Propagation: Does it Ever Make You Wonder?©

Tim Brotzman
Brotzman's Nursery, Inc., 6899 Chapel Road, Madison, Ohio 44057, USA
Email: tim@brotzmansnursery.com

PLANT AGING AND ITS EFFECT ON STEM CUTTINGS

Have you ever experienced or heard someone say that a certain plant is not as easy to root as it used to be? Have you ever wondered why different clones of the same species differ in their rooting characteristics? In *The Plantsman*, McMillan-Browse (2010) writes about plant aging and its effect on stem cuttings. I have found this short article to be very thought provoking while he discusses chronological age of a clone as playing a major role in the ease/difficulty of rooting and how to "recover regenerative capacity by manipulation of the parent plant."

Propagators know that in many cases woody plants become much more difficult to root as they move from their juvenile growth phase into their mature phase (ontogenetic aging). McMillan-Browse states "…it would appear that the ability to regenerate asexually declines with an increasing ability to regenerate sexually.", until reaching their senescent phase when this "…potential is virtually lost." He continues, saying "…the ability of the stem to initiate roots, does not occur as a constant function throughout the life of the plant" — it declines as the plant ages. This context has particular significance for the continuous propagation of a woody plant. The physiological condition of the material is not represented by the immediate age of the individual parent stock, but is a function of the chronological (and physiological) age of the original selection. This is the case however many generations it is removed from the current material. For example, a 10-year-old stock plant of *Ribes sanguineum* 'Pulborough Scarlet' should be regarded as 80 years old because the cultivar originated in the 1930s. He further supported this statement by referencing some well-known groups of deciduous azaleas and the number of years they have been in production: Ghent (150-200 years), Mollis (130-140 years), Knapp Hill (~60-70 years), and Ilam (40-50 years). According to McMillan-Browse, each group gets progressively easier to root as the time in production decreases. The only "tip" I can recall learning about deciduous azalea cuttings is that they would root easier when taken before the bristly hairs on the young shoots fell off. This idea that the total chronological age of the clone can influence the amount of regenerative capacity that can be gained by restoring juvenility is something that I have never considered. No doubt the actual processes at work are much more complicated. In fact, Hartmann and Kester (*Plant Propagation, Principles and Practices*, 7th ed., p.617) commenting on systemic diseases (specifically viruses) in fruit tree production state "…plant virologists have demonstrated that very small, transmissible organisms were the cause of numerous plant disorders and the primary reason for clonal degeneration." Clearly there are multiple views on this question since as clonal vitality degenerates rooting can also decrease.

For better understanding of the concept of chronological aging, I asked Dr. Brent McCown to explain how the above referenced 10-year old *Ribes sanguineum* 'Pulborough Scarlett' could be seen as really being 80 years of age. In his response he said (pers. commun.) "…the oldest (most adult part of a plant) is the original meristem from the embryo. However, on this same plant, in any year much more juvenile shoots may be coming from the basal collar region. Thus your 80-year-old *Ribes* stock plant is only that old if the tips of the original seedling shoots still exist; however, in most shrubs, these shoots have long been replaced by new collar shoots, thus the plants are not physiologically 80 years old anymore." If I continue this same line of reasoning I would therefore have to conclude that the 10-year-old plant would not be 80 years old (physiologically), although I suspect it could easily be much more than 10.

In most cases the propagator is working with clones that were selected for characteristics exhibited in their mature phase, and successive generations of vegetative propagation has resulted in stabilization (fixing) in this condition at the expense of any

juvenile characteristics (*Hartmann and Kester's Plant Propagation: Principles and Practices*, 7th ed., p.610). For cuttings rooted from plants in this mature growth phase, the "fixed" physiological condition of the parent plant is passed on to the newly produced plants. For difficult-to-root plants the propagator must attempt to turn back the clock and restore their stock to a juvenile phase where greater regenerative rooting potential exists. This is usually accomplished by cutting the plants back severely so that only vigorous, upright, non-flowering shoots are produced. Dr. McCown points out that the juncture of the stem and root is referred to as the "basal collar" and remains the most juvenile part of the plant. Shoots which arise from this area will exhibit more juvenility than growth from the main stem of the original stock plant. Additionally, Mr. McMillan-Browse comments "the faster the growth achieved by this process, the higher is the level of regenerative capacity regained. Therefore, it is the speed of growth which is the critical factor in regenerative capacity."

