[Aliso: A Journal of Systematic and Floristic Botany](https://scholarship.claremont.edu/aliso)

Volume 17 | Issue 2 Article 7

1998

Plant Systematics: Beginnings and Endings

Billie L. Turner
University of Texas

Aliso, 17(2), pp. 189–200

PLANT SYSTEMATICS: BEGINNINGS AND ENDINGS

BILLIE L. TURNER

Department of Botany
University of Texas
Austin Texas 78713-7640
e-mail:billie@uts.cc.utexas.edu

When I was first asked to address this thirteenth annual assemblage of southwestern systematists, I was reluctant. My research these days is mundane, old hat, descriptive; what would I have to hold forth upon? Dr. J. Travis Columbus, one of the organizers, in his letter of invitation to me, noted that I might prepare "a historical overview of plant systematics in the southwest" [or you might . . .] "also address needs and priorities for future work [and if] this topic doesn't suit you, I reemphasize that you are free to speak on any subject pertinent to the southwest flora." With that statement I capitulated: I could develop my own agenda, however eclectic. Nonetheless, I hope to restrict my thoughts and comments to selected individuals and practices of southwestern systematists, in general, all of this flavored with feisty comments and observations about the current state of our "art" and its future.

BEGINNINGS

I am a southwesterner and hope to die sooner or later in a dead-end canyon at 5100 feet, seven miles west of Alpine, Texas, surrounded by madrones and oaks, smiling up at my wife Gayle with all the love I have ever felt for her and for plants. Anything, really, hoping I'll be ashed and strewn along the volcanic bluffs that surround our newly built adobe home so that I might look down upon her magnificence for the rest of *her* life, be she entangled in the arms of another, or merely an eccentric widow savoring her past, I hope to gaze down upon Gayle each spring, coming up anew, reincarnated as Boutelouas, Hedeomas, Tradescantias, Verbesinas, Viguieras, mostly herbs though, "lotsa comps," but no Rue anywhere, ever.

Actually, it would be quite easy to provide a historical *overview* of systematic botany for the desert southwest, as suggested by my host: up to about 1980, one can envision its development through two concerns: 1) those who collected and did field work (the wranglers, one might say); and 2) the describers (city folk, mostly Yankees, Asa Gray for example, and others of his ilk).

The collectors can be said to have either lived before cars (BC) or after their deliverance (AD). Before cars appeared in the desert southwest, there were few collectors, travel was tough, sustenance tougher. Not many plants were collected, but plenty were described. After the appearance of automobiles and the extension of roads everywhere, numerous plants were assembled, herbaria at both large and small institutions grew exponentially (like people), urban areas formed, academic institutions grew, arcane fields of botany developed, fewer workers did real field work, more workers at the larger institutions were tied to laboratories studying fewer plants, the names of which they knew because of information provided by label data, or else they were busily at work in their offices *next to* their laboratories, the latter humming away with graduate students and post-docs extracting DNA, analyzing its contents, and feeding any assembled data into a computer so as to know what they were about. "The world is too much with us, late and soon, getting and spending . . . ." Poetry sometimes tells it better!

Historical perspectives, whatever the subject under scrutiny, are largely centered around people, their activities and ideas. Accounts of the early plant collectors or describers of the southwestern flora abound, most of these painstakingly researched and published in book form (e.g., McKelvy 1955; Ewan and Ewan 1981; etc.) or succinctly summarized by yet others (e.g., Welsh 1982; Mathias 1989; etc.). I would not like to go over such terrain again. Because of this I intend to focus my attention in the account that follows (after indulging my ego with a short introduction to myself, as already noted) upon a few select southwestern systematists, some of these dead, some alive, mainly those that have impressed *me* as remarkable researchers and individuals.

I could have singled out other workers perhaps equally noteworthy, but not many. I have purposely picked prominent "Claremont" professionals to portray or dwell upon, because this symposium is held at a facility which, to some considerable extent, nurtured them. Professionals, especially academics, people of absurdity (myself included), have always intrigued me! With this as an introduction, I will now launch into my more formal delivery.

The present address was embarked upon after read-

ing the published keynote deliveries of previous speakers to this symposium series. I was particularly impressed with the lecture of my friend and colleague, Lincoln Constance, entitled *Plant Taxonomy in My Lifetime* (Constance 1988) This made my task easier: I, too, had something to say; we've all had a life!

In his address, Lincoln recounted how he became interested in plant systematics: something born in him, perhaps, but certainly his leanings were catalyzed by individuals along the way, as he aptly noted.

Unlike Lincoln, and many others, I selected the field of plant taxonomy as a career quite by accident, or perhaps by whim. At the age of 24 in my senior year at Sul Ross State College (now University) at Alpine, Texas, after completing (1947–1949) a pre-law program under the G. I. educational bill, I took a course in native plants taught by Prof. Barton H. Warnock. This merely to satisfy the need for a few required hours in the natural sciences as recommended by my law school advisors. What an impact that class made upon me: flowering plants had names, distributions, relationships, etc. And the best way to study these was out of doors. Warnock took me under his academic wing and showed me population after population of beautiful species growing across an expanse of the mountainous Chihuahuan desert regions. I helped him press plants (1600 specimens in one night, a record for him, he said), collecting these in the environs of Langtry, Texas, hangout of the infamous Judge Roy Bean (1825–1903) and frontier home of my own relatives on my father's side who homesteaded this dry, unforgiving portion of Texas in the early 1880s. During this period, my great-grandparents operated a ferry across the Pecos River near Sheffield, Texas. Subsequently my grandfather and his family, including my father and his three brothers homesteaded a section of land along Independence Creek in Terrell County, some 30 miles north of Sanderson, a newly established hub along the Southern Pacific Railroad.

The family enterprise soon went dry: who could make a living on 640 acres of mesa lands dominated by creosote and cacti? Naturally, they moved to Sanderson, the nearest community, and became cowboys, lawmen, or railroad workers.

I digressed as to my paternal background because my early childhood experiences in western Texas must have affected my decision to switch from a planned career in law to one of academician in botany. When I first sought the opinion of Dr. Warnock regarding this matter, his advice was: forget it, Turner, the field is flooded with bodies, academic jobs are hard to come by, the pay is awful, and frankly, you'd probably be unhappy with your career choice in the long run, teaching five courses a semester in some backwater institution, with little opportunity or time for research, etc.

Nevertheless, I changed my field of study. To do so (because I was on the G. I. Bill), I had to travel to San Antonio, Texas, to take a series of psychological exams so as to assure the government that this change was motivated by inclination and *not* whim. On the exam I answered all of the multiple-choice questions with a "green slant," if plants were mentioned in any occupational choice, I selected this. Never mind that it might involve growing, or *experimenting* with plants, something that never appealed to me personally.

In hindsight, I can now recall that the first plant I ever remember seeing in sufficient detail so as to apply thereafter a scientific name was *Cirsium texanum* Buckl. (Asteraceae), a common thistle in central Texas. I was four or five at the time and remember this event vividly, having seen a butterfly, indeed a *pair* of butterflies, alight upon its purple head. Did this viewing predispose me to a preoccupation with the systematics of that family? I doubt it, but who knows what kind of neural wirings occur in the mind of a youngster?

