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Sincerely,

The Reference Staff  
Harvard University Archives

7 Bell Road  
Hampstead N.W. 3  
June 20 1922

Dear Professor Kelley,

I think you have got the grip of the enclosed problem. Your method of proof is very nearly the same as mine & at any rate as good. I have been very busy recently with examinations, but should like to develop the result by making the combated quantities, not  $X = \sum_{i=1}^n (x_p)$  &  $Y = \sum_{i=1}^n (x_p')$  but  $X = \sum_{i=1}^m (x_p) + \sum_{i=1}^n (x_p')$   
 $Y = \sum_{i=1}^{m'} (x_p') + \sum_{i=1}^n (x_p'')$   
The  $x_p, x_p', x_p''$  being given in the same manners. This to follow up our talk of a day or two ago

Pearson

c/o American University Union  
50 Russell Sq., London, W.C.  
May 15, 1922.

Professor E. P. Cubberley  
Stanford University, Cal.

My dear Cubberley:

I have tried and have truly done my best to follow your injunction. I supposed from your recommendation that it would be delightful and that, being here, I could sort of lord it over you, but really my first, last and only struggle ended in my utter rout. I will do anything within reason for you, but pray don't ask me again to have a glass of that bitter stuff called Bass' pale ale. Mrs. Kelly is just as fond of it as I am and has had to forego being like the English in the little matter of drinking and smoking. Beer is served in the College lunch room and daily I see college girls sitting on the library steps smoking cigarettes. I must say I prefer Stanford ways.

The ideal here seems to be "do everything, but do it in moderation." Moderation is clearly defined; the breath may be strong and the tongue loose, but if you don't stagger, that is "moderation". I have not discovered a real yellow journal, an evangelist, a tee-totaler, a bargain sale or a Fifth Avenue robber, but withal the ordinary English servant, clerk, official, or gentleman, is a very agreeable and kindly person. It seems to me that, per vocation, their I.Q.'s run higher than in America. I find the college professors very helpful, the dreadfully ignorant of America, American journals and psychologists. They total the number of American psychologists to three - Terman, Thorndike and McDougal. Spearman is a hard working earnest fellow and Pearson another.

Pearson is methodical, keen and like a razor in the sharpness of the distinctions that he makes and like an Indian in the memory he has for the professional faults of others. The trouble with Pearson is that he is right in his charges of error on the part of others - true a man may do 99 things correctly and one thing incorrectly, but if so the critic is "right" in calling that one thing wrong. Pearson is attacked in the same way that he attacks - if he commits one error in a hundred his critics forget his other 99 points. My batting average is much less than 99 so if Pearson wants to take a fall out of me he can do so most easily. To date we have gotten along well, tho he is a hard man to know intimately. Like the rest of the English professors he "lectures" so there is no mutual give and take in class work.

My problem is developing very well, in fact so satisfactorily that I can push it more rapidly than I had expected to be able to, time and money permitting. You will remember that I spoke to you of the possibility of securing an appropriation from the Commonwealth Fund. My idea was that I would spend my time here in developing an experimental and statistical method for determining the degree of uniqueness of different mental traits, with a view

Prof. E. P. C. - 2

to securing a map or plan of mental life so that one could say function (a) is the most worth while trait of an individual to measure, function (b) the next most worth while, etc. - all with reference to its bearing upon scholastic, vocational and social success. The problem is a very big one and I had expected to gather the necessary basic data after returning to America. I am still of this opinion because the data now available from previous studies is generally of questionable value due to such things as the overlapping of judgments halo, the correlation between speed performances halo, etc., but nevertheless I think it would be very much worth while to work over some of the best of the extant data and I now see the method for doing this with sufficient definiteness to know that it is both involved and lengthy. I therefore want to ask if by any chance, such as the return of an appropriation already made, the Commonwealth Fund, or any other fund that you know of (I am like Percy Martin in expecting you to have a fund or two in each vest pocket) is available and might be assigned to me to work on this problem this present year.

I can divide my study into two parts, first, working over extant data and second, a research involving collecting of new data. I can very profitably spend \$2,000 for computation work upon the first part and \$10,000 upon the second part. If there is any possibility of my getting say \$2,000 this year for this first study I wish you would cable me so that I can secure the services of some of the Biometric Laboratory workers before they scatter for the summer. I can get very adequate help and at less expense for the computation work of which there is a very large amount for I must test every hypothesis tried by rigorous "goodness of fit" methods, right here in London than anywhere in the United States.

