in a three-variable system is illustrated in Figure 6. Some information is contained uniquely in $X_1$, or $X_2$, or $X_3$. Some is contained jointly between $X_1$ and $X_2$, between $X_1$ and $X_3$, and between $X_2$ and $X_3$. Other information is contained jointly in all three variables $X_1$, $X_2$, $X_3$. In addition there is information contained jointly between $X_1$ and $X_2$ that is not contained in $X_3$, and so forth.

If we make a multiple regression analysis of variance of the information system depicted by Figure 6, we obtain Table 2. The ordinary test of hypothesis for the significance of the partial regression coefficient for $X_1$ tests the amount of information that is unique to $X_1$ and not contained in either of the other variables, in this case, .05. This test uses the ratio of the unique information to the information yet unaccounted for. Both of these amounts of information are adjusted for degrees of freedom.
Figure 4. Venn diagram of a simple regression situation.

Figure 5. Venn diagram of multiple regression situation.
(Two variable case.)

Figure 6. Venn diagram of multiple regression situation.
(Three variable case.)
By studying Figures 4, 5 and 6 it can be seen that as more variables are added to the system we should be continually decreasing the unknown information. For example, the unknown information decreases from .55 in Figure 4 to .15 in Figure 6.

In Figure 5 we see that the amount of information unique to \( X_1 \) has dropped to .25 while the unknown information has dropped to .40. In Figure 6 we see that the amount of information unique to \( X_1 \) has dropped to .05 while the unknown information has dropped to .15.

I think this approach illustrates what happens to our various test of statistical hypotheses as we add more variables to the system. It is impossible to say whether an F test should go up or go down as we add variables to the system. The result depends entirely on how much of the information in the new variable overlaps that of the variables already in the system and how much new information is contained in the new variable.

The equations for making the information partition indicated in Figures 5 and 6 are given below:

\[
\begin{align*}
A &= r^2 X_1 Y \\
B &= r^2 X_2 Y \\
C &= r^2 X_3 Y \\
AUB &= R^2 X_1 X_2 Y \\
A\Lambda B &= r^2 X_1 Y + r^2 X_2 Y - R^2 X_1 X_2 Y = .45 + .35 - .60 = .20 \\
A\Lambda B\Lambda C &= r^2 X_1 Y + r^2 X_2 Y + r^2 X_3 Y - R^2 X_1 X_2 Y - R^2 X_1 X_3 Y - R^2 X_2 X_3 Y + R^2 X_1 X_2 X_3 Y = .45 + .35 + .60 - .50 - .75 - .80 + .85 = .10 \\
(A\Lambda B)\Lambda C &= A\Lambda B - A\Lambda B\Lambda C = .20 - .10 = .10
\end{align*}
\]

2. Composite hypotheses. The diagramatic approach also suggests other meaningful hypotheses which should be tested. For example, if we consider the shaded portion of Figure 6, we could test the hypothesis of the joint effect of variables one and three independent of, or adjusted for variable two. In this case we would get value of .50 for the partition of the multiple correlation coefficient that is accounted for by variables one and three adjusted for variable two.
As researchers start getting more complicated mathematical models involving squares and product terms of variables, they will find that the interpretation becomes much more difficult. For example, in the situation that we have been discussing in Figure 6, if variable two happened to be the square of variable one, it would be highly illogical to test the contribution of variable one independent of variable two, because variable one and variable two are functionally related. Therefore, to test the hypothesis that the independent contribution of variable one really measures the effect of variable one, or that the independent contribution of variable two measures the total contribution of variable one would be highly ridiculous. The only way we can possibly get a test of the effectiveness of the variable is to test the joint contribution of both terms.

The full implications of this can best be realized by an example. The analysis of a set of data from a study of the effect of various plant and ecological characteristics on the yield of alfalfa seed (9) is tabulated in Table 4. A number of cross product and square terms were used. Looking at the analysis of variance through term 20 leads to the conclusion that blotch is of no practical importance in interpreting the multiple regression model because none of the terms involving blotch are significant at less than the .25 probability level. Upon careful thought and by referring back to the Venn diagram approach, however, it is evident that blotch, blotch-squared, and chaff-by-blotch interaction terms are all direct functions of the disease rating blotch itself. If we first remove, in line 21, the effect of blotch and blotch-squared we still obtain no statistical significance for this disease rating. If we then remove, in line 24, all the terms that are associated with blotch (in other words, blotch, blotch-squared, and chaff-by-blotch interaction), however, we have a new subset with three degrees of freedom. This term is significant at the .005 level.

The meaning of the foregoing is that we must have a rather complete knowledge of the interrelations among the variables we are using before we can make intelligent assessment of which analysis of variance will be most useful.

It might be pointed out that the multiple \( R^2 \) in this particular set of data was increased from .583 to .745 by the addition of the curvilinear and interaction terms. The inclusion of these terms was indicated by the model-building approach heretofore discussed.

3. **Stepwise regression.** With the advent of efficient computers, the stepwise regression approach has become extremely popular. Two difficulties are associated with this approach. The first one is that the research worker is rather tacitly assuming that all of the pertinent variables are contained in this system. The second is that he is assuming that the
Stepwise regression is the most efficient way of winnowing out non-pertinent terms. Either of these assumptions may or may not be true. The computer certainly is incapable of suggesting terms to go into the mathematical model and in many cases it will even be unable to select the most pertinent variables.

For example, if we consider the correlations given in Table 5 we would be led to conclude that variables three and four could well be deleted from the system, and the stepwise regression program when applied to the same set of data made exactly this decision. If we know enough about the interrelations among our variables and how the variables have been constructed, however, we would include both variables three and four in this system with rather surprising results.

The $R^2$ value for including variables one and two in this system gives $R^2_{X_1X_2,Y} = .0877$. If we force the seemingly non-important variables three and four into the system jointly, we obtain a jump of $R^2$ to .5031. What hasn't been mentioned before, is that variable four is the square of variable three, and the range of variable three has been very carefully constructed so that it creates a parabola.

For example, if we consider the correlations given in Table 5 we would be led to conclude that variables three and four could well be deleted from the system, and the stepwise regression program when applied to the same set of data made exactly this decision. If we know enough about the interrelations among our variables and how the variables have been constructed, however, we would include both variables three and four in this system with rather surprising results.

The linear relationship between $X_3$ and $Y$ gives nothing but a flat line with zero slope and hence the low correlation. The simple quadratic, or $X_4$, as a linear function of $Y$, doesn't do a much better job. When we know that $X_4$ is the square of $X_3$, and that $Y$ is a parabola with respect to $X_3$, we are able to get the tremendous jump from .08 to .50. This of course can only be done if variables three and four are forced into the problem simultaneously. None of the stepwise computer programs to date attempt to make multiple inclusions. These programs assume that only the independent contribution of the variable needs to be interrogated as a basis for deciding whether or not to include it in the system.

I suggest that research workers who plan to use stepwise procedures should make an initial run in which they force all the variables into the system. They can use the $R^2$ from this run as a check on the stepwise run. If the stepwise run does not produce substantially the same $R^2$ as the complete run, groups of variables may need to be forced into the system.

4. Non linear relationships along the independent variables. It is possible to have a set of data, with zero correlations among the independent variables and zero or near zero correlations of the independent variables with the dependent variable, which will show a large multiple $R^2$. This may be partially illustrated by the material in Table 5.

The sum of $r^2_{X_3Y}$ and $r^2_{X_4Y}$ is equal to .016, which indicates that neither variable is important. The model, with both variables included,
produces an $R^2$ of .50. The latter value is much greater than the sum of the independent contribution. This is a direct result of the curvilinear relationship between $X_3$ and $X_4$.

Curvilinear relationships among independent variables indicate a need for special care in interpreting the standard statistical analysis. The standard analysis assumes linearity. The diagramatic approach also assumes linearity. Non-linear relationships may show up as negative areas for some intersections and segments.

**Summary**

Probably the best summary is simply to state that although the theory of multiple regression is well developed, there is room for a great deal of improvement in the art.
Table 1. Factor Analyses on Alfalfa Yields.

<table>
<thead>
<tr>
<th>Correlations</th>
<th>Factor Analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Crop</strong></td>
<td><strong>1st</strong></td>
</tr>
<tr>
<td>Field one</td>
<td></td>
</tr>
<tr>
<td>First crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Second crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Third crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Variance</td>
<td>2.081</td>
</tr>
<tr>
<td>Field two</td>
<td></td>
</tr>
<tr>
<td>First crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Second crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Third crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Variance</td>
<td>2.355</td>
</tr>
<tr>
<td>Field three</td>
<td></td>
</tr>
<tr>
<td>First crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Second crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Third crop yield</td>
<td>1.0000</td>
</tr>
<tr>
<td>Variance</td>
<td>1.919</td>
</tr>
</tbody>
</table>

Table 2. Multiple Regression Analysis from Figure 6.

<table>
<thead>
<tr>
<th>Source</th>
<th>D. F.</th>
<th>Sums of Squares</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Symbolical</td>
</tr>
<tr>
<td>Total</td>
<td>25</td>
<td>( r^2_{XY} )</td>
</tr>
<tr>
<td>Model</td>
<td>3</td>
<td>( R^2_{X_1X_2X_3Y} )</td>
</tr>
<tr>
<td>Due to (X_1)</td>
<td>1</td>
<td>.05</td>
</tr>
<tr>
<td>Due to (X_2)</td>
<td>1</td>
<td>.10</td>
</tr>
<tr>
<td>Due to (X_3)</td>
<td>1</td>
<td>.25</td>
</tr>
<tr>
<td>Due to (X_1 + X_2)</td>
<td>2</td>
<td>.25</td>
</tr>
<tr>
<td>Due to (X_1 + X_3)</td>
<td>2</td>
<td>.50</td>
</tr>
<tr>
<td>Due to (X_2 + X_3)</td>
<td>2</td>
<td>.40</td>
</tr>
<tr>
<td>Residual</td>
<td>22</td>
<td>( 1 - R^2 )</td>
</tr>
</tbody>
</table>
Table 3. Canonical Correlation Analysis, Eugene Peck Data.

<table>
<thead>
<tr>
<th>Meteorological Factors</th>
<th>Canonical Vectors</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$c_1$</td>
</tr>
<tr>
<td>$X_1$ Initial Vorticity -- A measure of the absolute vorticity at the time of the observation. Is an indication of the cyclonic curvature of the airmass. Related to the activity of the storm.</td>
<td>.2260</td>
</tr>
<tr>
<td>$X_2$ Vorticity Advection -- A measure of the change expected in vorticity from upper air charts. Large positive values would indicate more cyclonic curvature thus more precipitation.</td>
<td>.2885</td>
</tr>
<tr>
<td>$X_3$ Vertical Velocity -- Computed value from vorticity and upper air maps. An index to the vertical motion in the airmass. Large values upward motion.</td>
<td>.4735</td>
</tr>
<tr>
<td>$X_4$ 700 mb DD -- Direction in tens of degrees of wind at 700 millibar level (approximately 10,000 feet).</td>
<td>.2059</td>
</tr>
<tr>
<td>$X_5$ 700 mb WV -- Speed of wind at 700 millibar in knots per hour.</td>
<td>.9434</td>
</tr>
<tr>
<td>$X_6$ 500 mb DD -- Same as $X_4$ except for 500 millibar level (approximately 18,000 feet).</td>
<td>-.2881</td>
</tr>
<tr>
<td>$X_7$ 500 mb WV -- Same as $X$ except for 500 millibar level.</td>
<td>.2733</td>
</tr>
<tr>
<td>$X_8$ CCL -- Measure of the moisture content and somewhat of the temperature of the lower airmass. Higher values indicate more moisture.</td>
<td>.5009</td>
</tr>
<tr>
<td>$X_9$ $\theta^<em>_E$ 700 - $\theta^</em>_E$ 850 -- Difference in the equivalent potential temperature ($\theta^*_E$) at 700 millibar level (approximately 10,000 feet) and at 850 millibar level (approximately 5,000 feet). Values below 50 indicate unstable air; those above, stable air.</td>
<td>-.1192</td>
</tr>
</tbody>
</table>

12 Hour Precipitation

<table>
<thead>
<tr>
<th>Location</th>
<th>$c_1$</th>
<th>$c_2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>SLC Airport (Valley bottom)</td>
<td>-.0201</td>
<td>.1064</td>
</tr>
<tr>
<td>Cottonwood (West face)</td>
<td>.1588</td>
<td>.8336</td>
</tr>
<tr>
<td>Brighton (Top)</td>
<td>.8884</td>
<td>-.5230</td>
</tr>
<tr>
<td>Echo Dam (Shadow)</td>
<td>.4303</td>
<td>-.1422</td>
</tr>
</tbody>
</table>

Canonical $R^2$  

|          | .309  | .229  |
Table 4. Multiple Regression Analysis of Variance of Alfalfa Yield Data (9).

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>M.S.</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Intercept</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Rac./Ac.</td>
<td>1</td>
<td>9,462.9</td>
<td>.61</td>
</tr>
<tr>
<td>2. Stem/Ac.</td>
<td>1</td>
<td>171.0</td>
<td>.01</td>
</tr>
<tr>
<td>3. Rac./Stem</td>
<td>1</td>
<td>4,146.7</td>
<td>.27</td>
</tr>
<tr>
<td>4. Seeds/Pod</td>
<td>1</td>
<td>8,501.8</td>
<td>.55</td>
</tr>
<tr>
<td>5. Chaff</td>
<td>1</td>
<td>8,504.7</td>
<td>.55</td>
</tr>
<tr>
<td>6. Blotch</td>
<td>1</td>
<td>12,270.4</td>
<td>.79</td>
</tr>
<tr>
<td>7. Soil Tension</td>
<td>1</td>
<td>32,810.9</td>
<td>2.13(.25)</td>
</tr>
<tr>
<td>8. Bees (yd.²)</td>
<td>1</td>
<td>92,496.3</td>
<td>6.00(.025)</td>
</tr>
<tr>
<td>9. Sugar</td>
<td>1</td>
<td>43,963.3</td>
<td>2.84(.10)</td>
</tr>
<tr>
<td>10. Flowers/Ac.</td>
<td>1</td>
<td>12,816.2</td>
<td>.83</td>
</tr>
<tr>
<td>11. Rac./Ac. x Seeds/Pod</td>
<td>1</td>
<td>38,841.6</td>
<td>2.52(.25)</td>
</tr>
<tr>
<td>12. Rac./Stem x Seeds/Pod</td>
<td>1</td>
<td>887.9</td>
<td>.06</td>
</tr>
<tr>
<td>14. Soil Tension²</td>
<td>1</td>
<td>20,802.7</td>
<td>1.35(.25)</td>
</tr>
<tr>
<td>15. Sugar²</td>
<td>1</td>
<td>44,427.6</td>
<td>2.88(.10)</td>
</tr>
<tr>
<td>16. Chaff x Blotch</td>
<td>1</td>
<td>38,553.9</td>
<td>2.50(.25)</td>
</tr>
<tr>
<td>17. Chaff x Sugar</td>
<td>1</td>
<td>2,986.9</td>
<td>.19</td>
</tr>
<tr>
<td>18. Tension x Flowers/Ac.</td>
<td>1</td>
<td>5,023.2</td>
<td>.33</td>
</tr>
<tr>
<td>19. Tension² x Flowers/Ac.</td>
<td>1</td>
<td>3,112.8</td>
<td>.20</td>
</tr>
<tr>
<td>20. Sugar x Flowers/Ac.</td>
<td>1</td>
<td>15,943.9</td>
<td>1.03</td>
</tr>
</tbody>
</table>

**Sub-sets**

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>M.S.</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>21. Blotch (6*, 13)</td>
<td>2</td>
<td>21,432.0</td>
<td>1.39</td>
</tr>
<tr>
<td>22. Tension (7, 14)</td>
<td>2</td>
<td>22,158.8</td>
<td>1.44(.25)</td>
</tr>
<tr>
<td>23. Sugar (9, 15)</td>
<td>2</td>
<td>22,240.6</td>
<td>1.44(.25)</td>
</tr>
<tr>
<td>24. Blotch (6, 13, 16)</td>
<td>3</td>
<td>152,905.3</td>
<td>9.92(.0005)</td>
</tr>
<tr>
<td>25. Tension (7, 14, 18, 19)</td>
<td>4</td>
<td>48,792.7</td>
<td>3.17(.025)</td>
</tr>
<tr>
<td>26. Sugar (9, 15, 17, 20)</td>
<td>4</td>
<td>31,314.6</td>
<td>2.03(.10)</td>
</tr>
</tbody>
</table>

**Residual**

<table>
<thead>
<tr>
<th>df</th>
<th>M.S.</th>
<th>F</th>
</tr>
</thead>
<tbody>
<tr>
<td>51</td>
<td>15,406.3</td>
<td></td>
</tr>
</tbody>
</table>

*Numbers refer to items listed above.*
Table 5. Simple Correlation. Monte Carlo Data used to illustrate nonlinearity problems.

<table>
<thead>
<tr>
<th></th>
<th>$X_1$</th>
<th>$X_2$</th>
<th>$X_3$</th>
<th>$X_4$</th>
<th>$Y$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$X_1$</td>
<td>.7764</td>
<td>-.0116</td>
<td>-.0352</td>
<td>.2757</td>
<td></td>
</tr>
<tr>
<td>$X_2$</td>
<td>.0023</td>
<td>-.0484</td>
<td>.1549</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$X_3$</td>
<td></td>
<td>.9716</td>
<td>.0508</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$X_4$</td>
<td></td>
<td></td>
<td>-.1180</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>


11. Shupe, J. L., et al., The Effect of Fluorine on Dairy Cattle. II 
Clinical and Pathologic Effects, American Journal Vet. Research 

12. Snedecor, G. W., Curve Fitting: An Art or a Science?, Iowa State 

13. Struble, G., Non-linear Least-Squares Curve Fitting Program, 1620 
EVIDENCE OF SOLAR-CLIMATIC RELATIONSHIPS

Hurd C. Willett

I. Introductory Remarks

The volume of synoptic and statistical analysis of weather and solar data which has been performed in the investigation of solar-weather relationships is vast and much of it so inconclusive statistically as to results. Thus in a brief survey of the subject like this one definite limitations have to be placed on the area and the quality of the investigations to be considered, and some rational outline or plan must be followed in the discussion. In the effort to achieve these ends the following discussion is selective and progressive in these respects:

1. The discussion is restricted to solar-climatic (long-term) as distinct from solar-weather (short-term) relationships. This restriction is made partly in the interest of brevity, partly because the short-term relationships are more controversial and less readily susceptible to statistical proof, and partly because the climatic evidence is presumed to be of greater interest to this conference.

2. Wherever possible the present discussion is based on the use of normalized climatic data. Unfortunately, in the past relatively little of the statistical work dealing with climatic fluctuations has concerned itself with normalization of the data. In my experience this lack makes objective statistical evaluation of the evidence difficult and leads to serious misunderstanding both seasonally and geographically as to the significance of the relationships.

3. Particular attention is paid to the western plains section of North America, partly because this is an area which appears to be quite susceptible to sunspot-oriented climatic fluctuations, and partly because the agricultural utilization of much of this area is marginally sensitive to these fluctuations.

4. The discussion progresses from the hemispheric evidence for the 80-90 year solar-climatic cycle and that for the double sunspot solar-climatic cycle of 20-24 years, to the evidence for the same cycles in the climatic records of the western plains of North America. It concludes with a few pertinent remarks on possible links between solar activity and the weather.

---

1/Professor, Department of Meteorology, Massachusetts Institute of Technology.
II. The 80-90 Year Solar-Climatic Cycle

Inspection of the record of annual mean relative sunspot numbers, which extends in reasonably homogeneous and dependable form back to 1749, discloses through this period a rather pronounced cycle of 80-90 years. The outstanding feature of this long cycle is a sudden break from very high sunspot numbers and a curtailed length of the 11-year sunspot cycle (a very actively disturbed sun) during the fourth quarter of the cycle, to very low sunspot numbers and a pronounced lengthening of the 11-year cycle (a relatively undisturbed sun) during the first quarter of the new cycle. During two of the long cycles, those ending with the very strong 11-year sunspot maxima of 1787 and 1957, the two middle quarters of the long cycle were marked by a progressively increasing level of sunspot activity. In the intervening long cycle, which terminated with the strong sunspot maximum of 1870, the first inactive quarter of the cycle was followed by a sudden flare-up of strong sunspot activity during the 1830's, which subsided gradually until the strong flareup towards 1870.

The evidence is strong in many quarters—including the levels of non-outlet lakes, the advance and recession of glaciers and the older fragmentary climatic data—that the general circulation and climate of the northern hemisphere (and probably also of the southern) responds significantly to this long cycle of solar activity. Unfortunately a climatic record adequate to demonstrate this response clearly exists only since the turn of the century, but for that period it does tell an interesting story which we shall consider in detail in a moment. However, by piecing together the fragmentary climatic record extending farther back, it is possible to obtain the broad outlines of a climatic cycle in relation to the long cycle of solar activity.

The primary features of this climatic cycle may be expressed in simplest terms essentially as follows:

1. The first quarter of the cycle, following the sudden break from a very high to a very low level of solar activity, is marked by the predominance of a low latitude zonal pattern of general circulation.

This circulation pattern is associated with a cool wet climate in lower middle latitudes, cool and dry in higher latitudes, and relatively steady conditions—involving a minimum of maritime-continental contrasts—in middle latitudes, and a minimum of summer-winter seasonal contrasts over the continents. This is the circulation pattern most favorable for the growth and advance of glaciers wherever they exist in middle latitudes, and over a long period of time, for the development of an ice age.

2. In the second and third quarters of the cycle solar activity increases slowly and steadily as it has in the current cycle. There is a tendency of the zonal pattern of the circulation to shift poleward, at first slowly and then more rapidly, so that by the third quarter a pronounced high latitude zonal pattern predominates.
This circulation pattern of the third quarter is associated with a warm wet climate in higher middle latitudes, essentially poleward of 50° N, and a warm dry climate, with strongly-developed continental-maritime contrasts in lower middle latitudes. This condition favors warm dry summers and cool dry winters in the interior of continents in lower middle latitudes. It is therefore most favorable to the rapid recession of glaciers in middle latitudes, and over a long period of time, for the maintenance of an interglacial epoch.

3. The fourth quarter of the cycle, when solar disturbances reach a high peak of activity, is marked by a pronounced tendency for the strong zonal circulation pattern to break up into a cellular blocking pattern of strong meridional circulation in all latitudes. Strong solar continental anticyclones develop in the higher latitudes and block the maritime lows, particularly during the winter season. But in summer strong maritime highs over the oceans in middle latitudes alternate with stronger than normal continental heat lows.

This pattern is known as one of climatic stress. It is typically one of maximum east-west contrasts of temperature and storminess in all latitudes, of maximum maritime-continental contrasts and of maximum seasonal thermal contrasts between cold dry winters and warm dry summers in the interior of continents. In general it is unfavorable to glaciation or glacial advance except on the western (maritime) side of continents in higher middle latitudes, where winter snowfall may be heavy and the summers cool.

This general picture of the 80-90 year cycle of climate and solar activity was gained from a number of past studies of climatic variations, most notably perhaps from Lysgaard (1), Willett (2), Murray Mitchell (3) and Willett (4).

Lysgaard's study is the only one of the four that deals with precipitation as well as with temperature, and his principal contribution to this discussion lies in that area. He expressed precipitation totals for the 30-year period 1880-1910 (essentially the low latitude zonal phase of the 80-90 year cycle) in the form of ratios to the totals for the 30-year period 1910-1940 (essentially the high latitude zonal phase of the cycle) at each of 130 stations scattered around the northern hemisphere. His map of the geographical distribution of these ratios showed quite generally ratios in excess of 100 percent, or wetter during the latter period. poleward of 50° N, and less than 100 percent equatorward of 50° N. The relative deficiency during the latter period was particularly notable in the central and southern United States, in contrast to western Canada, and in southeastern Europe and southwestern Asia, i.e., in areas of continental climate in lower middle latitudes. This pattern of change is exactly that to be expected with a trend from predominance of a low latitude to that of a high latitude zonal pattern of the general circulation.
Willett's paper (2) deals primarily with successive five yearly changes of annual mean and winter mean temperature by $10^\circ$ latitude zones from about 1850 to 1940, based on Clayton's World Weather Data. Willett's data were amplified and extended forward through 1959 (four additional pentads) by Mitchell (3). The gist of the results of this study is given in Table I, taken from Mitchell's paper, which tabulates from 1880 to 1960 the difference between the successive annual and winter five-year means of temperature in each $10^\circ$ latitude belt, and that of the same $10^\circ$ belt during the last pentad, 1955-1959.

Looking primarily at the trend of annual mean temperature on the northern hemisphere, for which data coverage is much more adequate than for the southern, we may note in particular that a pronounced warming trend continued from the 1880-1899 period (the first or low-latitude zonal phase of the long cycle) through the third period, such that maximum warmth was reached generally between 1930-1945 (the peak of the high latitude zonal phase of the long cycle). Since 1940 or 1945, with the advent of the cellular blocking climatic stress phase, moderate irregular cooling has set in in the higher latitudes, again in line with expectations. However, in the lower middle latitudes, particularly in the subtropical belt of high pressure from $20^\circ$-$40^\circ$ N, warming continued well into or through the 1940-1960 period, i.e., the fourth quarter or climatic stress phase of the long cycle.

The study by Willett (4) investigates more comprehensively the changes of the general circulation and temperature of the northern hemisphere from the second phase (1900-1919, still primarily low-latitude zonal) to the third phase (1920-1939, strongly high-latitude zonal) of the 80-90 year solar climatic cycle. This particular phase change was studied partly because it embraced the principal period of recent warming of the higher latitudes of the northern hemisphere. Also because adequate synoptic data for the preceding phase were lacking, and when the study was made these data were not yet available for the final fourth quarter phase.

The primary results of the study of this particular phase change over the northern hemisphere completely confirmed expectations, notably as follows:

1. A strong increase of sea-level zonal westerlies between $50^\circ$-$70^\circ$ N from phase 2 to phase 3 was noted, for both the summer and the winter seasons.

2. The marked winter-season warming of the hemisphere poleward of $50^\circ$ N was contributed primarily by greatly increased zonal westerlies from the Atlantic across most of northern Eurasia. An equal and geographically similar increase of the zonal westerlies in summer produced no warming of the northern Eurasian land mass as might be expected of the maritime influence—rather it produced a slight cooling.
### Table 1: Mean Temperature by Pentads, Expressed as Departures in F Deg From 1955-1959 Pentad, Each 10°-Latitude Band From 80°N to 60 S

<table>
<thead>
<tr>
<th>Pentad</th>
<th>80°N</th>
<th>70°N</th>
<th>60°N</th>
<th>50°N</th>
<th>40°N</th>
<th>30°N</th>
<th>20°N</th>
<th>10°N</th>
<th>0°N</th>
<th>10°S</th>
<th>20°S</th>
<th>30°S</th>
<th>40°S</th>
<th>50°S</th>
<th>60°S</th>
</tr>
</thead>
<tbody>
<tr>
<td>1950-54</td>
<td>-0.3</td>
<td>0.2</td>
<td>0.2</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1945-49</td>
<td>-0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1940-44</td>
<td>-0.3</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1935-39</td>
<td>-0.5</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1930-34</td>
<td>-0.7</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1925-29</td>
<td>-0.9</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1920-24</td>
<td>-1.2</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1915-19</td>
<td>-1.5</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1910-14</td>
<td>-1.8</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1905-09</td>
<td>-2.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1900-04</td>
<td>-2.4</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1895-99</td>
<td>-2.7</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1890-94</td>
<td>-3.0</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>1885-89</td>
<td>-3.3</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
<td>0.1</td>
</tr>
</tbody>
</table>

Note: Values in parentheses based on data for one station only.
3. Equatorward of 50° N some weakening of sea-level zonal westerlies occurred, and with it an increase of monsoonal cellular contrasts, notably of the vast continental monsoonal cells over Eurasia.

4. Across the interior of the entire Eurasian land mass there was a significant lowering of the mean winter season temperature and a significant increase of the mean summer season temperature from the 1900-1919 to the 1920-1939 period.

No further discussion of the interesting results of this investigation nor presentation of supporting charts or figures is offered here. Rather it seems preferable to direct attention to a current investigation of a similar nature which extends the previous investigation to the fourth quarter of the long cycle, and which is based on normalized data and is therefore much more significant, statistically.

In this recent study, as yet unpublished, northern hemisphere seasonal mean charts of departure from normal of sea-level pressure, and of 500-mb (1900-1939) or 700-mb (1940-1959) contour heights are normalized by seasons, i.e., the departures from the 60-year mean at each grid point are expressed in the form of ratios to the standard deviation of the element at the respective grid-point for the respective season. These normalized departures of contour heights and sea-level pressures are all plotted and analyzed, and averaged by season for the three successive 20-year periods, 1900-19, 1920-39 and 1940-59. The 20-year seasonal mean charts are plotted and analyzed, and in each case the normalized sea-level chart is subtracted from the normalized upper-level contour chart to furnish a thickness chart of differences of normalized sea-level pressure and normalized contour heights. These difference charts are not quite identical with, but correlate very highly with the corresponding 20-year mean departures of normalized mean virtual temperature between sea-level and the upper-level contour height. Hence they represent in each case a very close approximation to the pattern of the 20-year means of the normalized seasonal departures from normal of the mean temperature of the lower half of the troposphere. These charts probably contain a far more truly representative picture of the long-term changes of the temperature of the northern hemisphere than can be obtained from zonal averages of surface temperature at scattered stations.

Figure 1 contains the northern hemispheric zonal profiles of the 20-year means of the normalized seasonal departures of sea-level pressure (solid lines) and of contour height–sea level pressure difference, i.e., mean virtual temperatures (dashed lines) for the period 1900-1959. Unfortunately, for the last 20-year period the sea-level pressure data commence only at 20° N instead of 10° N, while the contour heights, or thickness, extends only to 25° N for the entire period.

There are a number of features in these profiles that confirm the expected phase aspects of the 80-90 year cycle to an amazing degree. First we may note three general features of interest concerning the entire group
Fig. 1. Twenty-year Mean Northern Hemispheric Seasonal Profiles Sea-Level Pressure and Thickness (Temperature).
of twelve pairs of seasonal profiles. One is a consistent tendency for the 20-year seasonal means of sea-level pressure and of thickness (Tv) to vary inversely (correlation ranging from -0.94 in winter to -0.72 in summer). This correlation implies a strong tendency for thermal compensation of surface pressure anomalies in the lower troposphere, strongest in winter. The second fact to be noted is the large amplitude of many of the latitude circle anomalies, particularly in the lower latitudes. A departure of 0.5 \( \sigma \) means that for twenty consecutive years the average seasonal departure of the 36 grid-points on that parallel amounted to one half of the standard deviation of the individual seasonal grid-point departures over the 60-year period. Finally, of particular significance for the reality of the 80-90 year climatic cycle is the striking similarity of the departure profiles for all four seasons within each quarter of the cycle, compared with the differences between phases of the cycle.

Looking at each of the three quarter-phases of the cycle represented in Figure 1, we may note in particular the following features of interest on the northern hemisphere:

1. Second quarter, 1900-1919. Sea-level pressure averaged normal or above at all latitudes and all seasons, excepting only at 10° N during the summer season. The temperature of the lower half of the troposphere was also uniformly below normal for the period. Note in particular the consistently above-normal strength of the low latitude zonal westerlies in all seasons, primarily from 20°-40° in winter and spring, and from 30°-50° in summer and fall. Equally striking is the consistent coldness in lower middle and subtropical latitudes, particularly the seasonal progression of the zone of maximum anomalous coldness in lower latitudes with the sun, from 25° N in winter to 35° N in summer, and back again.

2. Third quarter, 1920-1939. Sea-level pressure is consistently below normal and temperature above normal in middle and higher latitudes during all four seasons. Most noticeable is the increase of the zonal west wind component since the preceding period between 60°-80° N for every season but spring.

3. Fourth quarter, 1940-1959. Most striking during this period is the consistent deficit of pressure and excess of temperature during all four seasons in the subtropical high pressure belt. Note the surplus of pressure from 50°-70° N in winter (zone of the winter season polar continental anticyclones) and at 50° N during the other seasons (poleward intensification of the major warm season maritime monsoonal anticyclones). All of this reflects the predominance during this quarter of cellular blocking patterns of the general circulation. Note again as during the 1900-1919 quarter the poleward progression from winter to summer (and equatorward return following the sun) of the major thermal anomaly in low latitudes, this time positive instead of negative.
Undoubtedly the most striking and most significant feature of these 20-year seasonal anomaly profiles is the behavior of the temperature anomaly in low latitudes. Not only does it represent probably the primary thermodynamic drive of these changing circulation patterns of the 80-90 year climatic cycle, but it appears to be directly responsive to the sun and to follow the 80-90 year cycle of irregular solar activity.

We turn next to a brief consideration of apparent effects of the double sunspot cycle on the world weather patterns, and then to a discussion of the manifestation of both the 80-90 year cycle and the double sunspot cycle on the climate of the western plains of North America.

III. The Double Sunspot (20-24 Year) Solar-Climatic Cycle

The basic cycle of sunspot numbers is the so-called 11-year cycle, which has actually varied in length from seven to 17 years between successive sunspot maxima, and from nine to 14 years between minima. However, the so-called Hale or double sunspot cycle is much more clearly reflected in solar-climatic relationships, at least outside of the tropics. That there is a physical reality in the double sunspot cycle on the sun is indicated by a tendency for the sunspot number to be alternately lower and higher with successive maxima, for the polarity of the magnetic fields associated with sunspot groups on the sun's surface to reverse from one maximum to the next, and for the corpuscular (charged particle) radiations reaching the earth from the sun to be quite differently related to alternate maxima. (See Figure 8.)

