des trois classes d'estimation de la forme des tiges d'après la flexuosité.

Le mélèze hybride de Dunkeld et le mélèze du Japon ont eu la croissance la plus rapide en hauteur et en diametre, et, de plus les mélèzes de Dunkeld sont nettement supérieurs en ce qui concerne la forme des tiges. Les mélèzes d'Europe d'origine écossaise dépassent legerement les mélèzes de Silesie pour la vitesse de croissance et leur sont nettement superieurs pour la forme des tiges. Les melezes d'Europe des Dolomites (Alpes italiennes) poussent ici tres lentement, de même que le *Larix dahurica*.

Les résultats de cette expérience sont confirmés jusqu'à un certain point par des observations préliminaires faites sur des plantations ou d'autres parties de l'Etat, plantations etablies a partir des mêmes graines. Si ces plantations donnent dans un avenir proche des resultats comparables, la valeur de deux de l'expérience s'en trouvera renforcee.

Literature Cited

(1) Aird, P. L., and Stone, E. L.: Soil characteristics and the growth of European and Japanese larch in New York. J. For. 53: 425–429 (1955). — (2) Baldwin, Henry I.: Provenance tests of common exotics in the Northeast. First Northeastern Forest Tree Improvement Conference, Williamstown, Mass., 1953, pp. 33–38. — (3) Cook, David B.: Characteristics of the Dunkeld larch and its parent species. J. For. 40: 884–885 (1942). — (4) Hunt, Stuart S.: A study of European larch plantations confined to certain localities in New York State. Unpublished manuscript, on file with the New York State Conservation Dept., Albany, N. Y., 1930. — (5) Hunt, Stuart S.: European larch in the northeastern United States. Harvard Forest Bull. 16, Petersham, Mass., 1932. — (6) Littlefield, E. W.: Comparative height growth of different larches five years after planting. Notes on Forest Investigations No. 27. New York State Conservation Dept. (mimeographed), 1939. — (7) Littlefield, E. W., and Eliason, E. J.: Observations on a plantation of Dunkeld hybrid larch in New York. J. For. 36: 1188–1192 (1938). — (8) Littlefield, E. W., and Eliason, E. J.: On the testing of somc conifer species and types in New York State. First Northeastern Forest Tree Improvement Conference, Williamstown, Mass., 1953. 38–41. — (9) McComb, A. L.: The European larch: its races, site requirements and characteristics. Forest Science 1: 298–318 (1955). — (10) Paton, Robert R.: Dunkeld larch in Ohio. J. For. 42: 452–453 (1944). — (11) Schober, R.: Die japanische Lärche. J. D. Sauerländer's Verlag, Frankfurt a. M. 1953. — (12) Stoeckeler, J. H.: European larch seed sources compete successfully with tamarack during 5-year test in northeastern Wisconsin. Technical Notes No. 440. Lake States Forest Expt. Sta., U. S. Forest Service, 1955.

The Design, Layout and Control of Provenance Experiments

By M. V. Edwards

Forestry Commission Research Branch, Edinburgh, Scotland

(Received for publication July 15, 1956)

A number of experiments to compare the growth of trees of one species grown from seed of different provenances had been laid out in Scotland before 1930. They were of a very simple type, consisting of a single plot of plants from each seed source, and though the ground on which they were planted no doubt seemed reasonably uniform at the time of the planting, it has not always proved so in practice. In such experiments there is the unavoidable fact that one plot must always be more exposed to prevailing winds, while the others shelter in its lee, or alternatively one is more exposed than others to damage from other directions. These cast some doubt on the validity of the results obtained.

These difficulties can be overcome by modern statistical techniques, in which replication of the plots of each provenance plays the leading part. Scottish experiments from 1930 onwards employed this device, but the next essential, that of randomising the plots within the experiment, did not at first invariably follow. Only when these two principles are observed is it possible to segregate the variance between the plots, in respect of whatever characteristics are being assessed, into errors due to differences in the environment and errors due to variation between the provenances on trial. This is necessary in order to determine whether the differences between the two sets of errors are likely to be significant, or only due to chance. A well designed experiment is therefore essential.

Some notes on the lessons learned from the conduct of old experiments have been collected, with a view to the guidance of future experimental work.

The Object of the Experiment

The term "provenance experiment" is used to describe an experiment with seed from stands of trees, either native to their place of growth or introduced from elsewhere. Provenance experiments are to be distinguishable from "progeny trials", which term is used to describe trials of the progenies of individual mother trees (The paternal parent may or may not be known) (c. f. Veen, 1954b). Strand (1952) recognizes three classes of progeny test: "I. Individual seed trees; — II. Several individual seed trees in each of a number of stands; — III. The progeny of groups of seed trees in each of a number of stands . . ." but even in the last case he refers to comparisons between the progenies and the mean values for the groups of selected trees, and not the mean for the parent crop. It is this distinction between selected trees and the whole stand which makes the difference between progeny trials and provenance trials. The latter refer to comparison of seed from complete crops.

The precise objects of the experiment must be clearly defined and the kind of experiment will vary according to the definition of the object, but in general, most experiments are required to compare the growth of stands of trees. They are *crop* experiments and aim at the raising and comparison of different crops, not individuals. They will accordingly be conducted along normal silvicultura! lines and the best practical methods of raising crops for the normal objects of management will guide the tending of the experiments. This general rule of conduct needs to be written into the working plan for the experiment, and if deviations from it are required by the particular object of any experiment, they must be clearly specified.

The Seed Collection Stage

It follows from the general object of the experiments that seed must be collected from all the seed bearing trees in a stand if the parcel of seed is to be a sample of the normal products of the stand. Sometimes the collection of equal amounts of seed from each parent tree is specified (ANON., 1952) but this either gives undue weight to the trees which are less prolific seed bearers, or may in practice have the opposite effect, because when a minimum per tree is applied, seed will tend to be collected only from trees bearing a great deal of seed. On the other hand, there is a natural tendency to collect seed for an experiment from the best trees only. It may be remarked that different trees bear differently in different seasons, and that therefore successive collections over several years should be tested, but, while this is true, if this degree of precision is required, studies of the progenies of individual trees had better be undertaken.

It is to be assumed that the stands will have been subject to the normal treatment for the removal of defective and inferior stems, and with this proviso, seed should be collected from trees as it comes, i. e. in the proportion in which it is borne, and only this will be a fair sample of the capacity of the stand and of normal seed collections.

It is rarely possible to collect seed from a species throughout its whole distribution in one season, and for this reason, many past experiments have not been as fully representative as they might have been. For many species, cold storage of seed is now possible, and where this is so, seed should be collected and stored until the required provenances have been obtained. This involves considerable forethought and planning, well worth while for a long-term experiment.

A most important record is the description of the parent trees from which the seed is obtained. The location of the stand, data concerning the environmental factors such as climate, elevation, topography, soil, vegetation and a description of the mother crop in respect of its age, size and habit are necessary. The amount of detail recorded varies with the objects of the particular experiment in view. It should be related to the future assessments of the experiment as described below, and may require elaborate explanations either by measurement, description, pictorial design, photograph, etc. If the collection is from the ground or from felled trees *en masse,* then the seed necessarily comes from trees of all sizes and qualities, and this fact must be recorded. If collected from standing trees, how this was done must be explained and if any selection of trees was exercised, full details must be given.

If the fruit or seed contains a proportion of poor quality material which would be rejected in normal practice, the same standard of rejection may be applied, and explained in detail, otherwise the fact that no grading either by size or quality was done must be stated. It is not sufficient to leave the exact procedure in doubt, as practice varies in different places and times, and what is normal here and now, may appear unusual elsewhere or later on.

The Nursery Stage

The size, germinative energy and capacity of the seed will first be determined and recorded. It may then be found that if all the seedlings are to have an equal chance of growth in the seed bed, different amounts of seed must be sown per unit area of bed. (See Appendix I.)