Mr. McMillan-Browse also speaks about the importance of including the "basal swelling" with the cutting. Although he probably means this to include the basal collar, this might also be interpreted as the swelling which is typically seen where twigs/branches/trunks intersect. Using *Salix daphnoides* as an example, but generalizing this concept to include other woody plants… "quality (size and vigour) of the new plant is greatest when derived from the basal cutting, with progressively, declining quality with cuttings taken from the tip of the shoot." "This basal swelling …represents the fastest growth rate of the new shoot…" and therefore, the greatest regenerative capacity. These basal portions are the oldest chronologically, but are the most juvenile in maturity (ontogenically) whereas the tips are the youngest chronologically but the most mature physiologically. It is a commonly accepted nursery practice to leave a "heel'' on the bottom of conifer cuttings, as well as many harder-to-root deciduous trees and shrubs. Many times at our nursery we like to leave a sizeable bit of the older stem and on compact shrubs, like *Fothergilla* and dwarf forms of *Hamamelis* and *Clethra* we will actually take a branched cutting that includes 2- or 3-year old wood. We believe this improves the quality and overwintering success of our rooted cuttings and we have been doing so without even realizing enhanced juvenility might be a factor.

In 2014 I thought I would try some cuttings from a plant my grandmother had grown of *Rosa* 'Harison's Yellow'. Regarded as difficult to root, I found that to be true. I also discovered that it was introduced in 1824. I asked Mr. Bill Hendricks of Klyn Nursery, Perry, Ohio what he thought of the idea of rootability declining with the accumulated age of the original clone. Like me he had never given the question any thought, but he did note that *Malus* 'Bob White', a cultivar introduced in 1876, was one of the most difficult crabapples for his propagator to root. Do these observations have anything to do with total chronological age, clonal degeneration, failure to restore juvenility (rejuvenate), or something else? Probably all of the above are involved.

HOW LONG SHOULD A PLANT BE MONITORED TO DETERMINE IF ITS DESIRED TRAITS ARE REPRODUCIBLE?

Growing in one of Ohio's highway rest areas are several unique and interesting specimens of *Ginkgo biloba*. Although they do vary slightly, their distinguishing traits are being tall, narrow, and possessing branches spaced tightly along the trunk, almost to the point of touching at their bases. Branching is so thick that it is difficult to insert your hand and touch the trunk. From a distance the trees appear like thin, tapered paint brushes held upright. Probably planted in the 1970s, I can only assume they were sourced from a commercial nursery, although no one I have spoken to has ever seen a cultivar which resembles this form. No graft unions can be observed — might these be nearly identical seedlings or clonal cuttings? No one really knows.

Dr. Bob Lipka was one of the first to notice just how unique they are and 5 or 6 years ago he gathered some propagating material. He called this plant *Ginkgo biloba* 'Grindstone', named after the area in which they were found. Technically, he felt that each plant could be considered a clone, so he focused on the plant with the thickest,

fullest form. He told others about the plants and soon several propagators were trialing it as well. In 2014 I received bare-root liners from two of these growers. Plants were in the 3- to 4-ft size range. Some of them were upright in form while others exhibited a wider branching habit. Ginkgo are notorious examples of plants exhibiting topophysis — the phenomenon of the location from which the cutting is taken influencing the habit of the plant it produces, and this can create real issues for growers. I contacted four other propagators who had plants in the 3- to 4-year-old range. They reported some plants had wide branch angles but the majority was upright; again, topophysis at work. What I find of interest is that the dense twiggy branch formation of the mother tree is not clearly evident in the young plants being produced from it. Although Dr. Lipka reports a small percentage of his plants have dense branching, none are thick as the mother tree. Should this characteristic not be reproducible? Instead of branches that are packed almost to the point of touching, the limbs on my plants are well within what would be considered "normal" for the species. Is this inconsistency due to vigor and in time will revert to what is expected for this clone? Would cutting produced plants look different than those that are budded or grafted? I would expect own root plants to develop more slowly and have shorter internodes. Again thinking of Mr. McMillan Browse, instead of using propagation material from tips of ascending branches, what would happen if we were to take scions from the base of the same branch or closer to the basal collar? In his article he states: "Woody plants grown from cuttings with basal swellings show the expected characteristics of the mature cultivar by the second season after propagation." Would this solve the problem seen in the 'Grindstone' ginkgo? Lacking time to visit the mother plant, in 2014 I took terminal cuttings from the more upright portions of the 3- to 4-ft liners which we had purchased. Ideally, I would like to take cuttings from different locations on the original tree. The answer must be in there somewhere.