Indeed, over my lifetime, I have asked literally hundreds of botanists how they became interested in their professions, mainly plant systematists. Most credit their early interests as having been stimulated by a particularly supportive teacher, sometimes in high school, sometimes in college. Interestingly, the few "geniuses" (or proclaimed to be such by their peers) to whom I've addressed this question usually claim to have discovered this interest on their own. Thus, the late Edgar Anderson told me, amongst an assembled crowd, that he became interested in botany at an early age, somewhere between the age of three and five, supporting this with the claim that he could remember tipping up on his toes each year, looking out his window for dandelions, knowing that these were the harbingers of spring. And my colleague at Texas, Verne Grant, graduate of the University of California, Berkeley, and a marvelous mind (that I will dwell upon in more detail later), acknowledges that he became interested in evolutionary botany at a very early age, perhaps at the age of 12 or so, having become interested in natural history through birding activities while attending summer camps in central Texas; from these interests he shifted to botany by degrees, "more by my own volition," he states, although his bent was hastened by his Boy Scout troop leader in California, and yet other teachers along the way. Similarly, big-name leaders in the field of evolutionary biology such as Darwin, E. O. Wilson, and many others, showed an early predisposition toward systematics and evolutionary studies as a result of a childhood interest in natural history. In short, given appropriate options and their own set of epigenetic preferences, they were preordained to study evolutionary biology or systematics.

As to myself, and not claiming to be even in the

shadows of any of the illustrious individuals mentioned above, I was a late bloomer. Most of my advents into nature after the age of seven (first grade), consisted of taking off for the beaches along the Gulf of Mexico in Galveston, Texas, romping over the dunes among sea grasses (*Uniola paniculata* L.), catching fiddler crabs, skinny-dipping in remote lagoons far from the truant officer's beat, etc., just having fun.

But, while not aware of this at the time, plants must have attracted me, for I can remember vividly, even now, my first encounter with a population of terrestrial orchids (*Spiranthes* sp.), their white spikes of spiral flowers appearing eerie and out of place in the mud flats along the railroad tracks near our Southern Pacific section house which, during the depression years of 1930–1933, accommodated a large collection of relatives, to some degree all idled. My parents' preoccupation with survival permitted me to roam the tidal beaches freely and even today I can still hear the raucous call of gulls and other birds as they fed along the shallow bays, much of these dominated by pickleweed (*Allenrolfia occidentalis* [S. Wats.] Kuntze).

I hope not to dwell too much about my own beginnings in this oration, for it would soon become embarrassing, but I can't help but add that the first scientific plant name I committed to memory (whilst in Dr. Warnock's native plant class) was *Solanum eleagnaefolium* Cav. (Solanaceae), a common weed throughout Texas and the desert southwest. To me, its drawn-out pronunciation was poetic; even today, cruising the highways of western Texas at age 72, I savor its sound, singing it out alone or in company. "What's that for? my friends might inquire. "Oh nothing, just an old lover, one of my first, you know petals purple with passion . . . ," and heaven knows what else I might add. I mention this personal aside because Constance (1988), in his address to this same body some years ago noted that the first scientific name he learned was *Osmaronia cerasiformis* (Hook. & Arn.) E. Green (Apiaceae-sic, really Rosaceae, the familial displacement in my oral delivery was occasioned by both ignorance and an assumption that Lincoln would have only remembered an apioid; thanks to Barbara Ertter for setting me straight on the spot!) which, as he lamented, gave way to *Oemleria,* "a word that evokes no nostalgia whatever." But, perhaps unknowingly, this set the stage for his lifelong interest in the Umbell family; for as noted earlier, who knows what neural rewirings take place in one's youth, given this or that stimulus?

Remembering the quirky uses of scientific names by yet others, I recall their careful use by my late colleague at the University of Texas, Professor B. C. Tharp (1885–1964) who, when irritated by his plight or those inflicted upon him by others, would utter "*Coreopsis cardaminaefolia,*" with the accent placed upon the *dam.*

Speaking of personal things, scientific inquiry, why we do what we do, etc., I can't help but redigress: what drives a research scientist to publish? Again, I've asked hundreds of scientists this question, from Nobel laureates (in physics and/or chemistry) to those of less exalted rank. The answer is nearly always the same: for recognition. Some exceptions, however: "because it is expected of me, otherwise I wouldn't," or, less often, "the only way to get tenure. . .or I wouldn't." One researcher, a good friend of mine, Dr. Martin Ettlinger, according to his peers a bona fide or near genius in mathematics and organic chemistry, stated to me (I'm sure he doesn't mind the revelation) that he *didn't* publish because he reckoned that anything he might discover of importance would almost certainly be discovered by yet some other researcher in the future, and that he was content to do his research without publication, or words to that effect. This puzzled me, but it makes sense: Malthus, Darwin, Einstein, Weinberg . . . whoever, their remarkable revelations almost certainly would have been discovered by others, so why publish? Perhaps it is all ego for most of the top workers, but the drive to publish is surely largely motivated by one's desire to obtain some form of respect from one's peer group while alive, although some precious few might prefer a more benign "immortality" of remembrance after death. I personally *like* to think that the reasons most workers publish are altruistic: to leave their particular field a little better structured with better perspectives; for myself, I like to believe this is so. I also strongly believe that those of us supported by the largesse of taxpayers owe it to the community to *publish* their research; otherwise we become freeloaders. This is particularly true at the larger institutions in this particular age, since teaching is no longer the primary reason for promotion, as it once was. Today, young researchers at a major university, to assure promotion, must demonstrate their abilities to obtain grants, whatever their source. To obtain grants one must publish.

Interest, of course, is probably the driving force behind all good research; its publication, however, is a responsibility. One who does research on public time or funds and does not publish is a slacker.

Research and publication by plant systematists is very individual. Some workers prefer to pick at the bones of the same taxonomic carcass from the day of their "birth" (doctorate degree) to near death, providing a plethora of "in-depth" papers on this or that genus, if not family. This has its rewards: most workers will then avoid working on the dissected corpse! Others, myself included, blithely flit across the botanical landscape, working on whatever group might arouse their curiosity, even picked-over cadavers. This creates problems. Inevitably, some other worker will

protest. "That's my group! Get your own!" I never understood this gut reaction. To me, it makes more sense to have two points of view on a given systematic problem, than only one. Indeed, too many systematic accounts are propped up by a single study, this performed ages ago before the advent of population biology and phylogenetic systematics. This is especially true for tropical plant groups in general, as well bemoaned by Peter Raven (1974) in his review of Plant Systematics for the period 1947–1972. Things have scarcely changed since.

Worldwide, there really are too few plant systematists to discern and describe taxa, much less propound their relationships using the latest techniques available to us. No doubt, the destruction of climax associations among biomes in the lesser-developed countries has resulted in the loss of numerous taxa never blessed with a scientific birth certificate! But, so what? Is mankind the more bereft? Obviously not, given a pragmatic perspective, but in my opinion deeply so, at least in an intellectual sense, the latter being the founding force of all scientific inquiry.