Experiments have shown that the difference caused by raising the seed in different nurseries, or the use of different ages and types of plants, can be much greater in the early years than the differences due to seed provenance. The former differences can persist for at least 20 years or until the trees reach the height of 30 to 40 feet, whichever is the longer period, and maybe more. (EDWARDS, 1954, and in press). Later the inherited differences will no doubt always tend to become dominant, but it is evident that if genuine differences between provenances are to be detected, then the seed must be sown in a replicated and randomised experiment in the nursery.

This is the first point at which the experimenter is faced with the problem of deciding between the equal treatment of all provenances, or differential treatment in order to obtain equality at a later stage. No doubt the purist would insist that from the strict experimental point of view, there should be no differences in the treatments, and that adherence to this principle will quickly and surely demonstrate differences between provenances. This is so, but it will also eliminate some lots from the experiment at an early stage. Also the effect of using one sowing density for all treatments would be to penalise the better seed, as the resulting plants would be overcrowded in the seed beds. Seed must be sown so as to produce, as nearly as possible, equal numbers of plants per unit area of seedbed.

Some experimenters have gone further, seedlings being thinned by hand to the required density after the danger of damping-off diseases had passed (ANON., 1952), but this is likely to penalize the better-germinating seed lots by removing the most vigorous plants and it is better to allow the density of the plants to vary to the extent imposed by the density of sowing.

All the plants required for one forest experiment must be raised in a single nursery, but if several forest experiments are contemplated, as will always be desirable when the reactions of provenances to different environments are under enquiry, separate nursery experiments are better. Several forest experiments can be supplied from each nursery to save the complications of a separate nursery experiment for each forest, and division of the seed into two and sowing in two nurseries is the minimum to ensure against loss by mishap.

The sites of the experiments will of course be chosen with due regard for the uniformity of all the environmental conditions, and either typical of the optimum conditions for raising plants, or otherwise if so required by the objects of the particular experiment, e. g. if a trial of the frost-resistance of the provenances is required, then a frosty nursery site should be selected. In each nursery the best local practice will be followed at the discretion of the local nursery-man. Care must be taken to see that there is no chance of contamination by other seed, either from adjacent trees, surrounding seed beds, by wash due to rainstorms, from delayed germination of a previous crop, or any other likely local factor.

At the end of the first season the number of plants per unit area and the mean height of all the plants will be assessed. Comparison of the number of plants with the number of viable seeds sown may give some indication as to whether differences in the number of plants obtained from that expected simply reflects the normal hazards of error in estimation of germinative energy and capacity, or whether provenances have differed significantly in their reactions in germination and growth. Often the two are hard to separate. The most important comparison between the provenances is usually their mean height, and such differences may be significant and of real importance. Other differences, such as susceptibility to frost or disease, are often assessed not by measurement, but by classifying the severity of the attack and scoring visually on a points system.

The normal measures for weed control, protection against frost, diseases, etc., appropriate to the species and locality should be taken. In all cases each plot should be treated on its merits and if disease attacks one plot, or all plots of one provenance, affected plots should be treated, but not the whole experiment.

The age and type (number of years in seedbed and transplant lines) of plants taken to the forest will vary in different nurseries and must be adjusted to local conditions. In extreme cases it may vary between different provenances in one nursery, and then plants of differing age and type have to be used in one forest experiment. This is to be avoided if at all possible as such differences take time to disappear. It may then be better to make two experiments, taking to the forest the fastest growing provenances one year and the remainder the next, and using one intermediate one as a standard in both experiments. If it is decided to make one experiment, using plants of different age and type, or all one age and type but widely differing size, then this may be the best for a long term result, but early results will not provide a reliable distinction between provenances.

The question of selection of plants for lining-out and forest planting again raises the problem of differential treatment of the plots. In progeny trials, for instance, it will usually be desirable to line out all plants (or a sample consisting of every *n*th plant), every plant that has germinated being taken into account. But the principal object of provenance experiments is to compare the growth of crops of trees from different lots of seed in terms of their ability to produce forests and timber. In this case it is desirable to adjust the treatment of the individual lots in the nursery phase so as to obtain, as far as may be possible, equal crops of plants in the forest or the time of planting. Once this principle is accepted, a number of consequences follow automatically. The experiment divides itself into two separate parts, the nursery phase and forest phase, and the results at the end of the nursery phase are brought to a conclusion. The differences in terms of height, survival, resistance to disease and pests, etc., are compared and the significance and importance of the results discussed. The forest phase then starts from a new datum line, as even a start as possible being ensured for all provenances. In this case some removal or culling of the smallest plants has to be carried out, as is done in normal forest practice. Otherwise, the irregularity in the experimental plots is so great, by reason of the great variation in the different sizes of the individual plants, that they do not produce normal forest crops.

The problems of grading or culling at lining-out and for forest plantings were discussed by MÜNCH (1949) and also by ROHMEDER in his appendix to that work. MÜNCH noted that the differences in growth between plots of better and poorer plants of one seed lot were small and irregular, varying with the site, and ROHMEDER noted that growth was more regular, and differences therefore more apparent, on old cultivated soil than in plots on forest soils. The same differences have been found in plantations in this country on ploughed land, when compared with growth on the same type of land unploughed. RIGHTER (1945) concluded "Selection within progenies according to either seed size or seedling size would have no adverse genetical consequences. Therefore, it is clearly justifiable to obtain any cultural benefits possible by culling the smaller seeds or the smaller seedlings."

Three methods of grading or culling, both for lining-out and for selection of plants for forest planting, may be considered. *(a)* In normal nursery practice all seedlings under a certain size are rejected as too small to line out, but if this is done in a provenance experiment, then the proportion of plants that are rejected varies as the mean heights of the provenances. *(b)* It may be preferable to fix a different minimum size for grading each provenance, as a proportion of its mean height (GALOUX and GATHY, private communication). Thus, in a Douglas fir experiment for example, if the mean heights of the one-year seedlings of different provenances range from 2.0 to 3.0 ins. (5—8 cms.), 0.7 of the mean height of each lot may be fixed as the limit. This will result in roughly 68% of the seedlings of the smallest lot being over 2.0 × 0.7 or 1½ in. (4 cms.) in height and the percentage of seedlings of the largest lot over 3.0 × 0.7 ins. or 2.1 ins. will be about 77%. This is an example from JEFFERS (1955), but figures from actual assessments would be used in practice. The minimum percentage of seedlings over the limiting usable size should not be less than 60 per cent (on the average of all replications) of any one provenance. When transplants are graded for forest planting, the minimum percentage of usability will be much higher and normally the limiting size can be fixed so that 80—90 per cent or more of the plants are usable. *(c)* Alternatively, 70% (or other suitable percentage) of the largest seedlings of each lot can be selected and lined-out, as at Ekebo, (JOHNSSON, 1952) after preliminary check to ensure that the chosen percentage of the smallest lot does not include seedlings below a usable size limit. This method necessitates counting all the plants to determine the number to be culled, and then selecting the smallest plants for culling. Delays in handling the plants are likely to ensue, and it would be preferable to adopt the second method, measuring the height of the seedlings in the ground, determining the mean height and the proportion of mean height below which plants will be culled and rejecting plants below this height as they are lifted. Either of these systems will provide comparable populations without the inclusion of unsuitably small plants, but the minimum sizes used will differ. At the forest stage, if the site for planting is weedy, provenances including the smaller plants will be at a disadvantage, on the other hand if it is exposed and windy, those with only larger plants will be at a disadvantage. The writer considers therefore that *(a)* the first alternative, the normal practical method of grading all lots to one size, provided that a high percentage of all plants in all provenances are included, will provide a better long term test of the value of a provenance in the forest. The sample plan in the appendix is based on this method. When definite grading limits are in use in normal practice, either by height, diameter of stems or otherwise, it will be better to adhere to these limits all through the experiment, recording proportions of each provenance that reach the standard at each phase of the operations; lining out, planting out and so on. From this the percentage of the plants that germinated that reach the final stage in the forest can be calculated.