When in New York I discussed this problem with Dr. Thorndike and as he is a member of the Commonwealth Committee I am sending copy of this letter to him.

If I get this fund it will mean that all or most of my summer, which I had reserved for touring the continent, will be spent in work here in London. I mention this to show you how much in earnest I am and how much value I expect would result from such a study as mentioned. Dr. Terman knows more of my purposes and I wish you would show him this letter and get his opinion upon it.

I promised both you and Terman that I would let you know if I would return for the fall quarter. My present intention is to return for the first of January. May we leave it as so settled unless I cable to the contrary. If I do cable it will be immediately upon hearing from you in answer to this letter.

Mrs. Kelley and I send our warmest regards to you and Mrs. Gubberley.

Cordially yours,

221 Bryant St.,  
Palo Alto, Cal.  
Jan. 7, 1929.

Dr. Truman L. Kelley,  
Chicago, Illinois.

Dear Dr. Kelley,-

Enclosed find my write up of the results found in connection with the solution of that twenty-one variable multiple by the two methods: Doolittle and Kelley-Salisbury iteration methods. You had hoped to use the results in an article for the Journal of the Am. Stat. Assn. My write up is merely a summary of the situation and findings as I see them, and will no doubt need correcting and supplementing by you. It would seem to me that perhaps the comparison of time required by each method is not quite fair to Doolittle since the computation by this method was carried to eight places thus giving seven place accuracy. You will also note that one error occurring in the 18th step was so evasive as not to be found after twelve hours searching. It occurred in the fifth place, thus not making the remaining computation in error for our purpose. I did not feel that spending more time would be justifiable, however if you think this error should be located I'll make an effort to do so. It sure baffled my ego into stupidity.

In the first paragraph of the write up you will notice that I have taken our friends Tolley and Ezekiel to task for mis-counting the number of arithmetic operations involved in the iteration method applied to the six variable as published by you in the original article. Perhaps you should make a check of their count also.

Even tho my time of 19 hrs. 20 min. is better than yours of 30 hrs. one min., I discovered that your solution is approximately 80% shorter than mine and consequently have used the number of operations involved in your solution to compare with that of the Doolittle method, and have used my time for comparison. My greater number of operations are probably due to poorer judgment, while my better time is due to making use of the machine to accumulate the products for each column of Table III, thus avoiding errors likely to arise in copying the table out in full, and also saving the copying time. That this is where I gained in time saving is readily seen by comparing your time of an average of, for the 2nd and 3rd table III, 392 min. to my average of 86 min. Both including time spent hunting errors.

The bill for the calculations amounts to \$37.10. (A total of 53 hrs @ 70 cents per hr.) Check can be made payable to me, and then I'll pay Harry Israel for his part.

Hoping the work is satisfactory and thanking you for the opportunity of working with you on this problem, I am,

Very sincerely yours,

*Quinn McNemar*

221 Bryant Street  
Palo Alto, Cal.  
Jan. 28, 1929

Dear Dr. Kelley,

Enclosed find statements of time for various steps in the solution of that twenty-one variable multiple. The time summary for your work I have copied directly from your work sheets. The time for the Doolittle solution is, as you will notice, given for each "large" step. You will also note that the summary of time for my Iteration solution is not as detailed as yours. This is simply due to my method of working like the deuce for a period without jotting down the exact time for each little step. However the summary is such that you can readily tell the relative amount of time spent on each step. Hope this is satisfactory for your purposes.

You may also be impressed by the greater amount of computation which I did in my solution. As I said before this was due to my poorer judgment in the making of the successive corrections. Which leads me to say that if I had to solve another problem, I would be able to greatly reduce my time by exercising more care in the making of the corrections. A conservative and modest estimate would be that I could now do the same problem in 15 or 16 hours, and with more practice one should be able to cut it to less than that.

If you need addition data or information, I'll be glad to send it on.

Respectfully yours,

*Quinn McNemar*

221 Bryant St.,  
Palo Alto, Cal.  
Feb. 15

My dear Dr. Kelley,

Enclosing the further data which you asked for in a recent letter. It took three search warrants to find the scant information concerning the various tests as presented in Table I. Hope it is as complete as you wish.

Have had Miss Siegfried type the tables and they have been carefully proof-read. I have also rechecked the last parts of the calculations for the weights in each of the three cases in order to be more certain of accuracy. Have also recalculated the multiple  $r$ 's in each case as a check.