Why one does what one does and who cares, has always fascinated me. These questions were brilliantly addressed by A. C. Smith (1969) in his response to the question: "Is my life work important?" He notes that from the standpoint of the "cosmic optimist," the answer must be:

> ". . . certainly not; it is of no consequence whatever. The reasons for spending a lifetime as a systematist must be sought elsewhere, and these reasons in the final analysis are the reasons why we do anything—to derive personal satisfaction and pleasure from our actions. And again I would suggest that no pleasure, in the area of the biological sciences, exceeds the pleasure of the contemplation and appreciation of reality. I believe this is the reason why we are systematists . . . Our obligation to our fellow humans is to help interpret reality. Our obligation to ourselves is to comprehend and appreciate it."

In systematics, quite apart from professional incentives, the urge to publish, as I have already intimated, is probably related to a considerable extent to one's desire to achieve some form of "immortality," however brief. Many historians of science have emphasized the fact that, except for the very few, most researchers will be forgotten by their peers within 10 or 20 years of their last paper. This is less likely in morphological systematics, since the last comprehensive treatment is generally followed by most workers, at least as to nomenclature, for 50 to 100 years, and perhaps longer. But in the more conceptually oriented research, that appealing to a larger audience, one's work, unless it is seminal, is unlikely to be cited or remembered for long.

Imagine for a moment, if you will, the thousands of publications in systematic botany and fields immediately related to this generated over the past 100 years by workers too numerous to mention. How many of the latter names are part of our current memory? Precious few. And so will it be in the future. Each generation of workers is buried atop the refuse of the past: few tombstones (publications) remain exposed above this debris.

Alternatively, the memory of some workers lingers on for several generations or more, as a result of personality quirks, compulsive traits, behavioral predispositions, etc. Thus, one remembers T. D. A. Cockerall (1866–1938) for his numerous publications on plants and their pollinators (he described or proposed new names for 5,480 taxa of bees, and many new names for plants, together these appearing in 3,904 titles, this attested to by W. A. Weber, as quoted in Williams (1984); H. N. Moldenke (1909–1991) for his compulsive need to record everything ever written about the family Verbenaceae (ca. ten file cabinets chock-full at LL, TEX), most of this published in the journal *Phytologia,* of which he was both owner and editor; M. E. Jones (1852–1934), well known to workers, including those at Pomona College, for his abrasive personality and tenacity in the field; E. L. Greene (1843–1915), one of the founders of western botany, and no doubt a brilliant man, for his belief in Creationism, this resulting in a plethora of proposed new species, many, if not most, of these now synonyms, these referred to snickeringly by the mentors of my youth as chloronyms. But alas, even chloronyms were accounted for by the various indices and monographers, and appended to each name so proposed was that of its author, a form of "temporary immortality," however obscure.

I have touched upon these few botanical eccentrics to emphasize how memories of a person and their works are often propped up by personality peculiarities. And so it should be: the human condition is fraught with endless variability, all of this flexible, unpredictable, and worth cataloging.

Unfortunately, too few historians of science, or at least biographers of systematic workers, are predisposed to seek out the human side of their subjects. Thus, the life of M. E. Jones as presented by Lenz (1986) does not do justice to the man: he was much more colorful than portrayed in that telling, as is readily ascertained through a reading of Jones's personal adventures as accounted for in his uncensored journal, *Leaflets of Western Botany.* Likewise, reading the life of Aven Nelson (Williams 1984), one can only imagine Aven as a conventional academician or administrator holding a pitchfork beside his two married mates, essentially American Gothic. *Surely,* such men had more to their personalities and experiences than alluded to in their biographies!

For me, a researcher, past or present, is something more than his professional productivity, neatly

summed up in a sterile C. V. Indeed, as I've already noted, one's personality and behavior provides an aura about his/her professional persona that illuminates to some considerable extent one's potential for being remembered after death. I hope to illustrate this, or its prospect, with a few previously unrecorded comments upon three selected workers: Lloyd Shinners (1918–1971), Verne Grant (1916–) and Sherwin Carlquist (1930–), the latter two, now retired, having spent at least some of their productive careers here in Claremont and the Rancho Santa Ana Botanic Garden.

Lloyd H. Shinners

Having obtained my master's degree under his tutelage at Southern Methodist University during the period 1949–1950, I knew him well. Pity that too few others passed under the academic tutelage of this man so as to perceive his brilliance, dedication to scholarship and things intellectual. To me, he served not as a role model (I abhor the term and its implications: work not with your particular angst; assume that of another), rather he broadcast ideas and philosophies that became infectious, at least in me, then aged 24 and never previously exposed to an academic intellectual.

In size, Lloyd was a midget of a man, about 4′8″ tall or thereabouts. He was so short in stature that driving an automobile of that generation was out of the question: his legs were too short to manipulate the clutch! I drove the university vehicle on field trips with him, Lloyd precariously perched on the front seat, feet fully off the floor. Years later he *was* able to drive his personal vehicle, having had the clutch elevated by the local car dealer.

While small in size, Lloyd was a giant of a man: he believed in what he was about, plant systematics. His character and perseverance are demonstrated by his near-death experience while botanizing in the Guadalupe Mountains of western Texas with the late D. S. Correll (1908–1983). Lloyd fell from the highest peak in Texas (Signal Peak, 8751 ft.) while reaching across a bare bluff to collect a peculiar Aster-like plant heretofore unknown to him. In his account to me, he claimed to have tumbled down two hundred feet or so, landing among a heap of rocks, one of his legs and an arm fractured, thus prohibiting his movement. He also suffered a near concussion, his oversized forehead thereafter wearing a pronounced jagged scar. Lloyd lay there, terribly injured, for 15 hours or more, passing in and out of consciousness, sniffed by coyotes or wolves (so he said), and slithered upon by snakes. Even so, he managed to pull from his pocket with his good arm a penknife, which he opened with his teeth and thereafter clutched in his hand so as to ward off such varmints, were their encounters to become threatening. Correll (1971), who recounted the incident, was his companion on this trip; he discovered Lloyd's "tumbled heap of broken bones" at 10 A.M. on 16 August 1946; his broken wristwatch timed his fall: 7 P.M., 15 August.

Rescue crews recovered Lloyd's broken body well upslope along the south-facing ridges, a steep expanse of bluff below him. Later, working over his collections from that trip, with his usual sense of humor and some irony, he contemplated naming the plant which occasioned his fall (having clutched the specimen throughout his descent), "*Aster humerus-fractorum* Shinners", but named this instead *A. correllii* Shinners, which, ironically, turned out to be synonymous with *Haplopappus blephariphyllus* A. Gray, ultimately becoming *Machaeranthera blephariphylla* (A. Gray) Shinners.