This begs the question, how long should a plant be monitored to determine if its desired traits are reproducible? I believe that this topic can and does stimulate considerable debate. Let me give you but one short example. Our company has in the last 18 years introduced two forms of weeping *Cercis canadensis*, 'Covey' (Lavender Twist® red bud) and Vanilla Twist® red bud. In each case the mother plants were observed for several years, as were young plants produced from them until it was easily discernible that the strongly weeping trait was maintained through propagation. However, I was always hoping to find another *Cercis* that possessed a more upright habit with a dominant central leader as well as weeping branches. Observing several hundreds of F_2 seedlings involving 'Covey' and 'Royal White' we found plants that were primarily strongly weeping or exhibited the normal, wide spread form typical of the species. Nothing impressed me as intermediate. However, now nearly 10 years later some plants are beginning to develop a form that appears going in the right direction.

During this time a *Cercis* was found in an old park in Cleveland. It was close to 15 ft tall and had definite cascading branches — very much like I had been seeking. We propagated it and after approximately 7 or 8 years the young plants still have not taken on the form of the parent tree. These examples, along with the previously mentioned *Ginkgo*, suggests two points:

1) Some mature characteristics that we select for may not develop in time to be present in the smaller, younger plants that we offer for sale and possibly.
2) That such a delay may limit the marketability of such a selection.

I have now initiated propagating some of the more interesting F2 *Cercis* hybrids and I will be watching the young plants they produce to see how long it takes for them to develop the same parental forms.

OBSERVATIONS ON DWARF SELECTION OF GINKGO

Continuing with *Ginkgo biloba*, I have been rooting softwood cuttings of this ancient specie for a number of years. They are quite easy to do but own root plants will be slower to develop into saleable stock than those that are budded or grafted. The fastigate form I am producing is called 'Elmwood' and for whatever reasons, I find this selection easier to

produce with a straight, central leader. Many of the clones I have tried were nearly impossible to develop and keep a central leader without staking. For me, 'Elmwood' is easier. As much as possible I limit my cuttings to new growth taken from branches that are strongly ascending. Even the young 2-, 3-, and 4-year-old plants are showing a very high percentage of central leader dominance and narrow branch crotch angles.

In 2013 I thought I would try rooting some of a dwarf selection called 'Troll'. Cuttings were taken on July 31, treated with 1:10 Dip 'N Grow and placed into mist. One year later, new growth is about 0.5" long and rooting appears weak. This is probably what I should have expected from a dwarf. But then a question occurred to me, does the rootability of any given dwarf form depend at all on the origin of the original plant? In other words could ease of asexual reproduction be additionally influenced by whether it is a genetic dwarf (from seed) or a mutated form (broom) of an otherwise normal growing plant? A broom generated plant would most certainly have arisen in an old, mature or even senescing tree where rooting percentages would be expected to be low. Grafts from these would be perpetuating the mature characteristics and unless the grafted plants were being cut back drastically, one would not expect to find a lot of vigorous new growth (cuttings) being produced. However, I think if I wish to do own root dwarf ginkgo, cutting them back to produce juvenile growth is what I will need to consider.

