Communications

Chemical Induction of Flowering in the Sweetpotato

An effective means of inducing flowering and seed production in sweetpotatoes would greatly facilitate the development of varieties with superior quality, higher yield, and disease resistance (*1*). However, many difficulties have been encountered by research workers in their attempts to discover methods for the initiation of flowering in sweetpotato plants, especially with those of the Jersey type. Hartman (*2*) used as many as eight different variables in an attempt to induce flowering in Jersey-type sweetpotatoes with no positive results. Similarly, Mikell (*3*) found that certain hormone applications were ineffective, either for induction, or for retardation of floral primordia formation in sweetpotatoes. It has recently been reported that flowering can be induced by grafting scions of sweetpotato plants onto root stocks of certain ornamental species that do not have storage roots (*1, 4*).

In an experiment initially designed to study the effects of foliar applications of 2,4-D (2,4-dichlorophenoxyacetic acid) on subsequent sprout production of the roots, sweetpotato plants (*Ipomoea batatas,* vars. Porto Rico and Gold Rush) were set 13 July, 1953 in a ground bed in the greenhouse. Twelve plants of each variety were randomized in four blocks of three plants each. Single blocks of three plants each were treated with 100, 500, or 2500 ppm of 2,4-D. A hand sprayer was used to make the applications at the time (4 Sept.) the roots had begun to enlarge, 53 days following setting of the plants. A group of three plants for each variety was left as a control. A second replicate was planted 31 July, and on 4 October, 64 days after setting the plants, 2,4-D at the concentrations listed here was applied to this replication.

By 7 November, 1953, it was observed that plants were flowering which had received certain 2,4-D treatments. The numbers of flowers that had developed by 5 Dec. and the dates on which the first flowers appeared are given in Table 1. No flowers occurred on nontreated plants or on those receiving 2,4-D treatments other than the ones listed.

Table 1. Effect of 2,4-D on flowering in sweetpotatoes.

Treatment		Porto Rico		Gold rush	
Chemical and concentration	Date of application	Date of first flower	No. of flowers to Dec. 5	Date of first flower	No. of flowers to Dec. 5
2,4-D 500 ppm	4 Sept.	7 Nov.	84	No flowering	
2,4-D 500 ppm	4 Oct.	7 Nov.	26	No flowering	
2,4-D 100 ppm	4 Oct.	No flowering		9 Nov.	11

Flowering in all cases was general, with many flowers occurring on each plant. Relatively small or nonenlarged roots were associated with the treatments that induced flowering (*1, 4*), and splitting of stems and petioles and tumefaction of stems near the bases of such plants were common.

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References

1. M. P. Zobel and G. C. Hanna, *Calif. Agriculture* **7**, 13 (1953).
2. J. Hartman, *Plant Physiol.* **22**, 322 (1947).
3. J. J. Mikell, *Proc. Am. Soc. Hort. Sci.* **60**, 295 (1952).
4. A. E. Kehr, C. T. Yu, and J. C. Miller, *ibid.* **62**, 437 (1953).

8 August 1954.

Source of Error in Determination of Chromic Oxide Using Perchloric-Sulfuric Acid Digestion Method

Recently, in a series of determinations of the chromic oxide content of cow manure, by the method of Bolin, King, and Klosterman (*1*), it was concluded that temperature, time, and acidity were critical factors during the digestion period with the digest mixture of perchloric-sulfuric acid. A search of the literature showed that Smith (*2*) reported that hot, concentrated perchloric acid (70 to 85 percent $HClO_4$, by weight) has reducing, as well as oxidizing, properties and that the reducing reaction of hot, concentrated perchloric acid increases directly with the concentration of the acid and with the temperature. These reducing properties are assumed to result from the formation of hydrogen peroxide as a decomposition product.

Dilute perchloric acid, upon boiling, loses water until it reaches a concentration of 70 to 72 percent. This acid distills at about 203°C, accompanied by some decomposition (*2, 3*). Mixtures of perchloric and sulfuric acids will yield traces of hydrogen peroxide at temperatures of 180° to 185° C because of the dehydration effect of the sulfuric acid (*4*).

Low chromic oxide content in cow manure frequently resulted when the digestion was prolonged for any length of time beyond the minimum time required to oxidize the chromic oxide to chromic acid. In the absence of precise knowledge of the reactions involved, optimum conditions for this digestion were determined empirically. When a 400 to 500-mg sample of dry cow manure was digested at a temperature such that a clear orange-colored digestion mixture was obtained in approximately 10 or 12 min, results were obtained with a standard deviation of ± 0.02 percent in

the range of 0.3 to 0.5 percent Cr_2O_3. Rapid chilling and dilution of the solution at the end of the digestion is effective in arresting the reduction of sexavalent chromium due to the formation of a small amount of hydrogen peroxide during the period of oxidation. Potassium permanganate, if added, would reduce the hydrogen peroxide preferentially, leaving the chromium in sexavalent form (*4*).

Reference 1 did not stress the critical factors of time, temperature, and acidity of digestion or mention the possible source of error and low results due to prolonged digestion.