Thinking that you might wish to know the relative accuracy of the three calculations I have again substituted the weights into the original equations, and find that for my solution by the Doolittle method the average error is something less than .0001 (actually about .00004); for your solution by the iteration method the average error is .0017; for my solution by the same method .0018. (Those figures are the same as the average amount that the final  $A_p$  values deviate from the criterion  $r$ 's, these final  $A_p$  values being obtained by a recalculation of Table III of the iteration method).

Assuming that the Doolittle weights are accurate to four places (they should be), I find that my iteration weights differ by an average of .0024, and yours by an average of .0043. We can easily stress the essential accuracy for practical purposes that the iteration method gives.

Hope this is sufficient information. Am enclosing your letter so that you can check on how well your requests have been filled.

Have you noticed Salisbury's article in the J. Ed. Psych. for Jan. 1929?

Sincerely yours,





UNIVERSITY OF LONDON, UNIVERSITY COLLEGE,

GOWER STREET, LONDON, W. C. 1.

TELEPHONE No. MUSEUM 8101.

CENTENARY YEAR  
1926 - 27.

11-1-1928

IN REPLY PLEASE

QUOTE \_\_\_\_\_

My dear Dr. Kelley,

On return home I find your book, which I hasten to thank you for and which I hope soon to study.

As regards my previous remarks about it and your reply, I certainly did not allude to your not being able to subscribe to the doctrine of a general factor as the proper explanation of the correlations between abilities. I meant rather your characterising my work as a mere "defence", which seems to imply unoriginality and unconstructiveness. Whereas it appeared to me that I had made some positive discovery; namely, that scores for "dissimilar tests" admit of expression in terms of a general and a specific factor with at any rate some fair approximation; also that even when "similar" abilities are included, such an analysis is still possible, provided that the specific factors are now regarded as overlapping. So far I have nowhere seen any ~~work~~ of yours that to my mind in the least conflicts with the above "discovery". For example, the mode of expression which you brought forward at the meeting, and in which you say the general factor tends to disappear, may perhaps turn out to be scientifically more valuable than my mode of expression; but it can never, I think, prove the latter to be incorrect.

My other point in my letter to you was that you represented my position as having been refuted by my own pupils, whereas I think that it has only been further developed by them; and this, in intimate co-operation with myself. I must confess that I cannot trace this divergence on your part to any share misunderstanding. But the alternative of an unfriendly bias is - in view of our personal intercourse *including* all your private writings - very painful to me.

Yours sincerely,

*Spearman*

*[Handwritten notes and scribbles on the left margin, including phrases like 'finding/' and various symbols.]*



February 7, 1928.

Professor G. Spearman,  
University College,  
Gower Street,  
London, W. C. 1, England.

My dear Professor Spearman:

I am sorry there is a misunderstanding between us. I am sure it is just a misunderstanding, but I feel it is pretty fundamental, for I find that I do not subscribe to your interpretation as given in your recent letter. You write, "For example, the mode of expression which you brought forward at the meeting, and in which you say the general factor tends to disappear, may perhaps turn out to be scientifically more valuable than my mode of expression; but it can never, I think prove the latter to be incorrect." Now it seems to me that if the general factor does disappear, then it definitely proves "the latter to be incorrect." Of course I mean incorrect as a description of phenomena, not incorrect as a noumenal structure. I certainly do not wish to question the ingenuity and originality of your hypothesis, but only to question its applicability.

In a book now in press I have credited you with what I consider both the assets and the debits in the works of your students. I have done this because I have felt, as I believe do all psychologists, that your students are not taking issue with you, but are expressing and developing your own views. In regard to your views, I judge from your writings and from those of your students that they have expanded greatly within the past twenty years. If I am incorrect in this then I have quite failed to understand your writings.

In the book in press there is little reference to Thomson. I wish you to know that this omission was in consideration of your views, for had I discussed his work at length I should have had to give him considerably more credit for developing the problem and making issues clear than do you. I did not find Thomson's views as inescapable in the development of my own work as your own, so that I decided that I could omit a discussion of his position whereas it was beyond reason to refrain from discussing yours.

I believe our views differ greatly upon what constitutes reasonable proof, and I have accordingly criticised you because of what I consider to be an oversight of certain matters, and insufficient rigor in certain matters of proof. I believe these things and I believe that they are very important in connection with the accuracy of one's views of mental structure, but that they have no necessary bearing upon matters of personal respect and friendship.

