D. I. T. WALKER

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DISCUSSIONS

P. G. C. BRETT (S. Africa): How many back crosses are needed in order to return to a commercial type of cane?

J. T. RAO (India): The work is not sufficiently advanced to be able to answer this question.

J. T. TATAY (Philippines): Have you had any success with selfing noble canes?

DR. RAO: No. Very few seedlings were obtained.

D. I. T. WALKER (Barbados): In Barbados after only two generations of S. officinarum selfing there was a big drop in vigour.

B. T. ROACH (N.S.W. & Fiji): I should like to question the statement in this paper that growth of the pollen tube is indicative of the cross compatibility of the parents involved.

DR. BRETT: In South Africa it has been observed when attempting intergeneric crosses with Saccharum by Benecium, that pollen tubes grow long but do not penetrate the stigma. No seedlings were obtained.

MR. WALKER: I doubt the usefulness of an inbreeding programme with S. spontaneum when eventually interspecific crosses have to be made with S. officinarum. Desirable characters may not be built up unless the progeny is tested in a common officinarum background. A recurrent selection programme crossing spontaneum inbreds with S. officinarum would be the way to measure any gain from this programme.

FAMILY PERFORMANCE AT EARLY SELECTION STAGES AS A GUIDE TO THE BREEDING PROGRAMME

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(Presented by the author)

INTRODUCTORY

An examination of accumulated records provides a description of the trends in breeding policy and selection methods which have developed over the years. An understanding of the processes operating in selection and of the statistical limitations inherent in the work, allows a more critical check on our efficiency than simply observing the rate of production of successful new varieties from an empirical programme. An examination of both together can indicate whether the genetic variability available to the cane breeder is being explored and examined to the best advantage and with maximum economy in the light of alterng variety requirements.

DESCRIPTION OF THE SELECTION ROUTINE

The broad plan of the sequence of variety selection trials in Barbados was formulated by McIntosh in 1933-35. The seedlings from crosses made in 1934 were the first to undergo this schedule, which has not since been radically revised, though criteria have naturally altered with increasing experience and with changing requirements.
This coincided with the replacement of noble-cane breeding by interspecific material. The schedule has been described in detail elsewhere. Briefly, seed is sown as soon as it is mature; seedlings are potted when large enough to handle and planted out in the field (the Nursery) at 5 ft. x 1 ft. 7 in. with the help of overhead irrigation in May, when the probability of rainfall is increasing. In October, a selection is made based on visual assessment, particularly of cane thickness and cane number, less so of length and condition of top, and 0–100% (dependent on family characteristics; average decreasing in recent years; see Fig. 1) replanted as cuttings into a second single-stool trial (the First Year Trial) at a spacing close to the traditional 5 ft. x 5 ft. used in Barbados. After 15–18 months’ growth under natural rainfall conditions, visual selection for thickness, stooling, length, absence of rotting, etc. is made, and these selections are then recorded in detail, including hand refractometer brix readings. Some 300–400 of each series are generally accepted, and are given serial B. numbers prefixed by the year of this selection stage. The selections are immediately extended into 10-stool plots, established under irrigation, and allowed to grow for 7 months to provide material for planting out the first replicated trials (Second Year Trials) at two contrasting locations in Barbados, and for despatch to other subscribing countries. These plots serve also to correct some of the mistakes inevitable in single-stool selection – invariably a few clones grow very poorly and can be discarded on inspection of the 10-stool rows. Subsequent trials contain only small samples of each family, and therefore are of no interest in the present paper.

**TRENDS IN SELECTION RATE SINCE 1935**

Fig. 1 shows the average selection rate from the Nursery. The general decline is the expected result of increasing experience and confidence on the part of the team in charge. A reduction was in any case obligatory to keep the First Year Trials to a manageable size while at the same time increasing the numbers of crosses made and seedlings examined. These numbered about 4,000 in 1935 (plus 8,000 seedlings from noble S. officinarum crosses), rising to 22,000 in 1945 and to a peak of 47,000 in 1958. Relatively few aspects of yield and desirability are considered in this preliminary
selection in young cane, which has consequently been called more commonly 'the elimination of rubbish'.

