

HISTORY OF THE BIOSYSTEMATISTS ORGANIZATION

some records and recollections from J.A. Powell

February 1999

This memo is a belated follow up to the discussion we held at Stanford last spring, the inquiry by Brent Mishler in May, which dealt more with ongoing changes in makeup of the 'membership' than with its history, and is a response to the subsequent request for historical information distributed by Bill Lidicker in September and his summary of the history of the organization dated January 7, 1999.

There was an annual Newsletter of the Department of Entomology, called the *Vedalia*, that was produced from 1930 to 1970. I read through it for the years 1936 to 1970 but found very few mentions of the Biosystematists. Evidently faculty who were participants and/or secretary of the group did not consider it sufficiently important to mention along with other academic and personal activities. R. L. Usinger's autobiography (1972, Robert Leslie Usinger: Autobiography of an Entomologist, Pacific Coast Entomol. Soc., S. F.) yielded a little more about the early years.

In 1936 E. G. Linsley was still a graduate student but was spending the first of two years at UCLA as a Lecturer, to return in July 1937; and Usinger, also a graduate student, was in Hawaii and Guam surveying insects with Swezey. Hence neither was present when the Biosystematists first met [assuming Lidicker's notion that fall 1936 is correct], in contrast to Stebbins' recollection at the meeting we held last year.

Usinger (1972: 68) recalled that he returned from Hawaii in August 1936 and "a few months later" was given the job of Assistant Curator at the CAS . . . "It was during this time that the Biosystematists organization was started by David Keck . . . the meetings were very inspiring . . . [a paragraph that mentions Babcock, Goldschmidt, Stebbins, Clausen, Ferris]." "I used to ride to the meetings whether at Berkeley or Stanford, with the Academy's new director, Dr. R. C. Miller . . . [so, RLU probably joined the group ca. early 1937, while still a graduate student]."

Usinger also mentions (p. 78) that while at UC Davis (he was on the faculty there from 1939 until WW II, after which he transferred to the Berkeley campus) that "during this time I regularly attended "Biosystematists" . . . and other meetings on the Berkeley campus" -- entomology at that time was taught at Davis as a part of the Department at Berkeley.

I assume that Linsley must have joined the Biosystematists when he returned to Berkeley in summer 1937; EGL completed his Ph. D. in 1938 and seems to have inherited the faculty position left by E. C. Van Dyke, his major professor, when the latter retired in 1939. I am writing him to ask for recollections. His only mention of Biosystematists that I found in *Vedalia* was in 1941 -- in February he addressed the Biosystematists on problems of evolution of floral relationships of bees. That would explain the letter from Miller that Lidicker found in which Miller expressed regrets for not being able to attend the meeting and likely had no relationship to the organizer/secretary person, although Linsley could have been the organizer/secretary.

Paul Hurd began serving as museum technician in 1948 while a graduate student, obtained his Ph. D. in 1950, and took a position as Senior Museum Entomologist. He had a Lecturer title in the late 1950s, and I believe was appointed to Associate Professor in 1959-60 when he was on sabbatic leave in Argentina. According to Lidicker's record Hurd took over as Secretary of Biosystematists in for the 1960-61 academic year. That corroborates my recollection that Hurd was in charge in 1961 when I joined the organization.

In Vedalia for May 1965, Hurd mentioned that he ". . . continues as major domo of Biosystematists . ." [which implies that he had the job in 1963/64 and 1964/65, although he had not mentioned it in 1963 or 1964]. I think he had been the Secretary since 1960 and probably continued until 1966 when Lidicker's record indicates Ned Johnson took over. Hurd left Berkeley in fall 1967 for a year as program director for Systematic Biology at NSF, and then permanently in 1970 when he accepted a position at the Smithsonian.

Lidicker suggests that Bill Clemens may have continued as Secretary through fall 1973 and John Chemsak seems to have functioned as Secretary in Spring 1974. However, I am sure that I took over as Secretary in fall 1973 -- I had been away the latter half of that summer, and when I returned Ev Schlinger, who was a member of an executive committee of Biosystematists , told me he had volunteered "us" to take over the job. Of course it was left to me. I am fairly certain that I held the position for two years, although I think I depended upon persons at CAS, UCD and Stanford to arrange meetings at their institutions each year.