Lloyd affected my thinking in many ways; foremost was his dedication to objectivity and detachment. He disdained the haughty and saw through the superficial. And he gave more to his field than he took from it, not only mentally, but materially. Lloyd spent nearly all of his pitiful wages while at SMU on taxonomic texts (he was never a favorite there, sort of tolerated as a peculiar alien; indeed, in hindsight, he could easily have been cartooned as an extraterrestrial [ET], as portrayed in the well-known movie), slenderly built, eighty to ninety pounds maybe, with arm movements gentle and precision-like, his oversized head possessing large, nearly emotionless eyes that always appeared absorbed with the distance, and a low, monotone voice that was never ruffled or raised in anger. He gave the books, which he mostly purchased from European book dealers at bargain rates, to the science library at SMU, so as to implement the research efforts in plant systematics in the biology department at that institution. In addition, he paid for his own collecting trips, mounting materials, and more tellingly, spent weekends and nights typing labels and mounting thousands of his personal collections and those of others. Single-handedly he developed one of the finest and largest collections of Texas plants anywhere, most of these collected from the northcentral portions of the state.

Knowing all of this, I used to twit him with the question, "Why, Lloyd? Why put so much effort, time, and money into a systematic enterprise that is certain to collapse with your demise?" His response was always the same, a resigned look, with the comment, "That might well be, but *I* believe in what I'm about . . . anyway, if it's meaningful, others of a similar bent will take up my cause." I understood this attitude at the time, but doubted. His outlook subsequently proved correct. The botanical books he purchased and the herbarium he mounted (once part of SMU) have now been safely ensconced at BRIT (Botanical Research Institute of Texas) in Fort Worth, Texas, and an oil portrait of Lloyd now graces the entrance to that

institution's offices. Finally, I would be remiss in my telling here were I not to note that Lloyd was a gay botanist, this at a time when any such knowledge would have accounted for his immediate dismissal. I knew his sexual proclivities, and perhaps others did, but to me at the time and thereafter, this fact merely added to the stature of the man.

Lloyd's social plight was poignantly portrayed by Correll (1971) in his obit of the individual: "The last thing he said to me [on his deathbed] brought home the fact that he had lived a rather sad and lonely life. In futility trying to cheer him, I offered to bring him anything he wished, but, as he desperately gripped my hand, he said that the only thing he wanted was sympathy. In retrospect, in the last analysis, perhaps sympathy is all that any of us really want, as well as need." I wish not to second-guess my good friend Correll, but I believe in this instance that he really meant to equate the word "sympathy" with "love"; it's very difficult for scientists to use the latter term, even in an obituary!

Praise the Lloyds of the world: those who give back to their field more than they take from it! Many, if not most, scientists of my acquaintance suck on the teats of their sustenance and joy, leaving to the future but a few ill-remembered publications and perhaps a sparse cadre of doctoral students. Their "immortality" as scientists is short, and deservedly so.

VERNE GRANT

I feel very privileged to share the academic halls with this remarkable research scientist, now 79 years old. Nearly everyone in the present audience over 50 is likely to know that during the heyday of cytogenetical systematics (1950–1970), Verne, along with Ledyard Stebbins, was the standard-bearer of experimental work in this area, at least for plants. Indeed, he even foresaw the future, molecularly speaking, publishing early on a very lucid book, *The Architecture of the Germplasm* (John Wiley, New York, 1964). The quality of his published work is outstanding, and most of his early work, which established his reputation as an experimentalist, was performed at Rancho Santa Ana Botanic Garden.

I have singled Verne's name out for this particular occasion because his developmental roots are anchored in the desert southwest, as already noted. Long before I met him, he was said to have the mind of a genius, always preoccupied with evolutionary biology, the origin of adaptations, whatever; so much so that he developed a personality that some might describe as eccentric. But during my tenure with him as a colleague, I have not found him to be so. Quirky, perhaps, but much more stable in his personal and married life than most, including me. In my many, mostly brief, conversations with him over the years, I've come to appreciate his precision in thought and his reluctance to charm. Asked questions regarding his views on life, his fears, faults, frailties, whatever, he gives what to me are weighty, insightful responses. Thus my question to him once: "Verne, what was the most adaptive behavior that you ever assumed of a moment?" or something to that effect. I was intrigued by his response. After a few moments of reflective cogitation, he said,

"I think, Billie, it might have been sometime in 1940, during field work in southern Mexico. I was traveling by motorcycle and collecting plants as necessary. Having collected until dark, I pulled my cycle off the highway next to a cornfield, spread out my sleeping bag, and settled in for the night, a knife and machete at my side. About 3 A.M. I was awakened by a noise and stinging sensation in my buttocks, this apparently due to someone firing a gun at my prone body from along the edge of the cornfield. This I surmised, because I could see sparks coming from an old rifle pointed towards my body, this held by a Mexican *campesino,* or some such, crouched on his knees taking dead aim. Of course, I was startled by, and fearful of, my predicament, believing that he had intentions of killing me and assuming my possessions. Aroused from sleep in such a fashion, I really knew not what to do, but thought I had better do something fast, and so energized, I sprang naked from my sleeping bag, rapidly took up my two weapons, one in each hand, the machete raised threateningly in my right, and dodgingly made for the culprit, screaming at the top of my voice like some wild banshee. This must have startled the shooter, for upon seeing this fearsome-looking gringo charging in his direction, he hesitated for a moment until the two of us stood face to face, ten feet apart or so, then he backed off furtively into the shadows of the cornfield, not to be seen again. I then retreated, remained hidden for a few minutes maybe, then gathered up my things hurriedly and drove off on my bike, which was rather painful, for the bullet had left a rather deep wound in the cheek of my right buttock, which had been grazed with that first shot. I suspect, Billie, that my intuitive reaction at that time to the dangers at hand must have been one of more highly adaptive momentary actions."

I was impressed: who would have believed that this relatively benign-looking colleague would ever react to a threatening situation in such fashion? Lest I doubt the veracity of his story, Verne later provided me with a Xeroxed account of this encounter, with both place and time: N of Huayapan, Oaxaca, 20 Oct 1940, (Grant, V., *Journals of Travels 1940–1942,* unpublished but scheduled to be deposited in the Barker Historical Center, University of Texas, Austin, at the time of his death). He also gave me a picture of himself