Fig. 2 traces the trends in selection rate over the first three stages combined as a single measurement, the number of varieties considered worthy of Second Year Trial being given as a percentage of the number of seedlings in the Nursery. This figure is of more significance than Fig. 1 because it embodies the observations made on up to three occasions, one of them involving a substantial proportion of the original population as mature cane when juice quality is also a criterion. The selection percentage is a measure of the overall merit of the cross, embracing all the aspects of desirability considered in these stages and the weights given to each component character by the selector. A high percentage selection indicates that the population had a high mean and/or variance for some or all desirable characters. The clones selected should have a wide range of adaptation preferences, since environmental response should not have been important among the criteria applied at one site for canes eventually destined for a variety of sites.

The marked fluctuations from year to year, within the overall trend towards more rigorous selection, are due to several causes. The samples of crosses probably vary in intrinsic merit: family sizes fluctuate and affect selection rates; the selection efficiency varies because, in spite of every effort to make it otherwise, it is somewhat subjective; and in 1954 (B. 56' series) bunch planting was introduced for at least a proportion of the seedlings of certain crosses, was used extensively in the B. 59' series, but is now being used very little.

In a carefully recorded set of 26 large families in the B. 61' series, planted both as singles and as bunches in the nursery, the average contribution of each selection stage to the overall selection rate (and year trial % nursery) has been measured by partial regression methods. The number of seedlings surviving in the nursery at the time of selection was checked. The percentages selected at successive stages were transformed to angles for analysis. In the results presented in Table I, $b_1$ measures the contribution of Nursery selection $x_1$; $b_2$ that of selection from the First Year Trial $x_2$; and $b_3$ that from the 10-stool propagation plots $x_3$; $b_4$ measures the relationship between the
average number of seedlings per bunch $x_4$ surviving and recognizable (five having been potted) at the time of nursery selection, and the overall selection rate from bunches $y$.

It is evident that the contribution of nursery selection is small and relatively

<table>
<thead>
<tr>
<th>Selection rate</th>
<th>Seedlings per bunch</th>
<th>Regression coefficients</th>
<th>Overall selection rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Singles</td>
<td>26.60 ± 16.52</td>
<td>44.53</td>
<td>0.149</td>
</tr>
<tr>
<td>Bunches</td>
<td>25.20 ± 19.45</td>
<td>43.22</td>
<td>1.35</td>
</tr>
</tbody>
</table>

$ns = \text{not significant}$

inconsistent from family to family, particularly in the case of bunches. Contrary to expectation, the number of seedlings per bunch bears no consistent relationship to the selection rate. This is true even in the nursery stage (simple regression $a_3$ on $x_4$: $b = -5.32 \pm 8.52$). Families selected at 6 months in the Nursery, on a common set of criteria, actually give a different order of merit at 15 months in the First Year Trial. Family differences are also greater in the First Year Trial.

**SHORTCOMINGS OF SELECTION METHODS AND ACCUMULATED RECORDS**

Seedling mortality at potting, due largely to horticultural management difficulties, ran at a high level for a number of years, and has only been brought under control in the last 4 years. Certain families suffered worse than others from chlorosis and poor establishment, which therefore meant that genotypic selection was perforce practised at the pot stage, possibly being correlated with commercial yield qualities. Establishment in the field was also variable, due to imperfect irrigation, etc., as well as possible inherent susceptibilities to disturbance, and since each family is planted nowadays in a compact block, some entire families suffer more severely than others from such unfavourable environmental conditions. Accidental selection is therefore operating. The records show, in general, only the number planted in the Nursery, but observations have shown that subsequent mortality in the field is variable, often high, e.g. in the 1959 nursery from 0 to 33%! Mortality levels are higher in bunch-planted seedlings, giving a range of 1.6 to 3.5 of the 5 originally potted being recognizable at maturity. This unrecorded mortality can clearly bias the conscious selection rates, e.g. in the above series true selection rates from the Nursery were 0.2 to 9.9% higher in singles, 3.8 to 38.3% higher in bunches, than the rates ignoring mortality. However, in constructing Fig. 2, two seedlings per bunch surviving at the time of selection have been assumed in all families where a count was not made.