You seem to insist in attributing my own remarks to "unfriendly bias" and not to difference in view. I sincerely hope you will reappraise my motives, for mental structure is a field in which I expect to do much work in years to come, as I hope you will, and it will be a continuing sorrow to me if we cannot mutually hold each other in esteem, in spite of our different psychologies. Meanwhile, I am

Yours with sincere regards,

TLK:im

ROTHAMSTED EXPERIMENTAL STATION  
HARPENDEN, HERTS

Director:  
SIR JOHN RUSSELL, D.Sc., F.R.S.

STATISTICAL DEPT.  
R. A. FISHER, Sc.D.  
J. WISHART, D.Sc.

August  
14th  
1928

*Note*  
1-1-49

Dear Dr Kelley,

I hope you will excuse my delay in expressing my appreciation of your book "Crossroads in the mind of man". The fact is that I had this year to put in a lot of work on the solution of the multiple correlation problem, which I hit upon some months ago, and it is only quite recently that I have been able to read your book.

I should like to say at once how much I admire the thoroughness of the section on Theory and Technique, the whole subject appears to me to be greatly illumined by the treatment you have adopted, and I am astonished at the efficacy of the method of correlations in mapping out mental structure. Some further clues to the genetic bases may now be expected from the study of the pairs of siblings, or so I should hope, but perhaps this is difficult material to collect.

Yours sincerely,

*R. A. Fisher*

Dr T.L. Kelley,  
Stanford, U.S.A.

October 10, 1924

Dr. Harold O. Rugg  
Teachers College  
Columbia University

My dear Rugg,

I am enclosing copies of communications between Dr. Holzinger and myself, and wish you would read them in view of the fact that Holzinger will probably send you a note upon an article of mine appearing in the September, 1923, Jour. of Edu. Psych. I do not see any point in Holzinger's note and rather feel that he has written it because he is sore at me on account of my criticism of his formula for the probable error of the mean. Let me say that I consider his reply (Jour. of Edu. Psych., Sep. 1923, page 377) to my note a piece of statistical sophistry, that Holzinger knew that at least 99 out of every 100 readers would not fathom. In fact I am convinced that he did not want them to fathom it, for the idea in the fifth sentence is simply ridiculous. To show this to the readers of the journal would have taken space that the thing was not entitled to, and furthermore I was not concerned with a personal matter,-- Holzinger's reasoning,--but merely with the appropriateness of a certain formula for the probable error of the mean, and that issue was settled; so I did not attempt to point out the absurdity in this fifth sentence.

I feel that Holzinger is planning the same sort of a smoke-screen with reference to my formula (8) (Jour. of Edu. Psych., Sep. 1923, page 325) and I am writing you simply to suggest that you exercise such editorial prerogatives as you can to see to it that the argument be made clear and precise. I consider my formula (8) to have wide application, and I shall be glad to defend it against any honest criticism, but I do not want to be called upon to do so against some murky, intangible charge.

The notes concerning the probable error of the mean were peculiarly murky because of the many serious typographical errors in make-up. The copy of proof sent to me stated that you would be responsible for the proof-reading

(OVER)

but judging by the results I doubt if you ever saw  
the proof before ~~it~~ was printed. You understand that  
my sole purpose in writing this is to insure that an-  
other unprofitable, meaningless discussion shall not  
obliterate the pages of the Journal.

Will you kindly consider this correspondence ~~con-~~  
fidential?

Cordially yours,

TKK:d

INTERNATIONAL COMMISSION ON EUGENICS  
**EUGENICS COMMITTEE OF THE UNITED STATES OF AMERICA**

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460 Prospect Street,  
New Haven, Connecticut,  
December 21, 1923.

Professor Truman Lee Kelley,  
Stanford University,  
California.

My dear Professor Kelley:

I have the honor to inform you that you have been elected by the Eugenics Committee of the United States of America, to be one of the Advisory Council for the ensuing year, 1924.

The undersigned committee was appointed by the Second International Congress of Eugenics, held in New York in 1921. It was then pointed out that the time is ripe for a strong public movement to stem the tide of threatened racial degeneracy following in the wake of the War. America in particular needs to protect herself against indiscriminate immigration, criminal degenerates, and the race suicide deplored by President Roosevelt.

The enclosed copy of the "Eugenical News" contains a sketch of the program to be gradually developed by the Eugenics Society of the United States of America.

Our work will consist in the promotion of eugenics research and education, and its gradual application, through legislation and otherwise, to economic and social betterment.