By that time, whatever controversy that existed about women membership had waned -- there were many women participants. However, we had an incident involving student participation. After a graduate student showed up claiming to be the guest of one of our members -- he had seen the announcement on the faculty member's desk -- and dominated the discussion. I made a pointed announcement at the next meeting that we should restrict the attendance to members and their invited and accompanied guests -- which had been a long standing tradition (I was unaware that Usinger and perhaps others had been regular attendees while still graduate students). Most or all of the membership agreed with me -- I remember Ned Johnson being particularly outspoken in support of this policy.

I am sure that John Chemsak never held the secretary role; I recently asked him, and he confirms that. John helped out for a considerable period by taking care of some of the local arrangements, such as providing the sherry for the pre dinner social hour. At some point the Faculty Club raised their prices such that we decided to provide our own sherry. Later they outlawed that practice, which was a deciding factor in the group electing to leave the Faculty Club as a dinner site, after nearly 40 years. That was after my tenure as Secretary. I know I joined the Faculty Club for the first time in fall 1973 and charged the dinners to my account through my tenure as Secretary.

In contrast to my habitual customs with almost everything else, I did not save past meeting notices nor any other records of Biosystematists (except when I had been the speaker, in 1964 and 1974); I realized this when acting as Secretary and found there seemed to be no record of its history -- we had no office, no dues, no elected officers, so nobody had ever been

responsible for retaining records of the organization so far as I could tell. Therefore, I solicited the membership for past records in 1974 or early 1975. The only appreciable result was from John Tucker at UCD, who had kept the upper half of the mimeo reservation forms we used to send, dating back to when he joined the organization, in the late 1940s I think. I made a xerox copy of those notices and returned the originals to John. I put together everything I could find in a 3-ring binder and passed it on to Nancy Vivrette, who succeeded me, at the end of the 1975 academic year. I am quite certain of that.

I was disappointed to learn that Nancy has no recollection of the notebook nor its fate. According to Lidicker's reconstruction, George Papenfuss and then Joe Gregory succeeded her, so maybe one of them may have left records.

I attended the meeting Lidicker calls the "40th Anniversary" testimonial, in May 1978, and therefore evidently was organized by Papenfuss. I remember thinking at the time that somebody should have brought a tape recorded, and I took some handwritten notes. It sounds like Bill did too or has discovered records made by somebody else. Mine were on my desk for years, but I never got around to transcribing them until now, so their content is subject to interpretation (see appendix). Probably I didn't record anything Bill already has, but because there were several of the founder members present, that was our best chance to recover some of the early history.

The only other records of Biosystematists I found are in a folder that I assembled between 1986-1993. It has an undated memo "To members of Biosystematists: A plan for the future" by G. L. Stebbins, and a copy of "A plan for the future of Biosystematists, approved by the council and membership, March-April 1986". There are some records for 1990-93, when I was Membership Chairman. Back then and before we used to consider any new member by having the person submit a C. V. -- generally anybody on the staff or faculty of any academic institution or museum in the area, including visiting post docs etc. was rubber stamped as a member. Lidicker's record indicates that committee was abolished mid year 1993/94 [when Mishler's influence began to take hold], so I was the last to receive such nominations. My recollection is the committee used to meet once each fall at the Botanic Garden to discuss membership and other matters, at least when Don Kaplan was the Chair.

I am forwarding this folder to Bill, but it looks like all these recent records are already available.

Addendum: My opinion on the changed nature of Biosystematists

When I joined I was told that the organization met to discuss topics in systematics and evolutionary biology that represented research in progress -- i.e. a forum for discussion of ideas not yet finalized and published. This has been reiterated every time Stebbins comments on the

organization, and certainly often the discussions have been of this nature over the years. Discussions of my presentations for example, in 1964, 1974, and 1984 provided me with input that I used in further research and rewriting, for publications in 1966, 1984, and 1994 -- my lag time grows!.