When McINTOSH first used the present selection schedule, seedlings of each
family were planted in several dispersed blocks in the Nursery, and single stools of all families were completely randomized in the First Year Trial. As the number of seedlings in the programme increased the problems of identification became acute, and families are now kept in compact blocks in both trials, and also in the propagation plots, and are therefore more likely to be grown in toto in particular environmental oases. Family merit becomes confounded with environmental luxuriance, and only by complete randomization is this avoided. If interest lay only in variety selection all seedlings could be randomized together as one population and parentage disregarded. However, since it is desired to compare crosses and to determine varieties potentially useful as future parents, knowledge of each seedling's pedigree is necessary.

The grouping also introduces bias at the time of selection. McIntosh originally inspected stools in groups of 12 in the First Year Trial, this being a compact area within which non-genetic variation would theoretically be fairly small, and which could, as a group, be graded independently of the next group of 12 stools. Seedlings of diverse parentage were considered simultaneously, and in ignorance of their parentage could be selected with minimum bias. More recently each family has come to be treated as a unit. These vary in size, but instead of accepting that small samples will sometimes contain few or none worthy of selection, the standard has tended to be lower for small groups and more rigorous for large groups. Taking the best from each family is not statistically sound, particularly when the means and variances are not known. An absolute standard is set after making allowance for environmental variation, and should apply to samples of all sizes. Since 1957 the First Year Trial has contained stools of a standard variety every fifth stool, in an attempt to attain some absolute standard for selection, though this has its dangers if the clone used has particular adaptations. Large and small families can now be selected with more or less equal intensity.

Juice quality has not always been tested early enough in the season for the maximum brix variation to be detected; only in the last four years has testing been carried out at a time when large differences are present, and selection has been correspondingly more rigorous and, it is hoped, more accurate.

Since rainfall is a dominant factor in production at the site used for these trials, having varied from 41 in. to 100 in. over the years concerned, it was suspected that rainfall received by the trials might have influenced the overall selection rate. The decline in selection rate with years (taken as linear) and any association between the rainfalls received by successive stages of one cohort (possible association in rainfall 'cycles' — again taken as linear) were allowed for by partial regression analysis. None of the rainfall totals for the respective stages showed significant correlation with overall selection rate, and rainfall therefore seems to produce no simple bias on the rate, though it may have affected the efficiency, since it markedly affected the expression of growth characters.

Close spacing, to the extent of bunch planting, has been advocated as a means of 'natural selection' for vigour; but our experience with bunches of known clones, of commercial performance in pure stands, is that some are suppressed to the extent that they would not be selected (e.g. B. 41227). At the other extreme, wide spacing to allow full expression of potential tillering, etc., has been advocated for single-stool trials. Since the variability between stools of a clone seems to be much the same
at quite wide limits of spacing – of the order of 50% coefficient of variation for weight at 5 ft. x 5 ft. and at 5 ft. x 2 ft. – efficiency is not altered. But in general, unless the industry uses various spacings, selection at the spacing used conventionally seems the best compromise, at least in these early stages of selection. The universal problem of selection from the single stool remains, and becomes more frightening with a knowledge of the high variability of a single clone in the same field, of the severe competition effects and of low correlations between successive stages. An attempts is being made to circumvent this in two ways. Firstly, by recording the stool twice, e.g. at 7 months and again at maturity, or as plants and again as ratoons, it is almost possible to achieve replication. Secondly, by paying little attention to the complex character yield, and concentrating on other attributes, particularly juice quality early in the season, it is hoped that with experience it will be possible to attain the same or a lower selection rate but only with reference to characters showing higher heritabilities than yield.

The review above explains and measures some of the fluctuations since 1935, and outlines some of the developments in operating these early stages of selection since that date. With these in mind statistical comparisons between families and years using these records are not very precise.

**FACTORS DETERMINING THE SELECTION RATE**

There are two separate aspects of selection to be considered:

*Sampling of the total variation.* How many seedlings are needed to give a mean approximating within close limits to the true family mean? On the basis of a small sample, what probability is there that further samples will contain selections in quantities sufficient to warrant repeating the cross?

A cross repeated for several years produces several samples of the population of different sizes. Assuming year differences (environmental and human) do not affect the merit used as the selection criterion, small samples will be expected to vary widely around the true mean, some containing a few selections, others none. Large samples will be less variable, and will more closely fit the true mean. The sampling errors of different numbers of seedlings are shown as fiducial limits in Fig. 3. The expected selection rate affects these limits: three examples are shown, a good population with 50% merit as might be found in the Nursery, a poor family with 5% merit, and a very poor family with 0.5% merit. The errors are asymmetrical when selection is from only one tail of the distribution and at low frequency. Low selection rates in small families will give rise to a proportion of zero selections; this frequency also has fiducial limits though they are not shown in Fig. 3. Such diagrams are applicable to any sample expected to be more or less normal, e.g. the Nursery and First Year Trial, and to overall selection rates over any number of stages based on one or other of these samples.