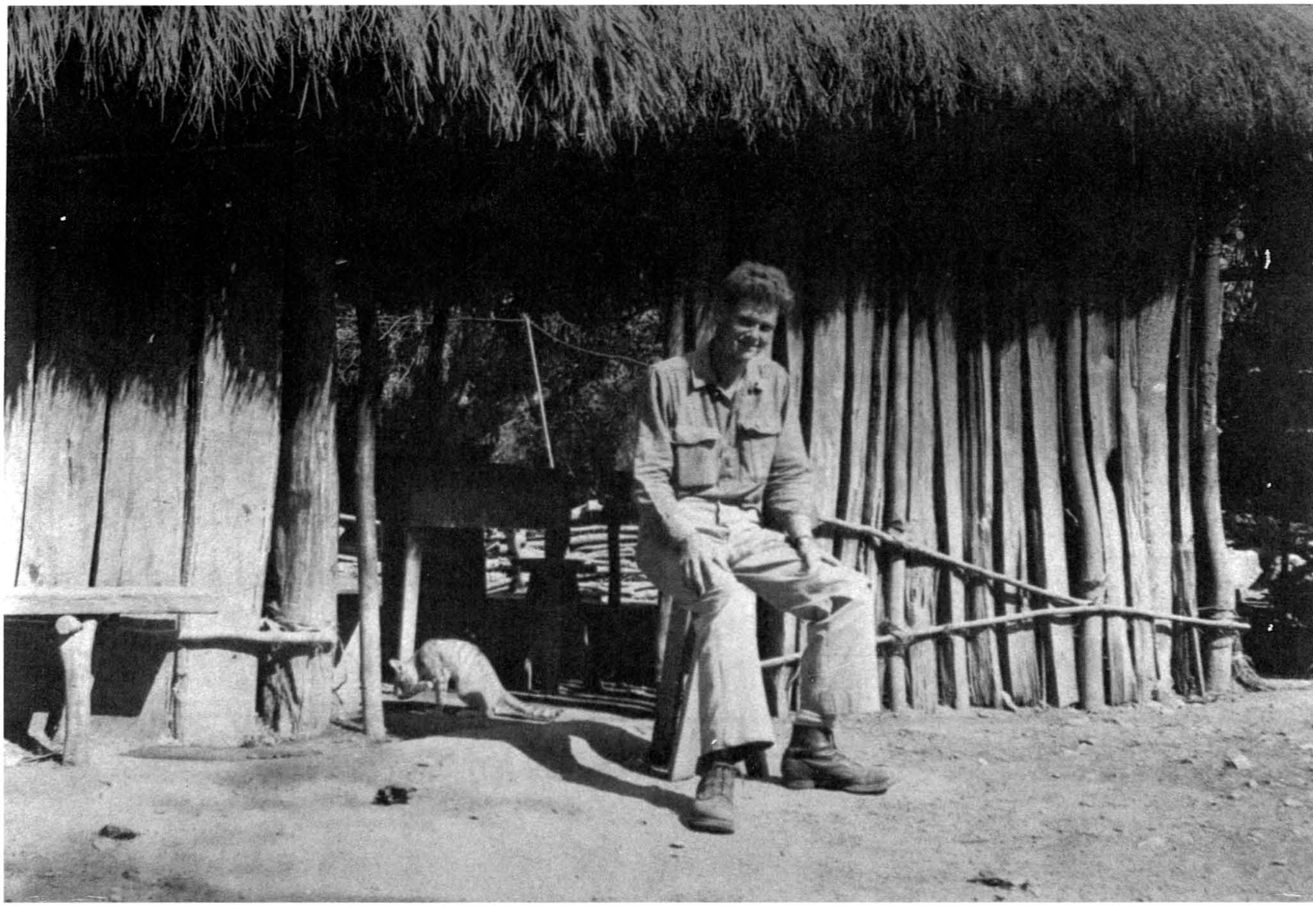

Fig. 1. Verne Grant during his Mexican-Central American field trip at age 23 (Nicaragua, February 1941; courtesy Verne Grant).

taken during this trip, a rather handsome, peaceful-looking fellow (Fig. 1).

SHERWIN CARLQUIST

Interesting fellow, this Carlquist, wonderfully productive and well known for his work on anatomy and biogeography, he has justly earned his kudos, as eloquently elaborated upon by Wagner (1994). I hardly know the man, except from the perspective of an admiring professional. We first met at an AIBS. meeting some 30 years ago, by pure chance you might say, my having sat next to him of a sudden, thinking he was a graduate student (he is younger than I by five years). Noticing his name on the convention badge adorning the lapel of his coat, I accosted him with a silly comment, "Ah, so you're the young anatomist Carlquist everyone is agog over, proclaiming you to be a genius. Is it so, the genius part, I mean?" Or something to that effect. He turned, looked me full in the eyes with that faint wisp of a smile he's prone to wear (cf. his portrait in Wagner 1994) and responded, quite confidently, "Yes, that's me." I was delighted with his comfortable response, intrigued even, not having a clue as to its meaning.

Never a friend, no doubt because of time and distance, I nevertheless followed his work, most of this performed at the same institutions that nurtured Verne Grant.

Carlquist's books and many well-honed research papers will insure him recognition and "immortality" for a generation or two after death, but ultimately his contributions will be shrouded over by yet another Young Turk taking up tunnel tomography or some such for his anatomical explorations, and certainly *molecular* biogeographers are likely to rewrite the natural history of insular adaptations and migrations, leaving Carlquist's contributions as but interesting asides in the long journey towards achieving reality. And so it is with most of our efforts to shore up our memories post-mortem.

Sherwin, however, has added something to his C. V. and well, what else, to his oeuvre, that will not permit his "immortality" to be eclipsed soon: his marvelously crafted two-volume pictorialized account of *The Natural Man* and *Man Naturally* (Pine Cone Press, Claremont, 1991; Pine Cone Press, Santa Barbara, 1996). I purchased both soon after Prof. Robert Thorne told me of their existence (at a symphony in Heidel-