Climatically the double sunspot cycle is manifested primarily by an opposite trend of the pattern of the general circulation as we approach alternate sunspot maxima. In passing from the sunspot minimum to the major maximum (highest sunspot number) there is a strong trend towards increasing prevalence of the climatic stress circulation patterns, meaning a predominance of polar continental anticyclones in high latitudes in winter, and warm dry summers in the interior of the continents. In passing from the sunspot minimum to the alternate minor maximum the trend is towards increasing prevalence of the low latitude zonal pattern of the general circulation, meaning a more southerly course of the prevailing storm tracks in middle latitudes, and in particular generally wetter conditions and cooler wetter summers in lower middle latitudes. Following the minor sunspot maximum in the double sunspot cycle there is a pronounced trend towards a shift of the zonal circulation pattern from lower to higher latitudes. This change implies a poleward shift of the prevailing storm tracks and precipitation, with a return to warmer and dryer conditions in lower middle latitudes.
The trends of the 20-24 year climatic cycle are superposed on those of the longer cycle. But they are less zonally uniform and they are seen most clearly in the winter season and in middle and higher rather than in lower latitudes. These facts imply that in contrast to the longer cycle, they represent primarily disturbances of the dynamics of the general circulation, perhaps by disturbances of the dynamics of the general circulation, perhaps by disturbances of the maritime-continental heat budgets, rather than a change of the effective solar constant working in the lower latitudes.

The 64-year series of seasonal northern hemispheric charts of normalized departures of sea-level pressure, of 500- or 700-mb contour heights and of thickness between sea-level pressure and the upper level contours, were averaged by the three-year phases of the double sunspot cycle. The clearest manifestation of the double sunspot cycle on the hemispheric scale is to be seen in the difference that the progression from sunspot minimum to major maximum, in contrast to the progression from sunspot minimum to minor maximum, has on the winter season normalized departures. This is seen best by averaging the winter season normalized departure charts of each element for all nine seasons of the 64-year period that lie in the major maximum phase (MM), in the preceding minimum phase (m), in the minor maximum phase (M) and in the preceding minimum phase (mm) of the double sunspot cycle. The change of the hemispheric winter mean departures from phase m to MM are given by the difference of the means (MM-m), and correspondingly from phase mm to M by the difference of the means (M-mm). The difference between the trend of the mean winter season circulation patterns of the northern hemisphere going into a major maximum and the trend of the same circulation patterns going into a minor maximum is the difference of the two sets of change patterns, i.e., by \( [\text{MM-m} - \text{M-mm}] \). The three sets of phase change difference charts (sea-level pressure, upper level contours and thickness) were plotted and analyzed for the winter and summer seasons, and the hemispheric profiles computed and drawn. These six profiles are contained in Figure 2. On these profiles the abscissae are differences of normalized (standard deviation ratio form) departures. Positive values mean the change is towards a relatively higher mean latitudinal value of the element going into the major sunspot maximum, negative values mean the change is towards a relatively higher value going into the minor sunspot maximum.

With regard to the difference of phase change profiles in Figure 2 we may note the following:

a. It is highly indicative of the climatic significance of the double sunspot cycle that the most distinctively contrasting trends of the general circulation and thermal pattern are found during the two periods of rapid increase of sunspot number to the alternate maxima. This indicates very clearly that it is not primarily any difference of sunspot number or trend, but some physical difference of the sunspots themselves (perhaps magnetic fields) that must be responsible for the apparently opposite climatic significance of the major and the minor halves of the double sunspot cycle.
Fig. 2. Mean Seasonal Hemispheric Difference Profiles.

Change From Sunspot Minimum to Major Maximum Minus

Change From Sunspot Minimum to Minor Maximum, i.e.,
(M M - m) - (M - mm)

(Units are standard deviations of seasonal mean pressures or contour heights.)
b. It is noteworthy that the climatic significance of the double sunspot cycle is much more clearly reflected by the winter season profiles than by the summer. The implication of this fact is that the solar disturbing influence in the double sunspot cycle is not one of electromagnetic radiational output, such as was clearly implicated in the pattern of the 80-90 year solar-climatic cycle, but rather one that is most effective in the winter season in disturbing the dynamics of the strong winter season circulation patterns. The significance levels which are assigned to each of the six profiles in Figure 2 represent F-tests of the significance of the between-group to within-group variance of the total 226 or 190 grid point values represented by each profile. The groups are the grid point values on each 10° latitude circle, the within-group variance is taken about the mean of each group; the between-group variance is that of the group means about the total mean.

c. The sea-level pressure profile in winter shows a strong relative tendency at the major maximum for mass of atmosphere to be displaced from the low-latitude high pressure belt into polar latitudes, with pronounced weakening of the zonal westerlies and subtropical easterlies and strengthening of the polar easterlies. This is completely typical of a strong relative trend of the general circulation towards a cellular blocking pattern going into the major maximum. In summer the trend is similar but much weaker, and the whole pattern is shifted 10° poleward. This corresponds to the poleward shift of the low latitude high pressure belt, represented at this season by the expanded Atlantic and Pacific anticyclones centered between 40°-50° N.

d. The contour height change profiles for both seasons are significantly weaker than those of sea level pressure, but they are similar in form, with the same poleward displacement of the pattern. Both of them, relative to the difference of phase change profiles at sea level, are displaced towards the negative in high latitudes and towards the positive in low.

e. The changes of temperature (thickness) in the winter season, and less strongly in the summer, are towards relatively lower temperatures in the high latitudes and relatively higher temperatures in the low latitudes going into the major sunspot maximum in contrast to the minor. These difference-of-temperature-change profiles reflect for the major sunspot maximum a relative minimum of temperature in winter at 60°-70° N (peak of anomalous continental cooling) which is displaced poleward to the Arctic Ocean in summer, and a relative maximum of temperature in winter in the subtropical high pressure belt, which is displaced in summer to 50° N (peak of anomalous continental heating during this season).

f. The clear implication of these seasonal difference-of-change profiles is that the general circulation at the major sunspot maximum, in contrast to the minor, is significantly disturbed by something that aggra-
vates the monsoonal (continental-maritime) thermal contrasts, thereby favoring cellular blocking as opposed to zonal patterns of the general circulation. In this connection it may be noted that high levels of solar corpuscular radiation are observed to have this effect at all seasons (5). Furthermore, solar-corpuscular radiation reached its highest level at the major sunspot maximum but is below its average level at the minor maximum. (See Figure 8.)

IV. **Manifestations of the 80-90 Year and the 20-24 Year Solar-Climatic Cycles in the Western Plains of North America**

Figures 3-7 are selected cumulative trend curves of seasonal departures of temperature and of precipitation across the western plains of North America for the period of reliable climatic record, starting usually in the vicinity of 1880 and extending through 1956, up to the time the study was undertaken (7). Each of these curves represents the average of either three or four stations lined up roughly along latitude parallels to represent conditions at three latitude belts as follows:

1. **Northern:** Canadian Wheat Belt, Edmonton, Alberta to Winnipeg, Manitoba.

2. **Middle:** west central United States, Denver, Colorado to Omaha, Nebraska.

3. **Southern:** close to the Mexican border, Phoenix, Arizona to Abilene, Texas.

The record of seasonal mean temperature and of total precipitation at each station is averaged for the entire period to define the normal. For temperature the seasonal departures for each station are expressed in departure/standard deviation ratio form. For precipitation the departures for each station are expressed as percentages of the normal. These individual station normalized seasonal departures of temperature, and those of precipitation, are averaged by each latitudinal group of stations to give the successive yearly seasonal departures that appear in the cumulative seasonal departures of each element in Figures 3-7. The four normalized seasonal departures of each element for each latitudinal group were averaged by years (December-November, inclusive) to obtain cumulative trend curves of annual means of the normalized seasonal departures of temperature and of precipitation for each latitudinal group of stations.

This normalization of the climatic data is absolutely essential if the long-term trends or fluctuations of climate are to be seen in proper perspective of their true statistical and therefore presumably their true physical significance, either geographically or seasonally. Since
departure patterns of temperature are more extensive and internally consistent both geographically and chronologically than are those of precipitation, we will pay first attention to Figures 3-5. These figures contain the cumulative trend curves of normalized departures of temperature at each of the three latitudinal groups of stations for the winter season, the annual mean of the four seasons, and for the summer season, respectively. On these curves it is the slope of the curve which expresses the magnitude of the departure; a downward slope indicates deficiency, an upward slope an excess.

Looking first at Figure 3, we may note in particular:

1. The raggedness of the cumulative departure curves at all three groups of stations, indicating year-to-year irregularity in the trend for the winter season.

2. Clearest evidence of the 80-year cycle in the southern group, where the first quarter to 1900 is obviously the coldest, and the quarters become progressively warmer to the fourth, 1940-1960. In confirmation of the hemispheric data, the warming trend in the higher latitudes is strongest in the third quarter, and in the Canadian group the temperature returns close to normal in the fourth quarter.

3. There is little evidence of the double sunspot cycle in these winter season trends, except some tendency to coldness at the major maximum (MM) at the northern stations, consistent with the hemispheric indication of the predominance of polar continental anticyclones in the higher latitudes at this phase.

In Figure 4 we may note in particular:

1. The long cycle appears more clearly and less erratically in the average cumulative trends of the four seasons than in those of the winter season alone.

2. It is in the arid southwest that the cycle clearly and outstandingly dominates the climatic sequence of temperature far beyond its effectiveness in the higher latitudes. Very obvious is the return of temperatures to near normal during the fourth quarter in the higher latitudes, while the warmth continues unabated in the south.

3. Quite striking in the southern section are the uniform severe coldness of the first quarter of the cycle, the moderate coldness of the second quarter and the persistent moderate warmth of the last two quarters.
Fig. 3. Cumulative Trends—Departures of Mean Temperature, Winter Season.
Fig. 4. Cumulative Trends--Departures of Mean Temperature, Annual.
4. The extreme warmth of the major maximum (MM) phase of the
double sunspot cycle is seen clearly in the southern section during the
last (active and warm) half of the long cycle, as might be expected.

When we look at Figure 5, the cumulative departure curves for
the summer season, the following facts may be noted with particular
interest:

1. In all three sections, it is the summer season that contri­
butes most strongly to the cumulative trend of normalized departures
of temperature, and much the most significantly so in the southern
section, where all of the features of the annual curve appear in height­
ened form.

2. During the first 20 years (cold quarter) every single summer
in the southern section was colder than the 80-year normal, with a
cumulative deficit of 26 standard deviations. During the 35 years
(third and fourth, warm quarters) since 1923 only one summer was slight­
ly colder than normal, a remarkable dominance of the long cycle.

3. The MM phase of the double sunspot cycle is markedly warm
in all sections during the third quarter of the long cycle, and equally so
in the southern section during the last quarter, less so in the middle
section. Again during this season a return to more normal temperatures
is clearly evident in the higher latitude sections.

We can conclude from these cumulative trend curves that the
thermal climate in west central North America has followed faithfully
the hemispheric pattern during the last 80-90 year cycle. By far the
most significant anomalies, statistically, occur in the lower middle
latitudes, during the summer season, and in the dry continental inte­
rrior, hence are strongly suggestive of a direct insolational effect. In
higher latitudes the warm phase of the long cycle was sharply concen­
trated in the third (high latitude zonal) quarter of the long cycle, with
a return towards normal during the fourth quarter. But in the southern
belt, close to 30° N, the warmth continued unabated during the last
quarter. The double sunspot cycle is in clear evidence, superposed
on the long cycle, only during the last half of the long cycle, the
warm half of high sunspot activity.

Figures 6 and 7 contain, respectively, the cumulative trend
curves of the percentage departures from normal of total precipitation
for the summer season and as averaged for the four calendar seasons,
for the three latitudinal station sections defined above. In general
climatic trend patterns of precipitation are much more complex and diffi­
cult of interpretation, both geographically and chronologically, than are
those of temperature. This relative non-homogeneity of the trend
patterns is caused in part by varied topographic response to change of
Fig. 5. Cumulative Trends--Departures of Mean Temperature, Summer Season,
Fig. 6. Cumulative Trends--Departures of Precipitation, Summer season.
Fig. 7. Cumulative Trends--Departures of Annual Precipitation.
prevailing wind. Also in part by the fact that the same latitudinal shift of prevailing storm track brings excess of precipitation to one region and deficit to another, in a pattern that may be quite different during one season than during another, depending on seasonal differences of the prevailing storm tracks, i.e., rain belts. A latitudinal shift of the zonal temperature pattern tends to affect broad geographical areas similarly during all four seasons. Consequently it is relatively difficult to obtain a clear picture of trends of rainfall climate compared with those of temperature. Furthermore, because of seasonal differences, the effective trend of precipitation climate is likely to be seen most clearly in the annual totals.

According to personal communications voluntarily sent to the author by the Samuel Roberts Noble Foundation of Ardmore, Oklahoma, and by the president of Hays College in Kansas, prolonged drought periods in the midwest, as represented by cycles of repeated crop failures, have occurred at approximately 20-year intervals, each with roughly six years duration. An extended drought period occurred during the 1890's, with greatest severity in the southern tier of states from Arizona to Texas. A less severe drought period but with seven failures of the corn crop occurred in the teens, with maximum severity in the south-central plains in Oklahoma, Kansas and Missouri. The greatest drought, the famous "dust bowl" period of the mid-30's, was centered further north, extending severe drought conditions practically to the Canadian border. The latest severe drought, that of the early and mid-50's, was located farther south again, with maximum severity extending from northern New Mexico and southern Colorado eastward across northern Texas, Oklahoma and southern Kansas. The intervening decades, the 1880's, the 1900's, 1920's and 1940's, according to these authorities, were comparatively cool and moist, with relatively few and minor crop failures.

It is interesting to note that this phasing of the rainfall cycle in the midwest places all four drought periods in the major maximum half of the double sunspot cycle, centered on or just preceding the major sunspot maximum, which is recognized as the climatic stress phase of the short solar-climatic cycle. The periods of relatively cool moist summers phase with the minor maximum half of the double sunspot cycle, centering on the low-latitude zonal phase of the cycle, all as might be expected.

It is also interesting to note that the entire pattern of this short climatic cycle shifts from farthest south during the 1880-1900 period (the first or low-latitude zonal phase of the 80-90 year solar-climatic cycle) to farthest north in the 1920-1940 period (the third or high latitude zonal phase of the long cycle), and finally shifts southward again during the fourth or climatic stress phase of the cycle.
Much evidence of this cyclical pattern of climatic change is to be seen in the cumulative trend curves of precipitation departures in Figures 6 and 7, although at some points not as clearly as might be desired. For the most part the evidence of the cycle is clearer in the annual totals than in the summer season totals, as might be expected by reason of local seasonal irregularities in the behavior of this element. To comment briefly first on Figure 6, the cumulative trend curves for the summer season, we may note primarily only the following points:

1. There is no such clear evidence of any long-term consistent cyclical trend of summer season precipitation as there is of temperature in Figure 5, but in the two more southerly sections there is fairly clear evidence that the major trends of the two elements are in opposition to each other.

2. There is no tendency for the trend curve at lower latitudes to be more impressive statistically than those from the higher latitudes, contrary to the cumulative trends of temperature.

3. In the southern section the period from 1879 to 1900, which was so outstandingly and consistently cold, had more dry than wet years. Hence there definitely is no indication in the precipitation data that this prolonged period of outstanding summer coldness can be attributed to an excess of cloudiness and precipitation rather than to a lowering of the intrinsic insolational heating.

Most of the interesting features of the drought cycles in west central North America are to be seen much more clearly in the annual cumulative trends of departure in Figure 7, than in those of the summer season in Figure 6, or, in fact, in those of any other single season. A number of interesting features of Figure 7 may be remarked, most notably the following:

1. There is even less evidence of any consistent 80-90 year cycle of precipitation in the annual trend curves of Figure 7 than in those of the summer season in Figure 6.

2. Contrary to Figure 6 for the summer season, Figure 7 indicates that the cyclical variation of precipitation is progressively of greater amplitude from the northern to the southern section. It is possible, however, that if the normalization of the departures were expressed as ratios to the standard deviation, as for the temperature trend curves in Figures 3-5, instead of as percentages of normal, that this difference would disappear.

3. Excepting only at the last two major maxima (MM) on the Canadian section, there is a definite tendency for every major maximum phase of the double sunspot cycle to be accompanied by or immediately preceded by a dry period of greater or lesser duration.
4. The minor maximum (mm) wet decade of 1880-1890 shows up strongly in the two southern sections. Records show that in southern Canada the period 1880-1895 (the Canadian curve in Figure 7 starts only in 1885) far exceeded any other period on record for dryness in southwestern Canada. Note that not only the first quarter of the 80-90 year solar-climatic cycle but also the minor maximum of the double sunspot cycle favor the low latitude zonal pattern at this time. That is, the combination of the two cycles favors the farthest southward displacement of the prevailing storm tracks, and therefore of precipitation, of the past 125 years. If the same cyclical sequence continues, the next 15 years are due to witness a repeat of the 1880-95 climatic pattern.

5. The severe drought period of the MM decade (1890-1900) terminated in 1895 in the Canadian section, where it really preceded MM and represented essentially the preceding decade of extreme low latitude zonal circulation. It terminated in 1902 in the two southern sections, where it was centered squarely on the MM decade, but meanwhile the return of the prevailing storm tracks to high latitudes made the 1895-1902 years of real drought in the two southern sections the wettest period of the record in the Canadian section.

6. The wet decade of the minor maximum of 1905 extended actually from 1904 to 1914 in the two southern sections, and appears again to represent the effective southward displacement of the prevailing storm tracks from the Canadian section, which was very wet from 1895 to 1902, when it was driest in the south.

7. The major maximum of 1917 appears as a brief moderately dry period on all sections, but not as a prolonged drought period in any section.

8. The dustbowl decade appears as the most prolonged severe drought on record in the middle section, but only as a minor drought in the southern section. Note that north of the Canadian border that decade was wetter than normal.

9. The second great drought of recent decades, with the major maximum of 1957, reversed the performance of the preceding drought in the two southern sections, starting early and being the severest on record in the southern section and relatively insignificant and close to MM in the middle section. It is interesting to note that these two most severe drought periods occurred during the active half of the 80-90 year solar-climatic cycle, in each case approaching MM of the double sunspot cycle, and that the pattern progressed southward as might be expected from the third to the fourth quarter of the long cycle. Note also in both of these drought periods that exactly the driest periods in the southern section were the wettest north of the Canadian border.
The evidence of the 80-90 year and the 20-24 year solar-climatic cycles in the rainfall data from the western plains of North America may be approximately summarized as follows:

1. The 80-90 year cycle is not as clearly evident as it is in the temperature data, because seasonal surplus or deficit of precipitation is associated with narrow zonal belts of preferred storm track, hence latitudinal shifts produce an increase of rainfall in one belt and deficiency in another. But the temperature pattern tends to be affected in the same sense in much broader zones.

2. The latitudinal shifting of climatic patterns which follows the four phases of the 80-90 year cycle is rather obvious in the latitudinal shifting of the relatively narrow zonal belts of recurrent drought and wetness.

3. The recurrence of periods of drought and wetness with the double sunspot cycle becomes fairly clear, primarily in the annual means, when allowance is made for latitudinal phase differences, i.e., the relative (compared to temperature) narrowness of the dry and moist zones. This cycle is clearest during the last, and more active, half of the 80-90 year cycle. Most striking during this period is the opposition of phase between southern Canada and the southern United States.

On the basis strictly of analogy with the past behavior of these two solar-climatic cycles, one may hazard the following predictions of climate in the western plains for the next few decades:

1. The next 15 years to be a period of coolness and of excess precipitation in the central and particularly in the southwestern plains, but north of the Canadian border to be much drier than any like period during the past 70 years.

2. The next major maximum drought period, probably about 1975-1985, should be severe primarily along the Mexican border, probably not in the central plains. Another dustbowl decade in the central plains is more probable at about the turn of the century.

V. Tentative Comments on the Physical Nature of Solar-Climatic Linkage

In the light of the above statistical evidence of solar-climatic relationships, it seems only fitting in conclusion to speculate very briefly on physical aspects of the indicated solar-climatic linkage. Unfortunately these comments even today must remain highly speculative and based perforce on statistical analysis rather than direct observation, because of the complete lack to date in our satellite and rocket
programs of any regular measurement of the pertinent solar quantities, notably of the total influx of energy in the visible solar spectrum, of the energy in that portion of the solar ultraviolet spectrum between 3200 and 2000 Å, and of the energy of the solar corpuscular radiation arriving both directly from the sun and indirectly via the Van Allen belts.

Essentially the physical link between solar activity and climate would seem to lie either in significant quantitative variations of the insolational energy, i.e., of the solar constant, or in variable solar effects on the transmissive characteristics of the atmosphere as they affect either the incoming short-wave solar radiation or the outgoing long-wave terrestrial radiation. Effects of the first category must lie in significant quantitative variations of the solar constant, either in the visible spectrum, which never has been satisfactorily measured, or in that 3200–2000 Å portion of the solar ultraviolet spectrum which never has been measured directly at all. Effects of the second category, those influencing atmospheric transmission and absorption of radiational energy, may be produced by variations either of solar ultraviolet, or by variations of the direct or indirect solar corpuscular radiation. The latter probably are quite insignificant quantitatively and not to be considered at all in any determination of the solar constant.

All of the characteristics of the 80–90 year solar-climatic cycle--its long-term regularity essentially independent of shorter sunspot cycles and its predominance during the summer season in lower latitudes in interior continental regions of maximum insolational heating--are all strongly suggestive of a significant long-term variation of the true solar constant of electromagnetic radiation, whether of the visible or of the invisible ultraviolet portion of the solar spectrum. However, the evident concentration of this thermal cycle in the lower half of the troposphere, as clearly indicated by the hemispheric quarterly phase profiles of Figure 1, strongly suggests that this cycle represents a variation primarily of the visible spectrum of the solar constant.

When it comes to the double sunspot solar-climatic cycle the pattern of the climatic cycle suggests something quite different. As noted above the cycle is seen most clearly during the winter season, in higher middle latitudes, where it finds expression essentially in intensification or weakening of the cellular pattern of the seasonal continental-maritime monsoonal cells. This cellular pattern tends to be relatively intensified at the major sunspot maximum (cellular blocking pattern of the general circulation) and relatively weakened (zonal maritime pattern of the general circulation) at the minor maximum of the double sunspot cycle.
These facts certainly exclude any intrinsic change of the solar "constant" of electromagnetic radiation as being a probably direct cause of the indicated climatic cycle. The implication is rather that something affects the atmospheric transmission of the long or short wave radiational fluxes in such a manner as to increase or decrease the seasonal continental-maritime monsoonal contrasts, particularly during the winter season in middle and higher latitudes. This implication is suggestive of the possible involvement of variations of total atmospheric ozone in the picture of the monsoonal heat balance.

In this connection it may be mentioned that an earlier study (6) has indicated a strong relationship between sunspot activity and total atmospheric ozone. We shall not attempt in this discussion to go further into this highly controversial question, which cannot be resolved satisfactorily under the present inadequacy of the measurements of total atmospheric ozone. We will close with the presentation of one figure which suggests interesting possibilities in the relationship of total atmospheric ozone, and of two other variables, to the double sunspot cycle.

Figure 8 represents graphically the sequence of the winter season mean values of four variables averaged for the eight successive three-year phases of the double sunspot cycle. The total number of years of record that were used for sunspot number (RSS) and index of geomagnetic activity (Cj) was 72, for the North American continentality function (OTl) it was 55, and for total atmospheric ozone (O3) it was 32 years. The function OTl is an empirical orthogonal function of seasonal mean temperature of an almost continentally symmetrical pattern centered in southeastern Iowa, for which positive and negative values appear to reflect, respectively, small and large winter season continental cooling. The O3 index is the seasonal mean of total atmospheric ozone as averaged from all stations reporting from all over the world.

The ordinate values plotted in Figure 8 for the winter mean phase group values of each index are the t values given by

\[ t = \frac{X - \bar{x}}{S/\sqrt{N}} \]

where

\[ X = \text{total mean of all } N \text{ values (72, 72, 55 and 32 respectively) of each index, } \bar{x} \text{ is the individual phase group mean of the index, and } S \text{ is the standard deviation of all } N \text{ individual winter season mean values of each index. The } 1\% \text{ and } 5\% \text{ lines as drawn represent very closely the corresponding levels of significance of the ordinate values of all four curves. These significance levels are not quite exact because the significance of } t \text{ is slightly dependent on } N, \text{ but the variation is very slight between } N = 32 \text{ and } N = 72, \text{ hence the true levels for all four curves lie very close to the lines as drawn, approximately within the thickness of the line.} \]
Fig. 8. Mean Winter Season Values of Selected Solar and Atmospheric Indices by Phases of the Double Sunspot Cycle.
It appears that the cyclical variation of all four indices is highly significant. The following features of Figure 8 may be noted with interest, but the physical implications will not be considered until such time as supporting statistical evidence may be found. Note:

1. The high positive correlation of the phase variation of OT1 and 03 (linear correlation of eight phase values = +0.81). The correlation is in the expected sense, more ozone, warmer continent.

2. The 03 correlates negatively with RSS, but not quite in phase, such that the maximum negative correlation between the two is found when sunspots are lagged about one half phase after ozone.

3. Ci (probably representing solar corpuscular radiation) correlates positively with OT1 and 03 at minus one phase lag, i.e., with the two atmospheric indices taken one phase later than Ci. This correlation with OT1 is +0.79 at one phase lag.

4. In particular, the phase relationship of the three indices Ci, OT1 and 03 with respect to RSS differ in the same respect between the major and the minor sunspot maximum in that all three indices lag one phase later at the minor than at the major maximum, and fall less to the following minimum than after the major sunspot maximum.

The above index phase relationships seem, in so far as they may be taken at face value, to support the very tentative suggestions which were offered above concerning the indicated physical basis of the 20-24 year solar-climatic cycle. Further speculation on this subject must await better ozone observations.
Bibliography


THE PREDICTABILITY OF CYCLES, TRENDS AND ANNUAL FLUCTUATIONS IN WEATHER AND CROPS

Louis H. Bean

In this paper I aim to go somewhat beyond the underlying scope of this seminar, which is the clarification of the separate roles of weather and technology in the upsurge of agricultural production in recent years. I want to focus on weather as the troublesome, unsolved "X" factor in agriculture. It has two aspects. One confronts us here in our efforts to unscramble the effects of nature from the effects of man's activities. It confronts us when we wonder how much of China's current food problem is due to weather, how much to Communist mismanagement. The same question hangs over the Russian grain production. Much the same problem is involved in judging Common Market grain output.

The other aspect of the "X" factor is the greater unknown, namely, the effect of weather on crops the next year and the next and the next. It is this aspect with which I wish to deal primarily. In fact, I wish to report on heretofore unrecognized annual characteristics in crop yield records. These somewhat novel findings are based on a considerable body of research that I have personally conducted over many years in an attempt to anticipate weather and crop yield changes a year or more in advance. And this has meant studying the interplay between trends, cycles and actual patterns of fluctuations.

Meteorologists have made great strides since former Secretary Henry A. Wallace asked the Weather Bureau forecasters to try their hand at forecasting 48 hours instead of 24 hours in advance. We now have extended forecasts for several weeks ahead. It may, therefore, be some time yet before year to year technical forecasts will be available in reliable form.

It is common belief among meteorologists, crop forecasters and nearly everyone else that weather and crop yield fluctuations are not predictable, since they appear to behave like random numbers. I hope to demonstrate to you why I think this is philosophically and statistically an erroneous view. I hope to show that these fluctuations appear to be governed by law and order -- to show why this view, if more generally recognized, could be of great help toward a better understanding of the interplay between long time trends, cycles and annual fluctuations -- why it would hasten the day when

1/ Economic Analyst, 3714 N. Randolph Street, Arlington, Virginia.
year to year or longer range forecasts in weather and crops will be officially possible.

As between the notion that weather and crop records represent random fluctuations and the view that they represent law and order I find myself readily accepting the latter, for it is in line with one of Einstein's last remarks. He said, "God does not play dice with the Universe." A corollary to this is that God, therefore, does not play dice with the weather since weather issues from an orderly universe. And a further corollary, in turn, is that God therefore does not play dice with crop yields since crop yields in the influence of man are the result of weather. If the universe is governed by law and order, if weather variations are the end product of an orderly system, if yield fluctuations are an index of the effects of weather, then both yields and weather must contain evidence of law and order and therefore must be predictable.

Evidence That Patterns Repeat Themselves

Though most of you may not be aware of it, part of the findings in this paper are not new. In a 1942 USDA bulletin entitled "Crop Yields and Weather" I called attention to numerous indications that weather and crop records are not just random numbers, that if properly investigated they reveal a large body of law and order and that both cyclical and year-to-year patterns of fluctuations tend to repeat. I illustrated with cotton and wheat yields and with rainfall in selected areas.

As a matter of record two other similar items are pertinent. On leaving the USDA in 1953 I supplied two forecasts to the new secretary and his associates. One was a forecast, dated January 1953, that the USDA December 1952 winter wheat appraisal, placed at a record low of 11 bushels, would turn out to be much higher, 14 to 16 bushels (the final figure was 15.5) and that the 1953 cotton yield, also based on the historical record no later than 1952, would be a record. This was based on a formula which first enabled me to give Secretary Henry A. Wallace a forecast in December 1936 of the coming record cotton yield in 1937 and which also continued to predict correctly, at least a year in advance, the record yields for 1942, 1944, 1948 as well as for 1953.

These findings, giving the results of these two studies in winter wheat and cotton yields, you will find in my testimony in the January 1954 hearings of the Congressional Joint Committee on the President's Economic Report. I used these wheat and cotton analyses and the proven forecasts for 1953 to justify my recommendations that long range statistical forecasting of weather and crops derived from the historical records be considered a field for basic research, since there was ample evidence that the accumulated records contained much "pay dirt."
Similar findings and views, more elaborately developed, are recorded in an address to the top officials of the USDA at the secretary's staff conference in June 1952. Here I dealt with evidence of rainfall cycles, repetitions in annual levels of runoff of the Missouri River at Sioux City and of peak levels of the Columbia River at The Dalles, Oregon. In the case of the flow of the Missouri River, my way of organizing the record revealed cycles and annual fluctuations that pointed to a flood in the spring of 1952 a year in advance. As some of you may recall this actually transpired. In the case of the Columbia River maximum flow, my findings would have given a year's advance warning of the great flood of 1948 when 16 lives were lost at Vanport near Portland.

That staff paper included three other examples of the way Nature repeats -- seasonal degree days at Philadelphia, potato yields in Maine and U. S. corn yields. In the case of corn yields my illustration showed that the corn crop failures of the 1930's were, to a large extent, repetitions of history. Had I known then what I have learned since I might have urged the U. S. Department of Agriculture to refrain from saying in 1935 that the chances were 100 to 1 against another crop failure in 1936 so soon after the crop failure of 1934.

**Updating of Studies**

Let me now update three of these studies -- those dealing with yields of Maine potatoes, winter wheat and cotton -- before turning to evidence of cycles in both weather and yields. This updating will permit me to put these studies to a severe test as to whether we are dealing with random numbers or not. The fact that a certain kind of auto-correlation analysis continues to hold good for five or six years or even longer after the period of observation is not necessarily the ultimate test of law and order in time series, but let me apply this test nevertheless. For purposes of simplicity let me merely indicate what year-to-year changes or deviations from trend were indicated during the five or six years following each study -- Maine potatoes after 1950, winter wheat after 1952 and cotton after 1936. No actual data are given in the accompanying charts. Here you have three actual cases where an observed correlation held good for five years beyond the period of the analysis, and in two of them for six years.

In the case of Main potatoes the historical pattern of changes applicable to the years after 1950 was as follows:

1951 -
1952 -
1953 +
1954 - (sharply)
1955 + (sharply)
1956 +
Figure 1. Maine potato yields — 1940-1956, actual and forecasting pattern. This is another case where a correlation for the period 1940-1950 held good for forecasting the variations for the following six years.
This extension of the 1940-1950 sequence actually took place.

In the case of winter wheat yields, the sequence called for after 1952 was:

1953 -
1954 +
1955 -
1956 +
1957 +
1958 -

The first five indications in this extension of the 1942-1952 sequence took place. The 1958 projected decline missed the phenomenal winter wheat yields of 1958.

In the case of cotton the year-to-year sequence called for after 1952 was as follows:

1953 + (record)
1954 -
1955 +
1956 -
1957 -
1958 + (record)

and these changes took place.

This projection of cotton yield variations is part of a longer experience. In the USDA bulletin on "Crop Yields and Weather" I showed that year-to-year changes in the yields for 1880-1890 repeated accurately the variations of previous years. This was also true for the period 1916-1927, and as already indicated, this led to forecasts of peak yields for 1937, 1942, 1944, 1948, 1953 and 1958. What is even more striking, the entire year-to-year sequence from 1937 to 1958, in terms of deviations from trend, turned out to be a striking example of historical repetition. There is a similar example of a continuous repetition over a span of 23 years, from 1924 to 1946.
Figure 2. U.S. winter wheat -- 1942-1952 and 1952-1957, and forecasting weather index. Shown here as an inverse correlation for the first 11 years, this analysis also provided a satisfactory basis for forecasting for five years beyond the date of the analysis in 1952.
Figure 3. **U.S. Cotton yields, year to year changes in two periods.**
This chart illustrates the high degree of correlation between yields of one period with earlier patterns of fluctuations, obviously strong indications of non-randomness in the yields of the two periods of 11 and 12 years.
Figure 4. Patterns of fluctuations in U.S. cotton yields, 1924-1946 and 1937-1958 and in forecasting "weather index." Even though these deviations depend in part on the cyclical trends shown in Figure 3, the almost perfect correlation in these two sets of data borders on the phenomenal, especially since these correspondences over 23-year periods were first observed in 1936 -- 28 years ago.
Do these illustrations point to law and order in the effects of weather on crop yields? The last word has not yet been said by mathematicians and statisticians as to tests of randomness in time series. We know from experience, as a matter of fact that tests in common use fail to differentiate between series known to be random and constructed series that are not random. But suppose we use the simplest test. You and I toss coins and I match your head or tail every time in five or six throws before missing. We try another round, say eleven throws. I match your head or tail every time again. We try once more and I match you in this game twenty-two times in a row. Wouldn't you say on the basis of chance that I could match your six throws only once in 64 tries, your eleven throws only once in 2048 tries and your 22 throws only once in over 4 million tries?

I have so far called attention to evidence that patterns of annual fluctuations in weather effects, and therefore in weather factors, are not random. This is only a small part of what I am now engaged in putting together for publication in the near future.

**Trends and Cycles**

Let me now say a word about trends and cycles, as they emerge in these auto-correlations and in other types of studies. In the nearly 100-year record of Maine potato yields, as shown in one of the charts, a 10-year moving average reveals peak periods around 1870, 1890, 1910, 1930 and 1950. Is this a 20-year cycle? There is another hint -- and merely a hint -- for the record is not long enough, of a still longer cycle with peaks around 1870, 1910 and 1950 -- a 40 year cycle?