Fig. 4 shows the overall selection rates in 5 repeated crosses. Taking the largest population in each case as the best estimate of the true selection rate, fiducial limits at $P = 0.05$ have been sketched in. As can be seen, selection has erred on the generous side in three crosses, but does not fall outside the lower fiducial limit. Selection has been cautious rather than over-confident.

In Fig. 3a, if a small sample falls above the fiducial limit shown, it has less than a
Fig. 3. Upper and lower confidence limits (P = 0.05) expected with various sample sizes, at three levels of selection: 1 in 2, 1 in 20, and 1 in 200. The shaded portion represents the expectation of zero selections being found.

Fig. 4. Observed selection rates from 3 repeated crosses with samples of different sizes. Confidence limits corresponding to the selection rate of the largest sample are sketched in. Ordinate: percent selection for Second Year Trial; abscissa: number of seedlings in sample (log scale).
I in 20 chance of being part of a population with 50% merit. Such a sample suggests the cross may be repeated with a good chance of the mean being greater than 50%, this repetition being planned on statistical grounds.

Setting the standard of selection. What proportion of the sample contains all the genetically good varieties? What allowance should be made for environmental variation? Both these questions have confidence limits attached; the possibility of missing a variety is inescapable, though the probability of its happening may be very low. Efficient selection is a compromise between missing the occasional good variety and selecting at an overall rate low enough to increase the mean, and decrease the variance in the next stage by a significant margin. The facilities must to some extent dictate the confidence levels applied.

If merit is measured in points and 100 points are taken as an acceptable score, then in the absence of environmental variation the selection rate is an area under the right-hand tail of a normal curve divided at this point on the abscissa. The group of seedlings selected have a mean score corresponding to half the selection rate. The selected seedlings will vary round this mean for environmental as well as genetic reasons, and, among those having a genetic merit of 100 or more, some may have phenotypes below 100, with decreasing frequency the higher the deviation. The environmental variance to which they are subject could be estimated from a plot of a clone of similar size to the seedling family growing nearby. Alternatively, an arbitrary correction could be made assuming a coefficient of variation due to all non-genetic causes of 20%: it should not be more than this for the combination of characters used as a measure of merit in single-stool trials, though is often higher than this for yield characters in clonal plots. Applying this figure, the lower fiducial limit to the mean of the selected group at any chosen level of confidence can be calculated, and this taken as the border line for selection in place of 100 points. The significance level applicable to a whole programme might be ideally as low as 0.000005, this being approximately the rate of occurrence of new varieties in our programme, but clearly this could not be applied in practice; probably I in 100 is as high as we can feasibly go if selection rates are to achieve a real advance between stages.

A PROGRAMME BASED ON CONSIDERATIONS OF PROBABILITY

McIntosh at the B.W.I. Sugar Cane Breeding Station, generally grew 200-300 of a new 'experimental' cross, discarding the remainder germinated. Such samples were not scored – means and variances were not estimated – but if the family looked good for either reason, the cross was repeated once or several times, and larger families raised and selected if possible. Such proven crosses occupied a good proportion of the time and facilities in the crossing season. More of each cross are now grown, in the hope that exploration in one year can be sufficiently thorough to avoid repetition of all crosses except those where few seedlings germinated. Effort in the breeding season can then be diverted into making many more of the innumerable combinations possible.

Where fewer than 200 seedlings germinate, however, the whole sample can be scored to give the best available estimate of the mean and variance of the population, so that repetition can be planned on statistical lines, for all but uniformly poor families.
The procedure could be as follows:

1. Score the entire family in the nursery stool-by-stool for merit on a scale so that 100 points is the minimum acceptable rating but not the maximum possible. This can be quite accurate if undertaken by one person who is experienced and who knows what weights to assign various characters, and who is consistent. Or the figure could be the average of several inspections by different people or at different times. From these calculate the mean and variance.

