RAY J. OWEN [illegible]

9/17/7-1

Box 9

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA

WILLIAM G. KERCKHOFF LABORATORIES
OF THE BIOLOGICAL SCIENCES

August 13, 1947

Dear Dr. Brink:

I just received your letter of the 11th. I am sorry to learn of the uncertainties connected with the teaching of the elementary course, and particularly that Russell's domestic problems have led him to withdraw from consideration. I had recently heard of his difficulties, and had wondered whether they would affect the situation. I am sorry, too, to learn that Clyde is not fond of the teaching assignment, though I think I can understand how his deep and intense interest in his own part of the field might make relatively unappealing the shallower and more extensive coverage of the subject necessary in that course. Perhaps there are other reasons, though.

The attitudes toward Lederberg I have encountered here are doubtless as varied as those you have collected. Numerically, the very great majority – Beadle, Sturtevant, Horowitz(?), Maas, Mitchell, and others are against him. These are for the most part people who have met him and know something of his work, but whose opinions are I think dependent on anecdotal and patchy acquaintance with him.

I understand that he was refused an appointment for study here because of this majority poor opinion. It seems to rest on (1) recognition of his arrogance in personal and intellectual matters (2) recognition of his abrasiveness, which leads Beadle, for instance, to believe that any claim of his should be confirmed by others whom he trusts more, before it will be believed. This distrust is not based on suspicion of "faked" results, "Josh has not been convicted of this": but on the suspicion that his ambition and enthusiasm lead him to advocate the most spectacular conclusions from experimental data that would only set more stable workers on a long series of cautious checks. In my opinion, he suffers here from being classed with Spiegelman. (3) reports of his sloppiness in the laboratory. Breakage of glassware (excessive) "borrowing" without permission from other laboratories; mislabeling strains, errors in dilution.

This "majority" opinion represents pretty well the slant I had received when I wrote you last. Although it sounds pretty damning, as I think I wrote you, I was not inclined personally to weigh it too heavily because of indications that Lederberg's personal objectionableness had colored opinions of his professional merit. Set against the letters I

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA
4

WILLIAM G. KERCKHOFF LABORATORIES
OF THE BIOLOGICAL SCIENCES

had seen from Tatum and Sinnott, the distrust of Lederberg prevalent here seemed weakly founded. I should say in this connection that Sturtevant and Beadle are both inclined to discount Tatum's and Sinnott's evaluations of candidates generally, as showing some "overtolerance" and being uncritical. This refers to judgments of men only, of course, and not to other qualities; both Tatum and Sinnott are highly respected.

This morning I talked to two other men, both of whom know Lederberg well, and both I think men of sound judgment. They represent a "minority" opinion considerably more acceptable to me as an outsider than that outlined above. (1) Bob Balentine knew Josh at Columbia. There were many times when he would "cheerfully have pushed him down the elevator shaft." Ryan had to institute a special fee system to make Josh more careful of glassware. Mrs Ryan couldn't stand (emphatic) Josh. But Bob thinks Josh one of the most brilliant minds he has ever seen. He likes him and trusts him without reservation. In Bob's opinion, whether

or not you take Lederberg will depend on what you want. If you want an all-round good fellow, who will do thorough work on small problems, don't take Josh. But if you want a brilliant investigator, who may well have several people "hating his guts" before long but who will be liked and respected by others, and who will enrich many discussions and produce, over a long career, a large volume of distinguished research, his young Bob says Josh recognizes his deficiencies in technique, is the first to distrust results of his own that are out of line and repeats them until he is satisfied that they are repeatable, and welcomes criticism and repetition of his work by others he trusts.

Galston knew Josh well last year. He says that Lederberg has turned down several offers, but will take the Wisconsin job if it is offered him because he is impressed by the prospects there. If he is not offered the Wisconsin position, he will return to the Columbia Medical School. Galston hopes you'll decide to give him a chance, because he thinks Josh's return to medical school would be a most regrettable waste of precious scientific talent. Like Bob, Galston thinks Lederberg one of the most brilliant minds in biology today, and he trusts his work implicitly.

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA
4

WILLIAM G. KERCKHOFF LABORATORIES
OF THE BIOLOGICAL SCIENCES

He says Josh is an avid student of the literature, accomplished in many fields; for example, although Lederberg is not a plant physiologist, Galston owes some good ideas on his own problem to Josh. He "made" the seminar, on many occasions; often catches points in foreign literature outside his field that other investigators (his colleagues) in the field have missed; always can be depended upon to take part in, and often to stimulate, discussions. When Barry Commoner (sp.? - but you'll know who I mean) gave his talk on genes and viruses, it was Josh who tangled with Commoner's easy generalization and retired Commoner more-or-less in confusion. Galston thinks Josh has become much easier to live with than he was in the past (i.e., he has improved remarkably in this respect), and he mentioned as Tatum did the "salubrious" effect of Lederberg marriage. In fact, Galston says if you extrapolate from Josh's behavior curve, you'll find him one of

the most popular men around, before long. But even if (Both Bob and Galston used this phrase) a sizable fraction of his associates "hate his guts", you cannot discount his solid and exceptional scientific merit, already established at 22 and with great promise for the future.

All this may be inappropriate, for me to write at such great length, Mr. Brink. But since I had already referred to the often-expressed opinion of Lederberg's "unreliability", I thought I'd better do my best to get to the bottom of this important case. Although two other investigators have failed to confirm the "sex in bacteria" work, and in spite of their suspicions of Lederberg's technical inadequacy, it now seems clear to me that to regard his work as untrustworthy is unjust. If I were still with you, and as far as I can tell without knowing the man, I am now quite sure I would vote to give him a fair trial, without of course a commitment on tenure. I will be interested to know how Lederberg's visit came out, and hoping the very best for all our friends back there. We are looking forward to Joan's visit. Sincerely, Ken.