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References

1. D. W. Bolin, R. P. King, and E. W. Klosterman, *Science* **116**, 634 (1952).
2. G. F. Smith, *Ind. Eng. Chem., Anal. Ed.* **5–6**, 229 (1934).
3. Willard and Gibson, *Ind. Eng. Chem., Anal. Ed.* **3**, 88 (1931).
4. G. F. Smith, *Mixed Perchloric Sulfuric and Phosphoric Acids and Their Applications in Analysis*, brochure (G. F. Smith Chemical Co., Columbus, Ohio, 1935), p. 55.

19 July 1954.

Genetics of Homothallic Fungi

Advances in Genetics, vol. V, contains an important and interesting paper by Pontecorvo *et al.*, on the genetics of the fungus *Aspergillus nidulans* (*1*). In the paper, however, the authors refer to this organism as the first homothallic fungus ever investigated genetically. They also report the discovery of a new phenomenon which they term "relative heterothallism." The validity of these claims seems extremely doubtful in view of the extensive genetic studies that have been carried out with members of the genus *Glomerella*. These studies date from 1914, when Edgerton (*2*) reported the occurrence of cross-fertilization between strains of *Glomerella*. References to most of the papers published since that time may be found in a recent review of the genetics of this fungus (*3*).

Wild-type cultures of *Glomerella* isolated from nature, as well as many mutant forms derived from them, are self-fertile when propagated from single, uninucleate haploid cells. Such cultures are clearly homothallic according to Pontecorvo's definition of the term. In wild-type cultures, mutant nuclei arise that undergo preferential cross-karyogamy, and, as a result, ascospores from many of the perithecia produced by such cultures segregate in a 1:1 ratio for wild-type and mutant forms (*4*). The phenomenon of preferential cross-karyogamy (relative heterothallism) also occurs commonly when *Glomerella* cultures are mated (*5, 6*), and an analysis of genes controlling this phenomenon in matings in which neither, one, or both of the mated cultures were self-fertile has been published (*7*). As a result of these behavior patterns, *Glomerella* has been termed homothallic, partially heterothallic, and homothallic with heterothallic tendencies. That a similar paradoxical situation exists in *Aspergillus nidulans* seems evident from the fact that this fungus is claimed to be both clearly homothallic and relatively heterothallic in the same paper (*1*). The results with both *Glomerella* and *Aspergillus* appear to emphasize Olive's (*8*) observation that "it is not wise to conclude that any fungus is strictly or irrevocably homothallic."

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References

1. G. Pontecorvo *et al.*, *Advances in Genetics* (Academic Press, New York, 1953), vol. 5, p. 141; reviewed by E. Caspari, *Science* **119**, 646 (1954).
2. C. W. Edgerton, *Am. J. Botany* **1**, 244 (1914).
3. H. E. Wheeler, *Phytopathology* **44**, 342 (1954).
4. S. J. P. Chilton and H. E. Wheeler, *Am. J. Botany* **36**, 717 (1949.)
5. G. B. Lucas, S. J. P. Chilton, and C. W. Edgerton, *ibid.* **31**, 233 (1944).
6. S. J. P. Chilton, G. B. Lucas, and C. W. Edgerton, *ibid.* **32**, 549 (1945).
7. H. E. Wheeler and J. W. McGahen, *ibid.* **39**, 110 (1952).
8. L. S. Olive, *Mycologia* **46**, 254 (1954).

29 July 1954.

Taken literally, my statement that "Genetic analysis of a homothallic fungus (*Aspergillus nidulans*) has been carried out for the first time" was quite wrong. I am glad that Dr. Wheeler has pointed it out. Even in 1948 (*1*) when I first communicated the technique of recombinant selection to the International Congress of Genetics, there had been already several attempts at analysis of homothallic species. Thus, earlier than the work of Wheeler and his colleagues on *Glomerella*, the pioneer work of Winge on yeasts was more than 10 years old, and that of Gries (*2*) on *Sordaria* was more than 5 years old. Though my statement is literally very misleading, when read in its context it may convey what I meant, namely, that with the technique of recombinant selection, formal genetic analysis on a scale comparable to that of classical organisms, such as *Drosophila* and maize, has become practicable. The previous attempts mentioned above, though exceedingly interesting in other respects, have given meager results of strictly genetic interest. The need of picking individual asci, among which the great majority were not of crossed origin, made it impossible to cope with the large numbers of products of meiosis required for effective genetic analysis.

As to the second point on which Wheeler takes issue, that is, our claim of having discovered "relative heterothallism," I am not sure that his doubts are founded. Our findings were communicated to the Genetical Society in 1951 (*3*), that is, before Wheeler and McGahen's paper (*4*). Furthermore, on our definition of relative heterothallism—the formation by two self-fertile strains of crossed asci in excess of 50 percent—only one or perhaps two of the many matings reported in their paper could be examples of this phenomenon. We still have no clue of whether relative heterothallism is the consequence of preferential multiplication of ascogenous hyphae with nuclei of two kinds, or of actual preferential cross-karyogamy. It would be