

DISCUSSION NUMBER FOUR

Johnson: I would like to ask whether or not the hybrids of *U. rubra* x *pumila* remained resistant.

Lester: Based on our wind-pollinated hybrids, I would expect a range of susceptibility.

Santamour: I recall a paper by Swingle that F1 progenies were uniformly susceptible.

McDaniel: There were examples of susceptible hybrids reported from Urbana, by pathologists in the Illinois Natural History Survey, but our hybrids, like *U. pumila* and *U. parvifolia*, are holding up better than native elms.

Polk: There seems to be a pattern developing in the findings that trees of southern origin outgrow northern sources of a species. This has proved true, for example, in studies of eastern white pine, eastern hemlock, and Douglas-fir. As an explanation of this, the thinking has been that southern origins continue growth later into the growing season. Such late-season growth has seemed to explain why there may be a cold hardiness problem when southern origins are introduced to northern climates. Now, we look at Calvin's data. If I understand the charts, southern origins were growing faster on a day-to-day basis through the growing season. I would like to ask Calvin if this interpretation is correct; and, if so, does he have some possible physiological explanation?

Bey: Not only did they grow longer, but they grew faster. If you look at the charts you will see that the southern sources took the lead early in growing season and increased their lead with time. This applied to height growth as well as to dry weight. We did not study why the southern sources grew faster, and I hesitate to speculate on an explanation.

Jokela: In connection with your question, I would like to speak on an experience we had with cottonwood this year. This is one-year-old data on a plantation at Stoneville, Mississippi, in which we had provenances from Mississippi, Louisiana, Missouri, southern Illinois, northern Illinois, and Minnesota, and measurements were taken at 3 different times during the growing season. Up to June 1, the most northern source, Minnesota, was the largest, and the smallest were some of the southern ones. At the end of the growing season, southern Illinois and Missouri were the tallest; these were followed by Mississippi and Louisiana which were the shortest. The growth rates were variable during the growing season.

Polk: Scotch pine is another species that doesn't have this strict north-to-south relationship in growth rate of provenances.

Kriebel: That was because the southern ones started growing later, wasn't it?

Jokela: I don't know if it was that way or not.

Gerhold: Dave, you suggested that the Rocky Mountain juniper may have migrated eastward to give rise to eastern redcedar. Is this hypothesis preferable to the opposite one—that eastern redcedar migrated westward and gave rise to the Rocky Mountain juniper?

Van Haverbeke: Buchholz's work with the conifers lends support to this view in that he suggested the center of distribution or region of maximum diversity of conifers to be centered around the periphery of the Pacific Ocean. The variation I found in the Rocky Mountains to the west was greater than that found toward the east. The direction of spread, in my view, would logically be from the region of maximum variation toward the region of less variability. Following the presentation of the paper I said that "one interpretation is as tenable as the other". This was perhaps an error on my part since the data available to me suggested a southeast migration. While I prefer the evolutionary divergence origin over the introgressive origin I wouldn't, however, argue with those who prefer the introgressive interpretation.

Sharik: In your studies of the putative hybrids of *J. virginiana* x *scopulorum* did you test for pollen sterility or for seed germination in the F1?

Van Haverbeke: No. Perhaps I did not make myself clear. What appeared at first to be hybridization is, in my view, more logically interpreted as the remains of evolutionary divergence. Thus, the half-way point in evolution—for lack of a better term—was referred to as F I -like, but not as F1 plants *per se*. This was simply to suggest the intermediacy of these plants.

Sharik: Then what really is the evidence for hybridization between what we have considered two biological entities? Why should we consider the parental types distinct?

Van Haverbeke: I based my judgment that the population represents two separate but not completely distinct species on the appearance of bimodality in the hybrid index frequency distribution. I believe the two groups should be considered different because in the total population we find two groups of plants possessing two basically different sets of character associations which are the physical manifestations of genetic differences.

Sharik: Why did you work only with meiotic cells?

Van Haverbeke: While no cytological work was included in this study, Ross and Duncan studied meiosis in pollen mother-cells of presumed hybrids and parental types of *J. scopulorum* and *J. virginiana* in the Driftless Area of Wisconsin. They reported an imbalance in the somatic chromosome complements of suspect hybrids.