OBSERVATIONS ON INCONSISTENT POD FORMATION IN 'DAVES' COLUMNAR HONEY LOCUST

Several years ago our company introduced *Gleditsia triacanthos* 'Draves', marketed under the name of Street Keeper® columnar honey locust. The nearly 50-year-old mother plant had been found near Buffalo, New York where it had been observed by Mr. Tom Draves for nearly 20 years. During this time he had not noticed it to be a seed producing plant. When one of the distinctive pods of this specie was shown to the owner, who had lived there since it was planted in his front yard; he asked "what is this"? But, shortly after being put into production the mother plant began to produce seeds, sometimes quite heavily. Growers also found that this was happening but oddly enough, not every tree, not every year and not in every nursery. *Gleditsia* is a plant that can change the balance of male and female flowers it produces from year to year, and stress may possibly be a factor in causing this. Trees that have been primarily male can start producing seed and plants that were primarily female can begin producing male flowers. Could it be that the stock plants being used by propagators have become "fixed" into male or female clones? Could some of the 'Draves' selection simply be replicating the mature physiologic age of the parent tree while others are expressing the juvenile (non-fruiting) traits of rejuvenated stock. One grower indicated he found a plant with thorns which would be an indication of a juvenile phase.

The first year we collected budwood we used a bucket truck to reach into the upper portion of the nearly 50-ft-tall mother tree. I recall that there was a small zone in the upper portion that had pods, probably limited to one branch. That was all. We avoided that area in harvesting propagation material. Several years later, however, the tree had started to produce pods throughout, sometimes in very large quantities. Could the scions we gathered after the 1st year have had a different sexual reflection than before? What might have happened to initiate seed production on the entire tree? One individual suggested that the use of the systemic insecticide, Bidrin®, to control honey locust plant bugs, might have encouraged the parent tree to set more flowers. This product has a phosphorus component in its chemistry and apparently there is antidotal evidence to support this side effect in flowering trees. Or, is it possible that the plant bugs had been limiting flower production and once they had been killed by the insecticide, the flowers could carry on as never before? Could it be that the 'Draves' selection is primarily a female clone that will not produce fruit unless it is pollinated by another male clone? With only two licensed propagators receiving scions during this time I thought it might be possible to determine if one of them was working with a primarily female or male clone,

but it appears that plants from both suppliers have seed producing capacity. Once again, the answer to the vexing question of inconsistent pod production remains to be found.

OBSERVATIONS ON PRODUCTION WITCH HAZEL

In 2009 the idea of a fixed juvenile form crossed my mind when I visited wild populations of *Hamamelis ovalis* (running witch hazel) in Camp Shelby, Mississippi. Mr. Harald Neubauer (Hidden Hollow Nursery in Belvedere, Tennessee) and I were being escorted by Mr. Steve Leonard who had discovered this new species in 2005. Three locations were visited and despite recent fires, we were surprised to find some plants over 8 ft tall and others that spread across the ground like a mat measuring approximately 12 ft wide by 18 in. high. Later that same year I went with Mr. Wayne Webb to observe other colonies in Clark County, Alabama. Here we found plants at least 12 ft tall that were surrounded by what appeared to be short rhizomatous outgrowths from a central location. We had observed the same spreading growth habit in Mississippi. Although I was told that a least one "mat form" had been observed in this area, I was not able to see it for myself.

The question of this low growing form still puzzles me. Could it be a genetic dwarf that still maintains the running characteristic? Is it some sort of abnormal habit that has become "fixed," the result of some environmental factor, such as periodic fires? Is a juvenile form that may not flower as well as more ascending forms? From the most pronounced of the low statured plants I gathered some rhizomes which have been established at our nursery. We are anxiously waiting to see just how they perform and if they will remain short. We are taking cuttings from these as well as about 12 other normal growing clones (grafted) in an attempt to get each of them established on their own roots. So far it has been possible to observe that some clones definitely do root better than others. I imagine the trick is going to be, as it appears to be for most efforts to do *Hamamelis* by cuttings, to get them into their 2nd or 3rd winter without dying. Own root *Hamamelis* are famous for dying during their first winter outdoors, even if they are 2 or 3 years of age at the time. Could lack of root hardiness on cutting grown plants be a sign of juvenility? Dr. McCown told me "…root tissues probably always remain juvenile, but we have no way to measure this since roots do not flower."