I am told that among statisticians it is common knowledge that a moving average of a time series automatically produces what looks like cyclical movements. So let me tell you about evidence of weather cycles in the Corn Belt without using moving averages.

In the chart dealing with Nebraska corn yields and annual rainfall, you will see that I set aside extreme variations. The rest fall in positions that can be zoned in by two parallel lines. This device marks out the changing level of the central tendency, or trend. This treatment reveals the low levels in the 1890's and 1930's and another low level in the 1950's, but in this case relative to a rising trend. The recent high yields centering around 1960 are like those of the cyclically high yields of the 1920's and 1880's. Dr. Louis Thompson's extensive correlations of corn yields with factors covering the period since the 1930's has defined both the trend factor, or technological factor, and the impact of weather for several of the corn states. The fact that these cyclical variations are essentially due to weather variations is indicated by the annual record which shows the same cyclical changes as the level of their central tendency. What man and his technology has done to Nebraska corn yields can be visually derived from the fact that the rainfall level around 1960 and that of the late 1920's is about the same. The yield level about doubled in that interval -- from about 25 bushels to about 50.
Figure 5. **Trends and cycles in Maine potato yields.** This chart presents fairly clear evidence of a long-time cycle, of around 20 years. The data are 10 year moving averages.
Figure 6. Cycles in Nebraska Corn Yields and Annual Rainfall, 1870-1961. This chart illustrates the presence of cyclical changes in yields which correspond to similar changes in rainfall, with the yield-trend from 1930 to 1960 obviously due to non-weather factors.
Figure 7. Corn yields, six states, 1890-1961. The central cyclical tendencies in corn yields reflect the central tendencies in a weather index (rainfall + temperature as deviations from 80°), and the yield trend from 1930 to 1960 is also clearly shown to be due to non-weather factors.
If this does not convince you that law and order in addition to man govern the food production in the Corn Belt, let me introduce another illustration. Take the yield per acre of corn for six most important corn producing states, from 1890 to date. Here too if you set aside the extreme years to track down the changing location of the central tendency, you observe clear cyclical changes and of course the technological lift as well.

These yield cycles for most of the Corn Belt are similar to those in Nebraska -- but there are also differences with which we are not concerned here. What is important is that as in Nebraska, weather underlies these cycles as well. The several cyclical weather influences I here represent in perhaps a novel way, for I have combined one rainfall series, June-July-August rainfall with another June-July-August temperature, thus making one broad variable out of six. The unusual operation is that I first deducted the temperature figures from 80 degrees and then added the difference to the rainfall figures, thus recognizing that lower temperatures are generally beneficial and temperatures near 80 degrees are detrimental.

Here too the sag in yields in the 1950's in relation to trend is associated with the low phase of the weather cycle.

I want to return to the unique cotton analysis to show what trends and cycles seem to be involved when they are derived by the application of the graphic method of multiple curvilinear correlation without benefit of the electronic computer. (See my articles on "Graphic Multiple Curvilinear Correlation" in the December 1929 and December 1930 Journal of the American Statistical Association -- and also Richard Foot's article on "The Bean Method," in the December 1953 issue of this journal.)

The problem here is conceived as simply a correlation of annual yields with two independent factors -- time and patterns of seasonal variations. By identifying those points in the series that are comparable with the historical "weather indexes" that are being repeated, it is possible to hold the influence of those points constant and thus obtain directly the net changes in the time factor.

The cotton charts show what historical batteries or weather indexes I have identified as being repeated and what trends emerge in both the actual and the forecasting series. For both the 1924-1946 period and for the 1937-1958 period, cycles of about six years in duration seem to emerge. The rising trend around which these cycles are located is of course evidence of influence of man and his technologies. The downward cyclical phases could also be due to man, but for the most part I suspect they indicate weather effects just as do the annual patterns of variations.
Figure 8. U.S. cotton yields 1924-1946 and a forecasting weather index. It is often said that it is one thing to correlate and another to predict on the basis of that correlation. Here is an illustration where a correlation for the period 1924-1946 made in 1936, held good for the 10 following years.
Figure 9. Trends and Cycles in U.S. Cotton yields, 1924-1946 and 1937-1958. This chart illustrates the net cyclical movements derived from our auto-correlation. For both periods the cycles are about six years in duration.
Figure 10. U.S. Cotton yields, 1924-1958. This chart illustrates the standard result of fitting a trend where the data are considered as random points around a trend line.
The main purpose of this paper has been to try to convince you that weather and crop yield data are not random but governed by a high degree of law and order. To summarize let me refer you to the figures which follow. Five of them deal with trends and cycles, four with evidence of law and order, and four with applications of the fact of law and order in actual forecasting. If these illustrations still leave you skeptical, I hope to have more success with you when I put these and many more similar studies from an even much wider range into book form, in which methods and problems will be dealt with in more detail.
Figure 11. July rainfall in three Iowa crop reporting districts (1, 5, 9) and forecasting indexes, 1947-1960. This is my most recent find. It illustrates that even current monthly weather for a local area or station is the result of a great deal of "law and order" that tends to repeat with a great deal of fidelity the fluctuations of preceding periods.
Figure 12. South Dakota wheat yields, year to year changes in two periods. This chart is also strongly indicative of the non-randomness in yields for the two 11-year periods.
CLIMATIC VARIABILITY AND CROP PRODUCTION

Wayne C. Palmer

Abstract

Investigations of the dry end of the climatic spectrum in various areas have led to the development of a method for delineating drought periods and classifying each as to its relative severity. Drought has been treated as a local abnormality; therefore the derived drought severity index values are comparable both in space and time.

This method of climatological analysis has been programmed for machine data processing and a number of areas have been analyzed by months for the period 1931 to date. Thus far, a number of interesting maps and summaries have been produced.

High Risk Produced by Excessively Dry Climate. In the southwestern portion of the Great Plains the climate is so consistently dry that dry-land wheat crops can be expected to be unprofitable about eight years out of 10.

High Risk Produced by Large Variability in Moisture Supply. In the central Great Plains the climate is much less arid than in the southwestern part, but the periodic variability of the moisture supply is so large that the individual farmer cannot expect to be but occasionally "in step with the weather." He has little alternative other than to try to use the "good" years to make up for his losses during "bad" years. This is difficult to do under farm programs which regulate acres rather than quantity.

Occurrence of Serious Drought. Almost the entire Great Plains region suffered -- not necessarily concurrently -- from severe drought 10 percent of the time during the past 30 years, and in portions of Kansas and Nebraska 10 percent of the months produced extreme drought. Severe and extreme drought are, as one would expect, less frequent in most areas to the east of the Great Plains, owing to the relative briefness of the periods of abnormally dry weather.

Evidence of Periodic Recurrence of Extreme Drought in Western Kansas. An analysis of the meteorological record beginning in 1887 shows a surprising degree of regularity in the occurrence of severe and extreme drought in the western third of Kansas. Periods of really serious drought occurred in 1894 and adjacent years; 1913, 1934 and adjacent years, and in 1954 and adjacent years. These four fairly regularly spaced occurrences of severe and extreme drought may be coincidence. However, historical accounts mention exceptional drought occurrences in the early 1870's and in the early 1850's.

This apparent regularity leads one to speculate concerning the possibility that a drought of extreme severity will again occur in western Kansas sometime around the mid-1970's. Understanding of basic atmospheric actions and interactions is entirely inadequate to permit one to formulate any physical method for estimating the probability of such an occurrence. But, on the basis of past history the early or mid-1970's may be years one might well anticipate.

**Drought Analysis for St. Louis, 1838 to Date.** A similar analysis was run on the longest continuous meteorological record in the middle United States, viz., that at St. Louis, Missouri. The record is continuous from January 1838 to date. Peaks of maximum drought severity at St. Louis appeared in 1838, 1845, 1854, 1872, 1895, 1914, 1931 and 1955.

With the exception of the peak at 1845, this may appear to support the periodicity found in western Kansas, but it certainly can not be considered to be entirely independent evidence, nor does the rhythm appear so clear-cut as in the data for western Kansas.

The only justifiable conclusion at this point is that there is some statistical evidence for suspecting that serious drought tends to occur about every 20 years in the central United States and that the subject requires looking into in much greater detail with more powerful methods and techniques.

**Introduction**

Farmers have only five kinds of weather -- too hot, too cold, too dry, too wet or too windy. I am going to talk primarily about the too-dry kind which we call drought. However, I'd like to start by showing a couple of illustrations of agro-climatic risks which appear similar, but are basically quite dissimilar. I'll follow that with a brief discussion of a method of drought analysis which I have derived, and then show an example of the sort of maps which have been prepared from 30 years of monthly drought analyses for some 25 or 30 climatological divisions in the central United States. I'll close with some examples of an apparent periodicity in the occurrence of serious drought.

**Agro-Climatic Risks**

There are numerous factors in weather and climate which produce risk in an agricultural enterprise. I'll confine my remarks to certain aspects of the risk of a moisture shortage.
High Risks Produced by Excessively Dry Climate. Figure 1 is a good illustration of very high risk produced by an agricultural undertaking in a climate which is on the average and almost every year too dry to permit the operation to be profitable.

One can define moisture demand as potential evapotranspiration -- I computed monthly values by the Thornthwaite method (1). Also, moisture supply can be estimated as monthly precipitation plus the computed decrease in soil moisture storage during the month. The difference between supply and demand is, then, a measure of the absolute moisture surplus or deficit during the month. The abscissa of Figure 1 is this estimate for the months of April and May, which make up one of the critical periods for winter wheat production in the northeastern climatological division of New Mexico. The ordinate shows wheat yield in Curry County, which lies in this division. Data are for the years 1931-1955. Note that in only three of these 25 years could one call the yields truly profitable. Obviously, the rather routine moisture deficiency in spring accounts for nearly all the cases of total or partial crop failure. Apparently one spring was too wet for wheat; I suspect lodging and rust. Overall, the odds are about 7 to 1 for winter wheat being unprofitable in this area.

High Risk Produced by Large Variability in Moisture Supply. Figure 2 is an illustration of a different sort of climatic risk. The abscissa is the same as in Figure 1 except in this case the entire 12 months, August through the following July, are represented for the northwest climatological division of Kansas. Wheat yield is for Thomas County for the period 1932-1955. There was apparently a trend in the yield, but this particular sequence of years is an exceptionally unsatisfactory sample on which to base calculations of long-term trend. The 1930's were mostly drought years and the 1940's and very early 1950's were mostly years of rather favorable weather, but with considerable variation from year to year. Therefore, the sample greatly exaggerates the long-term trend in yields.

For the purpose of this illustration, true trend is not particularly important and the ordinate of Figure 2 shows departures from an approximated trend of around +0.5 bu. per year.

The outstanding feature of Figure 2 is the great variability in the moisture picture. Nearly half the years in this particular sample were drought years and about one-fourth were years of surplus moisture. To the east and south of northwestern Kansas the variability, particularly in summer, is even greater than found here.

This exceptional variability in the moisture supply poses some extraordinary problems for the farmers in this central Great Plains region. This sort of risk exists in all agricultural undertakings, but it reaches some sort of supremacy in the central Great Plains where the summer variability in
SPRING WEATHER IN N.E. NEW MEXICO
(APRIL + MAY)

DEFICIT

SURPLUS

INCHES OF MOISTURE \((P+L-PE)\)

CURRENT COUNTY WHEAT YIELD \((BU/A')\)

Figure 1. Spring Weather in NE New Mexico.
MOISTURE VARIABILITY IN N.W. KANSAS (AUG.-JULY)

Figure 2. Moisture Variability in NW Kansas.
moisture supply is nearly twice what we find in Illinois and Indiana. In northwestern Kansas the driest summers produce weather similar to that one would expect to find around Del Rio, Texas. In such years, poor pasture is about the best one could expect from the land; profitable crop production is very unlikely. On the other hand, the wettest years produce regular corn-belt type weather suitable for the production of corn and alfalfa. Since these variations are, as yet, essentially unpredictable, it is rather obvious that it is impossible for agriculture in this area to operate in-step with the weather of each year. The fallow-wheat program which has, through trial and error, been adopted in this area is about the best one can do. One must recognize the great variability of the weather and expect to use the good years to make up for his losses in lean years. But, this is easier said than done.

There is a tendency for serious drought, once established, to persist for many months, or even for years. But, most of the dry spells last only a few weeks -- just long enough to cut crop production a little or a lot. There is no good way of telling which year will be a good crop year, so a farmer must manage in such a way that he is always in a position to take advantage of favorable weather, should it occur. The restrictions imposed by the acreage control program make it nearly impossible for any individual wheat farmer to hedge against future crop losses or to make up for past losses. If the restrictions were on marketed bushels per year, it seems to me that a wheat farmer could build up and personally store his own surplus for marketing in years when -- because of drought, winterkilling, duststorms, rust, hail or some of the other items which plague him -- his production was less than his marketing quota.

Most of these risks exist in other areas also, but nowhere else in the United States are they so pronounced as in this central Great Plains region, where the primary area of winter wheat production and the region of maximum summer moisture variability closely coincide. Farm programs for this region should take more account of the climate. Apparently, the humid area philosophy which Webb (2) has so lucidly pointed out is still operating to the detriment of the Great Plains.

Short-term Weather Events and Crop Yields. In passing, I would like to point out the lonesome case in the upper left portion of Figure 2. This was the year 1955. It was a drought year, but wheat yields in this area were surprisingly good. Early prospects were dim. At heading time the wheat was extremely short and rather thin. One or two good rains at just the right time produced long, well-filled heads. This case illustrates the difficulty of estimating local crop yields from meteorological data. Crop yields are greatly influenced by brief periods of very favorable or exceptionally unfavorable weather, especially, if they occur during the more critical phases of the development of the crop. Gross measures such as the annual values shown in Figure 2, or even monthly values, completely obscure some of the short-term but extremely significant weather events.
Drought

Risks Produced by Prolonged Drought. In both Figure 1 and Figure 2 the years of particularly large moisture deficit were sequences of years of drought. In my opinion, one of the greatest of all agricultural risks is the risk of prolonged disastrous drought. As a matter of fact, between 1948 and 1962 drought accounted for 39 percent of the indemnities paid to farmers by the Federal Crop Insurance Corporation. By comparison, the next largest cause of loss, flooding, accounted for only 14 percent of the payments. Other causes were insects, 11 percent; hail, 10 percent; freezing, 10 percent; wind, 6 percent; disease, 5 percent and other causes 5 percent (3). This places drought far ahead of any other single cause of crop loss. Of course, most farmers expect an unusually dry year now and then and the prudent ones are prepared to survive one dry year. The real danger is that the abnormally dry weather will stretch out for three or four or more years. Each successive year of drought takes its toll of capital and resources and each year sees more and more farmers -- and those with whom they do business -- with nothing left except debts.

The Drought Problem

A Definition of Drought. We have been saying a lot about drought in this seminar, but none of us has bothered to define it. When one does try to define drought, it soon becomes obvious that one's definition depends on viewpoint. I am sure I could get at least a dozen different definitions right in this room. Considering the agricultural interests represented, I believe the consensus would center around "too dry for crops." However, the economists might think in terms of "an adverse factor in the economy," while the hydrologists would think more in terms of "low streamflow and depleted reservoirs."

All such viewpoints are reasonable. But, on reflection, it becomes apparent that these are all concerned with the effects of a period of unusually dry weather. Therefore, I submit that the basic problem is meteorological and that an evaluation of meteorological drought may permit each special group to use such a measure to determine the effect relationships in which they have an interest.

To make a long story short, I have defined drought as a prolonged and abnormal moisture deficiency. By this definition drought severity is a function of moisture demand as well as moisture supply. Also, it depends on the climate itself because drought is a relative condition. For example, the imbalance between moisture supply and demand which is usual in western Kansas would be regarded as drought if it occurred in, say, Illinois. The other factor which must be considered is time. Floods can develop overnight, but it takes a good while to develop a serious drought situation. Therefore, drought severity depends not only on current weather but on antecedent weather as well.
Objectives. So far, I have only defined the drought problem. I'd like to digress for a moment and discuss how and why I got into this drought research and what I am trying to do.

I hung up my meteorologist's hat in 1951 in time to spend the drought of the 1950's operating a wheat and cattle farm in southwestern Nebraska. I had endured the drought of the 1930's out there as a young fellow and when I again returned to meteorology in 1956, I began to look into the drought problem. I had and have no illusions about predicting occurrences of drought. I am interested in defining, measuring, evaluating and classifying meteorological drought, and in determining climatological expectancies of drought severity.

A number of years ago I met quite regularly with the Drought Disaster Designation Committee of the U.S. Department of Agriculture. In large part, the committee was forced to make subjective decisions based almost entirely on the judgment of its field men. The chairman was acutely aware of the need for an objective criterion and urged me to do what I could to develop such a measure of drought severity.

After the exploration of numerous approaches which turned out to be blind alleys or closed circles, I have succeeded in devising an analytical technique which appears to provide drought severity index values which are locally significant as well as comparable in space and time. That work is to be published soon (4) and I'll not repeat it here. I will point out, however, that results are in terms of monthly index values to which I have rather arbitrarily assigned the following descriptive names.

\[
\begin{align*}
0 & \quad = \text{normal (for place being analyzed)} \\
-0.50 \text{ to } -0.99 & \quad = \text{incipient drought} \\
-1.00 \text{ to } -1.99 & \quad = \text{mild drought} \\
-2.00 \text{ to } -2.99 & \quad = \text{moderate drought} \\
-3.00 \text{ to } -3.99 & \quad = \text{severe drought} \\
\leq \quad -4.00 & \quad = \text{extreme drought}
\end{align*}
\]

As a rule of thumb, one can regard incipient drought as corresponding to the sort of dry spell in which the need for rain becomes definitely apparent. Extreme drought, on the other hand, is a very, very serious situation which results from many months, or even years, of abnormally dry weather. Very rarely, if ever, would one find a drought reaching the extreme category in less than four months. During extreme drought crop yields are ordinarily near zero or so low as to be unprofitable; industries and municipalities may face the need for rationing water, and the local or regional economy begins to become disrupted. So, extreme drought is not merely an inconvenience; it is essentially a disaster -- not of the sudden and spectacular variety but of the gradual and extended variety.
Incidentally, the index numbers also delineate and classify periods of abnormally wet weather, but this is sort of a by-product to which I have given little study.

Before going on to the results which have been obtained thus far, I'd like to again point out that the data handling procedure provides a measure of the character of the weather spells themselves. It does not measure the many and diversified effects of the weather. That is a separate problem -- in fact, there are a dozen or so separate problems there.

Results. This numerical method of drought analysis has been programmed for machine data processing on the Honeywell-800 computer at the Weather Records Center in Asheville, North Carolina. The method can be applied either to point data, i.e., rainfall and temperature data from an individual observing station, or to areal average data.

The Weather Bureau regularly publishes areal averages of monthly mean temperature and monthly precipitation for each of the climatological divisions into which each state is divided. To date, most of the drought analyses which have been run are areal analyses based on these climatological divisions. Many more areas and some point data are being or will be analyzed in the near future, including all divisions in the northeastern United States and all divisions in the Ohio River Basin. The northeast areas are being done by the Regional Technical Committee for the application of climatology to agriculture in the Northeast (known as the NE-35 Committee). The Ohio River Basin work is sponsored by the Resource Development Economics Division of the Economic Research Service of the U.S. Department of Agriculture. Before long most areas in the United States east of the Rockies will be included as well as some points in the west, but at present the only region for which a semblance of complete coverage is available is the central and southern Plains region shown in Figure 3. This figure is based on analyses for only 27 climatological divisions; so it must be regarded as somewhat preliminary.

Frequency of Serious Drought. Figure 3 illustrates one of the kinds of climatological analyses that can be derived. The machine results provide a drought index number for each area for each month for the 30-year period, 1931-1960. Figure 3 shows the drought severity that was exceeded during 36 of the months in the 360-month period analyzed. This figure indicates that almost the entire Great Plains region suffered -- not necessarily concurrently -- from severe drought 10 percent of the time during this 30-year period, and that in portions of Kansas and Nebraska 10 percent of the months produced extreme drought. In this predominantly agricultural region, extreme drought is almost synonymous with disaster, because the economic consequences of such a pronounced water shortage reach to nearly all levels of the local economy. On the basis of a few scattered analyses, severe and extreme drought are, as one would expect, much less frequent in most areas to the east of the Great Plains, owing to the relatively brief duration of the periods of abnormally dry weather.
DROUGHT 1931-1960

SEVERITY THAT WAS EXCEEDED 10% OF THE TIME

Figure 3. Drought Severity that was exceeded 10% of the time.
Evidence of Periodic Recurrence of Extreme Drought in Western Kansas. Among the few long meteorological records which I have investigated are those since 1887 for the western third of Kansas. Although no effort was made to discover "cycles of drought," the relative regularity of the occurrences of severe and extreme drought in western Kansas is rather striking. The periods of serious drought since 1887 are shown in Figure 4. The points represent the maximum severity for each year that severe or extreme drought was reached. The arrows along the abscissa mark the year of maximum severity in each of these four periods of drought. These four fairly regularly spaced occurrences of maximum severity may be coincidence. However, historical accounts (5) mention exceptional drought in Kansas in the early 1870's and in the early 1850's.

This apparent regularity produces a strong temptation for one to conclude that a similar period of serious drought will occur in this area in the mid-1970's, but caution dictates that one realize that this is merely a possibility rather than a foregone conclusion. Understanding of basic atmospheric actions and interactions is entirely inadequate to permit one to formulate any physical method for estimating the probability of such an occurrence. But, on the basis of past history alone, the early or mid-1970's may be years one might well anticipate.

Drought Analysis for St. Louis, Missouri, 1838 to Date. In an attempt to produce more evidence bearing on this question of periodic occurrence of severe and extreme drought, an analysis was run on the longest continuous meteorological record in the middle United States, viz., that at St. Louis, Missouri. The record is continuous from January 1838 to date. However, it does not appear to be a homogeneous record. The first 32 years seem to be much wetter than the remaining years. On the assumption that the climatic averages for the period 1838-1869 at St. Louis were similar to the climatic averages for the period 1931-1960, the early record was adjusted and the drought analysis was carried out. Peaks of maximum drought severity at St. Louis (marked by the arrows in Figure 5) appear in 1838, 1845, 1854, 1872, 1895, 1914, 1931 and 1955.

With the exception of the peak at 1845, this may appear to support the periodicity found in western Kansas, but it certainly cannot be considered to be entirely independent evidence, nor does the rhythm appear so clear-cut as in the data for western Kansas. However, it is interesting to see that the meteorological evidence at St. Louis tends to substantiate the historical accounts of major droughts during the latter half of the 19th century.

As an illustration of the "cycles" that one may find in a time series if he puts his mind to it, it may be worthwhile to point out that since the 1840's no serious drought is shown in Figure 5 during the last third of every other decade. These periods are marked by the short horizontal bars in Figure 5.
YEARS OF SEVERE AND EXTREME DROUGHT IN WESTERN KANSAS

Figure 4. Years of Severe and Extreme Drought in Western Kansas.
YEARS
OF SEVERE AND EXTREME DROUGHT
AT ST. LOUIS

Figure 5. Years of Severe and Extreme Drought at St. Louis.
Personally, I'll be surprised if this sort of thing holds true in future years. However, it is a bit difficult and probably unwise to completely ignore the possibility that there is a real physical mechanism of some sort behind this regularity.

One possibility that one can explore is the relation between these occurrences and the sunspot cycle. The annual means of the relative sunspot numbers as given by Chernosky and Hagan (6) are also plotted on Figure 5. On inspection, one can see that some of the periods of no serious drought -- as well as one of the times of maximum drought severity -- roughly coincide with the times of sunspot maxima, while other periods of no serious drought -- and again, some of the times of maximum drought severity -- closely coincide with the times of sunspot minima. Too, the transition periods, both before and after sunspot minima, were periods of no serious drought. There seems to be little or no evidence here that these aspects of drought are related to the cycle of mean annual sunspot numbers. I don't know what the basic physical mechanism is. In fact, at this point I am not prepared to say whether this apparent periodicity in the occurrence of drought is real or not. The "cycle" I have pointed out may be purely accidental. One can hope -- and, I think, expect -- that in one way or another the future holds the key to meteorological riddles such as this.

Conclusions

The problems and risks associated with weather and crop production vary considerably from region to region. I have mentioned only a few aspects of the moisture problem; there are numerous additional problems. We must recognize them, do our best to solve or at least understand them and efficiently incorporate the knowledge into individual as well as regional farm policies and procedures.

As far as drought "cycles" are concerned, the only justifiable conclusion at this point is that there is some statistical evidence for suspecting that serious drought tends to occur about every 20 years in the central United States and that the subject requires looking into in much greater detail with more powerful methods and techniques -- preferably with longer homogeneous meteorological data series which, unfortunately, are nonexistent. The real need, of course, is for a quantum jump in fundamental understanding of the atmosphere. So long as we are unable to really explain the major meteorological events, then so long must we grope through our data in search of clues -- and wind up with uncertainty.
Bibliography


A CRITICAL APPRAISAL OF PERIODICITIES IN CLIMATE

J. Murray Mitchell, Jr.¹

This history of weather science, dating back more than a century, is replete with attempts to analyze series of climatological data in terms of cyclical components. It might have seemed appropriate that my function today should be to summarize the results of all these analyses, and perhaps to consolidate information on a number of periodicities revealed by them into a statistical model to be used in foreshadowing the future. For reasons that I hope to make clear to you, I think that such a course would be unwise.

If one takes the trouble to amass the prodigious literature on the subject of cycles in climate and to try to collate all the conclusions thereof he becomes utterly perplexed rather than enlightened. Most investigators, rather than having merely confirmed the findings of their predecessors, have seemed to turn up with new cycles instead. It has, in fact, become a shop-worn joke among meteorologists that there are as many cycles discovered in climate as there are investigators who have hunted for them! In my own experience, this judgment appears to contain a modicum of truth, but all the same it is a bit harsh as an indictment of the well-intended and often well-qualified claims of many of these investigators who happen to have been highly reputable scientists in other fields of endeavor.

The important thing that I would like to stress today is this: It has taken modern insight into atmospheric behavior, coupled with a much better understanding of how very easily statistics can lead us astray, to place this historical chaos into proper perspective and to separate the wheat from the chaff where the reality of cycles is concerned. For this reason, I will be dealing here with some basic fundamentals of time series analysis, and will then be applying these fundamentals to several climatological time series, concluding with one that has a direct bearing on the problem of climate-crop relationships in the United States.

1. Harmonic Analysis and Cycle Hunting

Historically, most purported but unverifiable evidence of cycles in climate has come from the application of harmonic analyses to climatic series. It is important to realize that ordinary harmonic analysis is appropriate only to a series that is known to be, or likely to be, composed of a sum of strictly periodic components of variation. Under other circumstances, as for example, when the series being analyzed is random, or when it contains irregular fluctuations only, the results of harmonic analysis can very easily be misinterpreted. (See Jenkins ¹, pp. 148-149). Because of a widespread misuse of harmonic analysis and also because a clearer understanding of harmonic functions will be helpful to us later on in this presentation, I propose to dwell a bit on some basic principles.

¹/ Office of Climatology, U.S. Weather Bureau, Washington, D.C.
Were we to have a series -- let us say -- of 100 values, if we compute the mean and all of its 50 harmonic components derived from a harmonic analysis, and then add up the mean and the 50 sine waves that these components represent, each with its proper phase and amplitude specified by the analysis, we can exactly reproduce the original series, in proper time order, to any degree of precision we like. We owe this result to a mathematical identity, and it doesn't matter if the original series is really periodic in nature or not, because, in the parlance of the statistician, the analysis uses up all the degrees of freedom available in the original series.

If we are dealing with a bona fide periodic function, we should keep two facts in mind with regard to harmonic methods of analysis.

First, if the results of harmonic analysis are used to extrapolate the sample time series into the future (or the past), we should be able to predict future (or past) terms of the time series with some degree of skill. If the sample series came from a purely periodic function, predictions obtained in this way would be extremely accurate. This is exemplified by the methods of celestial mechanics, whereby the astronomer is able to predict future positions of the moon and planets with incredible accuracy many years in advance. If, on the other hand, the sample series consists in part of a periodic function and in part of a non-periodic or random variation, then predictions derived from the series by harmonic extrapolation will enjoy only qualified success, but some success nonetheless.

Second, if we are dealing with a series that is a bona fide periodic function, the efficiency with which harmonic analysis describes that function depends on its shape. If the shape of the periodic function is that of a pure sine wave, and the fundamental period of the analysis had been set in advance equal to a multiple of the period of this sine wave, then all the variance of the series would be contained in one component of the analysis (i.e., one sine and one cosine term whose wavelength corresponds to that of the original function).

If, however, the shape of the periodic function is not sinusoidal, then two or more components of the harmonic analysis are needed to describe the function. The additional components involved will be those corresponding in wavelength to higher harmonics of the basic period of the function.

Examples of this situation are shown in Figures 1 and 2. In Figure 1 we see that a spike-shaped periodic function whose basic period in six data intervals can be described by the sum of its three harmonic components, with phases and amplitudes as indicated. A rough approximation to the sum of these harmonics is shown by the dashed curve superimposed on the original spike function. Agreement is, of course, limited to the values for the data points themselves, and not necessarily to those for intervening points of the series where the shape of the function is unspecified.
Figure 1. A spike-shaped periodic function (solid line) with period equal to 6 data sampling intervals, and its resolution into harmonic components. Approximation to sum of harmonics is shown superposed on function.

Figure 2. An asymmetric saw-tooth periodic function (solid line) with period equal to 6 data sampling intervals and its resolution into harmonic components. Approximation to sum of harmonics is shown superposed on function.
In Figure 2 we see an asymmetric periodic function whose basic period is six data intervals. This particular one can be described by the sum of just two of the three computable harmonics.

These examples raise an important question concerning the interpretation of the results of harmonic analysis when applied to real periodic functions in nature. In Figure 2, for example, does the function shown at the top actually contain a periodic component whose wavelength is half its basic length? That is to say, if a function like this represents a physical phenomenon, does its second harmonic have any physical meaning? If you think about this you will agree that it doesn't have to at all: It may well be only a mathematical artifact arising simply because the language of harmonic analysis is sine waves, and the physical process we are attempting to describe statistically may be trying to talk to us in quite another language. In this case our translation to sine-wave language might be very inefficient indeed.

It is worth interjecting here that, from time to time, some meteorologists and others have claimed to find a periodicity in weather whose length approximates that of some higher harmonic of the 11-year sunspot cycle. Since the accurate description of the non-sinusoidal sunspot cycle, in terms of harmonic analysis, is bound to show a corresponding "period" in sunspot number, this coincidence has been interpreted as evidence of a genuine solar-weather relationship. From what I have just said, it should be apparent that this kind of reasoning is precarious as the basis of any working hypothesis about solar-weather effects.

Thus far, we have been talking about harmonic analysis of functions that are in fact periodic functions. When the functions we are dealing with are not basically periodic, then the problems of interpreting the results of harmonic analysis are compounded.

Consider for a minute a series of purely random numbers, that is, a series in which knowledge of the value of any term tells us absolutely nothing about the value of any other term. If we had a sample of this series, 100 terms in length, there is nothing to prevent us from going through the motions of performing a harmonic analysis on this sample. By adding up the mean and the 50 harmonic components calculated from the harmonic analysis, we could reconstruct these 100 numbers, in their proper time sequence, with all the precision we desire, just as we could have done in the case of a bona fide periodic function.

Just as before, we can also go through the motions of applying the results of the harmonic analysis to extrapolate the random series into the future (or into the past). But this time we will find a difference: the results of extrapolation will bear no systematic relation whatever to the actual values of the series to which they supposedly correspond. In other words, our predictive skill above chance will be zero.
Moreover, we will find that virtually every harmonic component of the analysis will contain some of the total variance of the original series. In fact if we were to perform separate harmonic analyses on a very large number of series of random numbers, each series 100 terms in length, and we then average the amplitudes computed in all these analyses, one harmonic at a time, all harmonics would turn out to have the same amplitude but with arbitrary phases.

Does this mean that series of random numbers are actually composed of a finite number of periodic components, all of equal amplitude, which we could use for prediction if only we knew their phases? Well, harmonic analysis is simple-minded enough to think of it that way, but we humans know perfectly well that this is nonsense.

2. The Fundamental Nature of Weather Variation

If you have been patient enough to bear with me this long, I am glad to say that we are now ready to close in on the problem of periodicities in climate. But, for the time being, we need to clarify some general principles before getting down to cases.

How are we to regard time series of weather and climate? Are they altogether periodic, are they in no sense periodic, or are they something in between? Are they deterministic in some respect other than being periodic, and therefore still predictable? Or, is climatic variability purely random, so that we can expect to gain nothing at all by looking at climatic data as a time series?

In order to answer these questions definitively, we find that we will first have to sharpen our statistical tools and make certain that our methods of analysis are flexible enough to distinguish between these various alternatives. It has been only in recent years that suitable methods have been developed to handle such problems. For most of these methods, we owe a debt of gratitude to the electronic communications engineers who are constantly dealing with the analogous problem of identifying signals in "noisy" electronic transmissions. It happens that when we use these methods to examine climatic series in which various periodicities had formerly been claimed by "cycle hunters" using classical harmonic methods, we are hard put to verify the reality of those periodicities. This, of course, gets us back to the reason why I did not begin my discussion today simply by rattling off all the results of these early "cycle hunts."
Before we inquire further into the observed statistical properties of climatic series, let us consider briefly what we might anticipate finding from our theoretical knowledge of atmospheric dynamics.

The atmosphere is essentially a thermally active fluid in motion. It derives its kinetic energy of motion primarily from a conversion of potential energy, which in turn is produced by differential solar heating of the earth's surface (mostly that between low and high latitudes). The resulting motion, modified to a considerable degree by the rotation of the earth on its axis, is predominantly a turbulent one. From our knowledge of hydrodynamic theory, we have to expect any fluid flow with Reynolds Numbers commonly found in the atmosphere to be more turbulent than laminar. The largest elements of turbulence are the so-called planetary waves that migrate erratically from west to east in middle latitudes, and the cyclones and anticyclones we see on the daily weather map which are carried along in these planetary waves. Smaller elements of turbulence, many of them thermally active, are present also. These are exemplified by hurricanes, thunderstorms and shower clouds. Still smaller elements of turbulence are identifiable as wind gusts, the bumpiness of airplane flight and the chaotic appearance of smoke plumes as they emerge from factory chimneys. Ultimately, all energy of motion in the atmosphere is frictionally dissipated by the smallest of all scales of turbulence, which is generated mainly by irregularities of the earth's surface terrain.