As far as factors other than size are concerned, it will be agreed that accidentally damaged plants can always be eliminated at any stage. Culling for such reasons as damage by frost, insects, or for the poor form or habit of growth of the plants, requires special consideration. When the damage is noted in the seedbeds or lines, it should be assessed. If the damage cannot be significantly related to any provenance, but appears to be random, then the damaged plants can be eliminated. If significantly related to certain provenances, that fact can be recorded, with full details, and then, to ensure that the forest stage starts off on as evenly comparable a basis as possible, the affected plants eliminated as would have happened in normal practice. Alternatively it can also be agreed to carry them on. letting the affected lots start off at a disadvantage compared to the others. Whichever alternative is adopted must be clearly recorded. Sometimes plants are damaged too seriously for them to remain in the experiment, and in such cases they inevitably have to be culled, when this is done, the percentage should be recorded, and a known lesser proportion of the whole population carried forward to the final forest stage.

The number of plants per provenance available for the forest experiment is limited by the number of plants in the provenance least succesful in the nursery (subject to elimination at that stage of any extremely unsuccessful example). Care must be taken not to reduce the grading standard in order to obtain more plants of any lots that are in short supply. A beating-up reserve must also be retained and re-lined in the nursery, up to 30% or even more of the number sent to the forest, in order to guard against accidents.

Plants from each block of the seedbed experiment should be kept separate and lined-out in the same blocks. If the plants are to go to several different forest experiments, they should similarly be kept separate and a separate nursery block allocated to each forest. Should however any mixing of plants from two or more blocks be required, they must be thoroughly mixed so that the uniformity of the plots in the forest is established. When plants leave the nursery they should be bundled and labelled for each forest plot, the correct number per plot (plus about 5% for accidents) being supplied.

It is rare that a particular provenance suffers so severely in the nursery that it has to be rejected entirely and fails even to enter the forest stage, but the point where the line between complete elimination and the continuation of a reduced proportion of the total population in the experiment has to be drawn cannot be settled beforehand and must depend on circumstances. The fact that one provenance does badly in the nursery is a matter of importance, but it vitiates the future of the experiment and its interpretation in relation to normal practice if provenance experiments carry forward deficiencies which normally would be obliterated before the end of the nursery stage.

The Forest Stage

Plot Shape

The shape of the plot to be used in the forest depends on the objects of the particular experiment in question. If only the early growth or form of the trees is to be studied, then a single line of plants may be used, e. g. as described by Osborn (1931) and Wettstein (1949), who planted the progeny of individual trees in lines alternating with various other seed collections as a standard against which to compare the better or poorer performance of the individual tree progenies. Wettstein replicated this by annual sowings of successive seed crops from the same mother trees. But unless planted very widely, single lines soon tend to interfere with each other, and the more vigorous ones gain an advantage and overtop adjacent lines. Once a small superiority of this kind is established, it is quickly exaggerated, the less vigorous lines suffering from competition and growing less well than under normal conditions, while the more vigorous lines between grow abnormally well owing to the absence of equal competition.

The plots should usually be square in order to minimise the length of perimeter and edge effects and to be as homogenous as possible. If plots are expected to be split for possible later sub-treatments, they may be twice as long as wide, but in that case the size of the whole plot will need to be increased.

Assessment Plot Size

If the experiment is to compare many widely scattered provenances, it is probable that those from much higher or lower latitudes than the experimental site, or from very dissimilar climates, will be unsuccessful, and their growth will either be much slower or they may even fail altogether in the early years. For such experiments plots of 30 to 36 plants (5×6 or 6×6) have been used. By the time the best plots have formed canopy, say in 10 to 20 years, the poorer provenances are clearly differentiated from the rest, being either dead or much less tall. From this time onwards, or even before, the results of such experiments may be misleading as the differences between provenances become exaggerated, as in the case of the lines cited above. Such an experiment is of great use in the preliminary phase as a means of reducing the number of provenances to be tested for their long term qualities. A new experiment must then be started with the provenances which prove successful in the preliminary experiment, and it is then safe to assume that these provenances will all grow reasonably comparably.

For these longer-term crop studies a plot must be of a size that will allow the individual trees to behave and interact as a crop. The final object of a provenance trial is the mature crop, even if short-term trials to eliminate provenances unsuitable for long-term trials have to be undertaken first. The plot must be able to go through the various stages of the crop, suppressing many of the originally planted trees, until a sufficient sample of the mature stand is left..

As Strand (1955) has pointed out, the problem depends on whether the trials concern related degrees of treatment and are unreplicated and will be analyzed by regression. or whether they are laid out as replicated experiments comprising different independent treatments (i. e. provenances) which are directly compared.

Most older references to plot size refer to the unreplicated plot in yield studies, thus Oudin (1930) aimed at 1.0 ha. plots (2.47 acres) for old crops with a minimum of 50 ares (1.24 acres) for the assessment area (placette) proper, and Fritsche (1927) concluded that 0.3 ha. (0.74 acres) was the minimum size. Fabricius *et al.* (1936) recommended plots of not less than 0.6 acres (0.25 ha.) and up to 2.5 acres (1 ha.) in irregular crops. Macdonald (1931) noted that in Britain it was the practice to use plots between 0.25 and 0.5 acre (0.09 — 0.20 ha). Formerly areas of 0.4 — 0.5 acres (0.16 — 0.20 ha.), were aimed at, but in a later replicated thinning experiment (*Picea abies* at Bowmont, Roxburgh) 0.1 acres (0.04 ha.) plots were used (Macdonald, 1932). As provenance experiments concern independent treatments which are replicated, the problem of plot size needs consideration on this new basis.

Within any one plot there is the difference between individuals to be taken into consideration. In any population, though differences between samples of less than twenty individuals are relatively large, they are decreased by increasing the size of the sample from twenty to forty, but beyond that the reduction in sampling error is small. The standard deviation of the mean is inversely proportional to the square root of the number in the sample. The curve (reciprocal of square roots) is given by JOHNSSON (1952). In the early years the variation between individuals is large but thinning gradually reduces the variation towards maturity, and it may be that much less than twenty trees will be adequate. In the case of plantation crops, which may be clonal and thus less variable than forest crops, PEARCE (1953) gives evidence that eight plants and upwards have been found enough in many experiments. Schleswig (1953—54) specified "at least 0.25 ha." and "0.20—0.25 ha." (0.5—0.6 ac.) for long-term observations of yield (apparently replicated) but this size included a surround. BEARD (1954) working in wattle *(Acacia decurrens)* when the crop is mature at an age of eight to twelve years when fifty feet high and 400—700 (mean 544) stems per acre, found that optimum results were obtained with 0.1 acre (0.04 ha.) plots. In younger experiments 0.05 acre plots (0.02 ha.) were sufficient, and he concluded by recommending a plot size of forty trees. STRAND (1955) also arrives at the conclusion that surrounds are not needed (in stands of about the same height), plots of 0.02 ha. (0.05 acres) are sufficient in natural forest, or rather less in plantations.

In Britain a plot of 0.1 acre (0.04 ha.) of Q. C. III *Pseudotsuga taxifolia* may be expected to contain 36 trees, some 60 feet tall (18 m.), at the age of thirty years (HUMMEL and CHRISTIE, 1953). At the age of 50 years there would be about 17 trees with a height of about 90 feet (27 m.). In Q. C. I. there would be only about 12 stems. In *Picea abies,* the Bowmont plots, aged forty-five years and with a height of 50 feet, include a heavy thinning treatment in which the number of trees is already reduced to an average of 21 per plot, and clearly in such an experiment a plot size of 0.1 acre will prove too small for continuation to full rotation age.

While 0.1 acre (0.04 ha.) is evidently suitable for many provenance experiments, in which, for example, the height at final assessments is not expected to exceed about 66 feet (20 m.), a larger plot will be essential in experiments where the trees are expected to reach large dimensions. In such experiments the size of plot as related to the width of the surround, and this is discussed below, but at this stage it may be concluded that the size of plot should be adequate to contain as near to forty trees as possible at the expected termination of the experiment, altough it may well be found that the variation between individuals has been so reduced by repeated thinning that half this number may be adequate (or even less), but data from uniformity trials are not yet available to support this conclusion.