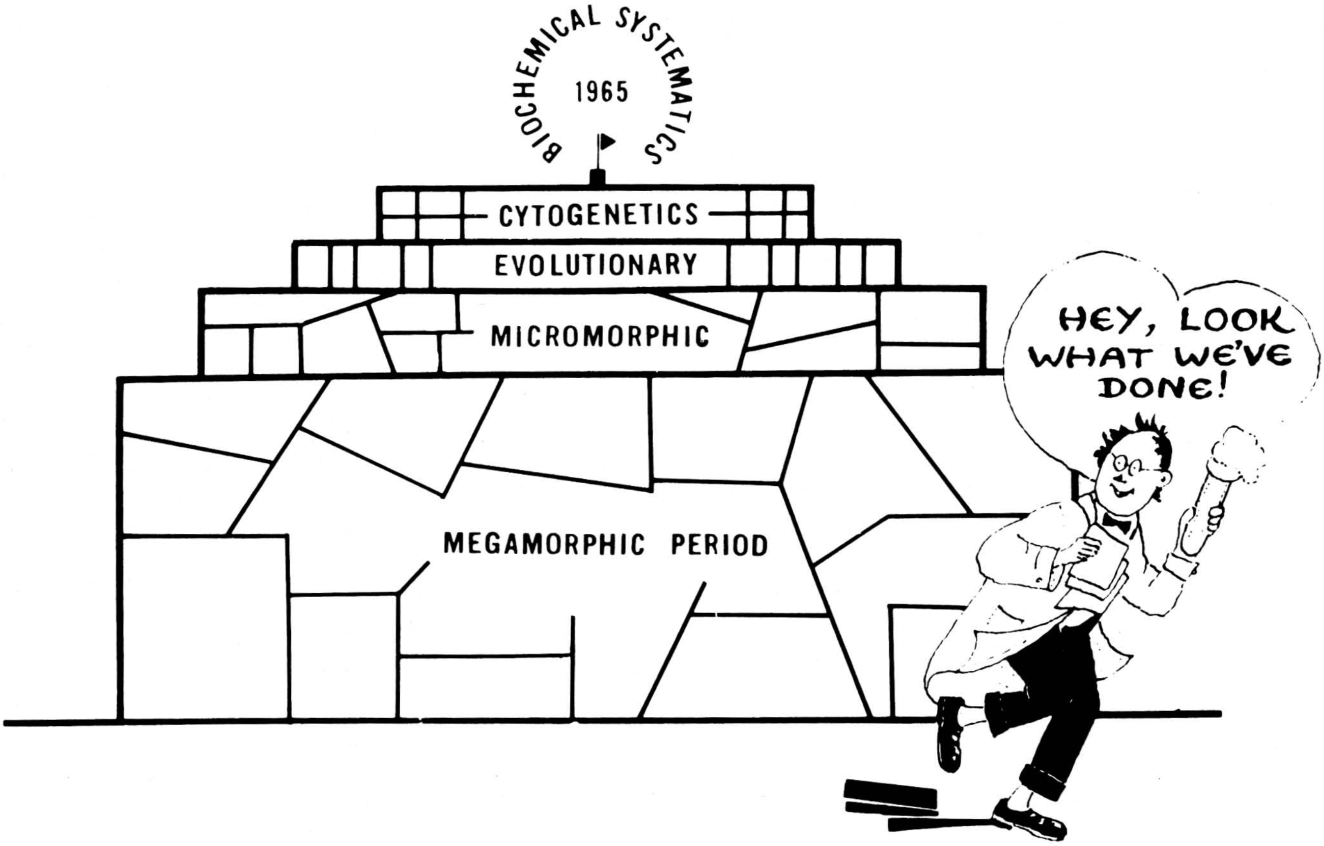

Fig. 2. Optimist's view of biochemical systematics, circa 1965 (Turner 1967).

berg, Germany!). Thumbing through these upon their arrival in Austin, I was astonished at the grace and charm with which Carlquist arranged his subjects, how they became part of the natural landscapes, pristine things welling up the purity of what once was, before man began to create taboos, tattoos and religions. And I've never discussed this with Sherwin, but I'm certain that his venture into the world of art will assure him a long and perhaps more deserved immortality than those of us who dabble in things scientific. After all, one cannot improve on a Titian or a Turner, imitate their creators even. Their artistic immortalities are "forever"; so may it be with Carlquist.

ENDINGS

In one of my more lucid epiphanies, while writing a chapter on the history of systematics for the text, *Biochemical Systematics* (Alston and Turner 1963), I portrayed the field as having developed over 2000 years, beginning with Aristotle, this symbolically represented by a pyramid as shown in Figure 2. I imagined the field of systematics to have grown by the accrual of inventions and mental artifacts over time, each of the latter producing a flurry of activities leading to the understanding or reality of what the course of organic evolution might have been on this planet. Each tier of the pyramid was elevated and enhanced by such inventions and conceptual developments, including (after its very large basal construction using megamorphic data) that brought on by the microscope, followed by the development of evolutionary concepts, cytogenetical concepts, and finally chemical concepts, largely comparative DNA. In 1963, contributions of the latter were represented by relatively few publications or research, this certainly magnified in the small box atop the edifice as shown in my cartoon, although the theoretical foundation for its huge impact upon the development of that edifice was obvious. I further envisioned that the various workers involved in the accumulation of systematic data, whatever their source, would lead ultimately to the formation of an integrated body of information, some large, quadrangular, finely polished block of knowledge, no cracks anywhere. I still think this is happening, but current workers of my generation, and perhaps the one thereafter (having now lived through three generations of aspiring academicians, many of the second my academic sons, these in turn spawning academic grandsons galore!) are not convinced. This is aptly portrayed in Figure 3.

In casual conversation with peers of my age group, many complain and carp thusly: "Hell, Turner, the

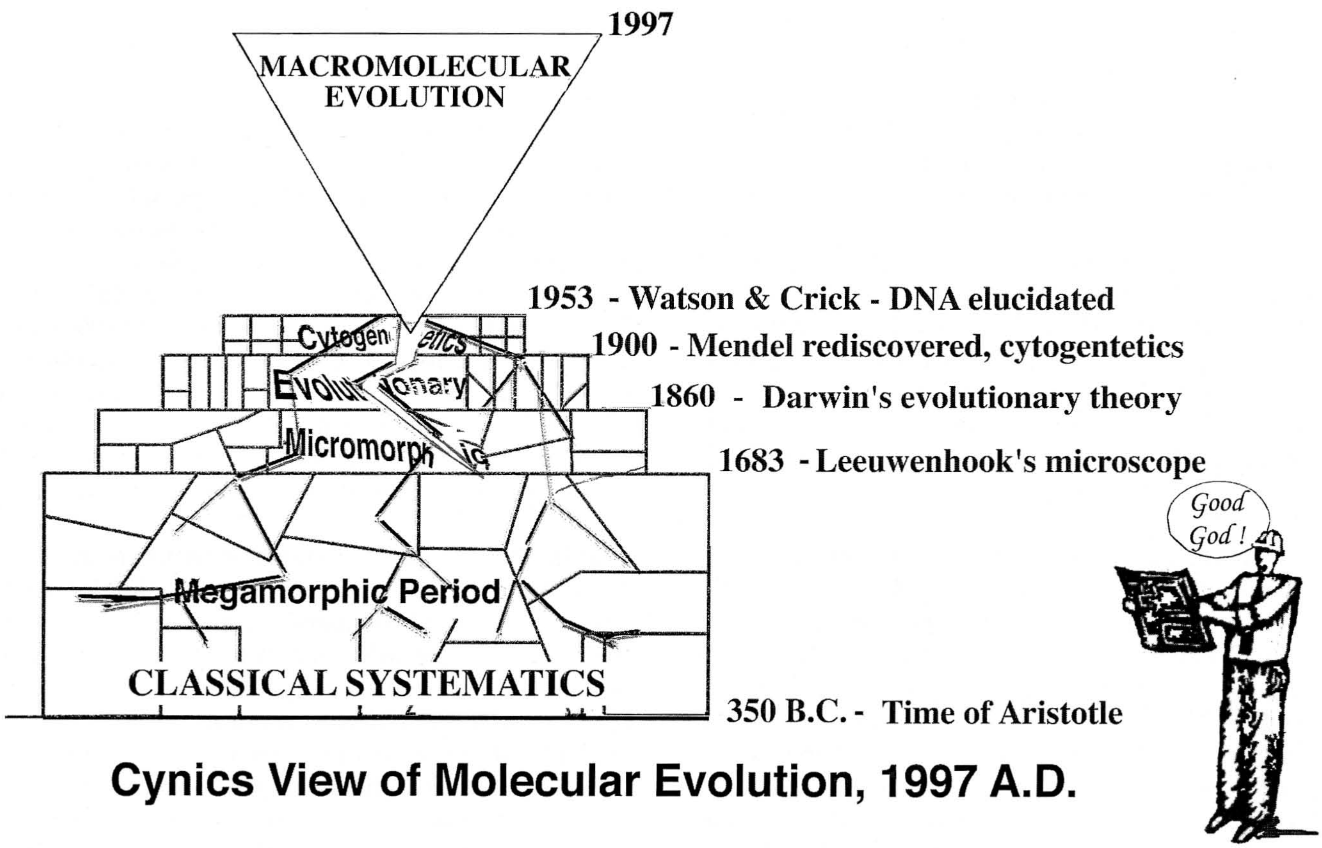

Fig. 3. Classicist's view of biochemical systematics in 1997, as viewed by the cynic.