This picture of atmospheric behavior, of which the chaos typical of all turbulent flows is an outstanding feature, would seem to leave little room for well-defined periodicities in weather and climate. On the other hand, we know very well that there is a well-defined diurnal period in weather, and an equally well-defined annual period. These arise because of the regular, astronomically controlled cycles of solar radiation, which are fundamental "forcing functions" to which the atmosphere must respond. These are easy to comprehend.

Beyond the diurnal and annual forcing functions, can we put our finger on any other periodic changes in our environment to which climate ought to respond? We could cite, for example, the moon's gravitational field, which exerts a well-known tidal influence on the oceans. Perhaps this influences the atmosphere, too, in some small way. Then there is the so-called "11-year" sunspot cycle. Sunspot numbers and other solar features change so much and so spectacularly from year to year that it is tempting to suppose the sun's energy output may vary appreciably in parallel with them. We don't know yet if this is true or not, but it is a possibility well worth keeping in the back of our minds when we analyze climatic data. Then, too, sunspots vary in their average number over very long periods of time, possibly following a rhythm 80 or 90 years in length. But again, real changes of solar radiation accompanying these slow variations of sunspot number have never been proved, so we can't be sure on theoretical grounds that climate should vary in an 80-to-90 year rhythm.
Other forcing functions on weather can be visualized, such as the tidal influence of other planets in our solar system. These, however, are so infinitesimal in amplitude that it is extremely difficult to imagine that they can influence terrestrial weather (or solar radiation) appreciably.

If we have a plausible a priori physical model of some forcing function on climate, we can be amply justified in searching for a weather effect of it. If the forcing function is likely to be periodic, and especially if the form of the forcing function approximates to a sine wave as tidal forces do, then we can put such methods as harmonic analysis intelligently to work for us in searching for the weather effect. Alternative techniques of analysis can be designed to study the problem too, because we know in a qualitative way what we are looking for and what time-period of weather variation should be involved.

On the other hand, if we find evidence of a cycle in climate that has an unanticipated period, and we have no a priori model of a forcing function with that period to account for it, then we must be very careful. Indeed, the analyst who finds evidence of such a cycle cannot claim to have completed his analysis until he has established by means of suitable statistical decision theory that the cycle is very unlikely to have arisen in his data from a spurious sampling bias. Even if he can do this, he can ill afford to accept the cycle at its face value unless or until the same cycle is found in other, statistically independent samples of climatological data.

From the historical viewpoint, if all cycle hunters had checked their results by these means, very few of their publications would ever have been written. Hasty and uncritical acceptance of the reality of evidence of cycles in climate has evidently been the source of more wasted effort in meteorology than any other kind of scientific misjudgment. Beyond a doubt, meteorology, has not been alone in this experience either.

Inasmuch as climate is obviously extremely variable, if we question that periodicities are the principal source of this variability, we must be prepared to offer a specific alternative explanation for it.

3. The Power Spectrum Approach

To handle the problem of time series in a way that does not presuppose anything about periodic elements in them, the concept of the power spectrum has been developed. Power spectrum analysis, otherwise known as generalized harmonic analysis, is based on the idea that a time series is not necessarily made up of a finite number of oscillations, each with a discrete wavelength, but rather that it consists of a large number of small oscillations having a continuous distribution of wavelengths. A spectrum, therefore, measures the distribution of variance in a time series over a continuous domain of all possible wavelengths -- each arbitrarily close to the next -- lying between
an infinite wavelength (trend) and the shortest wavelength analyzable by any form of harmonic analysis, namely that equal to twice the interval between successive observations.

Procedures for computing power spectra vary, but for the most part they follow the approach recommended by Tukey (2). Given N equally spaced observations, we start by computing all serial covariances or correlation coefficients for lags 0 to m, where m < N. Then we compute a cosine transform of these m + 1 values, in a manner analogous to finding the Fourier transform of a continuous variable. Finally the results are smoothed by a three-term weighted moving average with weights .25, .50 and .25. This yields the spectrum in terms of m + 1 estimates, each centered at a harmonic of the fundamental period of analysis, the latter being equal to 2m time units. If we then fit a continuous curve to these m + 1 estimates, making the curve as smooth as we can without violating the confidence intervals appropriate to the individual estimates, we arrive at an estimate of the continuous "population" spectrum to which our computed spectrum is a discrete-valued approximation.

The resolving power of a spectrum can be controlled at will by varying the maximum lag m; large choices of m lead to higher resolution but also to larger errors of estimation of the true shape of the spectrum. (See Tukey [2], Panofsky and Brier [3].)

In the spectrum, various kinds of non-randomness in the time series will show up differently. For example, the spectrum of purely random variation tends to be rectangular in shape. This condition is commonly referred to as "white noise," by analogy with visible light which is interpreted as white if all frequencies of radiation are of equal intensity.

If a pure sine wave is contained in the time series, the spectrum of the series will contain a comparatively narrow peak at its appropriate wavelength. If a periodicity having a complex shape is contained in the series, the spectrum is likely to contain two or more peaks, one at the fundamental wavelength and others at wavelengths corresponding to some of its higher harmonics.

If an irregular rhythm, or a quasi-periodicity, is contained in the times series, the spectrum will indicate this by a more or less broad hump spanning an appropriately large range of frequencies.

And finally, if the time series contains persistence, that is, if each term is influenced by its immediately preceding terms according to a Markov-type memory, the spectrum shape becomes distorted across all wavelengths. In particular, the amplitude of the spectrum is decreased at the shorter wavelengths, and the spectrum is said to resemble that of "red noise." (See Gilman et al. [4].)
In the case of simple linear Markov persistence, defined by the condition

\[ X_i = \rho X_{i-1} + \epsilon_i \]

where \( \rho \) is the lag-one serial correlation coefficient in the \( X_i \) series, and \( \epsilon_i \) is an uncorrelated random remainder, the shape of the resulting red-noise spectrum is shown in Figure 3 as a function of \( \rho \). I would emphasize this form of persistence because it is found to be characteristic of a great variety of climatological series. Moreover, the spectra of actual climatological data area usually biased toward longer wavelengths, in more or less the manner that the presence of such persistence would lead us to expect.

From what I have said, perhaps you can begin to appreciate the advantages of spectrum analysis over harmonic analysis. Certain disadvantages of the spectrum approach deserve to be mentioned, however. If real periodicities are present in a time series, the spectrum does not necessarily represent them as clearly as harmonic analysis could. Moreover, the spectrum throws away all information about the phase of real periodicities, so that other techniques of analysis have to be used to recover phase information.

In common with harmonic analysis, one has to be careful about the problem of aliasing in spectrum analysis. If data are sampled instantaneously at regular intervals, a situation like that in Figure 4 may result, where one wavelength is interpreted as another. To avoid aliasing and the ambiguity of interpretation it produces, the data can be suitably averaged before the analysis is made. I will have more to say about averaging shortly.

4. Examples of Meteorological Spectra

Now we are ready to look at some actual time series. We will look at them primarily in terms of their power spectra, at least to start with.

An example of the results of spectral analysis is shown in Figure 5. This spectrum is one of daily precipitation amounts at Woodstock, Maryland, a Weather Bureau Climatological Benchmark station and is based on a comparatively large sample -- 4383 daily values to be exact. The maximum lag in the analysis was set at 120 days, which yields 121 spectral estimates, all centered on harmonics of a fundamental period of 240 days.

The longer wavelengths in this spectrum are seen to be inflated relative to the shorter ones. Since the data were serially correlated (at one-day lag) with a value \( \rho = .12 \), the corresponding Markov red-noise spectrum was added. This is shown by the smooth curve and indicates that the general shape of the spectrum is primarily due to this serial correlation. (Had there
Figure 3. Shape of power spectrum for population of first-order linear Markov series for various values of associated lag-one serial correlation coefficient. Rectangular shape labelled "0" is case of "white noise"; other shapes correspond to "red noise." From (4).

Figure 4. Illustration of "aliasing," in which data available only for times corresponding to small open circles fail to distinguish between two different periods of variation.
Figure 5. Power spectrum of daily precipitation amounts at Woodstock, Maryland. Sample length $N = 4383$ days. Maximum lag of analysis $M = 120$ days. Fitted "red-noise" continuum is shown by solid curve. From (4) and (5).
been no day-to-day persistence in the data, the spectrum would have tended
to be horizontal, that is, to fluctuate around the horizontal dashed line
labelled "white spectrum."

In addition to persistence, might there be anything else of interest in
this spectrum? In the case of a spectrum like this which is largely dominated
by the effects of Markov-type persistence, further analysis can most con-
veniently be carried out if we first transform the spectrum to another coordinate
system, as in Figure 6. Then, persistence in the data causes the spectrum to
tend along a straight, sloping line (in this case the one labelled "population
spectrum for $\phi = .12$"). Without going into details, which are available else-
where (5), this method of representation has the advantage that various
confidence limits for the Markov spectrum also plot as straight lines. This
enables one to construct these confidence limits with minimum effort, which
we need in order to determine whether any of the irregularities in the spectrum
deviate by statistically significant amounts from the pure Markov model.

In this spectrum of Woodstock precipitation, only one excursion from
the Markov line appears to be large enough to justify a second look. This
occurs at wavelengths of about 2.8 days. An arbitrary spectral estimate would
be expected to exceed this particular extent of deviation in only one out of 10
independent spectra like this one. Since two adjacent spectral estimates are
involved here rather than only one, the unusualness of this excursion is some-
what greater than implied just above. However, no one has ever suggested a
reason why climate should vary with a period of 2.8 days. Inasmuch as there
is a pretty fair chance that a statistical result of this nature could have arisen
accidentally in our sample of data, we shouldn't get too excited about it
unless we should find the same period also in other statistically independent
samples of data. Tentatively, then, this spectrum may be assumed to contain
pure "red noise" and no periodicities.

Let us now turn to other spectra for Woodstock, previously published
in (6), but this time let us investigate the extent to which it maintains its
shape from one period of record to another. Even if no significant deviations
from "red noise" can be noted in individual spectra, it would still be relevant
to check if the same anomalies showed up repeatedly in different periods of
record.

Figure 7 illustrates five different spectra of daily mean temperature at
Woodstock, Maryland, representing the winter period January 1 to February 15
in different years. The heavy continuous curve in the figure is the average of
the five other spectra. No attempt was made in this analysis to smooth these
spectra to remove sampling biases, for smoothing would have reduced each of
them to nearly identical red-noise curves. Careful inspection of these spectra
will indicate that no peak in the average 5-year spectrum coincided with peaks
in more than three out of the five spectra for the individual years. Evidently,
Figure 6. Power spectrum of Figure 5 transferred to special coordinate system in which "red-noise" continua and their confidence limits plot as straight lines. For comments on evaluation of sample spectrum shown here, see text. From (S).
Figure 7. Power spectra of daily mean temperature at Woodstock, Maryland, for period January 1 to February 15 in each of 5 years, and the average of these spectra. Each spectrum based on a maximum lag m = 15 days. From (5).
if such peaks represent real periodicities or quasi-periodicities, they are not uniformly present in different years, and lack stability necessary for their usefulness in forecasting.

Figure 8 illustrates four unsmoothed spectra of daily precipitation amount at Woodstock, each based on five whole years of data as indicated. As in the case of Figure 7, none of the irregularities of the individual spectra are statistically significant, and the wavelengths of the irregularities are not particularly consistent from one five-year period to another. It is worth adding that if any of the peaks in these spectra were to be regarded as real periodicities and were to be used in devising a scheme of prediction, at best such a periodicity could account for only a few percent of the total variation of future daily precipitation at Woodstock.

Further comparisons of the spectra of daily precipitation amounts in different seasons and in different years are shown in Figure 9. A glance at this figure suggests great variety, and that each season and year appears to have its own unique spectral "signature." This circumstance is a clear indication that precipitation variations are essentially random, on the scale of days at least, and that if periodicities really exist their amplitudes are so minute that they cannot show up through the random noise.

Finally let us look briefly at a composite spectrum covering a much broader range of wavelengths. Figure 10 is a spectrum of mean temperatures at University Park, Pennsylvania (7), in which various ranges of wavelength were analyzed by separate analyses. (The data were corrected to remove the annual cycle before running these analyses.) The results for the longer wavelengths (left end) are based on suitably long time-averaged values of data covering many years. In contrast, those for the shorter wavelengths (right end) are based on unaveraged daily values, or short time averages of these, for only one or more years. In this way a fairly adequate estimate of the temperature spectrum could be obtained for better than three orders of magnitude of wavelength, with comparatively little computational effort. In order that the results of the separate constituent analyses could be dove-tailed together, the value of each spectral estimate was divided by its wavelength before plotting. This explains why the variance contributed by the longer wavelengths is indicated as being so small, and why the overall shape of this spectrum differs from that of the other, preceding spectra. But that is beside the point I wish to make right now, which is this:

Except for some rather minor irregularities, this spectrum indicates a more or less continuous distribution of variance with wavelength. That is to say, all possible wavelengths of variation, each being arbitrarily close to the next, appear to contribute something to the total variability of temperature in central Pennsylvania.
Figure 8. Power spectra of daily total precipitation at Woodstock, Maryland, in each of four 5-year periods. Each spectrum based on a maximum lag $m = 30$ days. From (6).
Figure 9. Power spectra of daily total precipitation at Woodstock, Maryland, in each of four seasons (advancing to right) and in each of five years (advancing from top to bottom). Season 1 is period January 1 to February 15, and subsequent seasons are contiguous 45-day periods ending with June 30 (Season 4). Each spectrum based on maximum lag $m = 15$ days. From (6).
Figure 10. Broad-band power spectrum of mean temperature at University Park, Pennsylvania, based on composite of several individual analyses (see text). Vertical bars indicate 5 and 95 percent fiducial limits of selected spectral estimates. From (7).

Figure 11. Deviation (in standard measure) of frequency of occurrence of maximum 24-hour precipitation in each calendar month, as a function of phase of the lunar synodic month. Results shown as approximately 3-day moving averages, based on 16,057 dates at 1544 U.S. stations, for two independent 25-year periods of record 1900-1949. From (8).
This sort of result, which I assure you is not peculiar to Pennsylvania, does not rule out the possibility of small contributions to climatic variability by genuine periodicities. On the other hand, it does indicate that if such periodicities are present they are too insignificant to be useful in forecasting practice.

### 5. Some Real Climatic Periodicities

It may come as a surprise to you that, my having spent so much time questioning the evidence of cycles in climate, I should abruptly turn around and say that I believe in cycles after all! Well, that is precisely what I am about to do! However, the cycles I have in mind are two very specific ones, and inasmuch as real cycles do seem to be so rare in weather, I think these two deserve a few words of clarification.

The first periodicity, hints of which were detected by meteorologists many generations ago but never generally accepted, is one found in precipitation that follows the lunar synodical period of 29.53 days. Recently, an intensive search for this period, by means of a variety of carefully designed statistical methods, was made using virtual mountains of daily precipitation data for the United States and some other areas of the world (8, 9). Not only have the results offered new confirmation of its fundamental reality, but they have shown a consistent modulation of the amplitude of the periodicity by other elements of the moon's orbital motion. Although no physical hypothesis has yet been proposed to explain this curious lunar control of precipitation, further statistical analyses show promise in helping to understand the causal chain of events involved.

A general idea of the nature of the lunar period in precipitation is shown in Figure 11. One can see that it consists essentially of a semi-lunar variation having a period of about 15 days. It should be understood that, in terms of the total day-to-day variability of local precipitation, this lunar influence is very weak. Consequently, it is not surprising that it should fail to show up clearly in power spectra based on data for single locations and for relatively short periods of record. By the same token, its incorporation into a routine forecasting scheme would not be very rewarding.

The second periodicity in climate that deserves comment is of a quite different character. It is an oscillation whose period approximates to two years, whence it has come to be known as the "biennial oscillation." In fact, this oscillation is not a strict periodicity, for its period in recent years has varied from 22 months to perhaps 30 months. As yet there is not generally accepted physical explanation for it. Ordinarily, an unanticipated "cycle" should be regarded with skepticism, but, as we shall see, this one is so very conspicuous in wind and temperature at high altitudes over the tropics that there can be no real doubt of its reality there.
In terms of mean zonal wind speed in the tropics, this oscillation amounts to a complete reversal between easterly and westerly flow in the stratosphere. Figure 12 (reproduced from [10]) shows that oscillation above Canton Island in the equatorial mid-Pacific, between 1954 and 1960. There, the double amplitude amounts to 45 m/sec (100 mph) at altitudes near 25 km (15 miles), and the oscillation completely overwhelms the local annual variation in wind.

In Figure 13 (taken from [11]), the zonal wind variation at an altitude of about 20 km (13 miles) is compared for six stations in the Pacific, ranging from Canton Island near the equator to Wake Island at 19°N. This Figure shows that the biennial oscillation in wind yields to the more familiar annual variation rather suddenly as one moves out of the tropics.

Figure 14 (reproduced from [12]) illustrates for Eniwetok Island (11°N) how quickly the oscillation damps out as one descends from the stratosphere to lower levels. The bottom curve in this figure represents the zonal wind at the 100-millibar pressure level, which corresponds to an altitude of about 16 km (10 miles). The oscillation is virtually absent at all elevations lower than this. The data in this figure, by the way, are in the form of 12-month moving averages, which serves to remove the annual cycle and to show the biennial component of variation more clearly.

There is accumulating evidence that this biennial oscillation is a feature of world-wide climate. In middle and high latitudes, it is revealed to a modest extent in surface temperatures. Indeed, if we refer back to the power spectrum of temperature in central Pennsylvania, shown in Figure 10, we can find an indication of it there. Another spectrum in which it shows up very prominently is one of annual mean temperatures for central Europe based on the period of record 1761-1953 (13). This is shown in Figure 15. Most spectra of long climatological temperature series show this feature, but it is prudent to point out that it is a relatively small component of variation in surface climate. It seldom accounts for more than 5 percent of the total variation of annual mean temperatures and has yet to be detected at all in precipitation.

So we see that we have good reason to accept at least two genuine periodicities -- or quasi-periodicities -- in climate along with the familiar annual and diurnal periods. Although no other cycles in climate have yet been established with as much confidence as these, several others that may turn out to be real after further study of them should perhaps be mentioned in passing.

One of these is the 11-year sunspot cycle, which, however, is not a strict periodicity inasmuch as its period has been known to vary between nine years and 17 years (recently, it has averaged only 10 years). Nevertheless, indications of a variation in terrestrial climate having a similar wavelength keep cropping up with just enough regularity to give us pause.
Figure 12. Mean zonal wind speed in stratosphere over Canton Island, showing variation with altitude (in kilometers) and with time, February 1954 to October 1960. From (10).

Figure 13. Variations of mean zonal wind speed, 1955-1960, at altitudes near 20 km at six stations in the tropical North Pacific, arranged by latitude. From (11).
Figure 14. Variations of 12-month moving average of mean zonal wind speed at Eniwetok Island, at three levels in the stratosphere. Upper curve corresponds to about 24 km, middle curve to about 20 km, and lower curve to about 16 km. From (12).

Figure 15. Power spectrum of annual mean temperatures for central Europe, based on record 1761 to 1953. Maximum lag of analysis m = 88 years. From (13).
A wide variety of physical changes take place on the sun, most particularly over the 11-year cycle, and sunspot numbers are an especially dramatic index of these changes. Professor Willett has already alluded to these and to their possible significance for varying the amount and kind of solar energy that reaches the earth. And yet, although we know that solar activity is responsible for important events in the earth's ionosphere, I believe it is fair to say that no one has yet been able to prove beyond a reasonable doubt that surface climate is also responsive to variable solar activity. The search for relationships between solar variability and tropospheric weather continues, however, and well it should until the problem is more adequately understood.

In addition to the 11-year solar cycle, sunspot numbers (and their relative abundance in the northern and southern solar hemispheres) appear to vary systematically over much longer intervals of time. In recent centuries this variation has given the appearance of a rather regular periodicity 80 or 90 years in length. It is not by any means clear, however, that this variation in sunspot statistics should imply a parallel variation of solar energy output and thus of world climate. There are a number of indications that this might be so, but, unfortunately, reliable meteorological data span too short a period of history for us to be able to verify this directly. The subject certainly deserves intensive study, and I look forward to devoting my own attention to it in forthcoming years.

6. Moving Averages and Time-Series Filtering

Before we pass on to the final part of my story, which will be concerned with an analysis of periodicities in drought at St. Louis, there is one other matter of technique in time-series analysis that needs to be considered. This concerns the practice of studying long-period fluctuations by smoothing with moving averages.

Along with harmonic analysis, the technique of moving averages has occupied a prominent place in the practice of cycle hunting. The idea, of course, is to smooth out the rapid variations in a series so that the slower ones will show up more clearly. If there are well-defined long-period variations in the series, this procedure can be quite helpful in revealing their form. However, if the long-period variations are ill-defined, as in cases where they arise purely from random numbers, to emphasize them by means of moving averages can give the unwary analyst a false impression of their true character.

An example of this problem is shown in Figure 16 (taken from [16]), which shows a series of annual precipitation totals for Philadelphia (above) and a 10-year moving average of this series (below). The moving average
Figure 16. Series of total "water year" precipitation values at Philadelphia, Pennsylvania, 1873-1950, after scrambling into random time sequence. Annual values above, and 10-year moving average of these below. From (16).
reveals slow, rather smoothly varying oscillations that might tempt one to conclude the presence of a 20-year periodicity (which however broke down toward the end), and perhaps to extrapolate the curve into the future as the basis of a forecast. That this would be utter folly will be obvious to you when I confess to a little sleight of hand here. The data in this figure were scrambled into a random order before they were formed into moving averages and plotted, and so their time sequence has no physical meaning whatever! The data are shown in their correct time sequence in Figure 17, where the 10-year moving average is seen to exhibit a very similar behavior.

Figure 18 shows how a simple moving average (of length one unit) changes the amplitude of variation in the original time series at each wavelength (expressed as a multiple of the length of the moving average). In this figure, the ordinate is the "frequency response," defined as the ratio of amplitude after averaging to that before averaging (see 14). You will note that only the very longest wavelengths are passed by the moving average with their full amplitude, a fact which helps to explain why so many series smoothed in this way appear to contain long oscillations and trends. Moreover, you will note that between wavelengths of 1/2 and one times the length of the moving average, the response is negative. This means that fluctuations in the original series between these wavelengths have a tendency to be shifted in phase, if not actually turned upside down!

Let me be quick to assure you that moving averages are not all bad. The ordinary form of them, in which successive terms are weighted evenly, is but one of many possible forms. If we wish, we may adjust the weights to impart to the moving average almost any shape of frequency response we desire. The procedure for accomplishing this has been described by Brier (15).

Suppose that we wish to isolate those variations in a series of annual data that approximate to 11 years in period. We could do this first by designing a moving average whose frequency response resembles that shown in Figure 19. Such a moving average would pass all variations near 11 years in period with negligible reduction of amplitude, but would almost completely suppress variations with wavelengths shorter than 5.5 years and longer than 22 years. A filter of this kind, which passes wavelengths somewhere in the middle of the spectrum, is known as a "band-pass filter." By the methods of Brier, we can calculate the weights for the moving-average filter required to achieve this particular frequency response. The results are shown in graphical form in Figure 20. The effect of applying this particular filter to sunspot numbers during the past 200 years is shown in Figure 21.

Band-pass filters are a useful tool for following the time history of changes in the phase and amplitude of a priori known periodicities or quasi-periodicities in a series. This is especially true when the series contains
Figure 17. Same data as Figure 16, but shown in proper observed time sequence. From (16).
Figure 18. Frequency response of a simple moving average, showing ratio of amplitude after averaging to that before averaging, as a function of wavelength (period) of variation. Length of moving average set equal to one; other wavelengths expressed as multiples of this. From (3) and (4).

Figure 19. Frequency response of special moving average (band-pass filter) that maximizes information about fluctuations with wavelengths near 11 years, and eliminates all fluctuations with wavelengths much longer or shorter than these. From (15).
Figure 20. Graph of 43 weights used to generate moving average with frequency response shown in Figure 19, assuming use with time series of annual values. Moving average is associated with date of central weight (W). From (15).

Figure 21. Series of annual Zurich sunspot numbers (thin line) and the corresponding values of the series after filtering by the weighted moving average in Figure 20 (heavy line). From (15).
variations at other wavelengths that tend to obscure the one we are trying to follow. It must be realized, however, that if there is no bona fide periodicity in the band of wavelengths to which the filter is "tuned," the output of the filter will still resemble a periodic function. Such filters must therefore be used with discrimination. An example of their proper use will be shown presently.

7. Time Series Analysis Of Drought At St. Louis

Mr. Palmer will shortly introduce you to a long drought-index series for St. Louis, Missouri. I should now like to show you some preliminary steps that are appropriate to take in analyzing this record for evidence of periodicities. Somewhat arbitrarily, I have considered this series in terms of its average value in successive summer seasons. Moreover, I have treated the series as a continuous variable, including both the wet and dry sides of the index, which is not strictly valid. And finally, although the record at St. Louis before 1869 appears to be somewhat inhomogeneous with the record since, I have not undertaken any adjustments in the series to allow for this.

The drought-index series, and the corresponding series of total summer precipitation, are shown together in Figure 22.

The first step should be to compute the power spectrum. A choice of 44 years for the maximum lag (m) of the analysis seems reasonable, inasmuch as this is a fairly small percentage of the total length of record and yet is large enough to provide a rather fine spectral resolution. Once the choice of lag is set, the computation of the spectrum can be left entirely to the electronic computer, using any of several available computer programs. In this case, the spectrum, which appears in Figure 23, was computed by the staff of the National Weather Records Center in Asheville, N.C., on a Honeywell-800 computer.

Since the lag-one serial correlation of the series, which was computed as a by-product of the spectrum, is +.44, the corresponding "red noise" spectrum may then be added along with its 5 and 95 percent confidence limits. (For the procedure of estimating confidence limits of spectra, see 2 or 3.) Using this "red noise" continuum as a null hypothesis, by which we postulate that the actual spectrum is a sample from a population of pure red noise with $\xi = .44$, we can see in the figure that only one spectral estimate exceeds the 95 percent confidence limit satisfying this particular null hypothesis. This estimate is the one at the far right, which will be recognized as our old friend the biennial pulse. Except for this feature, it appears that the drought index at St. Louis is pure red noise, and that if something in addition to red noise were present a much longer record would be needed to verify it.
Figure 22. Series of summer mean values of Palmer drought index (above) and summer total precipitation (below), at St. Louis, Missouri, 1840-1963. Dashed line at beginning of drought-index series shows index after correction for discontinuity of record in 1869, to render values more nearly comparable with those following discontinuity date.
Figure 23. Power spectrum of summer mean values of Palmer drought index for St. Louis, 1837-1963. Maximum lag of analysis $m = 44$ years. Red noise continuum and associated 5 and 95 percent confidence limits are added. Arrows locate peak near wavelength of 10 years, and two of its higher harmonics.
There is one oddity in this spectrum, however, that should not be dismissed without further checking. I am referring to the peak in the spectrum at the 9th and 10th harmonics, which corresponds to wavelengths very near 10 years. By itself, this peak would not arouse much interest because its amplitude has already been established as a non-significant departure from the red noise continuum. However, two other peaks in the spectrum, at the 18th and 26th harmonics, represent higher harmonics of a period near 10 years. Hence, we ought also to consider the joint statistical significance of these three peaks as evidence of a real periodicity near 10 years that has a non-sinusoidal shape. It turns out that the joint significance is rather high, being in the neighborhood of the 99 percent significance level for that particular choice of fundamental period.

If we view the joint significance of these three peaks in a more realistic way, however, we can relax again. Evidently, our test of significance should be based on a generalized null hypothesis, as follows. Instead of asking, what is the joint probability of finding peaks as large as these particular ones, we should be asking, what is the joint probability of finding a peak and two of its harmonics in an arbitrary location in the spectrum. The thought behind this form of question is simply that, had the peaks been at the 7th, 14th, and 21st harmonics of the analysis, or at any other equispaced positions in the spectrum, we would have been no less impressed by them. This brings a posteriori tests of significance into the picture (5), according to which this particular result fails to exceed the 95 percent confidence level.

Such reasoning may seem a bit obscure to the uninitiated. If you would prefer not to believe it, let us look at this indication of a 10-year period from some other points of view, and see if we can understand it any better.

First of all, we should check back to see if the same period shows in the spectrum of precipitation at St. Louis. This spectrum is shown in Figure 24, and, indeed, we do find evidence of it and its third harmonic, but not its second harmonic. Again, however, the joint significance of the peaks fails to exceed its 95 percent a posteriori limit.

Next, we might operate on the drought-index series by means of a band-pass filter tuned to periods near 10 years, and see how the phase and amplitude of the period behaves throughout the 126 years of record. Since 10 years is so close to 11 years, we may use for this purpose the same band-pass filtering weights that are illustrated in Figure 20. The results are shown in Figure 25.
Figure 24. Power spectrum of summer total precipitation at St. Louis, 1837-1963. Maximum lag of analysis m = 44 years. Red noise continuum and associated 5 and 95 percent confidence limits are added. Part of the 95 percent a posteriori confidence limit for the same continuum is also indicated.
Figure 25. Series of summer totals of precipitation (above) and Palmer drought index values (below) for St. Louis, after filtering by the moving average in Figure 20 to study variations in phase and amplitude of fluctuations with wavelength near 10 years.
This filtered series may best be used in the following manner. First, we plot the date and amplitude of each minimum point of the filtered series on a harmonic dial for which one revolution represents exactly 10 years. If the 10-year cycle is a genuine periodicity, all the plotted points should fall along one radius of the harmonic dial. The results actually obtained are shown in Figure 26. The minimum points of the drought index are evidently quite flexible in date (exhibiting a total leeway of four years); so we are not likely to be dealing with a precise periodicity.

Second, we might in passing inquire whether the cycle exhibits any coherence as to phase with the sunspot cycle. This, by the way, would seem unlikely, inasmuch as the sunspot cycle since 1838 has averaged appreciably longer than 10 years in period. For this purpose, we may plot the results on a harmonic dial for which one revolution represents one (variable length) solar cycle, as shown in Figure 27. The minimum points are seen to scatter rather evenly over the dial, a fact which lends no support to the notion that the apparent 10-year cycle in drought is solar connected.

If this 10-year cycle is found in spectra for locations other than St. Louis, then it would appear to deserve closer study. Conceivably it is real, and related somehow to the tendency to be discussed by Mr. Palmer for a 20-or-so year recurrence of severe drought during the past century in Western Kansas. On the other hand, it cannot yet be ruled out that it was introduced artificially into the drought-index series at some point along the complex route of its calculation. The fact remains that as yet, a 10-year cycle is not yet proven beyond a reasonable doubt as a real and persistent feature of climate in the Great Plains, nor for that matter anywhere else in the world.

8. Summary and Concluding Remarks

To summarize, although claims of a great variety of periodicities in climate can be found in the technical literature, only two (in addition to the familiar daily and annual astronomical cycles) appear to be established at impressive levels of statistical significance. These are the semi-synodic lunar cycle in precipitation, whose period is slightly less than 15 days, and the so-called biennial oscillation, which appears with dramatically large amplitude in the equatorial stratosphere but only with a very small amplitude in surface climate. Neither of these cycles evidently accounts for enough variance in surface climate to be worth incorporation into routine procedures of prediction.
Figure 26. Harmonic dial with period 10 years, showing phase and amplitude of each minimum point in Figure 25. Points are identified chronologically by numbers inside circle. Outside numbers are the last digit of the year in each decade.

Figure 27. Harmonic dial with variable-length period equal to duration of successive "11-year" sunspot cycles, showing phase and amplitude of each minimum point in Figure 25 relative to phase of contemporary sunspot cycle. Radius labelled "min" identifies beginning of each sunspot cycle; that labelled "max" identifies average phase of following sunspot maximum in all cycles. Time increasing counterclockwise.
In the future, other genuine periodicities in climate may be discovered. It is clear, however, that with one possible exception, if such cycles exist at all, they must be so small in amplitude and/or so variable in period that their practical significance for long-range prediction is vanishingly small. The exception is the possibility that climate varies rather appreciably over periods of 80 to 90 years, and perhaps over longer periods as well, corresponding to similar periods of variation in solar activity. This, however, is so long a cycle that ordinary meteorological data will have to be supplemented by other historical climatic indicators to demonstrate its reality. Such indicators as early weather chronicles, ancient Chinese accounts of exceptional sunspot activity, records of comet discoveries and aurorae which reveal secular changes of atmospheric seeing conditions, and long series of tree rings can be -- and to some extent have been -- used in efforts to study long-period cycles in climate. From such studies there have emerged a number of indications of the reality of an 80-90 year cycle in climate, and of even longer cycles, that appear to be related to similar cycles in solar activity. If such long cycles can be verified, they would have some value for predicting average climate (or climatic variability) of the order of decades in advance.

By and large, however, variations of climate from year to year, and from decade to decade, appear to be very irregular. Part of these variations may ultimately be traceable to atmosphere-ocean interactions, variable frequency of violent and dusty volcanic eruptions, or secular changes of atmospheric composition including an anthropogenic increase of carbon dioxide content since the 19th Century. In any case, a major component of the variations is of an irregular sort that one might expect to arise simply from the well-known fact of serial persistence in climate. This component, which has been defined as "red noise," is not amenable to long-range prediction by methods of time-series extrapolation.
Bibliography


FORECASTING CROP YIELDS

Bruce W. Kelly\(^1\) and John W. Kirkbride\(^2\)

Some hold that historians tell us about the past, that economists tell us about the future, and that it is only the present that is confusing. The discussions of the seminar may substantiate this belief.

We have heard about the past -- weatherwise, crop production-wise, and research-wise. We can look forward to hearing about the future and what it holds for crop yields. That leaves us only to worry about what is happening at the present time. That task is generally left to the Statistical Reporting Service along with a handful of professional estimators and thousands of self-appointed prognosticators.

We can all agree that crop yields are the culmination of a wide variety of variables, most of which show varying degrees of relationship to one another -- some positive and some negative in terms of crop output. One of the most controversial variables is weather, but even here we can agree that crop yields are dependent upon the weather -- assuming weather in its broadest sense. Other variables that exert significant influence on yields are soil type, soil fertility, plant population, variety, insects, disease and cultural practices.