A major shift in the composition of the membership and the discussions took place in the 1960s-early 1970s. The group makeup changed and many of the presentations have been more like seminars given by visiting firemen and finished products such as interview talks by new members who never knew the original intent. This has been a much more profound change than the addition of women that people seem preoccupied with in retrospect. I attribute the change to three factors: 1) a shift towards a more social evening atmosphere; 2) expansion of the membership to include a wider variety of institutions, such as the State Colleges and California State Department of Food & Agriculture, and 3) moving the meetings around to more institutions. The three are interrelated and not listed necessarily in order of chronological initiation or importance.

The last theoretically should not have affected the composition of the meetings if a program chair for the year had organized it, but it evolved that the chair saw it as a chance to hand off all responsibilities for that month. Moving to a site where teaching is the primary responsibility often or usually meant they picked a new member of the faculty who gave his or her interview talk or some other canned subject.

I trace the beginning of the shift away from origins to a meeting at the CAS, in December (1966, I think but it could have been a few years later), which was the first meeting held there that I recall. Somebody at the Academy was assigned the job of local arrangements and selecting a speaker. He selected the Director, George Lindsay, who was away on an expedition cruise, scheduled to return shortly before the meeting. His solution was to offer an open bar around the crocodile pit-- when introduced prior to the meeting upstairs, there was a standing ovation -- and then he showed a movie in lieu of research talk. That meeting more or less coincided with the early days of a broader diversity of members generally, and I thought it changed the image. After that, especially at places other than Berkeley and Stanford (but even at Stanford at least once there was an open bar in the faculty club and no host bar at others), more emphasis was placed on the social hour, dinner, and less on the substance of the speaker's presentation. Along the way the idea of in-progress research as a discussion topic often or usually was lost. When we left the Berkeley faculty club because the meals had become too expensive, and went to Spats or wherever with a bar available, that contributed to the shift in emphasis.

I do not remember there being any controversy about Biosystematists being a "men only club"; although some older members may have objected when the first women were suggested, as indicated in Lidicker's recent summary of the topic. Quite frankly, I think the "controversy" has been stirred up in retrospect by the historian (Smocovitis?) who spoke at our meeting at Stanford last May and others of her age. I think there simply weren't any women academics in systematics and evolutionary biology at Berkeley, Davis, Stanford, or the CAS in the 1940s and 1950s. Remember, we did not have any members from the State Colleges, as they were called prior to the Reagan governorship, so the membership was exclusive in other ways then too.

It is difficult for students and younger faculty now to imagine a time when there were no computers or even xerox machines, or a time when few women obtained higher degrees and if they did, often did not succeed in obtaining academic positions. For example, there were 0 female graduate students in systematic entomology at Berkeley throughout the 1940s and 1950s (ours was the largest such program in the country at the time). We had two women graduate students in the 1960s, both married fellow graduate students by the time they finished Ph. Ds., and both elected to not even apply for positions. Both accepted the role of homemaker and part time researcher at the institution where their husband obtained a position. In that kind of climate, I think Biosystematists simply existed without women, rather than excluding them. Times have changed in society as a whole, and Biosystematists did too.

Finally, we have what might be termed the Mishler era. His influence has been a positive one in reversing the trend from social evenings to stimulating discussion oriented meetings. Often the subjects have been controversial or current topics in evolutionary biology, not a return to discussion of in-progress research on specific projects, so another shift has occurred during the past 5 years. This has been augmented by the recent general invitation of non-academic staff and graduate students, even to their organizing and presenting the last meeting. Whether this shift in emphasis is "good" for the organization depends upon your point of view, and I suppose will have to be viewed after passage of time. Will it enhance the attractiveness of the organization and its meetings by attracting more faculty? I think we will see fewer faculty participating because the new format isn't essentially different from a wide array of seminars and noon meetings available on campus every day. After spending all day listening to graduate students one-on-one and in seminar discussions, is an evening meeting with more of the same attractive?

It appears to me that in recent meetings there have been fewer gray haired old farts like me who remember the original intent and composition -- and good riddance some will say -- but also fewer real organism current research topics, fewer faculty participants, fewer people from institutions elsewhere. It seems likely that each campus meeting will gradually tend to be dominated by local students.

Biosystematists was an unique organization: active research professionals in systematics and evolutionary biology from a few research oriented institutions who got together to discuss their ideas. Now it has none of that unique combination. It does not differ from many other discussion groups across the various campuses, and the changes are not reversible.