Surrounds

It usually happens that the "edge effect" influences the perimeter of plots either because of the absence of competition from trees outside the plot, or from excessive competition from an adjacent and more vigorous plot. If a gap is left unplanted around a plot, or planted with a less vigorous species, the edge trees in the plot grow abnormally strong and coarsely, and this often results in the trees in the second row inside the plot being eliminated by the edge trees. In short term experiments with no surrounds, no unplanted rows should be left between plots.

For longer term experiments, it is necessary to have a surround to each plot, treated exactly as is the plot itself. It then becomes necessary to distinguish between the plot itself, in which measurements will be made, and which may be called the assessment plot, and the assessment plot plus the surround, which may be termed the experimental plot, and which forms the unit of experimental treatment.

In the early thinning stages a two-row surround to the assessment plot is the minimum necessary, but in later stages the surround should be wider, for in provenance experiments the plots may grow at different rates. For example, when this happens the faster-growing plots stand out above the general canopy level and are exposed to the blast of the wind, while the slower growing ones may tend to form frost hollows. In order to avoid these difficulties, very wide surrounds are needed. Experience suggests that the distance between the assessment plots should be equal to the height of the trees, or in other words, the width of the surround must be half the expected height of the trees at the termination of the experiment.

For example, if the height of the trees in a tenth-acre plot is expected to be about 80 ft. (24 m.) by the time the number of trees is reduced to about 15 to 20, the surround should be about 40 ft. (12 m.) or as the side of a square one-tenth acre is 66 ft. (20 m.) the surround may conveniently be 33 ft. (10 m.) which allows 66 ft. (20 m.) between plots. The area of each experimental plot (assessment plot + surround) then becomes 0.4 acre (0.16 ha.). This may be compared with the recommendations of the British Forestry Commission's Code of Sample Plot Procedure (in the press) for unreplicated assessment plots of 0.3 — 0.5 acres (0.12 — 0.20 ha.) plus surrounds about 33 ft. (10 m.) wide, or a total plot size of about 0.75 — 1.0 acres (0.3 — 0.4 ha.).

This size of plot is somewhat formidable from the point of view of experimental layout, as it implies a block size of several acres. Although it is difficult to find such extensive homogenous areas, the necessity for protecting the plots from the errors attendant upon variations in canopy level or density is quite as important as the increase in site variation which follows upon the use of larger areas for blocks, and statistical considerations based upon the latter factor must not be allowed to over-rule other necessities.

The experimental plot, of which only one-fourth consists of the assessment plot in this lay-out, need not necessarily all be planted with plants of the same provenance at the start. A pure crop for the assessment plot plus a one-row surround is enough. Economy in plants can be effected by use of a "filler" species in the surround, diluting the plants of special provenance with a common provenance of the same species or of a rather slower-growing one. The filler plants are then cut back and removed as the canopy closes until a pure crop of the provenance required is obtained. A plan showing such a lay-out is attached (Fig. 1). This lay-out has even been suggested for the assessment plots themselves (Schleswig, 1953—54) but it has the disadvantage that the crop formed is then abnormal and different provenances may interact differently with the "filler" species.

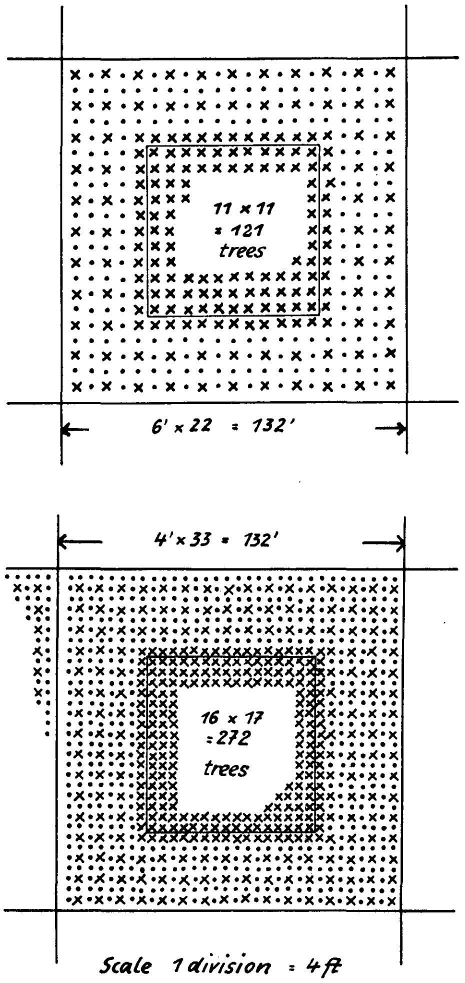

Fig. 1. — Plan of an assessment plot of one-tenth acre with surround thirty-three feet wide.

The area of the experimental plot (assessment plot plus surround) is 0.4 acres (0.16 ha.). The distance between adjacent assessment plots will be equal to the height of the trees when they reach 66 ft. (20 m.) and at this stage the number of trees within the assessment plot may be expected to be (vide HUMMEL and CHRISTIE, 1953): —

	Pinus sylvestris	*Larix*		*Picea*		*Pseudotsuga taxifolia*
		decidua	*leptolepis*	*abies*	*sitchensis*	
No. of trees per plot	25	18	20	37	36	31
Corresponding age Q.C.I	47	35	30	38	25	24
„ „ Q.C.III	75	60	45	55	33	33

At later stages, the trees in the plots will become taller than the width between the plots and the number per plot will be reduced. Examination will be necessary to see if the experiment has to be terminated. If growth is even, it may be possible to reduce the size of the surrounds, and to include part of them in the assessment plots, thus increasing their size to say 0.2 acres (0.08 ha.), and increasing the number of trees in them. Alternatively, if the growth rates of the provenances are markedly different, the full surround will be necessary, but it may be that the trees within the plots are sufficiently uniform to constitute an adequate sample, even with the number remaining in 0.1 acres.

The whole experiment plot of 0.4 acres may be planted pure if sufficent plants are available. If not, economy can be effected by using a "filler" species in the surround, as shown by dots, planted alternate plant by alternate row. At the extreme spacings of 4 ft. or 6 ft. the number of plants would be: —

		Assessment Plot	Surround	Total
4 ft.	Provenance	272	236	508
	Filler species	0	484	484
	Total	272	720	992
6 ft.	Provenance	121	120	241
	Filler speeies	0	200	200
	Total	121	320	441

N. B.: The total number is calculated to allow a blank line between experimental plots to demarcate them clearly.

Conclusion — Experimental Plot Size

There are few replicated experiments of sufficient age to furnish useful evidence from the statistical point of view for the best size of plot. Inadequate consideration has been given in the past to the size of surround required to protect the assessment plot. The question has to be considered primarily from the point of view of the termination of the experiment, and the width of surround then required depends on the difference between the rates of growth of the different provenances. If the differences are small then only narrow strips are necessary, but this cannot be assumed at the start. After determination of what appears to be the optimum assessment plot size, a suitable width of surround must be added and the size of the whole experimental plot determined.

Finally, around the perimeter of any experiment a belt of say 5 rows wide of some suitable common provenance should be planted, both to delimit the area of the experiment, to equalise the exposure of different plots, and to protect it against accidental incursions. Such a uniform belt may also help by indicating any general fertility trend over the area.

Statistical Design of the Experiments

The arrangement of the plots can be made according to many statistical designs. Those most commonly used have been complete randomisation, randomised blocks and Latin squares. Experience has shown that there is almost invariably a fertility gradient or some other gradation in the environmental conditions and that randomised blocks are superior to complete randomisation. Each block may include all the provenances, or in an "incomplete block" design of experiment it may include only a certain proportion of them. It is essential for the success of these techniques that each block is located on as uniform a site as possible. In some cases, blocks have been laid out according to text-book patterns but this only proves satisfactory on uniform sites. In practically all cases it is necessary to chart the area for uniformity of topography, vegetation and soil (if the ground is ploughed, charting both before and after ploughing is especially valuable) and then position the blocks accordingly, arranging their shape to suit the ground. Gaps between them can be filled with one local provenance. "Uniformity" is a relative term, and with short-term experiments using small plots it should be interpreted strictly. With long-term experiments in big plots, uniformity must be considered from the aspect of widely-spaced large trees. Smaller differences, as between herbaceous vegetation types which may make important differences in establishing the crop but which will disappear when the tree canopy closes, have then perforce to be ignored.