What is the interaction of these items with weather -- some of which have occurred during the growing season to date, some of which must still occur during the current growing season? These questions offer interesting thoughts for speculation. Researchers can and do isolate one or more of these items and present evidence of their impact on yield. One of the problems to date has been the rather wide variation in evidence. You are aware of the various opinions relative to the effect of weather on the recent sharp uptrend in yields for certain crops -- ranging from only minor effect to accounting for more than 80 percent of the increase. Similar differences are voiced relative to plant population, application of fertilizer, new varieties, etc. These are all interesting items for speculation and helpful in the evaluation of a given set of conditions in relation to yield, but how well do such opinions or results measure the combined effects of the many

---

\(^1\)Chief, Research and Development Branch, Statistical Reporting Service, U.S. Department of Agriculture.

\(^2\)Head, Grain and Hay Crops Section, Agricultural Estimates Division, U.S. Department of Agriculture.
factors that result in the amount of product removed from a given acre? These opinions and research results do illustrate the luxury enjoyed by some in speculating about the cause and effect of yields. We in the Statistical Reporting Service seldom enjoy such luxury -- ours is the role of being expected to know what is happening to yield month by month.

The Statistical Reporting Service has the responsibility for making (1) forecasts of crop production from current crop conditions during the growing season and (2) annual estimates of crop production. These are two separate and distinct functions. We use "estimate" to indicate a measure of accomplished fact, such as at harvest time or later; the term "forecast" is used to refer to expectations of what is likely to be accomplished at some time in the future.

It should be clearly understood that a forecast is a statement of the most likely magnitude of yield or production on the basis of known facts on a given date. This assumes weather conditions and damage from insects or other pests and disease during the remainder of the growing season to be about the same as the average of previous years when the reported condition on the given date was similar to the present reported condition. The yield potential of the current condition may be appraised accurately. However, if weather or other conditions between the date of the forecast and the time of harvest are not similar to those experienced in past seasons that have been used in the determinations, the actual yield may differ from the forecast. As the season progresses, the forecasts made at or just before harvest merge into estimates of accomplished fact.

Methods and procedures utilized in crop estimating have changed to reflect the needs of users of statistical data as well as adapting to the organizational changes of agriculture.

The general methods employed in estimating yield of field crops are based largely on the theory of sampling -- selecting a limited number in the universe whose behavior is used to describe the behavior of the whole. The sampling procedures embrace both mail and enumerative survey methods. The aim is to maintain as much objectivity as possible in sample data. It would be desirable to place all surveys on a random sampling basis so that measures of reliability may be mathematically calculated. In practice, this is difficult. For the most part, samples consist of farmers who report voluntarily on operations for the farm they operate or the locality in which they farm. Locality data provided by the volunteer reporters are largely subjective -- that is, reporters must exercise considerable judgment in arriving at the figures they report.
History of Crop Reporting

Assuming the role of the historian, I would like to devote a few moments to what has happened in crop reporting during the past 100 years. These historical developments have a direct bearing on where we are today.

The Department of Agriculture was established May 15, 1862. Its responsibilities included the collection and distribution of annual and current agricultural statistics.

The first Commissioner of Agriculture, Isaac Newton, announced that the first item on his agenda was "collecting, arranging, publishing and disseminating, for the benefit of the nation, statistical and other useful information in regard to agriculture in its widest acceptance." One of his first actions was to develop a corps of voluntary farm reporters who submitted reports that were used as a basis for estimating crops.

In early 1863, a Statistical Division was formed in USDA. The first chief statistician initiated a reporting program that consisted of a corps of voluntary reporters representing each county in the country who would be sent blank reporting forms to be returned by the 10th of each month in the growing season. These simple, easy-to-fill out forms asked for acreage of and prospects for different crops. The first monthly crop report was published in July 1863 based on replies from 2,000 farm correspondents. For this report, correspondents were asked (a) average amount of land sown compared with 1862 and (b) "appearance" of the crop at the date in tenths of average. Data published were the average of these reports for each state and for the nation. No estimates of actual acreage and production by states during the growing season were published until more than 40 years later.

In 1866, rather than "appearance" an estimate of "condition" of crops was asked, a term that has continued to the present. Because of the impossibility of averaging nonquantitative statements such as "excellent," "good," "fair," or "poor," a numerical scale was adopted, with 10 representing an "average" condition and lesser or greater numbers representing conditions poorer or better than "average." However, it soon became evident that farmers had difficulty in visualizing an average condition. This was demonstrated by the fact that over a period of years, the average of all reports of condition was somewhat less than 10. To get away from the use of "average," the concept of "normal" condition became the standard by which reporters were asked to rate condition of crops.
A normal condition is not an average condition, but a condition above average, giving promise of more than an average crop. Furthermore, a normal condition does not indicate a perfect crop, or a crop that is or promises to be the very largest in quantity that the area reported upon may be considered capable of producing. The normal indicates something less than this and thus comes between the average and the possible maximum. The normal can be described as a condition of perfect healthfulness, unimpaired by drought, hail, insects or other injurious agency, and with such growth and development as may be reasonably looked for under these favorable conditions.

The concept of what constitutes a "normal" condition of a crop obviously varies from one locality to another with difference in soil and climate. It also changes slowly, over time, in the same locality because of change in varieties, cultural practices and soil fertility. Shifts in the acreage distribution of a crop within a state, from acres of low yields to acres of high yields, may mean that the same reported condition will indicate a higher yield than it once did. A shift in the opposite direction may have the reverse effect. The relative constancy of the aggregate of all the individual reporters' ideas of normal condition has greatly enhanced its usefulness.

During these early years there was much concern about the reliability of estimates. Efforts were made to improve the data by increasing the number of correspondents. This began a period of transition in the method of making annual production estimates. Up to this time, estimates were based on reports by county reporters of the total crop production for the county as a percent of the previous year. Beginning about 1888, county indications were weighted to calculate state indications. During the season there were returns, first of area, then several consecutive returns of condition, then of yield per acre, and finally of production, compared with the previous year. These furnished data for three separate tests of amount of production, which were examined at the end of the season and harmonized for the final and only estimate. This was the beginning of the evolution that led to the current procedure of calculating crop production as a product of the two separate estimates of acreage and yield. During the late 1800's an increasing number of reports were received from handlers and processors of agricultural products. Their reports, which were used as supplementary indications became increasingly important, particularly as post-harvest check data on the amount of the crops.

As early as the 1880's some dealers began to interpret the reported condition of each major crop in terms of actual bushels, tons or pounds of probable yield. The desirability of having such interpretations made by the government and, therefore, available to all was recognized, and in 1912 the Crop Reporting Board began to publish forecasts of yields.
The method used originally was the so-called "par method," which assumes a proportional relationship between reported condition and final yield over the entire range of reported condition values. The inflexibility of the "par method" necessitated subjective modification of the condition index or of the pars to eliminate the disturbing effect of highly atypical years and of trends in the data. The marked superiority of the graphic regression method of translating reported condition into a forecast of yield led to the abandonment of the par method for field crops in 1930 and the adoption of the graphic regression method. However, this method did not fully explain the upward trend in yields due to the introduction of hybrid seed, improved varieties, increased use of fertilizers, mechanization and better cultural practices. Therefore, time is used as a separate variable in regression charts. The usual estimating procedure is to compute the net regression of yield on conditions taking into account time. Deviations from this line are then plotted against time. A reading of yield would be the regression value from the current condition level plus an increment for time.

The Crop Reporting Board does not forecast yield solely on the basis of reported condition. As a crop nears maturity, reporters are asked to estimate the probable average yield in their localities and the average of these crop reporters' forecasts are translated into yield forecasts by means of regression charts in which true yields are plotted against reported probable yields. For most crops, reported yields take into account weather conditions, cultural practices and other factors, consequently no adjustment for trend is necessary.

Regressions of final yield on current prospects are tools of major importance in our statistical workshop where forecasts of yield per acre are made. Current prospects which reflect the impact of weather, cultural practices and other factors to date are independent variables in forecasting equations. The impact of weather and other factors to date, as well as thereafter, is reflected in the dependent variable, final yield per acre.

It is very evident, therefore, that weather and yield forecasting are inseparably linked and that crop-weather relations are of vital concern in our work. While irrigation, mechanization and up-to-date cultural practices have given some measure of weather-proofing to crop yields, weather is still an important factor in determining yield per acre.

Since there is a logical cause and effect relationship between weather and crop yields, direct use of weather as a means of forecasting crop yields has been a challenge of long standing. Some of our earliest mathematical research in crop-weather relations consisted of simple correlation studies. In such studies, the final yield of a crop was charted against a single variable, usually monthly or total rainfall during a growing season or temperature during supposedly critical months.
It is very seldom that a single weather factor accounts for all of the variation from year to year in the yield of a crop. These studies were largely exploratory or educational and proved to be of limited use in estimating yield, except for winter wheat in the Southern and Central Plains area. In the states comprising that area, rainfall is usually light and seldom heavy enough to reduce yields. Thus, in most years a linear relationship exists, the greater the rainfall the higher the yield. Some years ago the simple rainfall-yield relations were of some use in estimating the yield per acre of wheat early in the season for that area. In recent years, however, factors other than rainfall have come into the picture and the simple relationship is not as dependable as heretofore.

Limitations of the simple correlation studies coupled with the challenge of improving early season estimates of yield brought multiple correlation studies to the forefront. Graphic multiple correlation methods were developed showing curvilinear relations that gave the statistician an understanding of the effect of a combination of variables on yield. Regressions of final yield using various combinations of rainfall, temperature, humidity, and other indices of weather were developed for most major crops by states.

During the late 1930's detailed special crop-weather projects were carried out for cotton, corn, and wheat. The projects involved special crop-weather plots at a number of experiment stations recording detailed plant and weather observations. Some exploratory work was also done at that time using complex equations. All of these studies added materially to the statistician's knowledge of crop yields in relation to weather. They showed the relative importance of weather by months, the effect of accumulated rainfall prior to the growing season and the general importance of factors other than weather.

While the correlations were significant and fairly high for some crops in certain states, the relationship when used in subsequent years would not be the same as for the years included in the study. For forecasting purposes, therefore, the previously observed relationships were misleading at times and generally much less reliable than estimates based on currently reported indices of yield per acre.

"Indirect" Weather Approach

While the so-called "direct" weather procedure in estimating crop yields per acre has not been abandoned, the emphasis has been shifted to what may be termed the "indirect" or supplemental weather approach.

In the present estimating program, considerable use is being made of multiple regressions in estimating yield with reported condition and/or yield, precipitation or indices of weather as variables.
Multiple regression equations and charts using combinations of current prospects reported by crop correspondents and precipitation as variables are being used for winter, durum, other spring wheat, corn and soybeans for some months and areas. In general, precipitation data contribute two factors to the equations: (1) accumulated precipitation for selected months before the forecast date, and (2) precipitation for the following month or combination of months. Precipitation after date has to be estimated from a knowledge of long-time trends, seasonal patterns in recent years and long-range weather forecasts. For most early season estimates, precipitation after date accounts for the major portion of the variance. The level of the indicated yield, therefore, is heavily influenced by the estimate of precipitation after the forecast date and the procedure becomes very subjective for current forecasting.

In appraising current prospects, crop reporters take into account seasonal progress, diseases, insects, quantity of fertilizer used and other cultural practices. The reported condition or yield, therefore, reflects the composite effect of weather and cultural practices to date and reporters' evaluation of such factors on final outcome. When these measures of current prospects are used as variables along with actual precipitation to date and after date, the regression coefficients measure the contribution of the components used. Any persistent tendency for farmers to underestimate or overestimate for a given pattern of rainfall, therefore, is appropriately adjusted.

In this approach we are not necessarily limited to use of actual weather data as a variable. Other factors which are, in themselves, measures of weather or effects of weather are also used. Estimating procedures for cotton and tobacco are examples of such methods.

Cotton fruits on a rather rigid time schedule in two dimensions, vertically and horizontally. For corresponding positions on the plant, it sets fruit up the stalk at about twice the outward rate along a given fruiting branch. With the fruiting rate fixed and the vegetative growth rate affected by weather and other conditions, the ratio of fruit to total vegetative growth is quite variable. Under lush growth conditions, internodes are long and the plants are large in relation to the quantity of fruit. Conversely, in periods of drought, internodes are short and the set of fruit is heavy in relation to the vegetative mass. Farmers tend to overstate prospects when plant growth is lush and understate during drought periods. It is necessary, therefore, to use an appropriate correction factor in our forecasting procedure.

The yield forecasting procedure used for burley tobacco is an interesting variation of the same general principle. To those directly involved in forecasting tobacco yields it has been apparent over the years that during the growing season procedures tend to overstate the relative yield
and condition of the crop when soil moisture is abundant and, conversely, understate its potential when drought conditions prevail. There seems to be a natural cause for this on the part of producers since the crop responds with luxuriant growth during moderately wet weather but remains nearly dormant during periods of drought. The crop has unusual ability to recover after a drought but tends to be deceptively thin and light when moisture is abundant.

To adjust for those factors we use pasture condition as one of the variables in the multiple regression equation. Pasture condition is readily available and serves as an index of soil moisture.

During the past 10 years a program involving probability area samples for use in estimating crop acreages has progressed from a limited pilot program in a few states to an operating level in 32 states and a pilot study level in 13 states for 1964. This program is intended to provide unbiased estimates of crop acreages, livestock and farm numbers and many other statistics pertaining to the farm. The probability area sample project also includes a program of objective yield studies designed to provide an unbiased indication of yield levels during the growing season and at harvest time. Objective yield studies are limited to cotton, corn, wheat and soybeans at this time, with exploratory work under way for sorghum grain. Much of the basic research work has been done through contracts with the statistical laboratories at Iowa State University and North Carolina State. Progress of the development of forecast and estimating models varies. Work is most advanced on cotton and just getting under way for soybeans. The cotton work is on an operational level in 10 states; corn is operational in 24; winter wheat operational in nine, and a pilot basis in six; spring wheat is on a pilot basis in six states. Soybean studies include surveys in 11 states, but these are still considered largely at the pilot level.

Conceptually, when a crop is mature standing in the field ready for harvest, obtaining an estimate of yield is nothing more than a sampling problem. Theory exists whereby properly designed samples of suitable size can produce sample estimates of yield with any desired precision.

**Pre-Harvest Sampling**

Techniques for estimating yields from objective counts, measurements or weights are of comparatively recent origin. Two Indian statisticians, Mahalinobis and Sukhatma, are generally credited with developing crop cutting techniques which give pre-harvest sample estimates of yield. These are based upon harvesting small sample plots of known size.
In the United States the Crop Reporting Service began experimenting with crop cutting just prior to 1940 with pre-harvest wheat surveys through the Plains States. These were discontinued after 1940 with no further work until about 10 years ago. At that time, an intensified program of objective counts was undertaken. One of the first steps taken was to approximately optimize plot sizes. Optimum sizes turned out to be rather small: two rows, 15 feet long for corn; two rows, 10 feet long for cotton; two rows, 3 feet long for soybeans; and a plot approximately 1/10,000th (0.00001th) of an acre for wheat. Experiments were conducted to find means of reducing the biases associated with these small plots. It was determined that bias could be controlled by making very precise measurements of the sample plots; by development of rules for handling border line plants, and by careful training and supervision of the samplers.

A sample design has been worked out for field and plot selection. At present, an allocation of sample fields is made to states with consideration given to the precision of both state and regional estimates. Within states a subsample is selected from the fields chosen in the spring general purpose probability sample survey. The fields in the subsample are selected with probabilities proportional to acreage, and two plots per field are located by a random process. This procedure results in a self-weighting sample of plots. Incidentally, the optimum number of plots per field appears to be something less than two, but one degree of freedom is desirable for analytical purposes, and the loss in efficiency is small.

The precision of the pre-harvest estimate of yield is of interest. A sample of 3,100 corn fields allocated to 24 North Central and Southern states gives a regional yield estimate with a standard error of about three-quarters of a bushel, and a sample of 2,150 cotton fields allocated to 10 Southern states gives yield estimates for individual states and for the region with a coefficient of variation of about 5 percent and 1 3/4 percent, respectively. The bias in the procedure for estimating corn yield has been measured by comparing sample estimates made by harvesting plots with the total production from the field; it has been found to be positive but less than 2 percent.

The timing of the objective yield surveys is geared to the forecasts and estimates published by the Statistical Reporting Service. During the growing season, forecasts of yield are made at monthly intervals beginning about two months before harvest. The surveys upon which the objective forecast of yield are based are likewise made at monthly intervals. For corn, soybeans and cotton the first survey is made about August 1; and for winter wheat, about May 1.

At the first visit to the sample fields the plots are carefully measured off and marked so that they may be found readily. At this and
subsequent visits, the number of plants and the number of fruit by maturity classes are counted, and a sample of fruit sent in to a laboratory for weighing and determining moisture content. Then, at the last visit before harvest time, the plots are completely harvested and their yield determined. Following harvest, gleanings are collected in similar sized plots for measuring harvesting losses.

Forecasting yield is more difficult than estimating it. Direct measurements of yield can be made only when a crop is mature. When plants are immature, yield as such does not exist and hence cannot be observed directly. But, components of yield such as plant numbers, numbers of fruit, and size or weight of fruit can be counted or measured, physiological observations of plant characteristics can be made, and the components of yield projected to harvest rather well.

Plant development and fruiting tend to be orderly processes. By the time fruiting occurs, many of the factors of heredity and environment which affect the plant's capacity to produce fruit have already exerted their influence, and yield potential tends to develop unless inhibited by abnormal growing conditions. Present forecasting procedures for objective yield include no explicit environmental factors. The time lag of weather effects upon plant development is well-known and it is recognized that historical averages rarely materialize. However, the effect of weather upon the relationships underlying present procedures is not known and cannot become a part of forecasting procedures.

In order to utilize the relationships found in growth and fruiting patterns, plant maturity in terms of the point of development in the plant's life or production cycle must be known. It is desirable to infer maturity from observable plant characteristics.

When maturity is known and the relationships based upon characteristic patterns of plant and fruit development have been determined, then yield components can be projected to maturity. The relationships seem to hold rather well within varieties and geographic areas and the resulting forecasts have generally been good.

**Corn, Cotton Objective Forecasting**

As illustrations of objective forecasting procedures, let's look briefly at cotton and corn. For cotton, two different forecasting models are being used to predict the number of bolls the plant will produce. One is known as the rate of fruiting model and the other, the rate of survival. The rate of fruiting model is more complex, and because of time limitations will not be discussed here.
The rate of survival model is based upon the fact that blooms and bolls which appear on the plant during the early stages of its fruiting period have a much greater probability of surviving to produce mature cotton than those that are set after the plant is carrying a greater portion of its ultimate fruit load. The survival rates for squares, blooms and small bolls, and large bolls were determined by noting the disappearance of tagged blooms and bolls averaged over several seasons with respect to a sensitive measure of plant maturity. The present measure of maturity is the ratio of large bolls to total bolls. By means of the relationship between maturity and survival rate, the fraction of fruit on the plants that can be expected to survive and produce cotton may be predicted.

In the rate of survival model an allowance must be made for the production of bolls from plots containing no fruit at the time of the survey. A satisfactory relationship has been worked out by averaging over several seasons the number of large bolls produced with respect to the maturity ratio.

To use the probability of survival model one computes the maturity ratio, multiplies the average number of fruit observed in each category by its expected survival rate, determines the bolls expected from plots in which fruit has not yet begun to form and sums these parts to obtain the forecast of bolls at harvest.

Although average boll weight does not fluctuate greatly between years, relationships have been found which permit predicting the boll weight at harvest. As cotton begins to mature there is a relationship between the weight of the cotton from maturing bolls and the maturity ratio. This relationship is the basis for predicting average boll weight.

Although these models are based on linear approximations of historical relationships, the resulting forecasts are generally good except when upset by abnormal growing conditions. In 1963 the August 1 prediction of yield was within about 5.5 percent of that actually produced as estimated by the pre-harvest survey. The corn forecasting model is also based upon simple linear relationships which were derived from experimental observations.

At the time of the August 1 survey, the corn in some of the more northerly states has not begun to form ears. When this is the case, the number of ears to be produced is predicted by a linear regression between stalk numbers and ears produced, derived from historical data, and a historical average ear weight is also used.

When ears are present, the problem is that of predicting ear weight. Fortunately, ears attain their maximum size by the time they reach the milk stage, and equally fortunate there is a linear relationship between length of ear and weight of grain. By means of this relationship, the length of the cob measured over the husk has proven a good predictor of ear weight, provided adjustments for frost damage and early harvesting are made.
Studies have shown that dry matter is laid down in the ear until the moisture content of the grain is below 30 percent. Where early harvesting occurs, it is necessary to adjust the weight per ear for loss of dry matter as well as for moisture content.

To adjust the forecast for possible early frost, the August 1 stage of maturity is used to estimate the number of days to maturity, and by comparing this date with a historical average of first frost dates for the locality, an adjustment for the likelihood of frost damage is made.

Last season, for 11 North Central states the August 1 corn yield survey predicted averages of 58.0 ears per plot weighing .413 pounds per ear. The pre-harvest survey found 58.7 ears per plot and an average ear weight of .438 pounds. Consequently the August 1 forecast of corn yield turned out to be 4.5 percent below the pre-harvest indications.

Objective yield techniques have been developed for tree crops as well. These include oranges, lemons, peaches, pears, walnuts, filberts, and sour cherries. These techniques are based upon concepts similar to field crops. The essential differences are that the sampling unit is a tree and that the crop of fruit is set before the time of the first forecast so that it is not necessary to predict the number of fruit yet to come.

On the whole, the objective forecasting procedures in their present state of development are performing reasonably well. However, further refinements are needed in the form of more sensitive relationships that are clearly defined and which incorporate the effects of environment upon plant production.

**Work for the Future**

Work still remains to be done in the area of improvement in forecasting crop yields as well as the true yield level. There is need for more intensive studies relating crop yields to weather factors and to early season plant characteristics. Detailed phenological and environmental observations are needed; the relationship of dry matter accumulation to weather factors over the entire growth period and the use of such relationships in predicting crop yields should be explored. Then special studies need to be separated into several areas of interest: (1) phenological events such as emergence of plants, fruit emergence and fruit counts by maturity category, and (2) the mechanism of growth and development over time as related to accumulated weather factors. Any early season forecasting method would be greatly enhanced by knowledge of the weather likely to occur during the growing season within even rather large geographic areas. Through the use of new statistical techniques and modern facilities, we can predict that crop yield forecasting in the future will give greater precision and usefulness.
LIVESTOCK CYCLES AND THEIR RELATION TO WEATHER AND RANGE CONDITIONS

Harold F. Breimyer and Alan R. Thodey

Probably from earliest times man took note of periodicity in natural events around him and in his own physiology. He was also aware of the recurrent ebbing and flowing in both individual and collective well-being. It was a simple next step to speculate on a connection between the natural and the cultural. It frequently led to seeing a clockwork rhythm in human affairs. The Old Testament is full of references to repeatability in various phenomena. Apparently three, seven and 12 were the most common intervals. In the Biblical account of the Pharaoh's dream of fat and lean kine, interpreted by Joseph as foretelling good and bad crop years, it is worth noting that the number of each was that digit of divination and superstition, seven. Still today the same number is said to be held in high regard in some circles.

Ancient perception of a link between the physical and the cultural or institutional worlds probably also brought varying behavioral reactions. Some individuals doubtless chose obeisance before the natural phenomena that seemed so governing. Others, the activists, preferred to do something about it. If the counsellor to the Pharaohs had only interpreted a dream he would never have made entry into the Book of Genesis. It was his program of action that brought him literary immortality.

Nor should we be smug about any change in the human psyche since those primeval times. According to the printed program we are to spend two days here at Ames reviewing the factual evidence of a relationship between

1/Visiting research professor, Department of Agricultural Economics, University of Illinois.
2/Department of Agricultural Economics, University of Illinois.
3/An ingenious and interesting modern attempt to ascribe cycles in economic affairs to periodicity in the physical universe is found in the sun-spot and similar theories of business cycles. Cf., Wesley C. Mitchell, "Business Cycles," National Bureau of Economic Research, New York, 1927, pp. 12-16. Mitchell quotes the German Werner Sombart in a distinction that obliquely bears on the thesis of this paper. Sombart held that "in the organic industries . . . the condition of business is determined largely by the yield of raw materials; in the inorganic industries the condition of business itself determines how much of the raw materials shall be produced." (Mitchell, pp. 15-16). This aphorism presupposes that inorganic materials are not employed in organic industries. This is not so generally true now as it once was. Capital inputs are utilized in great volume in agriculture, the output of which is no longer a mere manifestation of the bountifulness -- whether rhythmic or not -- of nature.
weather and our food supply. Then, in our own sequential pattern, we will devote a third day to the implications of those findings for national policy. It will be interesting to see how the empirical content of the first part of the seminar becomes translated into its meaning for action as sought in the second part.

The assignment for this paper falls in the first part of the schedule and the fact-finding category. The authors have been asked to sketch the relationship, if any, that exists between livestock cycles, weather and range conditions. This is the only paper at this conference that deals with livestock. The assignment takes on some complexity because livestock are no longer a primary product of agriculture. Only cattle and sheep that graze the western range are direct harvesters of "crop" (i.e., forage) production. They seldom subsist solely on range grass; most get some supplemental feeding. Furthermore, since range grass is grazed more closely in some years than others, the tonnage of beef or lamb produced is not an exact measure of the quantity of range feed that was available.

Thus any study of cycles or other variations in production of livestock by no means reflects with any precision how weather affects man's food supply. It reflects how natural events such as weather plus man's management of feed resources combine to affect our food supply.

Probably the only solid datum from which we can start is that cycles in livestock do exist. To certain time series of data relating to livestock there is enough patterned regularity to satisfy most test of cyclical configuration.

The most clearly revealed and best known cycle is in January inventories of cattle on United States farms. Since 1880 that statistic has traced a pattern of cyclically successive increases and decreases. It has completed its sixth cycle and is now near the turning point in a seventh. Edward Karpoff of USDA in an unpublished study noted that the first statistics on cattle inventories published years ago did not show a cycle. Only after the series underwent massive revision did a cyclical pattern emerge. A skeptic might wonder whether the makers of estimates introduce cyclical relationships as an estimating device. To inquire into techniques of estimation is appropriate to any study that employs estimated data. Nevertheless, very few observers of cattle trends in the United States would deny that time-series data for cattle inventories have a cyclical character.

The cattle inventory cycle as an historic reality may nevertheless be in some jeopardy for the future. It has always rested fundamentally on cyclical changes in the size of the basic producing herd, especially beef
cows on the western range. Recently the producing side of the cattle industry has lost some of its prominence to that vigorous upstart, modern commercial cattle feeding. Cattle feeding may now be generating the newest of all livestock cycles. The 1963-64 price difficulty in beef cattle can be attributed only partially to the old familiar cycle in cattle production. It had more of the marks of a fed cattle cycle, or possibly of a cattle feed-lot cycle.

For sheep the empirical evidence of a cycle in numbers or production is less convincing. Almost surely, if a cyclical phenomenon exists in range cattle it must also be present in range sheep. But the sheep business has been in turmoil for so long and has undergone so many changes in make-up and location that cyclical tendencies have been overshadowed.

Is there a cycle in hogs? As long ago as 1895, Samuel Benner said there was. Countless writers have since referred to a cycle in hogs. The answer to the question may rest on one's definition -- that is, on what minimum curvilinearity in plotted data one's eye may require. Also, a distinction is to be made between the years prior to CCC storage programs for feed grains, and those of their operation. This distinction will be discussed below.

Theories of Livestock Cycles

Cycles in livestock have been viewed in three different ways.

The first is pure empiricism. They are seen and charted. They are checked. Samuel Benner found hog cycles as "alternately certain as the diurnal revolutions of the earth upon its axis..." The presupposition is that cycles so clearly revealed in the past can be extrapolated into the future. The tools required are only a straight edge ruler and a French curve.

Nor need the technique be belittled. In spite of all the attempts to probe the inner workings of the cattle economy, most forecasting of cattle trends still rests heavily on graphic analogy with previous cycles.

Of more interest, nevertheless, is the question as to whether livestock cycles are self-generated or the result of outside forces. To what extent do they propel themselves in never-ending sequence, according to the principle enunciated in the Cobweb Theorem? Or to

---

5/Ibid.
6/This theorem describes the tendency toward successive waves of over-expansion and underexpansion that is seen in many sectors of the economy.
what extent are they the product of external events of which changes in
feed supply are likely the most important? Each school of thought has
distinguished adherents. Some years ago James H. Lorie built a cyclical
model that depended basically on its internal mechanism. John Hopkins, 8
Charles Burmeister 9 and Frank A Pearson 10 are among those who have
emphasized outside stimuli and would lay much importance on the feed
supply.

This contest can be viewed in other terms. Significantly, they
match an industrial against an agrarian view of things. To whatever degree
livestock cycles self-generate they are of a common mold with investment
cycles and building cycles and all other members of that big but unhappy
family of rhythmic instabilities. They thereby exhibit induced and autonomous
investment and an accelerator and doubtless other attributes of the cycles
of the industrial and business world. Cycles so viewed must be regarded
as products of human institutions. They are man-created. Presumably,
they can be man-corrected.

To whatever degree cycles in livestock are attributed to cyclical
fluctuations in feed supply -- now primarily range feed -- they are a
modern replica of an ancient phenomenon. They show that man has not
yet freed himself from bondage to erratic and sometimes niggardly natural
forces. Whether he cannot or merely refuses to is a separate issue; all
to be said in this instance is that he has not.

The Cycle in Hogs

Hogs are almost exclusively consumers of concentrate feeds,
primarily feed grains. Therefore, any analysis of hog production and feed
supply relates by definition to grain and other concentrate feeds only.

If a cycle could be said to exist in hogs before CCC feed programs
began, it would constitute evidence that feed grain production swings
cyclically up and down. Hog production was inexorably tied to annual
harvests of feed grains; and even though fluctuations in hog production
were not as erratic as those in feed grains, due to the evening out effect
of the life span of the hog, they were necessarily of similar cadence.

7/He acknowledged the disruptive effect of weather but only to the extent
it "can alter temporarily the typical pattern" of the cattle cycle. James
H. Lorie, "Causes of Annual Fluctuations in the Production of Livestock
and Livestock Products," Supplement to the Journal of Business, University
of Chicago, Studies in Business Administration, Vol. XVIII, No. 1, 1947,
(p. 60).
8/John A. Hopkins, Jr., "A Statistical Study of the Prices and Production of
9/Charles A. Burmeister, "Cycles in Cattle Numbers," The Livestock and
10/Frank A. Pearson, W. I. Myers, and E. E. Vial, "Interrelationships
among Farm Demand, Value and Supply of Cattle," et seq. Farm Economics,
Cornell University, Nos. 189-193, 1953-54.
Hog production was kept closely connected with feed supply through the working of the hog-corn price ratio. The price of corn, which responded to the size of each year's corn harvest, was the prime mover in that ratio.

Whatever one's interpretation of the statistical record for hogs in pre-CCC years, there is little room for disagreement as to the experience since. CCC price support and storage programs have not produced a perfect ever-normal-granary for feed grains, but they have attained a reasonable approximation to it. Already the stabilizing effect on feed supply and price is virtually taken for granted. Only persons of middle age or older can appreciate the contrast between the present relatively stable supplies and prices of feed and the alternate feast and famine that wrought such instability up to the middle 1930's. In the years since feed prices stopped bouncing up and down, the year-to-year variations in hog production have been smoothed out fairly well.

It may nevertheless be wondered if statistical trends in livestock have an aversion to stability. No sooner had sharp and erratic annual changes in hog production disappeared than a new pattern of fluctuation appeared in their place. It has most of the tell-tale signs of a cycle. The number of hogs produced now moves up and down in fairly smooth cyclical swings much as cattle inventories do. The hog cycle is shorter than the cattle cycle. The last couple of hog cycles have been moderate in amplitude. To expect such good behavior from them in the future would, however, be overoptimistic.

Analysts now depend more on projections of cyclical patterns than on the hog-corn price ratio to explain and predict changes in hog production. As the price of corn is less unsteady than before, the hog-corn price ratio is influenced more by changes in hog prices and less by changes in corn prices than was the case in years before feed storage programs entered the picture.\footnote{Cf., Harold F. Breimyer, "Emerging Phenomenon: A Cycle in Hogs," Journal of Farm Economics, Nov. 1959, pp. 760-68.}

Cycles in Cattle

Cattle feeding may now be cyclical, but the evidence is too recent to permit reliable analysis or lead to a firm judgment. Likewise, any cycle in construction of feedlots could be the most worrisome of all cycles relating to livestock. These cycles will not be discussed here.

Cattle production is now nationwide. Separate analyses should be made for the western range and the eastern pasture regions. To date, the
eastern half has not been studied in depth. For this reason it is not included in the analysis that follows.

The western producing region remains the most adapted to research analysis. From the historical record for that broad region at least a little empirical evidence can be found as to relationships between livestock cycles and range feed supplies.

The region used in this analysis, the 17 western states, contains 45 to 50 percent of all roughage consuming animal units and about 12 percent of total animal units in the United States.

It is well known that supplemental feeding of cattle on western range is more common now than it once was. It can be used to counterbalance changes in supply of range feed. Nevertheless, it can offset those changes only partially. Cattle production of the West remains closely linked to range conditions.

The customary method of analysis is to match data on livestock inventories with data on range feed condition. It has been adopted in this paper. The charts that follow compare the number of roughage consuming animal units fed each October-September year with the estimate of range condition in June, July and August as reported by the Statistical Reporting Service. The first chart summarizes data for the entire 17 western states. Separate charts relate to five smaller regions: Pacific States, Mountain States, Southwestern States, Northern Plains States and Southern Plains States.

The data for range feed condition are simple averages of indexes for each of the three months and for each state. They have not been weighted by size of state; Nevada counts for as much as California or Nebraska. More important to analysis is the nature of the range feed index itself. It is a "condition" index as reported by farmer reporters. It is an estimate of the visible supply of range feed in the locality reported on. It is strictly a subjective figure, and its usefulness rests on the skill of farmers and ranchers in appraising the range feed supply and on their judgmental consistency from year to year.