2. If available, score a clonal plot on the same scale and calculate mean and variance.

3. Correct the mean and variance for sample size. The sampling variances are \( s^2/N \) and \( 2s^4/N - 1 \) respectively, where \( s^2 \) is the variance and \( N \) is the number of seedlings. The figures really needed are the upper fiducial limits to the statistics at a conventional level of significance, say 0.05, and these could be tabulated conveniently. The practical range in the mean likely to be of interest lies between 40 and 140, with variance between \( 4M \) and \( 20M \). These limits have been observed with respect to weights in families in the Barbados nursery. Correct the figures from the clone in a similar way.

4. Using the corrected mean and variance, solve for \( x \) in the following equation:

\[
M = 100 - x(s)
\]

where \( M \) and \( s \) are the mean and standard deviation respectively, and \( x \) is the probit value (minus 5) corresponding to the value 100 points. The value of \( x + 5 \), when entered into a table of probits, gives the area of the tail under the curve below 100 points, i.e. the percentage which should be rejected, whence the percentage selection, \( q \).

5. Calculate the coefficient of variation \( e = s/M \) from the clonal data.

6. The mean of the selected group is to the right of 100, at a point corresponding to a selection rate of \( 1 - q \), for which the probit value can be found. Subtract 5, and enter this as \( x' \) into the equation \( M = y - x' (s) \) where \( y \) is the mean of the selected group. Solve for \( y \).

7. The standard error of \( y \) is estimated as \( cy \) or \( 0.2y \) if no clonal plot is available. The lower fiducial limit to \( y \) is then \( (y - cy) \times t \) at the selected level of confidence for \( Nq - 1 \) degrees of freedom.

8. The lower limit to \( y \) is the revised standard to which selection should be made which will be lower than 100 points. Let this be \( y_o \). Then the selection rate is calculated from the equation:

\[
M = y_o - x(s)
\]

as in (4) above.

9. For this estimated selection rate, nomograms similar to those in Fig. 3, or the tables on which they were based, could be used to provide:

(a) the lowest and highest selection rates which could be expected in other small samples; and, if the cross is to be repeated;

(b) the lowest number of seedlings which should be simultaneously grown and selected to give the minimum selection rate with high statistical confidence. Small batches of seed could then be stored and accumulated until the requisite number of seedlings could be grown together. Selection rates from low mean, low variance families demanding over 1,000 seedlings are probably uneconomic.
EXAMPLES OF SELECTION RATES FROM FAMILIES WITH VARIOUS OBSERVED MEANS AND VARIANCES ALLOWING FOR SAMPLE SIZE, ANDignoring non-genetic variation or allowing for it at two levels (Fiducial limits at $P = 0.05$)

<table>
<thead>
<tr>
<th>Coefficient of non-genetic variation</th>
<th>Observed mean</th>
<th>100 seedlings</th>
<th>200 seedlings</th>
<th>Observed variance</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>62</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0</td>
<td>88</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0</td>
<td>91</td>
<td>35%</td>
<td>42%</td>
<td>40%</td>
</tr>
<tr>
<td>0.14</td>
<td>62</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0.14</td>
<td>88</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0.14</td>
<td>91</td>
<td>99%</td>
<td>97%</td>
<td>97%</td>
</tr>
<tr>
<td>0.20</td>
<td>62</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0.20</td>
<td>88</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>0.20</td>
<td>91</td>
<td>100%</td>
<td>100%</td>
<td>96%</td>
</tr>
</tbody>
</table>

With further work it should be possible to simplify the calculations into a few tables. Some typical results are given in Table II.

It is a false economy to carry a programme larger than can be selected close to the appropriate rates indicated by the above procedure, because, if every seedling is unique, every genotype's chance of selection diminishes. Whole programmes where the first selection from single stools is no more than $z\%$, assume impossible levels of efficiency, and are wasting much good variability unless family sizes are very large, or unless the populations are far inferior in mean and variance to those we are used to seeing in the average programme. Finney even suggests a random discard of a proportion of the seedlings so that the remainder can be selected efficiently with the facilities available. In such a reduction it would be important not to bring family size down below, say, 1,000 seedlings, but rather discard entire small families. Further, in cane there are usually some, if not sufficient, major defects to make conscious selection preferable so long as there is room to grow the seedlings until they produce stalks.