DNA workers are constructing classifications based upon the analysis of only one or two genes, something we gave up eons ago!"—or more tellingly—"Christ! We're getting E. L. Greene stuff all over again, only this time it is more alien, classified from the green organelle, the chloroplast, I call cladograms derived from such data, chlorograms . . . " etc.; or worse yet, "Damned if they don't even use junk DNA [ITS regions] to ascertain relationships: that's like Aristotle using habit to classify taxa," etc. Ignorant, of course, these complainers, overawed by the direction their field has taken, uncomfortable in the swill and backwash of the *new systematics* (ever new!). But their complaints have a point, and some of their sneers are deserved.

Papers in plant systematics presented at national and international meetings are now predominantly DNA-oriented; journals, too, are plastered with classifications based upon one or two, often semisequenced genes, obtained from a single organelle, their samples (usually one or two plants) derived from an infinitesimal portion of the taxon's variability, etc. Most molecular workers counter their cynics with comments such as, "My data were gathered without bias, except as to gene, it was evaluated by a computer via algorithms developed by others, the construction of which I'm ignorant (even less bias!), and my product is in the form of a cladogram, whose outgroup selection for comparison purposes was suggested to me by yet others more familiar with the groups (less bias yet!), etc."

This was tellingly brought home to me recently by one of my own students who, during his doctoral defense, flashed upon the screen a Kodachrome slide of the University of Texas Tower (wherein resides our 1.3 million research specimens housed on five floors) and noted, with some pride, that all of his field work was done in that building; wryly stating, "that I collected [i.e., sampled] 41 of the 42 genera of my phylogenetic study (tribe Tageteae, Asteraceae) on the 3rd floor; DNA from these was amplified by the usual methods; identification of my samples were made by yet other workers, but I have annotated each as vouchers for my study; all of the work was performed in Dr. Bob Jansen's laboratory, and we are preparing this publication as soon as we complete our computer analysis of the data."

I mean not to cast aspersions, for his is a fine two-gene study that, in my opinion, lays the framework for a truly phylogenetic arrangement of the genera concerned. Indeed, the candidate also allowed as to how he did look at morphological characters, (largely as suggested by others more familiar with the groups), adding this to his cladistic analysis so as to provide some classical dimensions but, in my opinion, the two

sets of genetic data are best left unaltered by the morphological data. At the higher categorical levels molecular data are best used to test morphologically based arrangements, and not vice versa. What is needed to vouchsafe further his study are more genes.

I should digress here and note that the candidate, now Dr. Loockerman, was gobbled up almost immediately by the commercial marketplace and is a DNA forensic analyst with the Texas Department of Public Safety, and suffers little from his lack of field experience or ignorance of things megamorphic. Yet other fine systematic workers from Texas have gone this route; and who can blame them? Their pay is better with more likelihood of some form of tenure than exists in academia today.

Like all progress in science, changes in theory and practice encounter resistance, hostility even, more so when the field becomes unintelligible to those left in its wake. One might readily appreciate the classical worker's lament when he finds, for example, that the well-studied mostly temperate plant family, Saxifragaceae (s.l.), which was rigidly structured in Engler's *Syllabus der Pflanzenfamilien* as late as 1964, so as to encompass 79 genera distributed among 17 subtribes. But the work of Soltis et al. (summarized in Soltis and Soltis 1997), using topologies based on 18S *r*DNA and *rbcL* gene sequences, reckons that the family should consist of only about 30 genera, and that at least five of the subfamilies should be positioned elsewhere, outside of the Saxifragaceous clade. Indeed, the restructuring is quite profound; nevertheless, the workers concerned correctly note that "these relationships are also supported in large part by other lines of evidence, including embryology, serology, and iridoid chemistry."

In the future, comparative macromolecular data are certain to serve as the main criteria for the phylogenetic arrangement of higher categorical taxa, at least above the level of genus. Hopefully, comparative studies of several genes or more will be employed in these endeavors. (As an aside, I've even thought, in my idle musings, about the *International Code of Botanical Nomenclature* for the year 2100; under the chapter entitled Macromolecular Applications, the code will read, "at least 10 genes giving rise to a like number of metabolically important enzymes must be employed before such data can be used to realign families or "dangling genera," among many, the following genes are recommended . . . .")

The use of "gene trees" to ascertain the cladistic and/or phyletic relationships among closely, or even distantly, related species within a given, or among closely related genera, is fraught with peril. This is perhaps largely due to the frequency of natural hybridization among taxa, either extant or ancient. Ignorance or the denial of this fact has been elegantly groused upon by Grant (1992), especially as this relates to the southwestern *Ipomopsis aggregata* (Pursh) V. Grant. His comments quickly elicited rebuttal by Wolf, Soltis and Soltis (1993), all of this exciting repartee *if* one is interested in the relationships of *Ipomopsis aggregata.* Doyle (1992) especially deserves credit for calling attention to the ludicrousness of evolutionary trees based upon a single molecule, DNA or otherwise. Other workers have added to the protest, thus Kellogg, Appels and Mason-Gamer (1996) also decried the uncritical use of gene trees as applied to the well-studied grass genera of the Triticeae, noting that relationships among these are difficult to assess, this probably occasioned by a "complex phylogenetic history, possibly caused by extensive hybridization and introgression . . . ."

While the "modern" systematist, working away his life in the laboratory sequencing genes and/or doing restriction site analysis among a group of closely related species might *think* that this is the best and most efficient way to obtain a reasonable phylogeny, I hasten to differ. Systematists working in the field who are familiar with these taxa as populational systems, who understand their morphogeographical relationships and propensity to maintain populational integrity in *spite* of occasional hybridization with perhaps concomitant introgression, are much more likely to achieve an efficiently arrived at classificatory treatment or phylogenetic history of a given generic group than the DNA worker who might be ignorant of those relationships. Thus, the work of Elisens, Boyd and Wolfe (1992), using allozymes by which to clarify species relationships within the morphologically highly variable genus *Aphanostephus* (Asteraceae) scarcely improved upon the insights provided by Turner (1984), who used chromosomal and morphological data in his analysis, this in a paper published in *Phytologia*!

One of the problems facing a young doctoral worker entering the academic marketplace in plant systematics nowadays, at least at the larger institutions, is that of time constraints. It used to be (before our preoccupation with DNA) that fledgling systematists spent most of their research time doing field and herbarium work (character analysis, etc.), this accounting for 60–80% of their research time. Today's neophytes, however, may spend up to 80% of their time in the laboratory isolating and analyzing DNA. This is regrettable. Personally, I think experienced morphological monographers doing field and herbarium work will more certainly provide a more *efficiently* obtained, better systematic overview of a given small genus (up to ca. 30 species) than a similar study rendered by a laboratory worker using purely molecular data.