Latin squares are frequently difficult to fit in to the rugged Scottish landscape and they have often to be confined to areas with a more even topography so that experiments in randomized blocks are usually preferable. If the number of provenances is large, incomplete blocks and lattices of various types are more suitable, but randomized blocks are proving to have many practical advantages because they are *orthogonal,* i. e., if such an experiment is grouped by blocks, each is the same in respect of treatments; and if grouped by treatments, each is the same in respect of blocks. Also they are *robust,* i. e., comparatively little affected by mishaps (PEARCE, 1953). See example of a plan in Appendix II.

At one time, in Scottish experiments, as in other countries, the device of the "standard race" was used. In this design the provenances were grouped into blocks, and in each block one or more plots of a standard provenance were included. The results of the provenances under trial in the different blocks were compared by adjustment against the standard or by covariance analysis. As an example, the International larch experiment at Drummond Hill forest was planted in 1947 in this way, with the provenances divided into three groups, plots of two standard origins being included in each group. The interpretation of such an experiment has proved to be difficult. Comparisons between two standards, or between a standard and 'other' provenances, and between the 'other' provenances themselves have different standard errors and hence there are multiple differences necessary for determination of significance at the various levels. Moreover the standard provenances themselves may behave differently in different blocks, rendering adjustment difficult and the result of the corrections somewhat dubious.

The number of replications required depends on the variability of the site of the experiment and on the variation between the different provenances, and no hard and fast rule can be laid down as to the number required. It also depends on the number of provenances included in the experiment; as for example, if there are only two, and differences between them are likely to be small, statistical requirements make ten replications the minimum needed. A great deal more experimentation is necessary before reliable decisions can be reached on this matter.

The use of large plots (¼ ha., 0.6 acre) unreplicated and with strips of a standard provenance between and around them "in order to establish the uniformity of the site" has often been made in the past, and is still recommended in Schleswig (1953—54). Although such strips indicate differences in site where they occur they do not afford any method of adjusting data from adjacent plots nor any means of testing the significance of plot differences. A plot must be accepted as uniform, or if not so, then it must be divided and assessments stratified, and error due to site differences between plots can only be overcome by greater replication.

It is also desirable to carry out similar experiments in different localities, so that the interactions of the provenances with their several environments can be studied. This requires duplication of experiments as well as replication within each experiment, and if it is desired to ensure the elimination of abnormal seasonal factors, it is well to plan for the repetition of the whole series in time as well as in space. In the writer's opinion the latter is not necessary for provenance experiments which continue for many years.

It is frequently desirable to take material for special examination from the experiment. Often this can be done from thinnings, but not always, and it is desirable to have additional plots of each provenance for the purpose. An additional replication, above what is required for strictly statistical reasons, is very desirable.

Spacing and Method of Planting

The use of different spacings when planting the plots must be avoided in any one experiment, but it would be difficult to prescribe any one spacing for all experiments carried out in many different places. The optimum spacing will depend, just as does the optimum size of plants to use, on the locality factors, and what would be appropriate for a rich soil with a luxuriant non-woody weed flora would be quite unsuitable for an exposed and elevated site with sparse vegetation. The selection of the spacing to be used in any experiment must therefore be left to local experience. The same applies to other details of the method of planting employed.

Beating-up

Failures invariably occur in the early years, and the experimenter is again faced with two courses of action. The failures may be regarded as characteristic of the provenances and left untouched. Though this makes differences between provenances very obvious, it is not consonant with normal practice, and it is preferable to beat up the blanks in the first year, thus restoring the plots as far as possible to a position of equality. This will ensure as far as possible that at later stages the crops will be comparable, and the fact that one needed more beating up than another can be taken into account as one of the interim results of the experiment.

It frequently happens that there are insufficient plants of identical provenance for beating up and then it is desirable to use plants of a contrasting species, so that two provenances of the same species do not become confounded. This may, of course, introduce unknown interactions, such as nursing effects between the two species, but it is probable that the constrasting species can be removed as the crop forms canopy, leaving a pure crop of the provenance under trial in the later stages. Even if a contrasting species is not introduced, equality between provenances is not obtained because an interaction between the trees and the open space or weed growth sets in. It is therefore best to fill gaps in a crop in the most suitable way and get the remaining trees through into canopy stage, as they will frequently then grow normally.

Beating-up should not be repeated, unless for special reasons and then only after recording the first beat-up to ensure that a distinction between plants put in place of deaths in the original plants and deaths in the beat-up plants is made. If this is not done the survival of original plants cannot be traced and it is difficult to find whether deaths are cumulative and due to an inherent fault in the provenance, or merely the normal accidents of planting.

Thinning

Thinnings may be carried out with several different objectives. A normal silvicultural thinning has usually been made, based on the removal of inferior and suppressed trees and evenly spacing the remainder, as in standard low thinning practice. Alternatively a standard type of crown or high thinning may be carried out.

In either case some form of control is required, as differences between plots are bound to be introduced if the marking is left to individual and often changing officers and a personal factor thus introduced. Some form of Stand Density Index is necessary, and this may either be of a more complex type, involving the height, breast height, girth and number of trees in a crop, or a simple one relating the spacing (or number of trees per acre) to the height, with different coefficients for each species, or even for each separate provenance. Such coefficients are based on yield tables.

Silvicultural thinnings of this type are normal practice when the object of the experiment is to compare provenances as producers of crops and timber. But alternatives need consideration, and it may be desirable to thin so as to maintain the characteristic type of tree of a provenance. From the opposite standpoint, rogueing, or the removal of atypical trees may be of prime importance. Finally it may be desirable to preserve the whole range of individuals in a provenance to maintain the extremes of variation. Additional plots or replications of plots above the minimum required for the experiment are very valuable for such specialised thinning treatments. For this purpose, plots outside the main replicated experiment may be desirable, the thinnings being mechanical and removing every *n*th tree (MACDONALD, 1952—54).

For the normal experiment, the form and grade of thinning locally considered suitable to the species should be prescribed, together with a check by means of some kind of density index. VEEN (1954a) has suggested that as the relation between tree height and crown width is fairly constant the spacing index used in the Netherlands should be applied. In this case,

$$\left.\begin{array}{l}\text{No. of trees per}\\ \text{ha. after thinning}\end{array}\right\} = \frac{10{,}000\ d}{(\text{dominant ht.})^2} \text{ in ha./m. units.}$$

$$\text{or } d = \frac{N\ (\text{dom. ht.})^2}{10{,}000},$$

and d is for *Picea abies* 39 to 31, for *Pinus sylvestris* 31 to 25, for *Larix decidua* 25 to 20 } Graph opposite p. 544, VEEN, 1954a.

In Britain, HUMMEL (1949 and 1954) has described the same as the Spacing/Height percentage, the index being

$$d = 100 \sqrt{\frac{43560}{N}} \text{ in acre/ft. units.}$$

From the yield tables the indices are: —

Picea abies	16 — 17	Indicated on alignment chart. Fig. 2.
Pinus sylvestris	19 — 22	
Larix decidua	19 — 23	

However, the assumption that the relation between height and crown width is constant, which is true on a broad scale for a species, may not hold exactly for a collection of different provenances of a species. In fact, there are known to be differences in crown width characteristic of provenances, and GALOUX and GATHY (private communication) have pointed out that these differences in *Pinus sylvestris* are such that the range of spacing indices for pine (metric scale) must be increased from 25 to 31 (as noted above) to 25 to 45. In other words, it is not silviculturally correct to thin a provenance experiment to a fixed spacing index. Instead, it will be found that the only method is to thin to silvicultural grade, defined as the removal of trees of specified canopy classes, so as to obtain as equal a density of canopy as possible in all plots, and then to calculate the spacing indices. Study of these may reveal some discrepancies between replications which can be adjusted if thought fit and the index