*Cattle feeding is almost wholly divorced from range conditions. Ideally, the data in the charts that follow would have been corrected for the cattle-on-feed component. However, it is not so very large, except for some of the areas in recent years. It accentuates the upsweep in inventories in the Pacific area, for example. It does not affect the overall relationships shown.*
Moreover, being an indicator of the available feed, the range feed index is in no sense independent of the level of grazing being carried on. In a range feed-livestock inventory analysis the range feed index is not an exogenous variable. If the adjective "endogenous" can be compared, the range feed index is very endogenous indeed.

For whenever the level of stocking is high, the grass is grazed closer and the remaining supply of range feed is reduced. If stocking is light, the supply will appear greater. Thus when cattle and sheep numbers are down, the range may retain a fairly good condition even in a rather dry year. But when the stocking is heavier, a moderately dry season may appear as almost drought.

This characteristic of range feed statistics bears on the comparisons to be observed between livestock numbers and range feed condition in the West. Overall, the chart for the 17 western states (Figure 1) reveals year-to-year fluctuations in range feed conditions that sometimes are mild and other times sharp. It gives some indication of broad cyclical swings in range feed too. The inventory of roughage consuming livestock shows the familiar pattern of the cattle cycle.

It is hard to find in the summary chart for 17 states a clear tie between annual variations in range condition and in livestock numbers. But the pronounced cycles in livestock numbers and the milder cyclicality in range condition are interrelated. The timing of the relationship is important. If cyclical changes in range condition preceded changes in livestock inventories, it might be supposed that range feed does indeed go through cycles and that those cycles induce similar trends in livestock. Only during the early 1940's is there evidence that a cyclical swing in range led to a cyclical change in livestock. Usually, the relationship is one of similar timing but inverse pattern between cycles in range feed condition and in livestock numbers.

The conclusion follows that livestock cycles have more effect on range feed condition than vice versa. There is little reason to believe that range feed goes through periods of successively good years and successively poor years and that these give rise to cycles in livestock numbers. It is equally or more logical to say that livestock numbers go through cyclical fluctuations and these give rise to a cyclical pattern in the condition of range feed.

To be sure, individual years can be so dry as to force a reduction in livestock inventories. Such a year was 1934. There was a drought in several important cattle areas in 1956. Nevertheless, a year of short rainfall will be reflected in a poorer range condition, and will be regarded
Figure 1. 17 WESTERN STATES: Roughage Consuming Animal Units Fed and Index of Range Feed Conditions (June - August Average), 1923-1963.

Figure 2. PACIFIC STATES: Roughage Consuming Animal Units Fed and Index of Range Feed Conditions (June - August Average), 1923-1963.
as a more severe drought, if it is a year of high cattle numbers than if numbers are low. The 1956 drought appeared to be especially damaging because cattle numbers had been built up to a high level.

If the moderately cyclical pattern to range feed indexes in the West can be attributed to cycles in stocking level, it need not reflect a cyclical "beat" to western weather. Nevertheless, the analysis presented herein does not disprove the notion of a weather cycle in western country. It merely indicates that cycles in cattle do not prove it. Other data will have to be sought to confirm or refute the hypothesis of a weather cycle.

Data by Areas

Charts for the five subregions reveal differences in amplitude of fluctuations in both range feed condition and livestock numbers. They also show differences in the pattern of relationship between the two. Perhaps no one region presents as regular a pattern as does the West as a whole.

The Pacific States (Figure 2) have demonstrated relatively wide annual fluctuations in range feed conditions without any pronounced cyclical effect. In the period charted, a cycle in animal units became prominent when favorable feed conditions encouraged expansion during the war. That did not seem to change the pattern of range condition materially, although fluctuations since have not been as wide as before.

The movement in the number of animal units and in range condition in the Mountain States (Figure 3) is similar to that for the West as a whole. The two have tended to move in opposite directions, but with some exceptions. Range conditions held up particularly well during the war, for example, in spite of a high and increasing level of stocking.

The range condition in the Southwestern states (Figure 4) has not reacted noticeably to the gradual decline in the number of animal units, although the less drastic swings in the number of livestock in recent years are generally mirrored in a less changeable index of range condition.

The Northern Plains (Figure 5) have been subject to pronounced fluctuations in both livestock numbers and range feed conditions. Generally, the two have traced the same opposite path as in the West as a whole. Here, too, the range feed condition stayed high during the war years, in spite of sharply rising livestock numbers. Those must indeed have been bountiful years; they apparently favored the expansion in livestock numbers at that time.
Figure 3. MOUNTAIN STATES: Roughage Consuming Animal Units Fed and Index of Range Feed Conditions (June - August Average), 1923-1963.

Figure 4. SOUTHWESTERN STATES: Roughage Consuming Animal Units Fed and Index of Range Feed Conditions (June - August Average), 1923-1963.
The Southern Plains (Figure 6) also follow the same general pattern as the West as a whole. The poor range conditions in the early 1950's certainly appear to have taken the high peak off the livestock cycle at that time. They likely were more clearly due to dry weather than were the low range condition indexes in other parts of the West, even though the large livestock build-up had its usual aggravating effect.

Judgmentally, the above evidence by areas tends to confirm our doubt that apparent cycles in range feed in the West are due to cycles in weather. More probable is that they reflect alternate swings in understocking and overstocking of ranges. We repeat, this judgment does not entirely negate the possibility that weather cycles exist in western cattle country. It only says that livestock cycles are not to be taken as conclusive evidence thereof.

**Year-to-Year Relationships**

To study the year-to-year relationships between weather, range condition and feed supply it is necessary to turn to different sources of information. The broad regional and annual totals presented thus far are not useful for that purpose.

The short term analyses make a convincing case that yearly fluctuations in weather influence range condition and in turn affect the rate at which cattle are marketed.

Regrettably, the short term studies also have a deficiency. Whereas the cyclical analyses of Figures 1 to 6 only compare livestock numbers and range condition and omit data for weather, the short term analyses include weather data but take no account of the effect of stocking level on range feed condition. In the language of statistical procedure, most regressions made to date have been "partial."

Research conducted by Harold Abel and other members of the Western Livestock Marketing Research Technical Committee shows annual changes in summer range feed in the Northern Plains to be associated significantly with climatic conditions during the spring. "Throughout most of the northern and central Great Plains, April, May and June precipitation explains much of the variation in range-feed conditions." In a correlation study for range feed in five districts in Montana, "63 to 84 percent of the variation was explained by the log of precipitation for March through June." In other regions of the West, particularly the Southwest, fall and winter moisture also was significant in explaining changes in range conditions.

---

14 Ibid., p. 18.
Figure 5. NORTHERN PLAINS: Roughage Consuming Animal Units Fed and Index of Range Feed Conditions (June - August Average), 1923-1963.

Figure 6. SOUTHERN PLAINS: Roughage Consuming Animal Units Fed and Index of Range Conditions (June - August Average), 1923-1963.
Other climatic factors doubtless have some effect on range feed but they are much less important than rainfall. Temperature, for example, "has a statistically significant effect on ranges . . . when precipitation is below normal . . . Temperature was not significant in explaining residual variation when spring precipitation was normal or above."[14]

These findings are now being employed in livestock forecasting done by the Western Livestock Information Project at Denver.

An older study reported by Marion Clawson gave somewhat similar results. It correlated, for 1923-41, range condition in each year with precipitation in both the current and preceding years. The R²s state by state ranged from 0.2 to 0.8 but for 12 of the 17 states exceed 0.5.[15] These values are high enough to indicate a significant relationship between each year's range condition and rainfall. They are not so high as to rule out a cyclical relation between stocking level and range feed condition, evidence for which has been presented in Figures 1 to 6.

Yearly variations in range feed condition have a bearing on the decisions ranchers make as to marketing their cattle. Poor conditions speed marketings; good conditions slow them. It is our hypothesis that annual marketings may be more responsible to weather and range in the northern areas of the West than in southern ones. The reason is that the northern range country has long shifted back and forth between selling calves and yearlings in keeping with the range feed supply. Southern areas are more committed to selling of calves, and their marketing program is more flexible, season to season.

**Summary and Conclusion**

The first conclusion is that there is little reason to believe that cycles in livestock production are caused primarily by cycles in weather. For the classes of livestock that consume concentrate feed, any effect of weather is ironed out to an appreciable extent by national programs of feed grain storage. Even for forage-consuming livestock, supplemental feeding has become increasingly important over the years. The clearest case for a close connection between weather and livestock is that of cattle and sheep on western range. Yet data relating thereto yield no certain proof that cycles in livestock inventories are caused by cyclicality in range feed supply. The mild cyclical pattern to be observed in indexes of summer range feed condition in the West is at least partly a reflection of swings in the stocking level.

---

14/Ibid., p. 18.
Livestock cycles are to be attributed much more to the errors of fallible man than to the whimsy of a just God. They primarily are of the same family as building cycles, textile cycles and even the business cycle itself.

Weighty argument in support of this conclusion is the quickness with which fluctuations in hogs were transformed into a cycle once supplies and prices of feed became relatively stable. And now a cycle in fed cattle or in cattle feedlots -- it is hard to know which -- is making its unwelcome appearance.

All we are saying is that in its modern technology livestock agriculture is taking on a more industrial character. It is using more fixed investment. In gaining much of the physical efficiency of industry it is also falling heir to characteristic industrial instability.

This is not to deny that variable climate still contributes to instability in livestock production. It does. We do not even deny that there are weather cycles; we only say that livestock cycles are not indisputable evidence of them. Year-to-year variations in weather unquestionably affect all livestock that are grazed, in spite of the considerable supplemental feeding that is carried on. Occasionally a particularly bad weather year is crucial in halting a cyclical uptrend in production of cattle. Short term weather variations would affect all livestock, including grain-consuming species, were it not for programs that maintain feed reserves and keep feed prices fairly stable.

Whatever efforts are to be undertaken in the future to bring more order and stability to the production and marketing of livestock should definitely include steps to minimize the influence of capricious weather. In the division of the populace between those who sit on their hands and those use them, the authors of this paper hope to qualify for the second category.
THE SIZE OF GRAIN STOCKS THAT SHOULD BE MAINTAINED

Arthur T. Thompson

The title assigned for this paper seems to imply, first, that the nation should have a grain reserve at all times and, second, that its maintenance is a matter of public policy. As a sometime farmer and intermittent Department official who served previously during the drought years of the 30's and World War II, I have no difficulty accepting this implication. While the argument for carry-over stocks as insurance against yield variations has not been very convincing for several years now, there is at the same time a singular lack of guarantees of this recent weather luck for an indefinite run.

Anyway, even if yield variations due to weather could be ignored, there are other sufficient reasons today for larger holdings of grain than at any time in the past. For one thing, there is always the chance of nuclear attack, widespread and sudden. Then there is the greater worldwide dependence on U.S. grain supplies which has developed among importing nations, especially since the advent of Public Law 480 programs. Reserve stocks in the U.S. protect these countries not only against crop variations in the United States but also against the effects of adverse weather and other developments in their own agriculture.

History of Food Reserves

The maintenance of reserves against all sorts of possible eventualities has been practiced by man in some degree since time immemorial, sometimes only by provident individuals, at times even by nations. In ancient China, the followers of Confucius worked out a plan under which a part of the crop in good years was bought up by the government, first to keep prices stable, then to be held for later years of poor crops. According to the historians, the plan was fairly successful and in effect off and on for more than 1400 years.

In ancient Egypt, a similar plan was followed by Joseph of Biblical fame. As set forth in the 47th Chapter of Genesis, the grain surplus was stored up during "seven fat (good weather) years" to be released during the "lean" years.

1/Director, Grain Policy Staff, Agricultural Stabilization and Conservation Service, U.S. Department of Agriculture.
What seems to have been the first systematic effort to maintain national grain reserves in the United States as a public policy occurred in the early 1930's. After the searing general drought of 1934, which had followed the rather poor corn years of 1930 and 1931, the national administration then in power, through Secretary of Agriculture Henry A. Wallace, began to advocate the adoption of an ever-normal granary policy. This was not a new idea for Wallace. He had started urging the principle for practice by farmers as early as 1912. He campaigned for it editorially in Iowa in 1920, along with a plea for less corn and more clover to reverse the collapse of grain prices after World War I.

What was new by 1934 was the proposal that the matter of a grain carry-over level be linked up with a federal crop acreage control program. A device for handling the carry-over reserve problem had been developed in 1933 -- the non-recourse government loan on farm-stored commodities.

The per-bushel rate for that first government loan on corn was 45 cents -- considerably above the market price at that time -- and it brought under seal a total of about 270 million bushels. This enabled farmers who grew corn for cash to benefit from the later rise in market value and to keep more corn in their hands than otherwise would have been the case for helping to preserve livestock herds through the great drought of 1934.

By the late fall of 1937, in a speech at Indianapolis before a conference of farmers, businessmen and labor leaders, Wallace was quantifying the ever-normal granary in these words:

A long stride ahead toward stability could be made with a carry-over in the future that would average twice as much as carry-over has averaged in the past. That would mean an average carry-over (of corn) of about 350 million bushels.

It should be noted here parenthetically that Wallace also warned against having loan rates too high in relation to year-to-year levels of production and prices, since if there were persistent losses to the government, the program would be discredited.

"Corn Belt farmers," he said, "should cherish it (the program) as something which is not primarily the government's but their very own. If they do not cherish it in this spirit, but organize in the spirit of temporarily looting the government, the final loss will be a greater loss to the farmers than if there had been no program at all."

This Indianapolis speech and other representations of that period were the prelude to the basic Agricultural Act of 1938, which provided
the machinery for carrying a national grain reserve jointly in private and
government hands, facilitated by the non-recourse commodity loan device
and protected against over-accumulation by voluntary producer programs for
such land use adjustments as might be required.

This is not the place to recite further from history as to what then
followed subsequently through the rest of the 30's and up through the 50's
except to say that the grain supplies on hand at the beginning of World
War II, as a result of the ever-normal granary principle, went far to bridge
the gap until U.S. production itself could be brought to bear.

Grain Carry-Over Since World War II

It should also be added that the value of reserves for emergencies
was again underscored in the immediate post-war years when relief and
rehabilitation needs were substantial and U.S. stocks were relatively low.
From a total of a little more than 23 million tons in 1940, the feed grain
carry-in for 1947 was down to less than 8 million tons. Similarly the U.S.
wheat carry-in dropped from a high of nearly 631 million bushels in 1942
to less than 84 million bushels in 1947.

Fortunately, there were supplies in some non-combatant countries,
notably Argentina, which had backed up during the war for lack of shipping.
Later, there was extensive discussion internationally, particularly by
officials of the Food and Agriculture Organization, as to means by which a
system of buffer stocks might be established and jointly supported. Agree­
ment was never reached, however, as to a feasible means for financing
and administering such a system.

At the beginning of the 60's there was a lot of confusion and dis­
agreement over what American agriculture needed next, but there was one
thing just about everybody agreed on. The nation's granary was far too
full by almost any standard. In the absence of effective acreage controls,
the carry-in by 1961 had reached almost 85 million tons of feed grains
and more than 1.4 billion bushels of wheat. The government owned 85
percent of the wheat, about two-thirds of the corn and a substantial
share of other feed grains. Besides, a considerable part of the "free"
stocks of feed grains was under farm-stored government reseal.

Thus, the degree of underproduction that should be sought in the
first emergency feed grain program in 1961 was scarcely a major consideration.
It was rather generally assumed that such acreage diversion as farmers would
be willing to make voluntarily and for which there would be funds would not
reach the stocks reduction goal in one year anyway.
When producer response proved great enough in 1961 to effect a cut of nearly 13 million tons in feed grain stocks, despite record yields, the Department did begin the formulation of a minimum stocks level at which feed grain plantings might be relaxed enough to fully cover annual requirements.

It was about this time in late 1962 that Secretary Freeman indicated that this minimum for feed grains, based on preliminary staff work probably should be in the range of 45 to 50 million tons. In the months that followed, the problem was explored in greater detail, especially after the tension of the Cuban crisis in late October.

It is these more detailed recent studies that I shall now attempt to reflect in the balance of these remarks. I advisedly use the word "reflect" instead of "state" since Department staff people themselves have not concluded their recommendations on the reserve targets for each purpose and also since many of the figures have not been cleared by top government officials for public discussion.

As indicated at the outset, the question of a grain stocks level today necessarily must be related not only to yield variations but also to the possible contingencies of a nuclear attack and to variations in the needs of importing countries.

Defense reserve considerations today are greatly changed, even by World War II standards, from the days when emergency demands developed gradually and there might even be time to meet them through planting expansions in the following crop year.

**Carry-Over Needs Today**

Thus, any meaningful tabulation of carry-over needs today will have several headings: One for yield variations, of course; one for national disaster; one for strategic purposes to meet needs abroad and one for pipeline or working stocks.

As for the yield variation problem, this aspect has already extensively been treated here and will be further discussed in the next paper, by Mr. Upchurch. While his assignment is to relate yield variations to the task of farm program planning, stocks targets also must be related to such variations and it might be a helpful reminder to refer here to the swings in both wheat and feed grain average yields since 1901 through 1963.

In the five worst wheat years of that 63-year period yields were below average by about 4.3 bushels per acre. The corresponding short fall for other below-average years was 1.2 bushels per acre.
In the case of corn, the five poorest years for the same period were below average by 10.8 bushels per acre. The other below-average years were off by 2.1 bushels per acre. More or less comparable long-term variations were noted for the other feed grains -- grain sorghum, barley and oats. It is in the light of such data that the Department's current ideas on reserves are partly derived.

Computing a carry-over level for disaster or defense protection is very much a matter of judgment. As already indicated, I am not at liberty today to deal in specific levels, but I may say that Department staff people have been working on the assumption of having to supply for as long as 30 days somewhat more than the usual per-capita daily intake of wheat or wheat products, since availability of the usual foods may have been curtailed or terminated. In the case of feed grains, at least a 45-day supply has been under consideration.

As for a reserve against overseas contingencies, it is generally assumed that the principal areas or countries concerned would be in Southeast Asia and parts of Africa. As a kind of benchmark, one starts with the notation that annual wheat imports to 11 countries of that part of the world during the 1952-61 period had a high-to-low range of about 245 million bushels. It is not considered necessary, of course, to assume that all of the population of the aforementioned areas would need extra wheat at any given time.

Our staff people have approached the problem by first estimating what it would take to supply each 100 million persons with 3/4 pound of wheat per day for 3 months. The answer is slightly over 100 million bushels. Where it would be nine months until a new (and presumably larger) U.S. crop could be harvested, the reserve supply per capita would need to be three times larger.

So far, consideration of a separate stockpile of feed grain for strategic needs has been somewhat limited. It is recognized that wheat stored to meet an overseas emergency could, if necessary, also be used to preserve foundation livestock herds. Wheat is a dual purpose grain that fits in well as a two-way reserve.

Furthermore, quantities designated for one emergency purpose could be diverted to another purpose if necessary. The chance of a national situation coinciding with a big drop in yield and increased overseas needs is considered rather remote, but concurrence of two out of the three possibilities must not be ruled out.

Not much needs to be said about pipeline stocks, but estimations of a desirable carry-over stocks minimum should allow for a normal volume of grains in trade channels and normal merchandising positions. This volume
has been variously estimated at from 75 to 100 million bushels of wheat and
from 175 to 225 million bushels of corn.

To make a long story short, the opinion ventured by Secretary Freeman
in late 1962 that a feed grain reserve of about 45 million tons would be about
right still looks reasonable in the light of all the pencil work meantime.
Such a reserve would be equal to about one-third of our annual needs for all
purposes. If all of this reserve was in the form of corn, it would be about
1.6 billion bushels. Actually, our feed grain supply normally is about three-
fourths corn.

**Estimates of Proper Carry-Over**

Estimates of a proper national minimum wheat carry-over for all purposes
have usually ranged upward from 600 million bushels, the equivalent of about
our annual domestic use for human consumption and a little less than one-
half the normal total disappearance. The matter is reviewed periodically by
Department staff people. Some studies now indicate that a reserve approaching
700 million bushels could be justified.

You recognize, of course, that national reserves of unprocessed grain
are not the only elements in the national defense responsibility. A great deal
of work has also been done and is continuing with respect to the establishment
of a national processed or ready-to-eat food reserve.

It will be recalled that two years ago, the President submitted a
proposal to build a national food reserve through the use of existing USDA
funds. Committee spokesmen for the Congress countered by recommending
that a proposal be developed for operation under a separate appropriation.
This reflected a feeling, shared by most farmers, that the maintenance of
a national food reserve, including wheat and feed grains, is a general
public responsibility and should not be tagged as a regular USDA activity.

A bill authorizing a national food stockpile for domestic emergency
purposes has now been drawn up for consideration by the Congress, but
enactment in the current session is generally considered very unlikely.

Maintaining a national stockpile of the magnitude previously indicated
is not a small financial matter. Along with the initial investment in the
grains, there is the cost of annual storage and the handling charges incident
to freshening the stocks by rotation sell-out and replacement. The cost of
stocks rotation from government bins is a Department responsibility. In
warehouses, it is up to the warehouseman under the customary storage agree-
ment with the government to keep his total stocks so freshened that he could
make delivery at any time of the amount, grade and quality of the grain shown
on the warehouse receipts held by the government. At the moment, however, corn stocks in warehouses are from recent crops since most of the government-owned supplies in 1962-63 were bought by storing warehouses for current resale or loaded out to meet government sales commitments.

In this connection it might be mentioned that if stored initially in good condition both feed grains and wheat have been carried for at least 5 years without noticeable deterioration. In two tests with hogs at Purdue University and in one test with beef cattle at the Ohio Experiment Station, corn more than five years old from government bins compared favorably with new corn, even without Vitamin A supplementation.

Some part of the total national reserve, of course, will be carried by private individuals, such as grain dealers, feed manufacturers and farmers. However, when one considers the relatively low percentage of the crop that was carried over by the trade before federal farm programs came in, it is fairly clear that the government, one way or another, probably will have as much as two-thirds of the burden.

From the annual average corn (for grain) production of about 2,231 million bushels in the five-year (1925-29) period with a high and low difference of about 247 million bushels, the annual October 1 carry-out averaged a little less than 175 million bushels and non-farm holders accounted for less than 12 million bushels of this total. In proportion to the average annual wheat crop for the 1929-33 period, the average July 1 wheat carry-out was more than three times larger, and the private trade also held a much larger percentage of this total. However, even on a per capita basis it was only about 60 percent as large as the reserve not contemplated.

This past tendency of the trade to push on to the market for current use any grain above a certain stocks level is understandable. It costs money to carry grain and then there always is the chance of developments to cause a fall of the inventory value.

It is this tendency on the part of the private firms and individuals to use or otherwise dispose of even a large crop rather completely that leads to undesirable fluctuations in the production and price of both grains and livestock. Even if there were no defense considerations, a good case could be made for a fairly sizeable national grain reserve to ensure the American people a stable supply of farm products at reasonable prices and at all times.

Incidentally, as of April 30, the latest date for which I have figures, government feed grain stocks alone amounted approximately to 40 million tons, of which about 70 percent was stored in warehouses. Over 500 million bushels also were on farms under a continuing reseal price support loan agreement. As of April 30 the government also owned 813 million bushels of wheat, mostly all in warehouse storage. The amount of farm-stored wheat under continuing reseal was less than 60 million bushels.
Proper Location of Reserves

There are questions of location as well as quantity levels in connection with the national reserve problem. In general, and aside from the unpredictable logistics related to a nuclear attack, it is Department policy to keep government-owned grain or farmer-held grain under loan reseal as much as possible in the areas where produced. This minimizes the investment in transport of grain to areas where it may eventually prove not to be needed. A certain level of supplies is maintained at terminal points by in-shipments from the country to facilitate export sales and to meet large domestic commitments.

From a defense standpoint, however, location is determined somewhat differently. One school of thought holds that even under normal conditions it would be far more desirable in an emergency and good insurance to have a reserve of emergency food supplies, including wheat and feed grain stocks, available in non-vulnerable positions near the point of consumption than to have these stocks stored near the producing areas where they would have to be transported long distances to feed the consuming population.

This sounds reasonable in theory, but in many food-and-feed-deficit areas, the amount of space suitable for long storage is quite limited and in the case of food grains, processing capacity is also inadequate. Besides, it is difficult to decide which locations are really "non-vulnerable."

All I can tell you at the moment is that some progress has been made in this matter of food reserve location, but ideas are still somewhat fluid.

What we do take as no longer debatable is the fact that the federal defense responsibility for programs affecting the production, processing, storage and distribution of food, is that of the Department as delegated by the President. This is one responsibility which our national proclivity for abundant production does make easy. There is no nation on earth in which it is so easy really to arrange for an adequate carry-over of food in every form.
CONSIDERATION OF WEATHER IN FARM PROGRAM PLANNING

M. L. Upchurch

Since Cro-Magnon man took shelter from a storm in a cave near Dordogne, France, man has considered weather in his program planning. His consideration of weather in his planning grew when he learned how to invest time and effort in planting seeds so as to enhance his supply of food some months ahead. When man learned to invest at one point in time for his own benefit at a later point in time, he took one of the giant strides on the long road toward civilization. But the months between the act of planting and the enjoyment of harvest gave early man time to worry about the weather. He suffered with unfavorable weather and rejoiced with favorable weather. Thus, early man created rituals and prayers which he hoped would induce the favorable, and he devised charms and incantations which he hoped would avoid the unfavorable. He lived with forces beyond his control and he planned as best he could to avoid disaster.

We pride ourselves on having greater sophistication than our savage forefather, but, like him, we are still concerned with weather and our food supply. This seminar itself is evidence of our concern. Like primitive man we still live with a force largely beyond our control, but unlike primitive man, we have an immensely more complex setting for our concern regarding the effects of weather on our food supply.

When the first farmer's seed froze in the ground or his crops withered in drought, only he and his family went hungry. Today our failure to take weather properly into account may not have such drastic effects on us as individuals, but may have more far-reaching effects on the well-being of vastly greater numbers of people. The vagaries of weather can affect the price of groceries a thousand miles from the scene of a storm. More importantly for our purpose here, weather can affect the effectiveness of farm programs and their costs.

Agriculturalists are interested in weather for many different reasons. Farmers are interested in the production risk and the effect of bad weather on income. Agronomists are interested in the stresses plants must survive. Statisticians are interested in improving the accuracy of forecasts of crop production. Insurance firms are interested in the likelihood of losses. Conservationists are interested in erosion and stream flow problems. These are a few examples; there are many others. My interest in this paper is in the relationship of weather to total farm output and thus to the planning and operation of farm programs.

1/ Director, Farm Production Economics Division, Economic Research Service, U.S. Department of Agriculture. The author is indebted to Dr. Glen T. Barton and others in the Farm Production Economics Division for assistance on this paper.
We experience and observe problems in our agricultural industry; many of these grow out of the relationship between supply, demand and price of major farm commodities. We strive to design programs that will alleviate the observed problems. We seek authority and funds from the Congress to initiate and operate programs. In observing problems, in designing programs, in discussing the merits of programs in the Congress and in operating programs we make estimates and projections regarding the chief variables. A most important variable is volume of output.

If we underestimate future output under any given program situation, we find either that the program does not accomplish its intended goals or that it costs more than anticipated. If we overestimate future output, we risk possible shortages of a commodity and inefficiency in the use of public funds for program purposes. Thus, estimates and projections become crucial ingredients of program planning, enactment and operation.

Mark Twain observed that everyone talks about the weather but no one does anything about it. Under currently available technology we cannot do much about the weather in farm program planning, but it is imperative that we "consider" weather seriously in several respects. Before I discuss the ways in which we do, or should, consider weather, I should first give my interpretation of the terms "farm program planning" and "weather."

For purposes of farm program planning, I define weather in the "weather index" sense described by Messrs. Shaw and Durost in their paper for this seminar. Essentially, this regards weather as the net effect on production of variations in environmental factors which are neither under control of the individual farmer nor in constant supply over time.

Concern with weather in farm program planning presupposes something about farm programs. Here farm programs are defined to include actions of government that affect the supply and price of major farm commodities. Planning for such programs includes the projections and analyses which provide a basis for judgment regarding the effectiveness and efficiency of programs in achieving accepted goals. Such economic analyses include many facets. Chief among these are projections of: market demand, both domestic and foreign; production response, in terms of both crop acreages and yields; impacts on incomes to farmers and costs to consumers; and probable costs of programs to the government. Considering farm programs in the broadest context, the time horizon of our projections and economic analyses may range from one to two years to 20 or more years.

Of the major facets of farm program planning, weather considerations relate almost exclusively to production response. Relatively small errors in projecting production response, for whatever reason, can have substantial
economic consequences. Given the relatively inelastic demand for farm products of U.S. agriculture, relatively small changes in output produce major fluctuations in farm prices and income, or they produce wide variations in government costs of farm programs. Obviously, failure to take account accurately of weather can be a major source of error in projecting the volume of production that may be forthcoming under any given farm program.

Projecting Production Response

Economists generally agree that projections of production or of production response to programs is one of our most complex and difficult areas of analysis. Changes in technology and variations in weather are two of the chief factors contributing to this difficulty and complexity. Further complicating the analytical problem is the fact that technology and weather are not independent variables; they are becoming increasingly interdependent.

In considering the weather-technology interaction in production response, we ideally would like to become a combination of the proverbial bird that flies backward to see where he has been, and the seer with a telescopic crystal ball.

Our history of prediction has been none too bright since the advent of modern farm programs more than 30 years ago. I doubt that we have done much better than Joseph who foresaw the seven fat years and the seven lean years. Joseph, at least, was not confounded by a rapidly changing technology of production. Technical change has pushed our aggregate supply curve rapidly to the right. In program planning we have rather consistently underestimated the scope of the adjustments needed to achieve some semblance of balance between production and disappearance of farm commodities. By using output-increasing innovations farmers themselves have proved us wrong with great consistency.

For this and other reasons we failed for a long while to provide for programs big enough to achieve the adjustments needed in agricultural production. Furthermore, we found programs costing much more than anyone predicted because either the volume of commodities that came under price support was greater than estimated or the dampening effect of land retirement was less than estimated. The need for more reliable ways to project output of major commodities is obvious to all students of agricultural policy.

Since the two chief forces affecting trends in output are technological change and weather, we need to untangle, as best we can, the impacts of technological change and weather variation on production in past years. It is essential that we gain a clear picture of the time path of adoption of that bundle of farm practices we label technology. The latter step is a prerequisite to analysis of factors which influence the rate of adoption of technology and to projections of the effect of this controllable factor on farm production. As Shaw
and Durost emphasize, a weather index of some kind is required to achieve this goal.

Even if we could explain perfectly the past weather, technology and production relationships, weather would still plague us in projecting production response to farm programs in the future. The problem of forecasting weather effects into the future remains. Thus, the effects of weather limits accuracy in farm program planning. However, more adequate measurement of the effects of weather and technology in the historical sense would represent major progress. With it, we could project technology and its impact on production with more assurance. Also, knowledge of the range and probability of occurrence of weather and its effect on production in the past would provide the basis for developing features in a farm program that would minimize the economic impacts of unforeseen weather variation.

I have suggested ways in which we should like to consider weather in analyses of production response and in farm program planning. Let us consider these ways in which we have considered weather.

**Trends in Crop Yields**

In recent farm program planning, weather entered the analyses chiefly as it affects yields of crops. Even here, we have considered weather only in an indirect manner because of the lack of a comprehensive way to distinguish relative effects of the various forces affecting production. In practically all instances, our basic approach in program development has been to project yields of major crops by extrapolation of linear trends in yields since 1950.

Several modifications of this basic approach have been used:

(1) In cases where wide ranges in acreages of an individual crop were indicated under alternative program assumptions, the level of the projected yield has been adjusted to account for land selectivity and probably shifts in regional location of acreage.

(2) Similarly, the probable impact on production practices of farmers occasioned by wide ranges in price assumptions for an individual crop has been recognized and reflected in adjustments of projected yields.

(3) Also, specific knowledge of unique conditions regarding technology or weather conditions in recent years has been used to modify the linear trend in our final projections of yield. Grain sorghum is one example of the latter; here, knowledge of recent rapid adoption of hybrid seed provided the basis for moderating the sharp increase in yields indicated by the linear trend.
Over all, the net effect of these various adjustments to linear trends has been relatively small. For practical purposes, our technique is basically one of projecting linear trends fitted to the yield experience since 1950. Obviously, we would prefer to use more sophisticated approaches if proven technology and adequate data were available. Although, superficially, our technique may appear overly simple and too prone to major errors in projections, it has a substantial rationale. Several factors support this conclusion:

(1) Our analyses have been concerned chiefly with U.S. agriculture as a whole. Acreage of most of our major crops is widely distributed geographically. Widespread droughts and widespread good weather are rare. On a national basis we normally can expect compensating effects on yields of weather variations among regions in any given year. Casual inspection of the maps shown in the monthly "Crop Production" reports of the Statistical Reporting Service for the last several years illustrate this pointedly. The maps show that each year we have a mixture of areas ranging from "extreme drought" to "excellent" growing conditions.

As a nation we are fortunate that our agriculture is widely distributed and that all of our major commodities are grown over widely varying climatic zones. This provides much greater stability of production than many other countries enjoy. By contrast, most of Canada's wheat is grown in the Prairie provinces. Growing conditions are hazardous, so Canada's production of wheat varies from year to year tremendously more than ours.

(2) We grow crops over wide ranges of climate and weather; we also grow a wide variety of crops, many of which are substitutes for each other. Each crop responds differently to weather. Thus, there are compensating errors in projections of yields of crops which are close substitutes in total farm production. Projection of yields of the four feed grains -- corn, grain sorghum, oats and barley -- is a good example of this. (And for purposes of analysis of some alternative farm programs, wheat can be regarded as a "feed grain" and added to the list of close substitutes.)

(3) There is a growing body of evidence that rapid adoption of technology is the dominant factor in increasing yields. Available data on changes in farmers' use of fertilizer, improved seeds, pesticides and other key inputs of modern farming point in this direction. The supply equation reported by Shaw and Durost in their paper gives excellent statistical evidence of the dominance of inputs in explaining the rise in yields of corn in the Corn Belt.
Although more research is needed on the subject, it is clear that progress in technology has dampened the effect of adverse weather on crop yields. Good weather releases the potential of technology and good technology dampens the effect of bad weather. Farmers have more power and better machinery than formerly; so seedbed tillage, planting, cultivation and harvesting can be done in time to take better advantage of the season. Plant breeders have built better drought resistance and winter hardiness into our crops. Irrigation is more widespread and is done better than it used to be. Better forms of fertilizers and better techniques for using them are a part of the picture. These and other innovations tend to reduce the damage from adverse weather.