NOTES TAKEN AT THE "40TH ANNIVERSARY" MEETING OF BIOSYSTEMATISTS

Berkeley, CA, May 9, 1978

J. A. Powell, transcribed February 1999

I don't remember who organized the meeting, invited and introduced the speakers, but the meeting consisted of a series of reminiscences by several of the old time participants in Biosystematists, not necessarily all original founders. They were invited to visit Berkeley for the occasion; most had not been active members for some years.

George Myers -- Curator of Fishes at Stanford. Lives in Scotts Valley. He recalled that there was a group at the Smithsonian, attended by members of the US Geological Survey and biologists, which was meeting before 1933 [that he thought may have been the source of the idea for the Stanford-Berkeley group]. He seemed to think that Ferris came up with the name Biosystematists.

Wm. Heisey-- lives in Camino. Emphasized that the meetings were all very informal and that nothing discussed was to appear in print.

G. L. Stebbins -- still on the faculty at UC Davis. Was an instructor at Colgate in 1934, when he met E. Babcock at the 1934 AAAS Pittsburgh meetings, who invited him to come to Berkeley to work on a 4-year grant. In 1935 he was Babcock's assistant and was introduced to Biosystematists. [my note is not clear whether Stebbins thought the group was meeting in 1935 when he first came to Berkeley, but he has stated several times that he believes he was not a charter member. Lidicker's history questions whether 1935 was the starting date, but if the group did not begin until 1936, it seems Stebbins should have been involved from the beginning].

He mentioned Clausen, Turrenson[?] who invented the term or concept ecotype, visited in 1934, 1937; Ferris was the source of the name, Stebbins thinks too; Babcock was dedicated to the theory of evolution, [studied] changes in chromosome numbers. Alden Miller, concept of subspecies; Usinger; Goldschmidt.

David Keck -- also mentioned the 1934 meetings in Pittsburgh, where he met people of diverse fields, mostly evolution [oriented]. He thought the first group consisted of 15-20 persons, met in February 1935, at the Carnegie Institute with Keck as the first speaker & discussed Baker (H. O.?) pre 1934 ms for book on Wegener theory of Continental Drift. He thinks the 3rd meeting was at Stanford and that Myers or Ferris came up with the name Biosystematists [at that time].

Ira Wiggins -- remembers that "Father" Abrams opposed the ecotype idea.

Seth Benson -- came over from Inverness; went fishing yesterday and forgot notes [for this talk]. Mentioned that Grinnell had a "new systematics" philosophy but turned down an invitation to participate in Biosystematists. Cited A. F. Shell [Shall?] (1936) [book?] Evolution, controversy of subspecies relationship to speciation.

Herbert Mason -- now 82, says Clausen and Keck approached him and Scotwell (Forest Genetics Lab) to generate interest in the group at Berkeley; recalls that Camp & Gilley's little book invented the name [Biosystematists] and the group adopted it [but Lidicker says this book was published in 1943]. Species, macro- vs. microevolution, classes vs. natural taxa [were topics discussed]. Says they had no objection to women [as participants], just didn't invite any.

15 February 1999

TO: William Z. Lidicker, Jr.
Museum of Vertebrate Zoology

FROM: Jerry A. Powell
Essig Museum of Entomology

In response to your request of last September, I pulled together a memo that summarizes information on the history of Biosystematists, from recollections, a few notes, and some published and other documentation. This does not add much to the history and essays you distributed last month, but a few points are confirmed or contradicted.

It is disappointing that no trace of the 3-ring binder of records that I put together in 1975 can be located. I wonder if there is any use in contacting the herbarium at Davis to see if John Tucker left anything there. He had the original copies of meeting notices for a 25- or 30-year period to the mid 1970s and probably for another 10 years after that. Did you ask Joe Gregory if he remembers inheriting any of the past records in 1978?

I am writing to E. G. Linsley to inquire whether he has any recollections of the early years of Biosystematists.

Good luck with your continuing quest.

cc: J. Chemsak
W. Clemens
M. Ghiselin
N. Johnson
H. Kerster
E. Linsley
B. Mishler