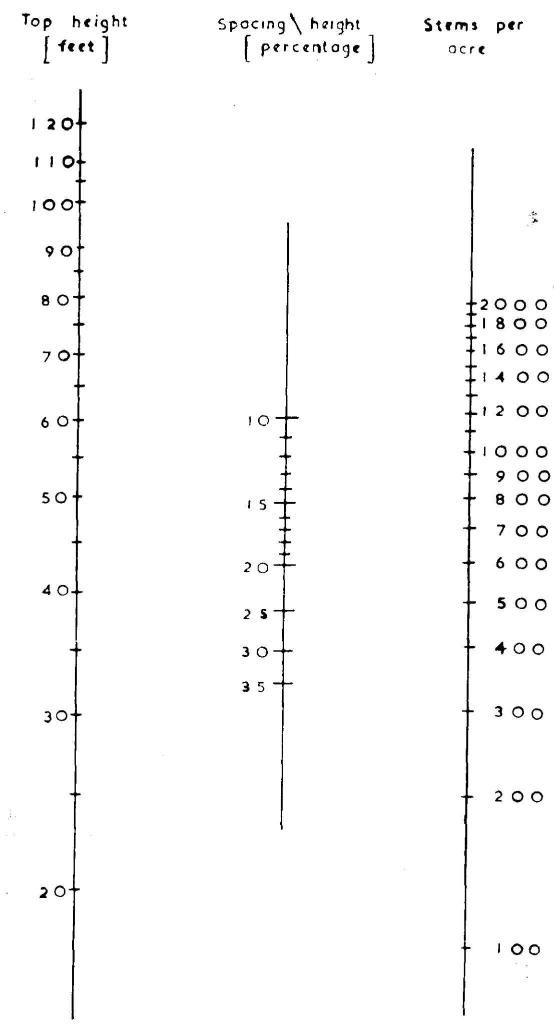

Fig. 2. — Alignment chart for number of stems per acre, top height and spacing as a percentage of top height.

recalculated. The differences in the indices can then be tested for significance between provenances.

It is possible that in provenance experiments a more complex index, perhaps including the factor of stem girth or diameter (as for instance LEXEN'S [1943] index) may be desirable.

It may be noted here that the principle that each plot be treated on its merits applies again, and that each will be thinned as and when it needs it. In early stages there will therefore probably be plots being thinned each year, but in later stages, it will be possible to synchronise them and, unless differences between provenances are extreme, thin all plots on the same cycle. Assessments should be made at the time of thinning so that it is desirable to synchronise thinnings as soon as possible so as to permit of easy comparison of data.

Assessments of Results

The assessment of the results of provenance experiments is basically no different from others, but the fact that there are international provenance experiments makes it especially desirable to obtain a certain amount of uniformity so that results from different countries can be compared. There are also a number of assessments concerned with the habit of the individual tree which are used in progeny trials and can usefully be applied to provenance experiments.

The basic assessments are those of survival, height. girth (or diameter) and volume, and the procedure for these is similar to that of the measurement of sample (yield) plots, as elaborated internationally under the aegis of the International Union of Forest Research Organizations.

This procedure has been in operation for many years in all countries, and an improved code for use in Britain has recently been compiled and is in the press.

Such a complete code cannot always be applied in experiments with small plots, or when sample trees cannot conveniently be measured, so that in Britain a simplified assessment code has recently been prepared. This was under discussion when Veen (1954a) visited the provenance experiments and it has since been expanded to take cognizance of his proposals.

The basic assessments are considered for the three stages of the crop, the individual tree phase, the thicket phase and the crop phase. The assessment of special characteristics (i. e. the non-basic assessments) is dealt with separately and may be applied at various phases. Each part of the code consists of general considerations, definitions and detailed instructions.

It is not possible to summarise this code, which is itself abbreviated, but it can be applied as and when suitable to provenance experiments. Experience in its use will no doubt enable improvement to be made.

The code does not lay down the timing of the assessments. Survival should normally be assessed at the end of first and third growing seasons after planting in the forest. Height should be assessed at the end of the third, sixth and tenth seasons, and thence quinquennially. Many of the special characteristics can best be assessed just before the end of the individual tree phase, but also at other times throughout the life of the crop.

A word of warning should here be uttered about the dangers of accepting early results as conclusive. Weidman (1939) noted the reversal of changes after a period of years, and in Scotland differences in age and type, size and nursery origin have been found important and persistent. "Nursery origin“ includes such factors as different levels of nutrition, etc. The danger of such differences being of importance and confounded with inherited differences in inadequately replicated forest experiments, especially if the plants were not sown in replicated nursery experiments, is very large, especially in the case of early results.

The records of the mean heights of the different provenances at the end of the nursery stage, and at successive ages in the forest permit of height curves being drawn for each provenance. Later the quinquennial assessments of top height, basal area, volume etc., permit of the usual growth curves being compiled, and from these the relative performance of the provenances can best be judged.

Finally, some general remarks by Richards and Kavanagh (1954) are important: "Growth is comprehended by measurement and analysis of measurements. In a given experiment the information gained is limited by the nature of the measuring method and the unit of measurement used. A single measure may be adequate for a practical problem, when the growth is a means for and limited to evaluating a single factor, ... Growth usually is a resultant sum of the growth of the component parts, which rarely grow at the same time and rate. To obtain a complete understanding of the growth of an organism the simultaneous use of different measures is required. When there is no change in form, certain dimensions may change in constant ratio to each other. On the other hand, changes in dimensions are an index of change of form and conversely. Much of the confusion in the literature on growth is due to the improper use and application of the measuring scales." The simultaneous use of different measurements is a complex problem, especially if different scales of time are also envisaged, (Weck, 1950), and it is important to organise and standardize the derivation and use of simpler measurements first, always remembering that the separation of chance and random errors from the variations we are seeking to determine compels the use of randomisation and replication and care to avoid subjective conclusions.

Summary

The objects of provenance (as distinct from 'progeny') trials are discussed and their conduct considered in the seed collection, nursery and forest stages. Seed collections should be made from whole populations, and they must be very fully described. Full records of collection, seed testing and grading are needed. In the nursery stage, plants must be raised i well-designed experiments, and methods of selecting and rejecting plants are described. The principles to be observed in these and other questions of treatment are outlined. In the forest stage, the shape and size of the plot must be that suitable for comparison of crops and not individuals, and this principle is of general application. It determines, partly, the size of assessment plot needed. The size of surround required and how it is composed is described, with a figure, the assessment plot plus surround forming the experimental unit of treatment. The combination of such plots into experimental designs should be as simple and as robust as possible, and must permit of the significance of differences being determined.

Various methods of spacing, planting, beating-up and thinning and the principles and precautions needed to determine the methods used are considered: spacing and planting varying according to site requirements, beating up once only, unless with special records; and thinning according to normal silviculture but with the use of a stand density index as a special form of assessment of differences.

A final section on assessments concludes that instructions, which are the same in many cases as for other kinds of forest experiments; are best detailed in a separate assessment code.

Zusammenfassung

Titel der Arbeit: *Die Planung, Anlage und Überwachung von Provenienzversuchen.* —

Erörtert wird das Gebiet der Provenienzversuche (zu trennen von Nachkommenschaftsprüfungen) und speziell ihre Behandlung von der Samengewinnung über die Pflanzenanzucht bis zur Bewirtschaftung.

Die Saatgutgewinnung sollte an eingehend zu beschreibenden, ganzen Populationen vorgenommen werden. Notwendig sind erschöpfende Aufzeichnungen über Samengewinnung, -prüfung und -qualität. Bereits im Baumschulalter sollten die Pflanzen in geplanten Versuchsanlagen aufgezogen werden und die Gesichtspunkte, nach denen selektioniert und verworfen wird, festgehalten werden. Die Prinzipien, die dabei und bei anderen in Frage kommenden Behandlungen zu beachten sind, werden umrissen.

Es ist von grundsätzlicher Bedeutung, daß bei der Ver-

suchsfläche die Form und die Größe der Teilstücke so günstig gewählt wird, daß Bestände und nicht Einzelindividuen verglichen werden können. Hierdurch wird z. T. auch die Größe der Teilflächen bestimmt. Größe und Anordnung der Randpflanzung gehen aus einer Abbildung hervor: Teilfläche und Rand bilden eine Versuchseinheit und werden in gleicher Weise behandelt.