A considerable stock of unused technology exists as a basis for a further rise in crop yields over the intermediate period ahead. For example, Corn Belt farmers who cooperated in the variety tests described in the Shaw-Durost paper have attained yields of corn 25 to 35 bushels above the average reported for these states in recent years. Because of their willingness to cooperate in the variety tests, it seems logical to classify these farmers in the upper range of users of advanced production practices. Also, the cooperators are widely distributed geographically over the Corn Belt. Hence, comparison of their levels of corn yields with average attainment provides some measure of the existing stock of unused technology.

Closely related to the last factor is the mounting evidence of acceleration in the rate of adoption of improved technology by farmers. The continuing rapid shift to fewer, but larger commercial farms is undoubtedly upgrading the level of management in U.S. agriculture. This structural change in the type and size of farms that together constitute American agriculture also points to an increasing ability of farmers to acquire and use the inputs that increase production. Further, our widespread studies of profitable adjustments on representative farms show, almost universally, that it would pay the individual farmer to adopt improved practices to a greater extent. These studies show also that further adoption of improved practices would be profitable to the farmer under a relatively wide range of price-cost relationships. Given the increasing quality of management, improved ability to provide purchased inputs and the profit motive to the individual farmer, we can expect an acceleration in the rate of use of available technology in the years ahead.

For these six reasons, we feel reasonably comfortable in the reliance we have placed on extrapolation of linear trends as a basis for projecting yields in program planning. Actually we feel that our technique of projecting
crop yields on the basis of trends since 1950 are likely to understate rather than overstate future yield achievements. In the past, use of our technique of projecting yields would have grossly underestimated yield levels in subsequent years. A chart prepared in the Farm Production Economics Division graphically illustrates this. (See chart.)

Linear trends were fitted to the index of crop production per acre for each decade beginning with 1910-20 and ending with 1950-60. Extrapolation of trend lines for each decade since 1920 suggested future levels of yield which significantly undershot actual yields in the following years. The record levels of crop production per acre achieved in 1961-63 strongly suggest that extrapolation of the 1950-60 trend will repeat the historical error.

Concluding Observations

Of necessity, we have placed perhaps too much reliance on analysis of national aggregates in our farm program planning. Rather, our aim should be to determine the nature, causes, location and importance of current and emerging maladjustments in agricultural production and to evaluate alternative programs designed to prevent or to alleviate maladjustment. We need especially to give more attention to the complex relationships among crops and among regions. This would sharpen our understanding of the effect of alternative programs on the allocation of production and resources within and among farms and regions. The Farm Production Economics Division recognizes the basic importance of this type of research. A recent reorganization of our Division's research program provides for much greater emphasis in this area.

In developing our research and in making it more useful in farm program planning, I am sure that we will have to give greater "consideration" to weather than in the past. A more comprehensive system of weather indexes, covering at a minimum major crops and broad regions, will be a basic requirement for the research program we envision.

At the same time we need greater emphasis on measurement of the impact of technology and other nonweather factors on future yields, perhaps even more than we need improvements in our ability to measure the impacts of weather. But until we have a firmer basis in research and improved and more comprehensive data, our present methods of considering weather in farm program planning will have to suffice.
CROP PRODUCTION PER ACRE*
Past and Prospective Trends

% OF 1947-49

TREND
'50-60
'40-50
'30-40
'10-20
'20-30

1910 '20 '30 '40 '50 '60 '70

*Crop land used for crops

U.S. DEPARTMENT OF AGRICULTURE

NEG. ERS 1239-62(7) ECONOMIC RESEARCH SERVICE
A great deal is known about the food situation in the advanced
countries of North America, Europe and Oceania. In these countries food
supplies are generally ample for healthy and satisfactory diets for all,
and only lack of income and buying power limit a small proportion of the
population to poor and inadequate diets. Not only is total food consumption
by quantity and composition quite accurately known here, but differences in
food consumption between different groups in the population have been
intensively studied.

Facts about the situation in the less-developed continents and
regions are less exact and far less complete. Two series of data are
available. The Food and Agriculture Organization has compiled food
consumption statistics based on "food balance sheets" for each country.
These arrive at average consumption per capita by setting up balance sheets
which take into account all factors -- production, farm use for seed and
feed, industrial use for non-food purposes, stock changes, exports and
imports, etc., and thus arrive at net amounts for human consumption.
Their production data are based on official national censuses and other
official figures. But in several regions such complete and authoritative
data are available for relatively few countries; so there are great gaps.
Detailed food consumption studies by different classes of the population
are also relatively few in less-developed countries.

A second set of world-wide food consumption data is available
from the studies and publications of the U.S. Department of Agriculture.
These are based on reports from the agricultural attaches of the Foreign
Agricultural Service, who use not only official data but all other sources
of information, and who supplement the published data by surveys and
estimates of their own. Where there are no U.S. agricultural attaches,
Agency for International Development technicians make the reports. This
U.S. service provides estimates of production and consumption for all
major countries and many minor ones, and thus provides a far more complete
coverage in less-developed regions than do the FAO data.

Chief, United Nations Division, Agency for International Development.
The major facts on the world food situation, however, are so striking that they appear the same whether studied from the FAO data or the U.S. data. The facts are broadly these:

1. Average levels of food supply and the adequacy of average diets have improved greatly in the more highly developed regions of the world since the end of World War II, and now are quite satisfactory both on the average and for all sectors of the population except the very poor.

2. Average levels of food supply per capita in most less-developed regions recovered most or all of the war-time losses, and increased fairly steadily though slowly until the middle or end of the decade of the 50's.

3. Since the late 50's, there has been a significant turn for the worse in less-developed countries with population levels recently increasing faster than the food supply levels. This threatening development is due to a general downward trend in death rates with as yet no corresponding reduction in birth rates, and a resultant rapid speeding up of population growth in all less-developed regions.

4. Production of non-food products -- largely fibers, wood, etc. -- has increased somewhat more rapidly than food products in less-developed regions, providing somewhat more buying power for food imports.

5. Supplies of food under concessional terms, mainly from the U.S. Food for Peace Program, have helped consumption levels somewhat. But even so, these now add only about 2 percent to food supplies for the less-developed regions.

6. Possible solutions to this problem include not only continued and intensified efforts to modernize and increase food production in less-developed countries and to check the upward surge in population growth rates, but also efforts to further speed up industrial and other non-farm progress so that less-developed regions could increasingly afford to import commercially more of their food from other regions with more ample land resources, particularly from the U.S. and Canada.

7. Help from the more advanced to the less-developed countries, to aid the improvement and more rapid development of both agriculture and industry, will continue to be needed. The help given them by the U.S. through its A.I.D. program is being increasingly reinforced by expanded economic assistance activities from other developed countries. Both will be needed and on an enlarging scale, for a considerable time ahead.
Food Production Relative to Population

The Second World War gravely disturbed food production and trade, and created or accentuated famine conditions in many parts of the world, developed as well as less-developed. The first formal international effort of the Allies was the creation of UNRRA -- the United Nations Relief and Rehabilitation Administration -- even before the war was won. UNRRA had a major hand both in relieving starvation and starting toward rebuilding war-devastated economies in the allied countries; and Military Government performed similar services for the vanquished countries with some notable successes, such as the Japanese land reform.

The war left the world with food production per capita reduced one-seventh below pre-war averages outside the Soviet Union, and probably far greater reductions there. The first rehabilitation efforts of national governments, UNRRA, and the newly-created International Food and Agriculture Organization were directed toward re-establishing food production. By 1950, food supplies per capita were back up to or in excess of pre-war production in most parts of the world, with the exception of some of the worst-devastated areas, especially the Axis countries, China and India.

Even so, 1950 food supplies were still far below minimum standards for health and efficiency in many less-developed countries, both in calories and in protein, and ambitious plans were developed by FAO member countries in 1953 for intensified efforts to raise world production. These called for increases by 1956-1957 of 6 percent for food crops and 11 1/2 percent for livestock products, above the levels in 1952-1953, with the steepest rate of increase in the less-developed regions where food supplies were furthest below minimum standards. Food production per capita did increase for the world as a whole over this middle period of the 50's even more than had been projected. The Far East ran substantially ahead of its goal; the Near East and Africa just about equalled theirs, but Latin America fell substantially behind its goal.

For the world as a whole, food production per capita regained the pre-war average by the beginning of the 50's, and reached a peak about 1/8th above pre-war by the end of the 60's, and then sagged

---

slightly thereafter. But the progress was not uniform throughout the world. Right through 1963, the well-to-do region of Western Europe continued to increase its output faster than population growth, reaching an average food production per capita 25 percent above its pre-war level. The Soviet Union continued to push its output to new levels of adequacy and in 1962 reached levels substantially above the pre-war average. Other eastern European countries also showed material increases. In 1963, there was a sharp set-back in this region, with a drop of 5 percent in Soviet food supplies per capita and three percent in other Eastern European countries from the preceding year.

The most ominous recent development is that the less-developed regions of the world -- Asia, Africa and Latin America -- failed to maintain their upward trends in food output per capita after the latter part of the 1950 decade. Latin America reached its peak in 1958 at just up to pre-war per capita food output, then dropped continuously to 12 percent below pre-war by 1962 and still further in 1963. The Far East (excluding Mainland China) reached its peak in 1960 -- 1961, at 96 percent of pre-war, and declined slightly in 1962, and slightly more in 1963. Africa increased to a level about one tenth above pre-war by 1953, varied slightly above and below that level to 1960, but since then has dropped markedly, to a level in 1963 of only about 5 percent above pre-war. Of the less-developed regions, only the Near East shows reasonably steady progress, which reached by 1959 a per capita level 13 percent above the pre-war level and remained at or near that level since.

In the Far East, the situation is even more discouraging, with only five countries out of 15 showing per capita food production in 1962-1963 above what it was a decade earlier, and only four out of 14 higher than the pre-war per capita average. In Africa, on the contrary, 18 countries

6/All Southern Hemisphere figures are for crop years; i.e., 1962 means 1962-1963, etc.
out of 29 show food production in the latter period higher than a decade earlier, with Africa south of the equator showing an average gain of 6 percent over the decade.9

Both overall and by regions, food production is generally expanding around the world. But in most regions the rate of expansion has slowed down in recent years, both in contrast with the rapid rate of improvement in the decade of the 1940's and also in contrast with the slower rate of expansion in the early 1950's. In the Far East and Latin America, where three-quarters of the population of the less-developed countries of the non-Communist world live, food production has generally failed to keep pace with population growth since the late 1950's. This general situation can be stated another way. In the less-developed world, recovery and expansion in food production got off to a rapid start after World War II, slowed down somewhat in the first half of the decade of the 50's, and slowed down even more in the subsequent period. But population growth in the less-developed areas has increased at a growing rate in the less-developed countries since World War II, as shown in the appendix table.

Population growth rates in Europe continued below one percent per year over the period from 1945 to 1959, declining slightly during this period; in Eastern Europe they rose to 1.3 percent per year, and in the U.S. and Canada stayed under 2 percent. In Latin America, however, the already high natural growth rates increased through the period, reaching in the latter half of the 50's new high levels of 3.2 percent per year in Central America and 2.5 percent per year in South America. In Africa and Asia, the net rates of increase also gained sharply, though still remaining at moderate levels comparable with those in North America. The efforts to help less-developed countries conquer disease and reduce deaths due to infectious diseases carried on both by the World Health Organization (WHO) and by AID and other bilateral assistance, have proved far more immediately effective than the efforts to improve agricultural production and output carried on by FAO and bilateral aid programs. Perhaps this is because we cannot inject new knowledge and methods into men with a hypodermic needle at the rate of hundreds per day!

9/"The 1964 Africa and West Asia Agricultural Situation," Supplement No. 5, to the 1964 World Agricultural Situation, p. 18, Economic Research Service, USDA, March 1964. (The dates cited here are for per capita total agricultural production rather than food production; the latter data were not shown.)
Some Latin American countries made material strides in food production, such as Mexico with a steady upward trend reaching 40 percent above pre-war levels by 1959-1960, and a sharp further increase indicated in 1963. Ecuador and Venezuela also show a substantial and continuing increase, and even Brazil shows a substantial increase in the last five years above the average for the early 1960's. But even with these increases included, Latin American food production as a whole, fell further and further behind population growth from 1958 to 1963, with a net decline of 7 percent in food per capita over that period.\(^{10}\)

Some of the poorest-fed countries did show substantial improvement in nutritive content of the average diet during the decade of the 50's -- India, 10 percent in calories, and about 8 percent in protein (but all in plant protein); Japan, about the same in calories, but marked increases in plant and animal protein; and some gains in Peru, Venezuela and the Philippines. Pakistan, at just below 2,060 calories, hardly held its own in calories, and slipped slightly in protein.

The average pre-war daily energy intake in all these countries, of 1,700 to 2,200 calories a day would be regarded as near-starvation levels in more advanced countries. Even the slightly improved levels were obtained only with the help of substantial PL-480 food imports from the U.S.A., donated or lent under our Food for Peace Program.

**Composition of the Diet in Less-Developed Countries**

Besides the low level of total energy (calories) in the diet in most less-developed countries, the quality of the diet is very poor. This is especially marked in the low proportion of proteins as a whole and particularly in the proportion of proteins derived from livestock products. While North America obtains 30 percent of its total energy from livestock products and all developed regions as a whole obtain 20 percent, the less-developed regions obtain only 5 percent of their energy from livestock products. As a result, most underdeveloped regions are deficient both in the total amount and the composition of their daily protein intake as compared to minimum nutrition standards for health and efficiency, and half the world's population lives in countries with such protein deficiencies.\(^{11}\)

\(^{10}\)/Food production in Cuba has dropped sharply and progressively since 1961. In 1963 it was only about two-thirds of the pre-war average.

This has influenced the Latin American averages as a whole.

International Efforts to Assist Economic Development

The increases in food production after the war reflect a very determined international effort both to reconstruct the war damages and to help the less-developed countries advance to better levels of production and living. The first phase -- the reconstruction of Europe -- was most effective. Europe, though seriously ravaged by the war, still had much of the apparatus of modern civilization, though badly damaged and deteriorated; even more important, it still had a population of educated, modern people, skilled in all that it takes for modern society. With generous help from the U.S.A. under the Marshall Plan and with American industry helping Europe rebuild its industries on modern lines and with the stimulus of its own progress under new European programs including the Monet Plan for France and the Common Market for much of the Continent, Central and Western Europe has forged ahead to new levels of efficiency and output in industry and agriculture and to new standards of living for its people, standards which begin to approach those of North America.

At the same time, Europe has developed a dynamism and adventurous spirit in both industry and government which are worlds apart from the flat feeling of stagnation and despair which characterized so much of the inter-war decades from 1920 to 1940.

As Europe gained new wealth and strength, European countries helped nearly all of their earlier colonies to emerge as free and independent states, and began to share with the U.S. more and more of the burden of helping the underdeveloped regions of the world. They have been joined by Australia and New England, and more recently, by Japan as aid donars.

By 1956, when the U.S. contributed two billion dollars to the economic development of the rest of the world through its development assistance program, other countries contributed only a little over one billion and much of that went to their colonies and ex-colonies. By 1960, U.S. contributions in grants and loans had grown to 3.9 billions, but contributions by other (including now substantial amounts from Japan) had grown to 1.8 billions. For 1962, the U.S. contributed 4.7, while other nations contributed 2.4 billion dollars to the less-developed world. Private capital flows varied between 2.5 and 3.5 billions of dollars of dollars during this same period with a slight downward trend.

---

Besides the directly financed assistance and the technical help extended through the bilateral aid programs of the U.S., the U.K., France, Germany, and many other countries, the advanced countries have paid the major part of the cost of establishing and financing the new United Nations institutions of all sorts -- not merely to keep the peace and to aid industry, agricultural, health and labor sectors, but also to help finance the economic development through the International Bank Group and to meet and ease international monetary emergencies and balance of payments difficulties through the International Monetary Fund.

Some idea of the magnitude of these activities may be given as follows:

Besides the heavy financial assistance to aid the less-advanced countries to speed their economic development, the U.S. Government has been maintaining a staff of roughly 5,000 experts in less-developed countries to provide them direct technical assistance, guidance, education and training, and on helping prepare national development plans. The United Nations and the specialized agencies of the U.N. system are supplying and supervising about the same number of international technical assistance experts, recruited from all the developed countries and some less-developed ones. Other bilateral aid programs -- those of the British, French, Belgian, Dutch, Swiss, Scandinavian countries, and many others -- are supplying experts where needed. Some of these, the British and French at least, on a scale as large or larger than the U.S.

Altogether, there are probably about 25,000 technical assistance experts at work in less-developed countries round the world, supplied by national governments, directly or through international agencies. Others are recruited and contributed by private foundations, religious groups and other sources from the advanced countries. In comparison, there are approximately 6,600 agricultural extension agents at work in the U.S. today and approximately 4,100 women home agents. Despite the great world-wide effort, the expert advice and assistance to the two and one-quarter billion people of the less-developed world is thus far less intense than it is to our five million American farmers.

Relation of U.S. Food Donations to Levels of Food Consumption

The data quoted on supplies of food available in less-developed countries include the food supplied by our Food for Peace Program as well as the much smaller donations from other countries made directly or through their cooperation in the new experimental international World Food Program, operated jointly by FAO and the U.N.
The U.S. food utilization activities, both direct and through the World Food Program, represent roughly 3 percent of total grain supplies in the less-developed countries and about the same proportion of their supplies of milk products. Since these supplies were not distributed uniformly through all less-developed countries, they represent a much larger proportion of total food supplies in some of the recipient countries. While small relative to total supplies, their concentration in areas of greatest need or suffering from exceptional crop failure made a substantial contribution to the prevention of starvation and acute malnutrition and to their maintenance of at least minimum food standards. At the same time where the food was sold locally for cash which was mostly turned over to the local government as grants or loans for economic development, the process also enabled the government to put more of its population to work on economic activities than it could otherwise have done.

Relative Place of Food and Agriculture in Future Economic Development

In the past of all presently highly developed countries, the proportion of the population working on the land declined as cities grew and developed, and as more and more of the population shifted to work in manufacturing, transportation and commerce as well as in all the now highly developed skills and professions.

Early in our own country's history it took nine families on the land to raise the food for ten families in all. Today, with the aid of many industrial products produced by non-farm industry, it takes less than one family on the land to produce the food for 10 families off the land, with a lot left over for commercial and non-commercial export. And the development happened here and in Canada even while plenty of good land still was not under cultivation.

Except in some parts of South America, the pressure of population on the land is very heavy in most less-developed countries, and the farms are so small -- less than four acres of cultivated land for each farm worker -- that even with a great increase in per-acre productivity they would never be able to raise enough food to feed their population at even a modest standard of dietary adequacy. With the general high rate of population increase, crowding on the land is getting progressively worse in most less-developed countries, even with strong efforts to raise their cultivated area by great new irrigation and land reclamation projects.

The only real way out is to increase their nonfarm and industrial activity and employment far more rapidly than they have heretofore, so that they can put workers not needed on the land into effective production elsewhere. Eventually, they can even begin to reduce the number of people on the land, so that they, too, can begin to substitute machines for men and can raise their per capita productivity on the land as well as in the cities beyond what can be obtained alone by higher yields per acre.

This long-range possibility ties back into the previous discussion of the contribution which PL 480 supplies are now making to their food supply. As less-developed countries begin to develop profitable markets for some of their new industrial exports they will be able gradually to begin importing commercially more of the food products they now receive as donations to meet emergencies, or as long-term local-currency loans to aid economic development. And over the longer period, if this process goes still further, their commercial demands for food to supplement their diets might provide a basis for us to give up all crop restriction efforts and to develop the food productive possibilities of our continent to the utmost, to produce all the food which the less-developed countries need so badly and for which they would then be able to pay.

Before this time could come, though, there are many difficulties to be overcome. The rate of economic progress in the less-developed world is slow as compared to the rate at which their population is growing. Most advanced countries, notably our own, are becoming weary of the continuing needs of foreign relief and development. Moreover, there is a scant supply of skilled men and of financial resources in most of these countries to tackle their problems. There is also the difficulty that we are trying to help the less-developed countries to achieve, in a single generation, a modernization and transformation greater than we or other advanced countries made in a full century.

This is the challenge we face for the future. But when we look at the vast sums now being spent on defense and on adventures in space, we can hope for great reductions at least in defense costs as the world continues its return to sanity. It does not seem impossible to hope that we and other advanced countries should be willing to devote a significant part of these savings to expanding our help to the development of the poorer countries of the world. We can change our export donations from spears into ploughshares. If we do shift in these direction, we can move forward together toward a more prosperous, more peaceful and less-hungry world.
## Appendix

Average reported natural population increase rates, by region, per 1,000 populates\(^{14}\)

<table>
<thead>
<tr>
<th>Country</th>
<th>1945-49</th>
<th>1950-54</th>
<th>1955-59</th>
</tr>
</thead>
<tbody>
<tr>
<td>United States</td>
<td>13.4</td>
<td>15.0</td>
<td>15.2</td>
</tr>
<tr>
<td>Canada</td>
<td>17.6</td>
<td>19.1</td>
<td>19.8</td>
</tr>
<tr>
<td>Western &amp; Central Europe</td>
<td>8.4</td>
<td>7.7</td>
<td>7.7</td>
</tr>
<tr>
<td>Eastern Europe</td>
<td>9.8</td>
<td>13.8</td>
<td>13.2</td>
</tr>
<tr>
<td>Central America</td>
<td>27.2</td>
<td>30.8</td>
<td>32.2</td>
</tr>
<tr>
<td>South America</td>
<td>21.3</td>
<td>23.6</td>
<td>24.7</td>
</tr>
<tr>
<td>Africa</td>
<td>15.6</td>
<td>17.5</td>
<td>19.6</td>
</tr>
<tr>
<td>Asia</td>
<td>10.8</td>
<td>12.9</td>
<td>13.4</td>
</tr>
</tbody>
</table>

\(^{14}\)Data are simple averages of countries reporting in regions stated, omitting small countries and territories, except in Asia, where weighted average computed.


(average regional population growth rates, reported by Lester Brown, loc. cit., p. 9 are substantially higher in Latin America and in Asia.)
I'm sorry that I wasn't able to come to some of the sessions myself. It's a little difficult to say anything very intelligent when one does come in cold toward the tail end of the conference. Also, I think it is particularly difficult in an interdisciplinary conference for a person who is steeped in one particular discipline to make any kind of brilliant comment or wise crack that won't be strictly "old hat" to members of another discipline.

I would like to mention one thing that I came upon in my reading just a few days ago. This was a statement by the inventor of the barometer, Toricelli, to the effect that "man lives submerged at the bottom of an ocean of air." Of course, his crops and livestock are also living submerged at the bottom of this ocean of air. There is no escaping the importance of the weather!

I doubt that anyone during the conference presentations up to this point has paid much attention to a man named Henry Ludwell Moore. I would like to read you three or four short quotes from two books which were published by Moore in 1914 and 1917. One of these is on the forecasting problem -- crop forecasting or yield forecasting, if you will. If there are USDA people in the audience, especially Crop Reporting Service people, let me say that this is not a current criticism but was made in 1917. Henry L. Moore said then that on the basis of some regression analyses he had made "it is possible for any person from the current reports of the Weather Bureau as to rainfall and temperature in the states of the Cotton Belt to forecast the yield of cotton with a greater degree of accuracy than the forecasts of the Department of Agriculture," and also "from the prospective magnitude of the crop to forecast the probable price per pound of cotton with a greater precision than the

1/Professor and Head, Department of Economics and Sociology, Iowa State University.
2/Farmer, Waukee, Iowa.
3/Professor and Head, Department of Economics, University of Illinois.
4/Professor, Department of Economics and Sociology, Iowa State University.
5/Editor of the Editorial Pages, Des Moines Register and Tribune.
Department of Agriculture forecasts the yield of the crop." Now I'm sure this endeared Henry L. Moore to the Crop Reporting Board! But he did start some people thinking and researching and, I suppose in some cases, rationalizing on problems of both weather and economic forecasting.

I note that the theme of periodicities appears at least in the title of the conference and I would like to read you a quotation from Lord Kelvin, 1876, out of a footnote from Moore's 1914 book. Lord Kelvin is being asked by another member of the Royal Society of England, "Suppose you make observations on weather during the sun spot cycle -- 11 years let's say -- now can you analyze these 11 years of observations and regard the job as done once and for all -- or will you then want to go on and analyze 22 years, 33 years and so on." Lord Kelvin's reply was: "I cannot say whether anything with reference to terrestrial meteorology is done once and for all. I think probably the work will never be done," So from the employment standpoint, I think we're safe, either as meteorologists or economists!

There are about three more short passages I would like to mention here. The title of Moore's 1914 book is Economic Cycles -- Their Law and Cause. Moore investigated yields of four crops in Illinois and weather records from about 1860, and came out with this kind of summary: "The investigation of these four crops taken together leaves the general conclusions: (a) That there is a rhythmical movement both in the yield of crops and in the rainfall of the critical (growing) periods which is summarized in the compound cycle in which the constituent elements are a ground swell of 33 years, and its semi-harmonic and a short superimposed cycle of eight years with its semi-harmonic; (b) that the cyclical movement in the weather conditions represented by rainfall is the fundamental persistent cause of the cycles of the crops."

Just one more quotation along this line and I'll turn the rostrum over to Harold Halcrow. You see, Henry Moore was primarily an economist. I think he had been influenced at Johns Hopkins University by an astronomer -- a man named Simon Newcomb -- who introduced him to periodogram analysis of time series and other techniques, but Henry L. Moore was really an economist. And this is where he came out toward the end of his book: "The links in the sequence of causation were completely established. The fundamental persistent cause of the cycles in the yield of the crops is the cyclical movement in the weather conditions represented by the rhythmically changing amount of rainfall. The cyclical movement in the yield of the crops is the fundamental persistent cause of economic cycles" (and by this he meant business cycles).

6/Hay, corn and a couple of others.
I am simply saying here that over 50 years ago there was at least one man who was vitally interested in the interconnections between meteorology, crop yields in agriculture and fluctuations in the rest of the economy. Now some of his fellow economists had a higher opinion of his meteorology than of his economics, but that is another story!

Harold Halcrow has been to, I think, all of the conference sessions here. Harold Halcrow, most of you know, is head of the Agricultural Economics Department of the University of Illinois. I am going to ask Harold to make the opening statement for the panel.

Harold Halcrow

This conference has added to knowledge which should be useful to economists. Those who are engaged in economic studies and analyses may have gained additional insights into effects of weather on agricultural production and output. Some of the information presented should be useful as background for additional economic studies of agricultural productivity and policy. We are more aware of the limits of weather forecasting and of the range in production response of agriculture to weather. As information becomes more adequate for estimation of weather cycles, trends and related output responses, we can be more realistic in assumptions concerning national policies and programs. I feel, although I cannot be certain, that the years since 1958 combined with the poor crop years of the 1930's are distorting our picture of agricultural production potentials even though we must admit that our potential has greatly increased and is continuing to increase as new technologies and new methods are built into agricultural production systems.

The conference has brought together new information and I hope that it will be followed with additional work on the effect of weather and related policy problems. My interest in the relation of weather to crop production was developed while doing graduate work at the University of Chicago. My Ph.D. thesis title was "The Theory of Crop Insurance" which dealt with the problem of weather and yield variation.

Dr. Fox: Thank you, Harold. The next panelist to speak will be Lauren Soth, Editor of the Editorial Pages of the Des Moines Register. Lauren, you were living out here in the Iowa weather way back in the 1930's, weren't you? You take over from here.
Since Harold Halcrow has brought up the subject of crop insurance, perhaps I should tell you that when President Roosevelt appointed a committee on crop insurance back in 1936, I was a flunky on the committee. So I share a little responsibility with Dr. Halcrow for it. The fact that they haven't followed Halcrow's thesis, though, I had nothing to do with that. I don't have any qualifications for this panel, but I can contribute to this ocean of air the chairman says we're living in.

I have lived and been reasonably awake for the last 30 years, and it has always impressed me the tremendous impact -- we're suppose to be talking about agricultural policy and the weather -- it has always impressed me the tremendous impact that the 1934-36 period has had on agricultural policy in this country in several different ways.

This was a period of such drastic weather effects that we had (I looked up the figures) just about half of normal production of grain in those two years. In 1934 we raised 1.1 billion bushels of corn and half a billion bushels of wheat; in 1936, 1.3 billion bushels of corn and 600 million bushels of wheat. In the years preceding that we had been harvesting 2.5, 2.8 billion bushels of corn and around 800 or 900 million bushels of wheat.

The shortfall of production was a shock, and it came to us just at a time when we were all worried about surpluses. The farm organizations at that time had more or less "got together," after a long period of debate in the '20s, that production control was necessary. And here came this period of '34 and '36 when our production was cut in half on these two important grains.

Art Thompson mentioned this morning that a lot of people felt this was a divine visitation on us for fooling around with "natural laws" of economics. I think it has had an impact ever since. People opposing production control in agriculture always hark back to this period and ask, "What would happen if we had a '34 - '36?"

In the '30s, you'll remember, this curbed our application of crop controls. It influenced the Agricultural Act of 1938. The Farm Bureau at that time was very strong for production control and for marketing control. The Farm Bureau said that this '34 - '36 thing was once in a century and would never happen again and we had to get control of production and get parity prices for the farmer. But the fear of shortage -- I'm oversimplifying, of course -- as a result of that recent experience with the dust storms and the chinch bugs was such that it had more political power than the desire of the Farm Bureau and other people to control production.
As another effect of that '34 - '36 period we got into crop insurance. Bill Rowe of the BAE had done some studies on crop insurance which became useful after the drought years. President Roosevelt appointed the crop insurance committee with Roy Green as chairman. We made a report; Congress passed the legislation. There were heavy losses in that program from 1938 on. Congress wanted to call a halt in 1944, but FDR had enough authority to change Congressional minds and wound up getting a larger program than before.

Another indirect weather impact on our policy, I think, was the World War II period. We had good weather and big crops, but we still used up the surpluses acquired during the late '30s. And the use of the reserves, along with the drought experience, was cited by people to argue that production control was dangerous as well as morally wrong. Look here, they said, in 10 years we had two severe droughts and are in great danger of being short of food. And then we had a World War in which we needed to call upon all our reserves and we didn't have much. These experiences added a lot of emphasis to the arguments that production control was unwise.

Now, according to Louis Thompson and others, we've had abnormally good weather for a period of years in the '50s and early '60s and this is the reason for the surpluses. I would make the point that this analysis again encourages wishful thinking that we don't need to worry about production control in agriculture. If I can oversimplify, I'd say the theory is that God will save us, because He will produce poorer weather later on and we won't have too much.

So my conclusion from the record of the last 30 years is that no matter what happens to the weather or to the demand for farm products it can be contrived into an argument against production control.

Dr. Fox: Everything seems to turn out to be an argument against production control. Don Kaldor will be the next panelist to speak. Don Kaldor is a Professor of Economics here at Iowa State.

Don Kaldor

I'd like to make just a few brief comments about two unresolved issues which I think have been tossed around here and which have some real relevance to agricultural policy. There are others, but these are two rather important ones, I believe. There is still a good deal of uncertainty and lack of knowledge about the nature of the relations between weather variables and farm output. As Louis Upchurch has
pointed out, these relationships are relevant to predictions for short-run planning of farm programs including production control, and for assessing and evaluating the size of the long-run resource adjustment problem facing the farm industry.

The point I want to make is that this problem of the nature of the relations between weather variables and farm output is part of a larger problem on which we need a great deal more work. This larger problem involves the explanation of past changes in farm output and our ability to predict these in the future. Economists have generally assumed that technology and weather are exogenous variables. In other words, they are things that occur outside of the economist's model and he plugs them in (whatever the values are) and goes on from there. I think this may have been justified years ago, particularly before World War I when much of the increase in agricultural output appears to have been the result of increases in what we normally measure as land input, capital input and labor input.

But since World War I and in particular in the last 20 years practically all or at least the very large part of the increase in output has apparently been associated with things we call technology, and probably weather. Thus the economist, in using his traditional models, even when he attempts to throw time in as a variable, is simply covering up (at least recognizing) that there is something here but he really doesn't know what. We have reached the point where so much of the change in our output is no longer explained or explainable by our typical economical models. The upshot of this to me is that we need more interdisciplinary work in which the economist, agronomist, climatologist and others join forces and try to develop better, more complete models for predicting, not only past changes in output, but future ones as well.

The other issue which has some implications to agricultural policy involves the instability problem in farm prices and incomes and the source of this instability. I think economists generally recognize weather as an important source of this instability, but there is still a good deal of question as to whether the year-to-year weather effects are of the essence of a random variable or whether they have periodicity patterns that are predictable. I gather from what I heard in the sessions that this is not a resolved issue among meteorologists, climatologists and agronomists. I think it's again another area of work in which we need to spend some effort.

Dr. Fox: Now finally we'll ask Bob Buck of Waukee, Iowa and Washington, D. C. to speak. When I say Washington, D. C., I am thinking of the various national advisory committees Bob has been on over the years. He has been very deeply involved in national agricultural policy in addition to actually making a living as a farmer and facing up to the effects of weather on his own operations. Bob, I'd like to have your comments.
Robert Buck

People claim a farmer is never satisfied about the weather or anything else. I know about a farmer who is supposed to have complained, when he had a severe drought and raised practically nothing, that it was a tragedy. The next year when he had a bumper crop, he said "This is terrible because it takes so much out of the soil."

I'd like to mention that as one Iowa farmer I'm real pleased to see the Center for Agricultural and Economic Development sponsor this kind of conference. I think you, Dr. Fox, and Louis Thompson and the others here deserve the thanks of Iowa farmers for bringing scientists together from different disciplines to work on a common problem. One reaction I have as a farmer is that you generally tend to be in your own little cubby holes and it is real encouraging to see you talking over problems from different angles.

My memory as to what has been going on in the past 10 or 15 years on weather and technology is about this: In the late '50s our feed grain production began to get out of hand. Stocks built up. Increasing pressure developed to do something about it -- to get some kind of feed grain program. The USDA, under Mr. Benson, tended to regard this as just a temporary thing due to unusually good weather. About that time there was a good deal of work going on by scientists taking a look at technology. Several journal articles referred to the explosion in technology and the technological revolution. Beginning in 1960 the USDA, under Mr. Freeman, tended to ascribe most of the increase in feed grain production to technology. Using a straight line trend of feed grain yields from the 1940's through 1960 as reflecting mostly improved technology, there was a tendency to regard this as an "explosion" in technology requiring rather drastic control measures. At this conference we're looking at both weather and technology. The debate is -- how much of a factor in our increasing production is weather and how much is technology?