Evidence has been obtained that there are upper limits to family size beyond which it is not useful to go. Clearly once a large population has been raised, assuming no adaptive selection is being practised at the first stages, it is wasteful of effort to rear and select from smaller samples of the same family if these have much wider fiducial limits. Thus we would not raise further samples of the family B. 37161 x B. 4098 smaller than 500 seedlings (Fig. 4e). When the variance is very low, i.e., genetic variation is low, a modest number of seedlings explores the variation exhaustively and growing more seedlings will lead only to a confusing set of varieties differing by little more than the environmental variance. However attractive the family, selection is difficult and ineffectual. These are statistical limits and demand measurement of means and variances to be accurate. The contention that it is essential to have very variable families in order to select easily is true when the plant breeder is prepared
TABLE III

RECORDS OF REPEATED CROSSES YIELDING COMMERCIAL VARIETIES AND SELECT SEEDLINGS, B. 37' TO B. 61' SERIES

 Varieties selected from Percent Percent Times Total se-
Cross made seed-
1st 2nd 1st 2nd selection lection (years) selection mean rest
1,000 1,000 2,000 up

<table>
<thead>
<tr>
<th>Cross</th>
<th>Times made</th>
<th>Total seedlings</th>
<th>Varieties selected from 1st 2nd selection</th>
<th>Percent</th>
<th>Percent selection mean rest</th>
</tr>
</thead>
<tbody>
<tr>
<td>Co. 421 x B. 34104</td>
<td>4</td>
<td>2,388</td>
<td>nil B. 5121 nil</td>
<td>0.6</td>
<td>1.0</td>
</tr>
<tr>
<td>Co. 421 x B. 4098</td>
<td>10</td>
<td>3,504</td>
<td>B. 4738 nil *</td>
<td>1.1</td>
<td>0.7</td>
</tr>
<tr>
<td>Co. 421 x B. 4111</td>
<td>8</td>
<td>13,016</td>
<td>B. 4737 B. 51298 nil</td>
<td>0.8</td>
<td>0.8</td>
</tr>
<tr>
<td>P.O.J. 2878 x B. 34104</td>
<td>6</td>
<td>1,799</td>
<td>B. 47409 - -</td>
<td>0.5</td>
<td>0.6</td>
</tr>
<tr>
<td>B. 349 x Co. 290</td>
<td>6</td>
<td>1,184</td>
<td>B. 4908 - nil</td>
<td>0.8</td>
<td>0.9</td>
</tr>
<tr>
<td>B. 349 x B. 3337</td>
<td>7</td>
<td>2,408</td>
<td>B. 4915 nil *</td>
<td>1.1</td>
<td>0.8</td>
</tr>
<tr>
<td>B. 349 x B. 3703</td>
<td>4</td>
<td>3,744</td>
<td>B. 5009 nil *</td>
<td>0.6</td>
<td>0.6</td>
</tr>
<tr>
<td>B. 33207 x P.O.J. 2878</td>
<td>16</td>
<td>7,546</td>
<td>B. 54109 - -</td>
<td>1.0</td>
<td>0.8</td>
</tr>
<tr>
<td>B. 33218 x B. 4098</td>
<td>5</td>
<td>1,057</td>
<td>B. 54297 nil -</td>
<td>1.0</td>
<td>1.2</td>
</tr>
<tr>
<td>B. 33447 x B. 3337</td>
<td>7</td>
<td>5,102</td>
<td>B. 54337 - nil</td>
<td>1.4</td>
<td>1.2</td>
</tr>
<tr>
<td>B. 37061 x P.O.J. 2878</td>
<td>13</td>
<td>4,115</td>
<td>B. 4362 nil B. 4725*</td>
<td>2.3</td>
<td>0.9</td>
</tr>
<tr>
<td>B. 37061 x B. 4098</td>
<td>15</td>
<td>11,137</td>
<td>B. 4730 nil B. 50114 -</td>
<td>1.4</td>
<td>0.7</td>
</tr>
<tr>
<td>B. 37172 x B. 4098</td>
<td>5</td>
<td>1,883</td>
<td>B. 4740 - -</td>
<td>1.3</td>
<td>0.9</td>
</tr>
<tr>
<td>B. 39255 x B. 34104</td>
<td>9</td>
<td>376</td>
<td>B. 45151 - -</td>
<td>2.4</td>
<td>0.7</td>
</tr>
<tr>
<td>B. 40722 x Co. 290</td>
<td>7</td>
<td>4,034</td>
<td>B. 45152 - -</td>
<td>2.4</td>
<td>0.7</td>
</tr>
<tr>
<td>B. 4154 x B. 41227</td>
<td>2</td>
<td>1,007</td>
<td>B. 4725 nil -</td>
<td>1.5</td>
<td>0.8</td>
</tr>
<tr>
<td>B. 45341 x B. 3337</td>
<td>2</td>
<td>606</td>
<td>B. 4725 - -</td>
<td>1.5</td>
<td>0.8</td>
</tr>
<tr>
<td>B. 4870 x B. 41227</td>
<td>4</td>
<td>1,877</td>
<td>B. 51414 - -</td>
<td>0.5</td>
<td>0.7</td>
</tr>
</tbody>
</table>