As is stated in the program, the work will begin

Professor Truman Lee Kelley #2.

with chief emphasis on the three following subjects:-

1. Working out and enacting a selective immigration law.  
(Active work is now being done by the Committee on Selective Immigration, already definitely appointed.)
2. A brief survey of the eugenics movement up to the present time.  
(To be worked out by the proposed Committee on History and Survey of the Eugenics Movement.)
3. Securing the segregation of certain classes, such as the criminal defective.  
(Plans to be worked out by the proposed Committee on Crime Prevention.)

Very sincerely yours,



Irving Fisher, Chairman.  
Charles B. Davenport, Vice-Chairman.  
Henry E. Crampton, Secretary-Treasurer.  
Madison Grant.  
C. C. Little.  
Harry Olson.  
Henry Fairfield Osborn.

MA

COPY

University of London, University College

Gower Street, London, W.C.1

26th April, 1935

Galton Laboratory

Professor F. L. Kelley  
Graduate School of Education  
Harvard University  
Cambridge  
Mass. U.S.A.

Dear Professor Kelley.

I very well remember that great body swinging across the road and driving you into the stone wall, and I hope you were successful in getting compensation.

About your enclosed paper I feel some little difficulty. I am not the editor of "Biometrika," which is Professor Pearson's private property. Moreover, he has, I fear, not got over his suspicion and antagonism towards my work. If, therefore, you want to publish in that journal, I am sure that it would be far better for your paper to come to him direct, rather than through me. The journal I am now responsible for is the "Annals of Eugenics," which will of course be open to statistical as well as genetical papers, and I hope some time you may think of it, a propos of any psychological or other work you may think of publishing there.

I quite see the point of your paper, which I am returning herewith, and think it may be of use to a good many readers.

Yours sincerely,

[Signed] R. A. Fisher



1004 Physics Building  
May 2, 1930

Professor C. Spearman  
University College  
University of London  
Gower Street  
London, England

Dear Professor Spearman:

I have just read with little satisfaction your review of my Crossroads in the Mind of Man. You say that I have in this book reversed my position, but the gropings toward the view of Crossroads are clearly discernible in Educational Guidance, 1914, and ever more clearly in every book of mine since then, except Statistical Methods, which does not treat of this matter.

You have clearly misunderstood my position upon probable errors. If I can find the time in the near future to expand upon this issue, I shall do so.

You charge me with being biassed against your views. I have not been aware in the past, nor am I now, of any bias of this sort. I am only trying to keep an open mind and follow where the data lead. I am sorry that you who are dealing with the types of subject matter that I also am investigating should make this charge, for it will handicap freedom of future reference to your work. My last wish would be to participate in a discussion in which questions of bias rather than of evidence were paramount.

Regretting that our viewpoints seem so far apart,  
I am

Sincerely yours,



UNIVERSITY OF LONDON, UNIVERSITY COLLEGE,

GOWER STREET, LONDON, W. C. 1.

TELEPHONE No. MUSEUM 8101.

IN REPLY PLEASE

QUOTE \_\_\_\_\_

20-5-1930

Dear Professor Kelley,

As you read "with little satisfaction" my review of the Crossroads, so did I get little from your handling (both what it said and what it did not say) of the Abilities, and so probably I shall be dissatisfied with what you now reply, and then You again with my rejoinder. And so the vicious circle goes on getting even worse; as silly and as harmful to all concerned as an ancient blood feud. ~~XXXXXXXX~~  
~~XXXXXXXX~~ I see no escape. But I <sup>may</sup> ~~am~~ say that when you **h**egan it, it was one of the bitterest disappointments I have ever had. And not merely on scientific grounds, **h**ut above all as a personal matter.

Yours sincerely,

*C. Spearman*

*Save*

June 3, 1930

Professor C. Spearman  
University of London  
University College  
Gower Street, London W.C.1  
ENGLAND

Dear Professor Spearman:

I am indeed sorry that you take my differences in judgment,--my differences from you in inductions from data,--as ever having been instigated by any trace of personal ill will toward you. It would not have been professionally honest to have refrained from expressing differences of view upon fundamental matters because of your personal kindnesses to my wife and self which helped make our stay in London so enjoyable. If we can differ in our psychologies and be friends, I want it to be so, but it seems to me that friendship can only be upon this basis, for I believe our differences in viewpoint are not readily reconcilable.

I do and always have granted your honesty of intent in your interpretations. If you grant the same to me, need such differences in interpretation as develop particularly matter so far as personal relationships are concerned?

