

Harry's last seen John + Betty More in 1972 (↔ Anza Bryozoa), we called them Monday evening to see if they would join us here for a day. And sure enough they did, driving from Riverside to meet us at 1000. His car was a bit higher slung than our rented Outback; we nevertheless left it about  $\frac{1}{2}$  way up Fish Creek and walked mostly in the wash approaching Sandstone Canyon.

Johnny was very positive about historical myrmecology and about doing this with Harriet — whom he recalled as a graduate student!

Some points about the milieu: Francis entered a highly competitive, cartoonish arena in the department. He was a bright young embryologist when he took his fellowship at Stanford [? how deferred from selective service for the fellowship  $\frac{1}{2}$  some minor physical disability may have sufficed  $\frac{3}{4}$ ] and it was already settled that he would return as an instructor to teach Zoo 1-2 (as it happened the V-12 pre-meds); but meantime he switched to Neurospora. He had also (mildly) antagonized Professor Barth by switching at the last minute to Pollister as his dissertation sponsor. Real reason not recorded: he told John it was because "Pollister had never had a good student!" (He + John were fellow grad. students; the temperature-relations expt. derived from some of John's observations; in retrospect the developmental index may be spurious: gastrulation can be delayed while other maturation processes continue.)

When Francis returned to Columbia, the great lights (Ours, Dobry; even Barth) were not too thrilled about his pursuing biochemical. — perhaps not even about his invading "genetics." [This, not the choice of organism, was the problem. Later when John, perhaps also Francis recommended that I be soundbored about coming back to

Columbia, Dobry still thought microbial genetics was a "flesh in the pan".  $\frac{1}{2}$  Consider implications of this attitude as a danger for Avery!  $\frac{3}{4}$

{ Paradox that it was Dobry who brought Griffith to the attention of most geneticists via his comments in GATOS. 3. }

Francis was viewed as an energetic, aggressive investigator - perhaps almost too much so in competing for space etc. (Bairdment work needs more.) - he was just in the wrong field. Research was the paradigm for promotion; but it needed to be sparked by an offer elsewhere; and jobs were scarce. When Johnny was offered a tenure position at Hopkins he was able to negotiate a promotion for himself but only on the further condition (unthinkable!) that Francis also be elevated in a reasonable time.

Francis set up his own seminar series in the department. (Doubtless the one I also talked at in Fall '45) - which had many, since illustrious visitors. "D + D never came," to Francis' great hurt!

Betty describes this as *puni-dorsa conydx*. When John became chairman he spent almost his full time trying to ameliorate the personal feuds and help the department members get along with one another as decent human beings.\*

As to graduate students, it took John a while to focus on 1945-8; he thought later there were quite a few. During the war, graduate students were scarce; perhaps of lower quality. They would tend to be prompted by the senior faculty; and in genetics in particular, D + D were better known, and had earlier access to them in the standard course sequence. It would take something for Francis' reputation to be established, to be an independent

\* At Stanford only Pharmacology comes close to this level of intercol disciplinary. An important subject for sociological inquiry: how much academic energy is wasted in such conflicts; what are the preconditions for effective cooperation? Is personality; leadership an adequate answer?

attractant. { all of which facilitated a niche for myself! } (3)

I tried to press John on the question of how that atmosphere might have shaped Francis' own research program; but did not get a clear focus on the affective - cognitive intersections. He just returned to the theme it was "difficult."

[I should have turned the question on to John's own career, where he might be better informed! But he would probably have been too modest and self-deprecating to come through. But John is also plainly much more diplomatically successful than Francis I.]

He focused on Selig Hecht as a victim: "brilliant, Jewish, aggressive in research, <sup>fresh</sup> resourceful at getting funds, autonomous but isolated at Purvis Hall. — felt very lonely. John tried to mediate — e.g. to persuade Schwabe, with some difficulty to hold faculty mtg. over at Purvis. — "brought tears to Hecht's eyes!". "Entertained them royally { codices, tea... } when they did come over!"

---

P.S. Francis continued to teach Zoo 101-102 because that was what he was originally hired for; who else could have done it!; he was too loyal to the department.

{ The nabobs' ~~attitudes~~ attitudes may also have been conditioned by his being home-grown. Wouldn't they inevitably have difficulty effacing their recollections of him as a green student from their overall appreciation of him. Is this a verifiable principle, ~~that~~ that deviance is best accepted in such circumstances. }

Note: adogen cartons of Dept. records were deposited in Columbia by Moore in August 1968. In addition about 6 on the history of genetics were sent to the Am. Phil. Soc. L.C. Dunn's papers are also there! He does not know what was discarded. He knows nothing logically.

Cy Levinthal knows that many E.B. Wilson papers were in the "Wilson house" he used to rent at Woods Hole. (These could be priceless for history of American Biology < 1940).

Soci

12/24/76.

"Ryan credited [John] with starting his work as reverse-mutations in factery."

---

(I may have discussed research programs with him as backup to recombination & probably did look at  $\text{meth}^- \rightarrow \text{meth}^+$

(cf. Kohn & Harris); possibly an eye to  $\text{meth}^+ \rightarrow \text{meth}^-$ .