- = not attained; * = some varieties still under test in trials;
† = selection rate (2nd year trials % nursery) of all crosses in the programme for the same years, excluding those selected only for parent material, e.g. early nobilizations.

Table III summarizes the results of 'proven' crosses and other repeated crosses which have given rise to commercial and near-commercial varieties in the West Indies. All the populations were raised and underwent preliminary selection in Barbados, though some of the varieties are used only in other parts of the area.

As expected, most of the crosses are above average in merit when compared with the programme as a whole, but not markedly so. In most cases the good varieties were selected in the first 1,000 seedlings raised, in many cases from the very first population examined. Of the varieties already extensively grown, B. 49119 is the...
only exception to this. Small families limited by infertility, notably B. 39254 x B. 34104, are the only ones where further exploration is needed, but the effort needed to obtain further small samples might be put to better use with other crosses.

Table IV examines the selection of the important varieties in more detail. B. 429 is not considered because the male parent of this variety was discarded and the cross was never repeated.

**TABLE IV**

<table>
<thead>
<tr>
<th>Variety</th>
<th>No. in Nursery</th>
<th>Selection rates as % of Nursery First year</th>
<th>Second year</th>
</tr>
</thead>
<tbody>
<tr>
<td>B. 37161</td>
<td>200</td>
<td>67</td>
<td>3.5</td>
</tr>
<tr>
<td>B. 4098</td>
<td>128</td>
<td>77</td>
<td>1.8</td>
</tr>
<tr>
<td>B. 41200</td>
<td>420</td>
<td>67</td>
<td>2.9</td>
</tr>
<tr>
<td>B. 4362</td>
<td>49</td>
<td>71</td>
<td>1.0</td>
</tr>
<tr>
<td>B. 45151 &amp; B. 45152</td>
<td>60</td>
<td>83</td>
<td>5.0</td>
</tr>
<tr>
<td>B. 4744 &amp; B. 4746</td>
<td>293*</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>B. 4747</td>
<td>494*</td>
<td>--</td>
<td>0.6</td>
</tr>
<tr>
<td>B. 4728</td>
<td>96</td>
<td>63</td>
<td>3.1</td>
</tr>
<tr>
<td>B. 49119</td>
<td>179</td>
<td>95</td>
<td>2.8</td>
</tr>
<tr>
<td>B. 52017</td>
<td>304</td>
<td>53</td>
<td>1.6</td>
</tr>
<tr>
<td>B. 52142 &amp; B. 54277</td>
<td>309</td>
<td>38</td>
<td>1.3</td>
</tr>
</tbody>
</table>

* Bs series: seedlings planted direct into short-season First Year Trial.

The high first-year selection rates indicate that the families were all above average in the years concerned. The overall selection rates were not consistently different in these years from other years in which the crosses were grown, but in only two cases were these rates lower than the year average for all families. All these varieties were selected from quite small family samples, but with a system of generous selection in the earliest stages.

While the variation displayed by 1,000 seedlings cannot have universal or profound significance, it appears that it could have usefully been the upper limit to some of the crosses listed. Some possible reasons for the real and perhaps inescapable limits to phenotypic variation follow:

1. The parent phenotypes may differ markedly, but the genetic constitution may not be as heterozygous as we think, particularly in desirable characters. So much of the world’s cane has ancestry traceable back to the two clones P.O.J. 213 and P.O.J. 287. In Table III crosses involving two ‘species’, e.g. B. 39254 x B. 34104 (sinense x Co. 281) are among the best, probably having high mean and variance.