OBSERVATIONS ON PRODUCTION OF RED MAPLE CULTIVARS

When it comes to the production of *Acer rubrum* cultivars the nursery industry has come a long way from the days when budded stocks suffered high failure rates due to incompatibility. Production by cuttings or tissue culture has become the accepted and preferred standard. Several years ago a local landscape architect suggested that I grow him some plants of *A. rubrum* 'Columnare' so I immediately looked to the mainline producers of tree liners for a supplier for this very old selection. Finding none, I asked Kris Bachtell at the Morton Arboretum to send me some budwood from their majestic specimen. This material arrived in October 2006 and the only understock I had available were some containers of own root *A. rubrum* 'Somerset' and 'Brandywine'. Due to the size mismatch I chip budded these anywhere from 3 to 5 ft up from the bottom. Enough survived that I was able to plant out about six of them. At that point I did not record on which understocks the grafts had succeeded or failed. For several of the following years we asked Hidden Hollow Nursery to bud for us additional trees using *A. rubrum* as the understock. The observation I wish to make is that for the stock budded onto *A. rubrum* seedlings a number of plants eventually formed a swollen, bell-shaped flair at the graft union and died from incompatibility. This is consistent with the problem that was expected from budded propagation before the days of own root cultivars. On the other hand, my two remaining plants which are on 'Somerset' and 'Brandywine' have reached a caliper size of 3.5 in. and have perfectly smooth unions and no symptoms of incompatibility after 8 years. This suggests to me that had not the industry developed own root techniques for commercial cultivars of *A. rubrum* and *A.× freemanii* then it might have pursued clonal understocks and cultivar compatibility studies. Of course such a

project might have been academic in nature, since to develop clonal understocks begs the question, by what means would they have been produced if not by rooting or tissue culture (perhaps stooling?) As it stands today, budding onto a specific clone might prove useful only in limited instances where a particular cultivar remains difficult to root or to establish new stock. I would like to point out that we have tried to root this cultivar using softwood cuttings with very limited success. It is my understanding that J. Frank Schmidt & Son and Klyn Nursery have found it difficult as well. I bet Mr. McMillan Browse would suggest the fact this cultivar was introduced prior to 1889 has a lot to do with that.

WINTER DAMAGE OBSERVATION

Let me make an observation on winter damage, of which we have seen a lot occur in our fields as of late. Brotzman's Nursery is primarily a producer of field grown stock and we maintain only about 10,000 ft^2 of container production. A wide range of both deciduous and evergreen plants are overwintered in white, single-poly covered houses measuring 110×14 ft and either 8 or 11 ft tall. In the past certain plants were chosen to be stored in specific houses only as dictated by available space or their height requirements. Once the houses are closed we try to enter once, if not twice during periods of thaw to water each container thoroughly.

After the winter of 2014 we experienced higher loses than normal, despite being able to water once in mid-winter. At the time I was surprised to find that the greatest losses were in the taller houses. Whereas *Ginkgo biloba* 'Elmwood' (own root) growing in quarts, 1- and 2-gal containers were fine in the short houses, 6-ft standards of *G. biloba* 'Troll' in 15-gal containers were mostly dead. The same observation was made for own-root plants of *Parrotia persica* 'Vanessa'. Quart and 1-gal containers in short houses were mostly alive, whereas most of the 3-gal containers from the high house were badly hurt or killed. Assuming all containers had adequate amounts of moisture during critical periods, I now realize that during the prolonged and extreme cold we faced, the tall houses may have offered less protection than the short houses, primarily due to the greater heat loss from the taller house. Using the formula for Heat Conduction Loss Factor [TSA (Total surface area) × T (max. temp. inside – min. temp outside) × HLV (heat loss value of poly covering, which is .83 for 4 ml)] I determined my 8-ft houses would lose 118,695 BTU/h and my 11-ft houses would lose 178,000 BTU/h. Apparently the shorter houses held more of the ground heat closer to the container, whereas in the higher houses the ground heat escaped more quickly into the ambient air and eventually, to the outside. Clearly during the long periods of below zero temperatures the issue became a matter of root damage from the containers freezing. In the future, using air inflated double poly, laying the plants down and covering with a sheet of poly or a frost blanket or utilizing an alternate location for the suspect species may need to be considered.

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