I assume that we're in an early stage of studying weather, and I think it is just about as early in the study of the influence of technology. I doubt if the study of weather and weather forecasting is any less sophisticated than is the study of technology. I would like to see economists regard technology as an input that is part public and part private. Whichever the source, the result is an over-rapid investment of capital in agriculture. That isn't the burden of our conference on weather, but my point is that these are two areas in an early stage of development and I hope both will go forward.
No farmer can be growing corn in Iowa and not be impressed with the importance of weather, especially of July and August temperature and rainfall. My own experience has been that I have had practically the same level of technology and have had a corn yield difference of 20 to 30 bushels to the acre depending on whether or not I got rainfall at the right time in the summer. It may be that other parts of the country, particularly in Illinois, are not so dependent on summer rainfall as we are here. I'm also impressed with the crucial importance of technology in our increasing yields. We're now beginning to understand the role that hybrid corn has played, and my guess is that in a few years we'll be doing the same thing with nitrogen.

General Comments with Audience Participation

Robert Dale: I'll start this out by just pointing out that Mr. Buck assumed that Illinois might have a little bit more chance for summer rainfall than Iowa. One of the findings of the NC26 committee has been that the probability of getting sufficient summer rainfall is just about the same all over the Corn Belt. Where we do have a difference is in the winter. Illinois, Indiana, Ohio can plan to start with a full soil moisture profile in the spring in nearly all years but we cannot always plan to here in Iowa. And the same thing holds true in Nebraska. The entire Corn Belt is subject to about the same probability in summer rainfall as Iowa.

Harry Trelogan: (Statistical Reporting Service, U.S. Department of Agriculture) Contrary to your assumption, Karl, we are very happy that you quoted from H. L. Moore giving his comparisons with the USDA. It simply demonstrates that down through the generations through Moore and Bean and now Thompson, the comparison of excellence has always been against the performance standard of USDA predictions.

Karl Fox: Well said. I was very careful to relate my criticisms, you know, to 1914 and 1917.

Harry Trelogan: I have an interest in one question on which I would like to see whether anybody could help enlighten me. I am looking at the livestock economy now and recognizing that we've had sufficient dry weather with a cumulative decline in soil moisture over several years to make some people be concerned about what might happen if we had a further accentuation of the dry weather to cause the pastures and ranges to be short. These, we recognize, are areas of crop production very sensitive to the weather. Assuming that we had such an eventuality where the livestock were unable to get sufficient forage from the ranges and the pastures, to what degree can we depend on the grain stocks to substitute for forage to help maintain the breeding herds and avoid the kind of adjustments we experienced in past droughts? Has the technology improved in this area?
Harold Halcrow: Since the 1930's (as has been mentioned here in the conference two or three times) the livestock industry has shifted from marketing about one-third of the cattle through grain fed cattle to about two-thirds now or more. We're feeding a larger percentage of our cattle and this could have two consequences. One, it would mean (relatively speaking) that to keep up this rate of output you would have to feed more cattle than you did back in 1934. On the other hand, if you had adequate feed stocks the drought wouldn't have as severe an effect on the cattle industry as it had, let us say, in the 1930's. So the effect of drought depends on the adequacy of stocks and on the way in which we can maintain feed supplies.

Karl Fox: Well, first, is there another comment on the panel on this question? Are there comments in the audience on the question that Harry Trelogan raised. If not, I would like to get another comment, another question, or another statement from the audience.

Louis Bean: It seems to me that the commentators have been essentially those from the institution here or from Washington. I don't have the feeling that those who represent industry here have said much, and I'd be interested in knowing what observations they want to make about the field as they see it and the lines of interest that ought to be emerging from this.

Karl Fox: What do the conference topics mean to industry? There are a number of representatives from industry -- some engaged in forecasting, I guess, or inventory control. Will someone from an industry position speak up? What can any of these lines of research do for you?

Allan Leffler: (Agronomist, Pioneer Hi-Bred Corn Co.) As an agronomist I feel, with deference to Dr. Wadleigh and Dr. Thompson, the complete lack of contribution on the part of the agronomists in the fertilizer response area and the effect of weather on it. I believe that for the next conference of this type people from soil fertility could make a contribution to your understanding. I agree with Soth that dry years affect our thinking. I can remember that in 1947 we got very satisfactory response from plowdown nitrogen, at low rates, even though side dressing was completely futile. We found that year that starter fertilizer had a positive effect on the corn yields in contrast with what farmers believe about fertilizer. In 1956 in the Ankeny area we averaged 36 bushels of corn to the acre. The neighbors were sure we were going to burn up our seed fields because we had used more than $20 an acre on fertilizer. Actually our corn held on a week longer than theirs did. It varied from 13 bushels on alfalfa sod to better than 50 on oats stubble. This is one place where I got interested in soil moisture reserves as a contributing factor. I think the soil physicists and the soil fertility people could contribute to the understanding of the effect of weather on variability in crop production at future conferences if we get the facts dug out that are in the files.
Karl Fox: Does someone else with an industrial association want to comment?

Thomas Army: (International Minerals and Chemical Co., Skokie, Illinois) I can say unequivocally that we in the fertilizer industry have a great deal of interest. Weather affects our business just like it affects the business of an individual farmer. Whether he succeeds means whether or not we succeed. I might add that the International Minerals and Chemical Company now has its own weather impact program. Meredith Smith, who is here, organized this program for IMC and I would like him to say just a few words about our own weather program. I think it's an excellent step forward and shows how we as a company are going to use weather information in our business and how our customers -- both the manufacturer and the farmer -- can benefit. This new service involves not only our agricultural or agronomic recommendations but it also bears on the problems related to transportation, storage and the like. Meredith, why don't you say just a few words because it has a direct bearing on the whole subject under discussion.

Meredith Smith: (International Minerals and Chemical Co., Skokie, Illinois) Our weather impact program is an audit of what is happening weather-wise on a daily basis from the Rockies east. With this information we compute the soil moisture and ground temperature in macro-areas of the United States. This was done, and Tom has already alluded to it, principally for governing the operation of our business, which is a major fertilizer concern. It is important that we know where the planting seasons are breaking, where the fertilizer demand is going to be, at what time and where, on a county to county basis. You must realize that we move millions of tons of fertilizer material in a very, very short period of time. If we warehouse that material in the wrong spot, it is very costly for us to transfer it to one warehouse from another to meet the shift in the demand.

Secondly, we have found a great deal of use from this computation as a prognostication tool -- not of what the weather is going to be, because we don't believe that there are techniques developed that will allow us to see the future weather much beyond 48 hours. But, by knowing the present moisture condition and the subsoil condition we can evaluate how much rain must fall before any given condition will change.

I paid close attention (with corresponding interest) to Dr. Thompson's paper and also Bob Shaw's paper. I am not trying to refute the necessity of observing cycles and using these as estimating tools, but I do feel that the two men I have mentioned are really on the verge of a breakthrough for you people in the economic community. They are starting to define for the first time some of the underlying principles of weather effects on
plant growth. I think Bob Shaw's work on stress days can give a yield criteria, and certainly we can audit these inputs. Dr. Thompson's work, particularly where he is referring to optimal temperatures and optimum rainfall for yield is a sound, usable tool, much more precise than cyclical evaluations. However, I feel that total rainfall alone is a very poor statistic to use to find out what is happening in agriculture. The effect of rainfall and the sequence of rainfall which is important can be audited and these things (which I think you will see happening in a very short time) can be built up into yield expectations of accuracies of which we have never known before.

**Karl Fox:** Well, that sounds like one of the meatiest statements and one of the most forthright statements I have heard so far! Are there any other reactions or follow-ups in the audience?

**Lauren Soth:** Well to get back to agricultural policy and the weather, what I was trying to get at, and I hope somebody in the room can comment on it, is the political problem of how we get away from making policy on the basis of dramatic incidents and begin to apply brains to long-range agricultural planning on the basis of the best we know about the weather and the best we know about other factors. It seems to me we plan just like the generals -- always preparing for the next war on the basis of the last one. We make our agricultural policy on the basis of what happened in 1934 and 1936 or on the basis of a war period or something else.

**Harold Halcrow:** I made the comment that I believe we are going to come closer to predicting or anticipating production response to climatic changes and cycles, and I feel that our production policy, our storage policy and our insurance policies need to take these matters into account. We have in our projector two charts that will help provide a refresher to people who were in the conference. The first chart shows the years of extreme drought in Western Kansas, and although this is fragmentary data it was to me one of the most dramatic evidences of the possibilities of crop prediction that we had in this conference, The production or drought index shows severe drops in crop yields around 1894 and 1895, 1913 and 1914 or 1915, 1934 and 1936 and again in 1954 and 1955. When this was flashed on the screen Dr. Willett of the Massachusetts Institute of Technology remarked that these coincided with maximum sun spot activity. I checked this with one of the other meteorologists who said, "Well only in an approximate way." Well, assume that it is only approximate, or assume that our data are fragmentary, it seems to me that there is something more than randomness suggested in this particular chart. One of the difficulties we have at this stage in our prediction process is that accurate data on weather go back perhaps, 80, 90 or 100 years,
that is data which are sufficient for drawing up a scientific model. One thing that was most impressive to me in this conference is the possibility of an 88- or 89-year cycle in sun spot activity with harmonic or minor changes from periodic intervals such as we have here approximately every 21 or 22 years. Now this is one evidence of weather phenomenon.

The next chart that we have is from the work of Dr. Louis Thompson of Iowa State University in which he shows relation of weather and technology to the trend in the yield of corn. Instead of one straight line projection one might show a curve for the adoption of hybrid corn and another curve starting in 1958 showing years of rapidly expanding fertilizer use in the Corn Belt. Obviously the weather is much better from 1958 on than it had been back in the thirties. Perhaps this is part of an 88- or 89-year cycle. Each time that we add a new group of technologies to farm production we really move to another prediction surface. The implications for agricultural policy and for storage operations are rather extensive.

Louis Bean: I dislike to disagree with a panel member, but in the first place I am a little bit surprised that having said that our records are in a way too short when they go back to only about 80 or 90 years and then to have such faith in an 89- or 90-year cycle of which we are now a part seems to me a little bit dubious. Personally I would not worry about that 89- or 90-year cycle. I'll let my grandchildren be concerned about that one. I am concerned with the failure and, Dr. Halcrow, if you don't mind my saying so, the failure to get one of the things out of this conference that I had hoped would be clear. These movements that you just pointed to in the case of Iowa corn are cyclical changes that are to a large extent reflections of weather. Now you don't accept these as weather cycles from Dr. Thompson's studies because he hasn't revealed to you the weather elements that go into it, but you may want to recall the chart that I showed you for Nebraska which indicated that the cyclical movements from 1890 to date are very definitely reflections of the rainfall cycle -- almost identical cycles in two separate series, rainfall and corn yields per acre. And I showed you similarly that you could take the whole Corn Belt and find the rainfall cycles in the Corn Belt corresponding to the cycles in corn yields. So I'd like to emphasize once more that we are witnessing cyclical weather movements. Your suggestion to draw lines for the period of hybrid adoption and another one to represent something else sounded to me as if you were minimizing the evidence, which you earlier seemed to accept, the effects of the weather cycle.

Harold Halcrow: I think this is a misunderstanding. Actually I'm not minimizing this or attempting to minimize the idea of cycles or periodicity. This is the very point that I was trying to bring out that I think cycles and periodicity are characteristic of the phenomena we were observing.
Robert Buck: There has been some discussion of interaction of weather and technology. I'd like to see the economists and agronomists work more in this area. I can see it on a farm. A limited amount of rainfall with proper fertility will go farther and will not have as depressing an effect on yield as it would have had in 1934. The information now available on subsoil moisture and the situation we face month by month as we go into the spring has something to do with whether a farmer goes out early and works his fall plowing. I never touched my fall plowing this spring until I was ready to plant because I didn't want to waste any moisture.

Louis Bean: I'll stick my neck out again since I know nothing about this subject of agronomy or interactions. This takes me back to the 1930's when I had an argument with Henry Wallace. He was trying to educate me to the meaning of hybrids and to weather and to a lot of other things about which I was ignorant and still am. The point at issue at that time was whether or not adverse weather in the 1940's would have the same impact on yields as occurred in the 1930's, and I felt that sharp weather changes probably would have the same impact and that we would not have this counteracting influence of technology. I have been looking at the records, and I observe that in 1947 corn yields suffered as much as they did in the 1930's, and by looking at the yields for various districts and counties in Iowa I observed that you had even greater impact of weather in the 1940's than you had in the 1930's. I was rather amazed at the many cases of greater declines in yields in 1946 when compared with what happened in 1934 and 1936. So I have a question as to whether the interaction that you seem to bank on is really as certain as you think.

Thomas Army: (International Minerals and Chemical Co., Skokie, Illinois) The interactions probably haven't been in the past as great as has been implied but remember that in the past we've been controlling the chemical environment of plants, and fertilizer has been part of this control. You are controlling the chemical environment around the plant root or altering it; but now we are moving into a phase through research where we believe we can alter the physical environment of the plant both above and below the ground. Take this problem of drought that we've been discussing for the last three days. Dr. Zelitch of New Haven Agricultural Experiment Station recently has published several papers on chemical control of stomatal openings. This research suggests that we may soon have a practical way to reduce transpiration. Dr. Zelitch has also shown that as transpiration is reduced, photosynthesis is also reduced, but to a lesser degree. I believe that we will soon be able to minimize drought effects at least temporarily. There's no reason why chemicals should not play a role in the future for evaporation control from soil surfaces or to increase or decrease infiltration of water. I think that within the next decade you'll see the use of such chemicals become part of the production practices in farmers' fields.
**Harold Halcrow:** In addition to the physical influence on the soil, chemical weed controls and controls of insects and diseases which are often associated with drought offer other possibilities.

**John T. Pesek, Jr.:** (Professor of Agronomy, Iowa State University) If we define interaction as the different effect of one input in the presence or absence of another input or environmental factor, then I think there is evidence that there are interactions of the controlled input like fertilizers or stand with the environment. We know this occurs with stand; we know this occurs with response to fertilizers. I have not looked at this from the standpoint of weather interactions with machinery, for example, or size of machinery. I suspect that your response surfaces or response curves on farms with four-row machinery are different from those obtained with two-row machinery on the farm. Whether there is an interaction here or not or whether it is just a net gain over all the levels of the environment is a question that has to be answered. We have examined our data to determine what effect weather has on responses resulting from fertilizers. Observing the difficulty in measuring the important elements of weather and its affect on yields reported at this conference, I feel sure the problem of measuring the relevant elements of weather which influence yield responses to fertilizers will be even more difficult. We are having to learn which are the important factors to measure from the standpoint of the effect on fertilizer response. It turns out that the drought-day or stress-day concept that Dr. Dale presented has been drawn to be an important factor in determining response to fertilizers.

At the same time the drought-day concept that has been used up until now does not take into account the effect of distribution. As an agronomist, one thing I learned soon after I came to Iowa was that it wasn't how much rain that was important, but how it was distributed. At least everyone said that. It is not easy to express the distributional pattern of rainfall or temperature or stress-days in a meaningful equation. This is another area in which we are working and hope to make some progress. I think that if we have another conference there may be more material which agronomists could contribute than they have in this one, although I certainly am not apologizing for the part the agronomists and climatologists have played here.

**Robert Shaw:** I'd like to make one comment regarding weather research. Sometimes I think we tend to forget one of the problems that we have. We have many different days when weather in a growing season affects a corn plant, and out here at the end we have one yield in which we have to try and integrate all the pluses and minuses and zero effects of many variables. Dr. Dale's work here is an outgrowth of several things. We've tried to break down some of these periods and
some of these factors and look at them individually. This is one step which we think is in the right direction. After you get them broken down you have to put them back together again. But we have to break these segments out because there are a lot of pluses and minuses that go together to make this final yield.

**Wayne Palmer:** I am rather concerned about the emphasis that is being placed on cycles and periodicities. I hesitated for some time before I presented that slide, the one which shows serious drought about every 20 years in western Kansas. I think we have some difficulty with definitions. Some people here apparently think of cycles as being very rhythmic, while others apply the term to variations which are rather irregular. I don't think you people would be well advised to put too much stock in cycles and periodicities in weather. Most such cycles are only suggested by the data and are not supported by any semblance of physical theory. Of course, Dr. Willett presented a physical theory -- the sunspot theory -- to support certain cycles, but most are without support. I presented only some statistical evidence of a 20-year cycle for which I really have no physical theory.

The meteorological literature is so filled with "cycles" that one can find ideas and suggestions of cycles of nearly all lengths from a few days right on through the spectrum of time to thousands of years. Dr. Mitchell remarked yesterday that there are almost as many proposed "cycles" as there are investigators of the subject. I suspect -- but I have no proof -- that this apparent 20-year cycle in serious drought isn't going to show up too well except in a certain area in the Great Plains. Don't be too ready to accept cycles and periodicities in weather.

**Karl Fox:** If I can just toss in one comment: It sounds as though some cycles represent nothing more than an interaction between the data and the investigator! This is sometimes the case in economics too.

**Louis Bean:** I think perhaps you've misjudged the matter of cycles as discussed in this conference. It is pretty well established that cycles of the sine curve type are no longer what is generally expected. I think even the Weather Bureau people have learned that cyclical movements do not have to come exactly in the same shape and in the same time interval. Your remark that there are cycles perhaps only in the central part of the country, I think, is denied by the fact of the even more striking evidence of cyclical features in Maine potatoes, California potatoes and in other crops; so I would like to amend your remark or you have amended for me that the cycles which you do see in the Grain Belt may also be found in the South and the Northeast and in the West.

**Harry Trelogan:** I again want to express a note of gratitude for the compliment paid to the estimates that we issue from the Statistical Reporting
Service of the U.S. Department of Agriculture, because in none of the discussions thus far has anybody attributed any of the deviations between the predictions and the actual prediction estimates to errors in the estimates. I don't think we're that good! We think we have been making improvements in the estimates of acreages, of yields per acre and of production, but we have considerable room for further improvement in these estimates. Part of the technological improvement that has been referred to as a bundle of improvements includes such things as improvements in record keeping, improvements in management especially farm management, and improvements in the technology of statistics; that is, in the statistical methods.

**Louis Thompson:** We find that we can get better correlations in our studies in data since 1935 than we had for data available to us prior to 1935 and we have suspected that this is due to more accurate yield reports. At least that's what we believe and we think that you're doing a fine job. Please bear in mind that even though we talk about taking weather data in developing prediction equations, our prediction equations are only as good as the data that are used to develop the prediction equations.

**Lauren Soth:** I have a question that may have been answered earlier and I wasn't able to be here the first two days of this meeting, but I've been very much interested in today's proceedings. I want to endorse what Bob Buck, my neighbor, said about the importance of this study. Now comes my question. I wonder if anyone here, Louis Thompson or Louis Bean or anyone else, feels that we know enough about weather at this stage to have any utility in program planning?

**Louis Bean:** Probably it would be honest to say that at the moment we don't have it. We have so far dealt chiefly with the corn and Kansas wheat yields. But I think the cycles are much more pronounced in winter wheat and spring wheat, and these I think you can begin to take much more seriously than the cyclical evidence in U.S. corn yields. I'm afraid your caution will always be appropriate. I'm reminded of an item in a book that Dr. Thompson reminded me of the other day on sun spots. It is a very striking case of the fact that even where you know that cycles exist there is room for some doubt.

This is a case of Dr. Douglas, who had tracked tree rings down through the centuries and had worked out correlations between rainfall and tree rings and knew that rainfall cycles were real because they tied in with the sun spots cycles and nobody could have any doubt about sun spot cycles. Well as he tracked back the sunspot cycles still further he came to a period where there were no sunspot cycles.

Can you imagine that? Suppose you had an agricultural policy that rested on the expectation of sunspot cycles continuing into the 1970's and they didn't appear! Well at the time when he discovered the years of no sunspot cycle in his long record (and perhaps wondered whether those who compiled the records were on the job and had accurate statistics) he
received a communication from a fellow astronomer or a meteorologist in England who found that the record for that particular set of years showed no cycle in rainfall. So this was a true event both in rainfall and in sunspots and not due to inaccurate statistics. But the point I want to make is that in sunspots you have about as neat a certainty as you could get. Yet the sunspots failed at a certain time. When they will fail again I don't know. That's why I say that your type of cautionary mind is needed always, but I think not too many of them!

Mordecai Ezekiel: I just wanted to make one comment on the subject of statistics. Working with some of these earlier data years ago we found that there were some strange things in the data. One of them that showed up was in an early period in the last century. Sometimes when the crop estimates over 10 years were tied together with the next census they apparently were 5 to 10 percent in error. You had to re-adjust the data on the assumption that the census was right and the intervening data were wrong. Now you may get some peculiarities which are due to adjustments in data that may or may not have been well based. Another phenomenon as I recall it in early statistical data was that when the farm control programs of the 1930's were put in and we really began to actually measure up the area in land, we found that in many parts of the country that the acreages in fields were not what the deeds said or what the farmers thought they were. In some cases they were substantially larger in area and other cases they were substantially smaller. There were some pretty drastic revisions in some of the basic figures which went into the acreages on which the crop yields were calculated. So when you are using early data, you can't always count on the crop estimates year by year or even the census data as being as accurate as we like to think.

Karl Fox: I wonder, Dr. Ezekiel, in view of your many years of work in FAO, did you actually study weather response data, or weather effects on crop yields in a number of countries? Did members of your staff make such studies and do you have anything to say on the quality of our knowledge about the weather and the weather effects?

Mordecai Ezekiel: As a matter of fact we're so used to dealing with the comprehensive voluminous weather data available here that it is quite a shock when you try to relate production to weather in other countries. In the first place, the weather data or yield data themselves are of much less high quality in many of the less developed countries than they are here. But in the second place the weather data in the past have never been compiled and put together in anything like the shape they are here. You could get the rainfall and the temperature in a few selected cities, but what was happening out in regions where the crops were grown simply wasn't published, and sometimes wasn't even recorded.
We took that up with the World Meteorological Organization and one reform that they agreed to make -- I haven't checked back to see whether they have carried it through as well as they said they were going to -- was to collect and publish in their current international report, country-wide or province-wide average monthly data on temperature, precipitation, and so on for each country they cover, in somewhat the same shape they have been published here regularly in the States. After a time we will begin to have long series available from many underdeveloped countries comparable to those available here, which in the past would not have been available without a mammoth investigation to bring together, average and record the data for the recordings scattered over the agricultural area. It is hard to collect the data. It would have been quite impossible for anyone here to do it.

**Karl Fox:** I think some of these cautions on data from earlier periods particularly are very much in order. I recall a statement, I think by Whipple, who published a little book in 1919 or 1923 on vital statistics. The statement was simply this: "With statistics the unscrupulous deceive the unwary and the innocent deceive themselves!" None of us here are unscrupulous but when we get far back in the early weather data or in the yield and weather records of other countries we might be in the position of "innocents deceiving ourselves."

**Louis Bean:** There appears to be a better way to ask this sort of question. If you have an adequate reserve, then I think you can take cyclical movements seriously because if by chance you should err on the down side, that is if you expect a decline in yields and it doesn't come, you are adequately protected. That I think would be the practical application of this kind of information as it is developing now.

**Paul Waite:** (Iowa State Climatologist) And in regard to earlier weather records I'd like to point out that Douglas' tree ring data are now being re-analyzed at the University of Wisconsin and some of the results have been very strikingly different from the earlier analysis.

**Robert Shaw:** I'd like to make just one comment here regarding Mr. Soth's question. I think I'm basically in agreement that as yet we can't use weather much in planning, but I can remember not many years ago quite an extensive article on the editorial page of our Ames paper regarding present yield levels and technology. Weather was noticeably absent. There was not one place in that article where weather was mentioned as having any possible affect on our present high levels of production. So if we have done nothing else I think we have got some people thinking that maybe there is a little weather factor in our recent high yields.
William Shrader: (Associate Professor of Agronomy, Iowa State University) I want to speak just a moment on this interaction question. There is no doubt as you look back over the records that the fluctuations -- extreme fluctuations -- are as great now from year to year as they have ever been in any of our records. Yet in thinking back, too, we can think of things like Mr. Buck mentioned where the interaction has definitely shown up, and these are things that we've been able to do that make the extremes of weather less severe. I think that we can reconcile those two things if we realize that weather can be a definitely limiting factor and that if we have no moisture we have no crop. The crop under certain conditions can drown out. But in any of these data that we've looked at you have this upward trend that has been called the ignorance factor. And I'm wondering if this upward trend isn't in large part an interaction of technology and weather. The fertilizers widened the base under which a crop can make a satisfactory growth. If you're short of moisture one of the first things that shows up is actually a nitrogen deficiency. Eventually it doesn't matter how much nitrogen you have; if it's dry enough the crop fails but under conditions that prevail maybe nine times out of 10, the crop would not fail if it had adequate nitrogen. The upward long-time trend is in part an expression of this interaction.

Robert Dale: I think Mr. Soth's question can be answered in two parts, and I can't answer either part. It seems to me that it depends upon first, "how far can we forecast in the future?" and second, "given the weather data, can we estimate a crop yield?" The forecasting in the future has been alluded to several times. I think we owe a debt to Dr. Wadleigh in pointing out that some private meteorologists as well as the Weather Bureau have not found that they can go out beyond about 30 days with any great accuracy. The most we can call any day-to-day weather is about three days in advance and we put out a five-day forecast on a sort of "call-the-trend basis." We hope that research will improve our capabilities.

Dr. Willett mentioned that he hopes we will eventually be getting out to 10 years, but before Dr. Willett's comments I hadn't heard of anything beyond one or two months. It was a great privilege to comment on John Morgan's paper about two years ago at Fort Collins on using weather to estimate short-run crop forecasts. In my discussion I made a statement that given a 100 percent accurate forecast, we probably still wouldn't be able to accurately determine crop yields. In the Weather Bureau clearance process the paper was returned to me with an indication that this was a terrible indictment. But I submit to you that this is one of our big problems here today. We have a 100 percent accurate forecast. It's right up there plotted on the board for the last 30 years. We have temperature, precipitation; it's all published; and we can't decide how much is weather and how much is technology. If we knew exactly what rainfall and temperature conditions we were going to have any place in the United States this year
we might be hard pressed to say more than the weather is sufficiently favorable to allow technology to take over. I hope that maybe with more efforts like this and with more individual efforts at the universities, the Weather Bureau, the Agricultural Research Service, Statistical Reporting Service and other USDA agencies we will have this information.

Louis Thompson: Bob, did you say that if we knew the weather that we're going to have each month during the next year that we would not know how to deal with it and make a yield projection?

Robert Dale: I said this is what this conference is about. In other words, some of us say that it's all technology; some say it's weather; and some say it's interaction.

Louis Thompson: Let me see if I can summarize where we are, and if we ever come back together where we need to clear the air. The people working in economics have treated weather as a random variable; so that means you might take a set of yield data and run a linear trend through the data and assume that weather is random, and that the trend will measure technology. The assumption would be that any deviations from the trend then would be deviations due to weather, and then one can establish a weather index. That has been the economists' approach and I am not criticizing them. This is just the way the concept of weather has developed. So the economists who have worked on this problem have considered weather as one variable. If you include in this upward trend some improvement in weather then some of your weather influence is confounded with technology. Now, our approach has been that of trying to measure the influence of various weather variables. We used Dr. Ezekiel's technique of putting a time trend in to help us adjust for technology, and we got pretty high correlations. We have been considering the time trend as a trend in technology. This conference has brought out very clearly that this technology now needs to be divided into its components and that weather cannot be treated as a random variable. In other words, we can now share each other's experience and make further improvement in prediction equations.

I certainly wish to express the appreciation for the Center and for Iowa State University for the time that you people have taken to come here. I know we have some very busy people who came here Sunday and have attended every single minute of the conference, and we deeply appreciate that.

Louis Bean: May the other Louis speak? Are you about ready to close this meeting or not yet? Well, I'd like to offer a resolution somewhere near the close of the meeting; if this is it, I'd like to offer it now; if not, I'll wait.
Karl Fox: I expect this is substantially the close of the meeting and this would be a good time, Louis, if you have a statement to make.

Louis Bean: I've tried this out on two or three people and I think it probably expresses the sentiment of many other people here. Whoever is the literary person in this group may be able to take this and shape it into the right form.

BE IT RESOLVED, That (1) the members of this conference express their appreciation to the Center for organizing and making possible this "Seminar on Weather and Our Food Supply."

BE IT RESOLVED, That (2) the conference members feel that the success of this conference in bringing together representatives from government, industry and academic research centers warrants considering the advisability of holding similar conferences annually as a non-governmental supplement to the USDA Agricultural Outlook Conference.

(Now this suggestion occurs to me because several of us here, including Dr. Ezekiel and myself, have been intimately involved in the beginnings and the continuity of the annual Agricultural Outlook Conference in Washington, and I have a strong feeling that the present stage of our interest in weather and the food supply is equivalent to the stage we were in in 1923 when a similar conference of economists in business, agriculture and the USDA were brought together. That conference has been repeated.)

BE IT RESOLVED, That (3) the Center appoint a committee of five (or some appropriate number), representing the several areas of interest in government, industry and the academic work in current and prospective weather relations to our agriculture and to the food supply, to draw up recommendations for planning the next annual and continuing conferences on weather and our agriculture; that the program to be developed and the research to be stimulated be developed in close association with the USDA, so as to capitalize on the 40-year experience with Agricultural Outlook Conferences.

I offer that as my sincere expression of interest in this subject in this conference and would like to come back here 41 years from now and see that you have done as nice a job as the Department of Agriculture has done with its Outlook Conference.

Karl Fox: Well, thank you very much.

Louis Bean: I offer that as a motion or whatever would be appropriate.
Karl Fox: Suppose we consider it as a motion. I guess it would be for the most part advisory to the Center but let's not lose it. Is there a second to the motion?

Harold Halcrow: This is an excellent idea with a lot of promise and I wish to second the motion.

Karl Fox: Is there some discussion on this? Does anyone else feel that this is a good thing to do to recommend to the Center a plan for one or more additional conferences? Is there discussion on the motion?

Harry Trelogan: Louis, do I interpret you correctly when I think that you're trying to suggest a conference of a somewhat different nature than this -- to be held preceding the Outlook Conference to give us some guidance on what the weather prospects are for the forthcoming year. The Weather Conference would then provide some grist for the mill at the Outlook Conference?

Louis Bean: That might be the ultimate utility of a series of conferences for the moment. I just have a feeling that there certainly is a great amount of interest all over the country in this subject. Louis Thompson's work, I think, has demonstrated that. There are many questions that have not been answered as has been indicated by discussion here the last few days. There's a great deal of need for additional research. This is exactly the situation we were in 40 years ago in the Department of Agriculture. We entered that field of agricultural economics; we didn't know the answers; we didn't even know the topics that were to be developed. So I have expectation that a weather conference could be pulled off next year at the time of the Washington conference economic conference for guidance in agricultural programs. No, I'm not that optimistic. My attitude is that it is an opportunity to bring together the different interests in these broad fields where there can be an interchange of minds and where there can be an interchange or development of techniques. The first Outlook Conference consisted chiefly of industry economists because people in the departments weren't quite certain of themselves; so they leaned heavily on the outside. Well, presently they discovered talent within, and in the agricultural colleges, and they became the basis of the conferences. I have no motion as to what personnel and papers on weather and agriculture would be considered for next year. I would leave this open for the recommended group to think about it and to make recommendations.

Mordecai Ezekiel: I believe Louis' explanation indicates that we should have continuing conferences of this sort to develop the subject further. I'm not quite sure that he wants to go as far as his resolution read, that the conference should necessarily be held at Washington as part of the Outlook Conference -- particularly if it's going to be organized under the present auspices.
Louis Bean: The suggestion wasn't that the conference be tied into the department's conference. It was a suggestion that the committee might be appointed to examine the advisability of another weather and food conference, to check with people in the Department of Agriculture who have had long experience with this type of conference for the development of research in this broad area of so many facets. No, it wasn't that this conference -- if it is to be repeated next year -- should be in association with the Department. It should be quite a separate non-government operation. Have I made that clear?

May I say that this is a recommendation which I offered to the Bureau of Agricultural Economics 11 years ago -- and then I did urge that if the Department itself developed a weather-agriculture interest in the form of supplementary conference it might be tied in with the Economic Conference and possibly held at the time as the Outlook Report.

But I'm not suggesting that now. I'm suggesting that we begin to develop our long-range weather-crop wings a bit. If four or five years from now a weather-food conference has something to say in the way of weather and crop outlook then we could send Karl Fox to Washington and have him negotiate a wedding with the Economic Conference.

Karl Fox: I didn't come to this conference to be a marriage broker! But putting the motion to a question: Are we recommending that the Center set up a committee to plan at least one more conference of this type -- one or more? Now isn't this about the essence -- to recommend to the Center that it appoint a committee and organize at least one more conference and maybe set up a cumulative research and experience interaction here which might run as many years as it really seems fruitful -- not necessarily 41. After all, we -- well, I, can understand Louis' interest in having it run 41 years and his being at the last conference. All in favor of this motion, would you please say "I". (I's) Opposed? (None heard.) The motion is carried. Louis, I guess you'll just have to take this under advisement. Do you want to make any response at this time?

Louis Thompson: I'm not so sure that I should be the one to respond. There's Mr. Stucky, the educational director for the Center, who might more appropriately respond to this because I'm serving merely as chairman of this particular conference; but I can assure you that I will do all I can to help in this kind of development and so . . . Bill.

William Stucky: Well, usually we're soliciting suggestions for ways in which we can provide Center resources so as to make opportunities for leaders to get together and work on common problems of man. We're pleased to receive voluntary recommendations from people like you and these will get very weighty consideration.
There is a certain limit to what the Center can do in terms of its very scarce resources. But it can cooperate with others (agencies, institutions, groups, etc.) and sometimes get quite a bit done.

The Center has limitations on the amount it can invest to deal with purely procedural research or scientific method type of problems. It's interested in the implications to be derived from the results of this conference. For example, one of the things we'd like to see go on from here is to specify the implication the understanding of weather variables has on the capacity of the nation to provide food for a growing population. Can we predict something about further changes in agricultural structure that we need to take into consideration? The Center is interested and we will respond to the recommendations you submit.