Morphological monographers are a disappearing breed. Currently, there are too few of these to do the large number of studies yet needed, especially in trop-

ical and subtropical regions. But this is not new; what is new is our realization that many species will be born to bloom unseen. I mean *biological species,* not "herbarium species," or "cladistic species," or "phylogenetic species" even, as espoused by some authors. I need not cover again the various arguments for and against such concepts. Suffice to say, that the integrity of a taxon, populationally speaking, is best determined by its relationship *in the field* to yet other potentially interbreeding units; character analysis, both within and between populations, especially as related to coherence, however inferred, is an important aspect of this work. Those who diligently work at such studies will have the edge over most DNA analysts that lack such knowledge.

Some 25 years ago (Turner 1971), looking into the future, I speculated that any first class institution espousing a first class program in plant systematics might ought to have at least two systematic professors: one morphologically oriented, the other molecular. Unfortunately, most departments of today (the future then is now!) are attempting (indeed are) to hire two-headed Januses: any new, tenure-track faculty member must be both a molecular and morphological expert; even prospective *curators* are often expected to know DNA laboratory procedures.

As a professional systematist working in the trenches (i.e., mostly out of doors collecting and describing new species and/or genera), I am dismayed by the diminishing few who can, or even care to, provide this service. Specialists do exist for this or that selected group, but increasingly their focuses are narrower than their predecessors, either as to taxon or geographical region. Other workers have lamented this loss as well, but few administrators who might correct this imbalance have emerged.

The loss of experienced, field-oriented taxonomists, however, is not all bad! The slack is certain to be taken up by others. Two recent developments lead me to this conclusion: one sociological, the other technical. Modern society, in America, Europe and parts of Asia, at least, has seen the development of highly intelligent amateur botanists with time on their hands and *interest* in plants, this occasioned by increased wealth, early retirement, or both. Such workers exist in large numbers, not only in California, but Texas and elsewhere. They are really a silent majority. But probably not for long!

With the development of the Web, e-mail, home pages, etc., the "amateur" is likely to flood the world networks with new observations, new records, new species, etc., this all documented with localized maps, ecological observations, colored photographs, flowers dissected down to detail, even as to stereodepiction, this all to be downloaded within moments by anyone anywhere. The International Organization of Plant Systematics must become aware of this prospect and make plans accordingly. What will constitute legitimate publication in the future, etc.? That international body faces a daunting challenge, and I wish it well.

For myself, in the few years left to me, I will fret but little over all these developments. The Web and all of its accouterments I will continue to use as suits my fancy, mainly for bibliographic purposes. *My particular ending,* as I've already noted, will be in that little dead-end canyon just west of Alpine, composing an illustrated microflora of those 40 acres or so (I've already started this!), a sort of Thoreau's pond without water, one might say, fully documenting the natural history of this and that taxon, their wider distributions, how these managed to gain a toehold in this particular place. My study will never be completed, of course, but that's not important. Working on this is bound to give me pleasure, and even after my deliquescence I will continue to relate to these species, becoming part of them even. This too pleases me. My epithet (on a little plaque embedded in a volcanic boulder along the dry stream bounded by oaks and madrones) will read:

In this canyon are strewn
the ashes of Billie, a botanist.
But what matter his name?
He went as he came, naked, unashamed.

LITERATURE CITED

ALSTON, R. E., AND B. L. TURNER. 1963. Biochemical Systematics. Prentice Hall Inc., Englewood Cliffs, New Jersey. 404 p.

CONSTANCE, L. 1988. Plant taxonomy in my time. *Aliso* **12**: 1–5.

CORRELL, D. S. 1971. Lloyd Herbert Shinners—a portrait. *Brittonia* **23**: 101–104.

DOYLE, J. J. 1992. Gene trees and species trees: molecular systematics as one-character taxonomy. *Syst. Bot.* **17**: 144–163.

ELISENS, W. J., R. D. BOYD, AND A. D. WOLFE. 1992. Genetic and morphological divergence among varieties of *Aphanostephus skirrhobasis* (Asteraceae-Astereae) and related species with different chromosome numbers. *Syst. Bot.* **17**:380–394.

EWAN, J., AND N. D. EWAN. 1971. Biographical dictionary of Rocky Mountain naturalists. Bohn, Scheltema and Holkema, Utrecht, The Netherlands. *Regnum Veg.* **107**: i–xvi. 1–235.

GRANT, V. 1992. Systematics and phylogeny of the *Ipomopsis aggregata* group (Polemoniaceae): traditional and molecular approaches. *Syst. Bot.* **17**: 683–691.

KELLOG, E. A., R. APPELS, AND R. J. MASON-GAMER. 1996. When genes tell different stories: the diploid genera of Triticeae (Gramineae). *Syst. Bot.* **21**: 321–347.

LENZ, L. W. 1986. Marcus E. Jones. Western geologist, mining engineer, & botanist. Rancho Santa Ana Botanic Garden, Claremont, California. 486 p.

MATHIAS, M. E. 1989. The fascinating history of the early botanical exploratory and investigations in Southern California. *Aliso* **12**: 407–433.

MCKELVEY, S. D. 1955. Botanical explorations of the trans-Mississippi west. 1790–1850. Arnold Arbor., Jamaica Plain, Massachusetts. 1144 p.

RAVEN, P. H. 1974. Plant systematics, 1947–1972. *Ann. Missouri Bot. Gard.* **51**: 166–178.

SMITH, A. C. 1969. Systematics and appreciation of reality. *Taxon* **8**: 5–3.

SOLTIS, D. E., AND P. S. SOLTIS. 1997. Phylogenetic relationships in Saxifragaceae sensu lato: a comparison of topologies based on 18S *r*DNA and *rbcL* sequences. *Amer. J. Bot.* **84**: 504–522.

TURNER, B. L. 1967. Plant chemosystematics and phylogeny. *J. Int. Union Pure Appl. Chem.* **14**: 189–213.

———. 1971. Training of systematists for the seventies. *Taxon* **20**: 123–130.

———. 1984. Taxonomy of the genus *Aphanostephus* (Asteraceae–Astereae). *Phytologia* **56**: 81–101.

WAGNER, W. L. 1994. Sherwin Carlquist—recipient of the 1993 Asa Gray Award. *Syst. Bot.* **19**: 1–5.

WELSH, S. L. 1982. Utah plant types—historical perspective 1840 to 1981—annotated list, and bibliography. *Great Basin Naturalist* **42**: 129–194.

WILLIAMS, R. L. 1984. Aven Nelson of Wyoming. Colorado Associated University Press, Boulder. 407 p.

WOLF, P. G., P. S. SOLTIS, AND D. E. SOLTIS. 1993. Phylogenetic significance of chloroplast DNA restriction site variation in the *Ipomopsis aggregata* complex and related species. *Syst. Bot.* **18**: 652–662.