Die Zusammensetzung derartiger Teilstücke zu einem Versuchsplan sollte so einfach und klar wie möglich sein, und sollte eine statistische Auswertung auftretender Unterschiede zulassen.

Die Wahl des Verbandes der Pflanz-, Astungs- und Durchforstungsmethode sowie Grundsätze und Vorsichtsmaßregeln, um die angewandten Methoden festzulegen, werden erörtert. Verband und Pflanzmethode wechseln mit den Standortsbedingungen, Aufastung findet (abgesehen von speziellen Verhältnissen) nur einmal statt, und die Durchforstung wird nach normalen waldbaulichen Gesichtspunkten, aber unter Anwendung eines „Bestandesschluß-Index" als spezielles Mittel zur Feststellung von Unterschieden, durchgeführt.

Abschließend wird festgestellt, daß Instruktionen und Richtlinien, die für dieses Gebiet wie auch für andere forstliche Forschungsrichtungen gültig sind, zweckmäßig in einer besonderen Anleitung bis ins einzelne festgelegt werden.

Résumé

Titre de l'article: *Disposition, installation et contrôle des expériences de provenances.* —

Les buts et la réalisation des essais de povenances (qu'on distingue des essais de descendances — progeny tests) sont étudiés, de la récolte des graines jusqu'au stade de peuplement forestier, en passant par la pépinière. Les récoltes de graines doivent être faites sur des peuplements pris dans leur ensemble, qui doivent être décrits très soigneusement. Des renseignements complets sur la récolte, l'analyse des graines, les tris éventuels, sont nécessaires. La culture des plants en pépinière doit se faire en suivant un dispositif précis, et les techniques de sélection et d'élimination des plants défectueux doivent être mentionnées. Certaines règles doivent être observées dans la conduite de ces opérations. Sur le terrain, la forme et la dimension des placeaux doivent permettre la comparaison de rendements globaux et non d'individus. Ce principe d'application très générale, détermine en partie la dimension des placeaux. Des schémas donnent la dimension et la composition des bandes unitaires d'isolement; le placeau unitaire plus la bande d'isolement forment l'unité expérimentale de traitement. La combinaison de telles unités en dispositifs expérimentaux doit être aussi simple que possible, et doit permettre de tester par les méthodes statistiques les différences observées ou mesurées.

On examine les diverses méthodes d'espacement, de plantation, de regarnissage et d'éclaircie, ainsi que les principes de leur choix: l'espacement et la technique de plantation varient suivant les conditions écologiques; les regarnis doivent être faits en une seule fois, sinon il faut les mesurer séparèment; les éclaircies doivent suivre les règles de sylviculture normales, en utilisant toutefois un index de densité pour évaleur les différences.

Le dernier chapitre est consacré aux méthodes de mesures; elles sont les mêmes dans certains cas, que pour d'autres types d'expériences; on en trouvera une description plus détaillée dans un "code des mesures" spécial.

Literature Cited

ANON: Working plan for cooperative study of geographic sources of Southern pine seed. Southern For. Expt. Sta. 1952, pp. 35. For. Abstr. 14: No. 3273 (1953). — BEARD, J. S.: Investigations into experimental technique with Black Wattle. Empire For. Rev. 33: 158—171 (1954). For. Abstr. 15: No. 3921 (1954). — EDWARDS, M. V.: Scottish studies of the provenance of European larch. Comptes Rendus, 11th Congr. I.U.F.R.O., Rome, 1953: 432—437 (1954). — EDWARDS, M. V.: Third report of an investigation of 1931—32 into various races of European larch. For. Comm. Annual Rept. *(in press)*. — FABRICIUS, L.: The choice of plants from the seed bed. Forstwiss. Cbl. 60: 297—304 (1938). For. Abstr. 1: 34 (1939—40). — FABRICIUS, L., OUDIN, A., and GUILLEBAUD, W. H.: Outlines for permanent sample plot investigations. I.U.F.R.O., Oxford, 1936, pp. 33. — FRITSCHE, K.: Größe der Versuchsflächen. Tharandt. Forstl. Jb. 78: 129—157 (1927). — HUMMEL, F. C.: The use of stand density indices for describing thinnings. J. For. Comm. No. 20: 137—142 (1949). — HUMMEL, F. C.: Definition of thinning treatments. Comptes Rendus, 11th Congr. I.U.F.R.O., Rome, 1953: 582—588 (1954). — HUMMEL, F. C., and CHRISTIE, J.: Revised yield tables for conifers in Great Britain. For. Comm., For. Record No. 24, 1953. — JEFFERS, J. N. R.: Relationship between the percentage of usable conifer seedlings and the mean height of seedlings as assessed at stocktaking. For. Comm., Rept. For. Res. 1954: 101—106 (1955). — JOHNSSON, H.: Einige Fragestellungen der forstlichen Nachkommenschaftsprüfung. (Some problems in forest tree progeny test.) Z. Forstgenetik 2: 2—8 (1952). For. Abstr. 14: No. 1121 (1953). — LEXEN, B.: Bole area as an expression of growing stock. Jour. Forestry 41: 883—885 (1943). — MACDONALD, J.: Sample plot methods in Great Britain. Empire Forestry Jour. 10: 241—258 (1931). — MACDONALD, J.: A visit to Denmark and Sweden. J. For. Comm. No. 23: 12—19 (1952—54). — MACDONALD, J. A. B.: The value of early thinnings in Norway spruce. Scot. Forestry Jour. 46: 112—117 (1932). — MÜNCH, E.: Beiträge zur Forstpflanzenzüchtung. (Contributions to forest tree breeding. Selection trials of progeny of individual spruce trees. Further contributions to forest tree breeding from the papers of the late Dr. E. MÜNCH.) Edit. B. HUBER, München, 1949, pp. 116. For. Abstr. 13: No. 228 (1952). — OSBORN, J. B.: Seed selection in black wattle *(Acacia mollissima)*. Emp. For. Jour. 10: 190—202 (1931). — OUDIN, A.: Les méthodes actuelles de la station française de recherches en matière forestière. I.U.F.R.O., Proc. 1929 Congr. Stockholm, p. 98—105 (1930). — PEARCE, S. C.: Field experimentation with fruit trees and other perennial plants. Techn. Comm. No. 23; Commonwealth Bureau Hortic. a. Plantation Crops. East Malling. 1953, pp. X + 131. — RICHARDS, O. W., and KAVANAGH, A. J.: The analysis of growing form. Essays on Growth and Form: p. 188—189. Clarendon Press, Oxford, 1945, pp. VIII + 408. — RIGHTER, F. I.: *Pinus:* the relationship of seed size and seedling size to inherent vigor. Jour. Forestry 43: 131—137 (1945). — SCHLESWIG: Arbeitsplan für Anbauversuche mit ausländischen Holzarten. (Working plan for silvicultural experiments with exotic tree species.) Conference of forest research organizations. Allg. Forst- und Jagdzeitung 125: 237—331 (1953—54). — STRAND, L.: Progeny tests with forest trees. Hereditas 38: 152—162 (1952). — STRAND, L.: Plot sizes in field trials. Z. Forstgenetik 4: 157—162 (1955). — VEEN, B.: Report on the test areas of the International provenance tests with larch, pine, and spruce of 1938—39 and 1944—45, and suggestions for future treatment and assessments. Comptes Rendus, 11th Congr. I.U.F.R.O., Rome, 1953: 536—551 (1954 a). — VEEN, B.: General remarks on provenance research in forestry. Euphytica 3: 89—96 (1954 b). For. Abstr. 16: No. 374 (1955). — WECK, J.: Über die Brauchbarkeit von Wachstumsgesetzen als diagnostisches Hilfsmittel der Wald-Wachstumskunde. (On the utility of growth laws as aids to the understanding of forest growth data.) Forstwiss. Cbl. 69: 584—695 (1950). For. Abst. 14: No. 1420 (1953). — WETTSTEIN, W. VON: Selektion von Kiefern nach 4 Jahren. (Selection of pines within 4 years.) Züchter 19: 205—206 (1949). For. Abstr. 12: No. 1765 (1952). — WEIDMAN, R. H.: Evidence of racial influence in a 25 year test of pondorosa pine. Jour. Agric. Res. 59: 855—887 (1939).