Johnson: Would you not consider the second alternative to

- be migration? Couldn't you also include the Atlantic populations of juniper in Bermuda and the Bahamas? Their relationships are interesting.
- Van Haverbeke: Yes, migration would be involved in the second alternative, and the Atlantic populations could be included, although I have not studied them. Apparently, there is a phasing from one type of juniper into another. You might call it a mingling of the races, or a group of interlocking species, if you will. For example, in the southwest *J. ashei* apparently merges with *J. virginiana*; in the southeast *J. silicicola* apparently merges with *J. virginiana*. The same is apparently true in the west with *J. scopulorum*, *J. ashei*, *J. virginiana* and *J. horizontalis*, and *J. osteosperma* merging at the peripheries of their respective ranges. Probably, a more accurate interpretation would be to recognize that as the environment gradually changes from one region to another it selects different types of plants for survival, each type exhibiting a different set of distinguishing characteristics. When the differences become great enough to be generally recognized they lend themselves to species classification.
- Johnson: Your second alternative is possibly something of an oversimplification of the true situation.
- Van Haverbeke: Perhaps. I also recognize that while the area sampled in this study was fairly extensive, it did not include all the recognized species of juniper or include the entire juniper distribution. Therefore, as stated in the paper, the taxonomy and evolution of the junipers will continue to be not fully understood until more of the natural populations have been evaluated.
- Santamour: I have a question or a comment for Ed Wollerman. We have at the Morris Arboretum and the National Arboretum some of the early tests of selected black locust clones plus the 'shipmast' locust. 'Shipmast' has been the poorest clone in both these plantings. I was wondering if anyone had gone back to Long Island and looked for a 'shipmast' that might have lived up to expectations?
- Wollerman: I will agree with the fact that the 'shipmast' locust hasn't performed well at any location except Long Island. As I understand the history, the planting stock presumably came from Virginia. My observation of the plot at Beltsville is similar to what is reported from the Arboretum; 'shipmast' was the worst entry in the experimental plot. To my knowledge, no one has investigated why 'shipmast' doesn't do well anywhere else or how it is faring on Long Island at the present.
- Santamour: There was just one clone taken from Long Island originally?
- Wollerman: Yes. Henry Hopp tried to pin down the morphology by spine characters and to describe it as a subspecies.
- Voice: I have a question for Dr. Wollerman. I am not sure, maybe I missed it, that you tested the differences between these clones for significance—maybe you did. Were the differences between the clones with regard to the number of beetles significant?
- Wollerman: I haven't analyzed the data yet.
- Voice: What do you think functions as an attractant to these beetles? Do you think it's an optical pigment since there was a positive correlation between the diameter and the number of beetles? Do you think the bigger the stem is the more it looks to the beetle like a host tree and he then tries to enter?
- Wollerman: In insect activity, two of the greatest influences on movement are temperature and light, and when you speak of attractants I think you get into an area where arrestants is a better word. Their movement is random and I believe the adult beetle identifies black locust when encountered and lays its eggs. I don't think it does it from a distance; I think it has to find the host tree and that's why I am interested in space relationships. If-it has to use up its energy in sampling a dozen other trees before it finds a black locust, it may reduce the damage.
- Voice: What's the spatial variation pattern of the beetle? Did there exist some groups of trees in which there was much mortality or did there exist some groups of trees surrounded by open space that were preferably attacked?
- Wollerman: No, we didn't find either situation.
- Voice: No difference whether the tree was surrounded by other trees or by gaps?
- Wollerman: The general picture was early infestation. It was mild and increased generally over the whole plot. Practically every tree had at least one borer in it.
- Hunt: I just want to ask Ed if he did anything with the goldenrod nearby. Did you try correlations with the number of goldenrod stems per acre? Isn't this species a preferred host for the locust borer?
- Wollerman: Yes, a suggested answer to the locust borer problem is to get rid of the goldenrod. Goldenrod isn't essential. I have caged an infested tree and left the cage on for 3 years. Beetles matured, emerged, mated, and reinfested the same tree for 2 generations with no access to goldenrod. On the other hand, it may be a stimulating thing in its nutrition. And as to the occurrence of goldenrod, I mentioned being out here in Illinois. When we looked for beetles we looked for them on goldenrod, but couldn't find much goldenrod; this may be the result of clean cultivation. I don't know why else you would have less goldenrod through central Illinois than we do in Ohio.
- I might say that in addition to two types of interests that we have, for timber, and for shade trees, there is a third interest—a professor. of apiculture at the college of Guelph, Ontario, wrote me and asked me for my recommendations on a tree that would bloom early in its life. I think our strain 4193 has bloomed earlier than any of the others and has set seed earlier than any of the other clones. His interest was just in the stem staying upright and supporting the blossoms for his bees.
- Clausen: I have a question for Dave, regarding your figure 3, the hybrid index with zero as the low value and I believe 96 as the high specific value. My question is: How did you arrange your scores; on what did you base your scores when in fact on the east-west transect you didn't get any "pure" *virginiana* at all? Is there such a thing? How can you derive the scores since you could

not find a tree that would "qualify as pure *J. virginiana*?"

Van Haverbeke: The technique is as follows. The range of measured values for each character was divided into 5 classes, 0 to 4. A whole tree value was then computed utilizing the total of all 24 character values. Thus, a specimen possessing a minimum value for each of the 24 characters measured would theoretically have a whole-tree value of zero; whereas, the other extreme would possess a theoretical value of 96. The fact that I found no trees with a value of zero indicates that I did not find a single tree showing the minimal range for all of the 24 characters, nor a single tree at the other extreme where all 24 characters fell within the maximum range.

Clausen: I don't think you get my question. What bothers me is that you must have used a kind of hypothetical

type of specimen to arrive at your zero value. Otherwise, some specimens would surely have come up with that. I would assume that the Connecticut ones would have fallen in that range. Did you use the published description of the species or did you look at some specimens?

Van Haverbeke: Hopefully, the first part of the question was answered earlier. With regard to the Connecticut material—and I appreciate that a single sample is probably not an entirely adequate one—the environmental niches in Connecticut tend to be more like Rocky Mountain juniper environments than those in the lower Missouri and Mississippi regions; therefore, I would expect selection for survival of plants slightly more *J. scopulorum*-like in character, than *J. virginiana*, in Connecticut.