2. The amount of recombination may be limited by the pattern of chromosome pairing. Since most of the parents are interspecific hybrids with more than one complement of S. officinarum chromosomes, chromosome affinity – the preferential pairing of these chromosomes with each other leaving the spontaneum or other ‘wild’ chromosomes unpaired – may leave most of the variation from the wild parent to be inherited as blocks of genes, whole chromosomes passing to the gametes in irregular numbers, sometimes with no reduction in fertility.

3. The expression of heterosis, and even of dominance and additive variability, may
be limited by the environment. The B.W.I. Central Sugar Cane Breeding Station serves a wide area and accordingly uses, as far as possible, characters constant in all normal conditions. The variation in growth is of course not entirely ignored, and might be greater if the nursery was grown elsewhere or in another year.

4. There are physiological limits to the accumulation of sucrose, whatever the genes might dictate. In development, some characters may be intimately related so that correlated response to selection cannot be broken by genetic recombination.

CONCLUSIONS

Fig. 2 shows that repeated crosses — defined as 500 or more having been grown in earlier Nurseries — are on average a little better than experimental crosses, but by a smaller margin in recent series. Repeated crosses are giving diminishing returns both in terms of new varieties and information. It is therefore concluded that a programme should consist of as many crosses as possible, to explore more fully the diversity we are fortunate to have at our disposal from recent collecting expeditions. It is possible to guess at the additive variation by inspection of the parents, but until crosses are made, the more important 'specific combining ability' component is not known. It remains to be seen whether the new S. spontaneum clones carry much useful variation different from the Pasoeocean and Coimbatore Local, and whether it can be released by conventional breeding methods. Existing lines are being more intensively examined by generationwise assortative mating — a form of inbreeding — and polycrosses among sib progeny from some of the old proven crosses (promising, for example, in those from Co. 421 × B. 41211) in place of repeating the crosses themselves. Fig. 5 indicates the trend towards the use of younger parents now well in hand at the B.W.I. Central Sugar Cane Breeding Station. A more systematic programme involving some inbreeding will be the quickest way to release the variability from this highly selected parent material. In all cases attempts are to be made at the B.W.I. Station to examine a large population in the first season, and to avoid repetition unless the population is small, when its mean and variance may be used as a guide on the probability of success and the number of seedlings required.
1. Selection percentages derived from the first three stages of selection are considered useful as a measure of family performance and worth.

2. Trends in selection rate in the Barbados programme are examined, and sources of inaccuracy noted.

3. Efficient selection has two aspects:
   (i) good sampling at one time of the total genetic variation of the population; and
   (ii) a good rate of genetic advance between stages, involving knowledge of genetic and environmental variation.

4. Selection rates applied are a compromise between missing the occasional good segregant and obtaining a reasonable rate of advance, particularly in single-stool trials.

5. The value of repeating crosses beyond quite low limits is debatable in the light of Barbados results. Possible limitations to the theoretically very wide variability are discussed.

REFERENCES


DISCUSSIONS

J. T. Rao (India): Do you make selections in the seedling stage?

D. I. T. Walker (Barbados): Yes, when the seedlings are about 8 months old.

Dr. Rao: In India, it has been found that the expression of characters in seedlings and in subsequently vegetatively propagated material is different. All seedlings except those of spontaneous type, which are discarded, are planted from cuttings before selection.

Mr. Walker: This was the practice in Barbados in 1936 but at present early selection is 20 or 30% and based on characters which correlate in seedling and vegetative growth.

E. F. George (Mauritius): How many years were crosses repeated to give 13,000 seedlings?

Mr. Walker: Approximately 8 years. One cross means one year in our system.

Mr. George: How is it that after the initial selection of two commercial varieties, 10,000 seedlings did not give one approaching that standard?

Mr. Walker: The standard of selection had probably risen over the period and so too had the performance of commercial varieties.

J. N. Warner (Hawaii): Are you able to use the procedure indicated on all new crosses?

Mr. Walker: We shall try to use the method on small samples of seedlings when we wish to know whether it is worth repeating a cross.

J. Tapay (Philippines): If a cross is not to be repeated, what is the number of seedlings you would advise to have a useful range of variation?

Mr. Walker: We consider 1,000 to be enough.

Dr. Rao: In India, many proven crosses are repeated. Certain combinations have been particularly successful for special regions.