Appendix I

Example of a
NURSERY EXPERIMENT PLAN

NURSERY: . . . . Experiment No. . . . .

PROJECT: 232.12. Race and provenance trials.

OBJECT: To compare the size and number of plants obtained (and also other differences) from seed of various provenances sown and lined out in nurseries prior to planting in the forest.

SPECIES: . . . . , ...

EXPERIMENTAL TREATMENTS: Seed of the following provenances will be sown as follows: —
(see attached Sowing Form for calculation of sowing rate, based on an average of . . . . plants per sq. ft. and survival factor of . . . . per cent of the probable germination.

Id. No.	Pro-venance	Quantity	No. of blocks	Amount per block	Sowing Rate sq. ft. per lb.	Size of plot sq. ft.

GENERAL TREATMENTS: The ground for the seedbeds will be selected with regard to previous use, greengropping, manuring, etc., to give optimum results. It (will / (will not be sterilized.
Sowing, covering, etc. will be carried out by the best methods in force at the nursery concerned. The seed will be sown in blocks, each block containing one plot of seed of each provenance (see below). It is essential that a block be as uniform as possible, and all plots within it must be treated the same way, unless prescribed otherwise.

DESIGN: Randomized blocks. Within each block plots will be randomized and a plan of the layout recorded.

			No. of blocks actually used = ...
for 7	provenances (or more)	3 blocks	
„ 5—6	provenances	4 „	
„ 4	„	5 „	
„ 3	„	6 „	
„ 2	„	10 „	

PLAN: Standard nursery beds.

SEEDBED REQUIREMENTS: Total Area = (No. of provenances × size of plots) × no. of blocks
= . . . . , sq. yds. of seed bed (net).

REPORTS AND ASSESSMENTS: Nursery Record Forms 1, 2, 3a and 3b will be maintained. At any time, differences between provenances in rate of germination, damage, colour, etc. as they appear, will be assessed, either by counting seedlings or by classifying plots by a suitable key. At the end of the season, the normal assessment of height and numbers will be carried out. See Assessment Code. Nursery Record Form 6 for counts and a summary in Experiment Record Form 7 will be used for all assessments.

LINING-OUT: Normally all plots with 60% of usable seedlings will be lifted and lined-out at the end of the first year. This figure may be adjusted to allow for all plots in a block to be either lifted or to remain to produce 2 + 0 plants.

If the differences between plots are great, some will be lifted and some left and the experiment divided into two parts. The decision will be made after computation of the assessment data.

(a) Grading. A minimum height for lining-out will be fixed for each block, and for every plot the total no. of undersize plants below this will be counted and recorded in the Production Form (Cols. 4 & 5) (see below).

(b) Damaged plants over the size limit (e. g. tops or roots broken, etc.) will be rejected, and their number recorded (Col. 6).

(c) If necessary, other poor plants will be culled. If this is done, culling to exactly the same standard must be done by the same man for all plots. The number so culled, and the reason for culling, must be recorded. (Col. 7.)

(d) Lining. All the plants selected from each sowing plot will be lined out in one lining-out plot (Col. 8).

A suitable piece of ground for lining-out will be chosen, and each block of the sowing experiment (less any plots not to be lined) will be lined-out as one block. The lay-out of the individual lining-out plots should be re-randomized.

REPORTS AND ASSESSMENTS: Nursery Record Form 2, 3a, 3b and attached production form will be maintained. Assessments as per Assessment Code.

DESPATCH FOR PLANTING: A minimum size for planting will be fixed, and the number of plants required will be allocated in the usual way on stock sheets. In order that the growth in the forest shall be comparable it is essential that the same standard for relining or culling shall be observed in all lots. It is not desirable that culling should be done, either for lining out or for despatch to the forest, except to remove undersized or accidentally damaged plants. But there are cases when plants over the minimum size are so poorly furnished or so mis-shapen that they cannot reasonably be expected to grow well. These may be culled and the numbers recorded, with reasons, in column 14 of the Production Form. The same standards of selection and culling must be applied to all lots. It is important that one man should grade all provenances in one block (and not one provenance throughout all blocks).

As far as possible, all plants for one forest experiment should come from the same block or blocks of the lining-out experiment. If more than one block is utilized for one experiment, the plants must be very thoroughly mixed.

PROVENANCE EXPERIMENT SOWING FORM

Identity No. of seed	Provenance	1000 pure seed weight. gms.	No. of pure seed per lb. pure seed. thousands	Purity per cent	No. of seed per lb. unsorted seed. thousands	Germination per cent		Corresponding no. of viable seed per lb unsorted seed. Thousands		Sowing rate sq. ft. per lb.	Assumed Survival factor. per cent	Expected No. of plants per sq. ft. at end of season	Actual	
						A	A + B	A	A + B				No. of plants per sq. ft. at end of season	Survival factor per cent

PROVENANCE EXPERIMENT PRODUKTION FORM

Nursery: *) record reasons separately Experiment No.

			Lining-Out						Despatch to Forest							
										Undersize						
Plot Reference No.	Id. No.	Pro-venance	Minimum height - ins.	Undersize number rejected	Oversize damaged rejected	Oversize otherwise culled*	Number of plants lined out	Total (Cols. 5—8)	Minimum height - ins.	Lined Out	Des-troyed	Oversize damaged rejected	Oversize otherwise culled*	Number of plants despatched	Remainder re-lined	Total (Cols. 11—16)
1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*
							Block No................ (Complete all plots in one block before proceeding to next block)									

Appendix II

Example of a
FOREST EXPERIMENT PLAN

GLEN LIVET Forest, Ruthven section
Experiment No. 5. P. 54

GLEN ISLA Forest
Experiment No. 3. P. 54

LAIKEN Forest
Experiment No. 2. P. 54

PROJECT: 232.12. Race and provenance Trials.

OBJECT: Comparison of the survival, growth, yield, form, disease resistance and any other differential characteristics of Scots pine of eleven selected East Scotland provenances, embracing various climatic zones, and planted in three situations of varied climate.
(Extension of Teindland Heathland Nursery Experiment No. 1. P. 52.)

SPECIES: *Pinus sylvestris* Linn.

EXPERIMENTAL TREATMENTS:

Seed Id. No.	Pro-venance	County	National Grid Reference	Elevation	Quality Class	Age	Seed stand No.	Seed stand classification

BASIC TREATMENTS: Climate: Severe — Glen Livet
(Anderson's zone D/E, Matthews IV)
less severe — Glen Isla
(Anderson's zone C/D, Matthews VI)
Favourable — Laiken
(Anderson's zone B, Matthews I)

Ground preparation: Ploughing at Glen Livet
Nil at Glen Isla and Laiken

Spacing:
Glen Livet 5 × 4 ft. = 2178 plants per acre.
Glen Isla $4^1/_2 \times 4^1/_2$ ft. = 2151 plants per acre.
Laiken 5 × 5 ft. = 1742 plants per acre.

Manuring: Nil.

Beating up: All plots will be beaten up 100% after first season, but not normally again, unless failures are heavy, when another species will be used.

DESIGN: Balanced incomplete blocks, eleven provenances with five replications arranged in eleven blocks of five plots each at each forest.

PLAN: See separate figure (not reproduced).

ASSESSMENTS: (a) At the end of the first growing season: —

All as laid down in Forest Assessment Code.

Survival. Also to provide data for beating up.

(b) At the end of the third season: —
Survival }
Height }

(c) At end of the individual tree phase, i. e., When the tallest plants are about 8 feet high: —
characteristics of branch number, size and form, form of tree, needle length and colour and any other morphological characters.

(d) After brashing but before thinning: —
Height
Survival
Any other characteristics.

(e) After thinnings — Crop phase assessments.

(f) At any time when needed: —
Incidence of disease
Phenology
Suppression of ground vegetation
Resistance to exposure
Any other differential character which may become apparent.