Hoping that our future productivity will not be marred by needless recriminations, and wishing you ever continued success, I am,

Sincerely yours,

TLK:d

UNIVERSITY COLLEGE, LONDON.  
~~TEMPORARY ADDRESS~~  
9, MARINE TERRACE,  
ABERYSTWYTH.  
~~TELEPHONE - 463.~~

GOWER STREET, W.C.1

20, NEW STREET,  
ABERYSTWYTH.

14th January, 1944.

Dear Professor Kelley,

It was very good of you to send me a reprint of your interesting article on 'Periodicity in Short Time Series'. Your mode of attacking the problem will be of great interest and help to us. As I expect you know, we have carried out in our laboratory a good many investigations on fluctuations in mental work and in attention, and my assistant, Dr. Philpott, has published several articles on the subject. I myself have experimented with harmonic analysis following for the most part the lines of the economists, using Fisher's method of testing significance, with which no doubt you are already acquainted.

I will send under separate cover one or two reprints and (if I can obtain permission) some of the memoranda for which you ask. So far, however, Government Departments in this country have been rather chary of allowing us to pass on detailed accounts of the work of psychologists in the Army, Navy or Air Force. Early in the war, in the hope of stimulating interest in this country, I compiled a memorandum on work done in America and in Germany, and I can certainly enclose an abridged version of this. My last Presidential Address to the British Psychological Society, described work in this country, had to be limited to members only; but I hope that in the near future I may be permitted to print a duly censored version in the Brit. J. Psychol., and will gladly send you a reprint.

I condensed into an article for Psychometrika a summary of the chief statistical methods which we were using up to the beginning of last year, and have just obtained sanction for sending it off. (There has been little change in methods since.) There is no reason why I should not include a copy of this.

Journals from U.S.A. and accounts of American method, reach us with little hindrance, though at times after some appreciable delay. We have throughout been extremely interested to hear of developments from time to time in American military psychology. Over here our great handicap, of course, was that there were so few trained psychologists in the country: the numbers working for the Army, Navy, and Air-force now number over 5,000.

We have also succeeded in establishing contact through the Embassies with our Russian colleagues. I get a certain amount of psychological literature from Moscow; but it would seem that their censorship is even more rigorous than our own. So far we have received no details of personnel selection as such.

It was very pleasant to hear from you again after so long. I have very vivid recollections of our pleasant meetings when you were in London with Karl Pearson very many years ago. I cannot help seizing this opportunity of saying how valuable all your books have been to us. After twenty years, Statistical Method is still the book on the subject for every psychologist in every teaching department!

With all good wishes,

Yours very sincerely,

*Amos*



July 10, 1950

Federal Bureau of Investigation  
Washington, D. C.

Gentlemen:

Knowing that you must receive many "informing" letters I will make this as short as possible.

I have just returned from family conference of religious liberals (Unitarians and Universalists) held at Camp Radford, N.E. of Redlands, California.

Though, as a university professor, I have seen pressure groups operate in committees and in conventions of national societies I have never before seen such evidence, as at Radford, of an organized, unscrupulous and smooth working clique endeavoring to stampede a lot of Christian folk into support of the USSR.

I do not believe that Unitarians incline toward Communism, but, like the Quakers, there are many pacifists in the Church. Working upon this putty a small clique, perhaps not more than half a dozen, introduced and got passed a resolution, by a vote of 43 to 29, condemning President Truman and the militaristic U.S. in attacking the poor Koreans (untainted by Russian influence) struggling for independence. I was most impressed by the brilliance of the leadership in playing upon the good pacifists, who would vote for anything if presented as the means of world peace, and by the utter dishonesty of the leadership in reporting facts and motives.

I would say that the spearhead of this communistic effort was Martin Hall (966 1/8 Palace Ave., Los Angeles), Anna Louise Strong (2317 El Moreno) and A. H. Morgan (Englishman, 3737 Canyon Crest Road, Altadena). They were supported by a number of radical, but I judge just immature youths. They were also supported by two undoubtedly smart men, Robert L. Brock (3860 Globe Ave., Culver City) and Carole Richardson. I must mention these men because they certainly played Martin Hall's game at Radford. They seem to me like fine gentlemen, perhaps merely disgruntled from frustrating army experiences. Finally ~~these~~ they were supported, at least to some degree, by such Unitarian reverends as John K. Findly and Frank Ricker and a lot of similarly minded church people who just can't believe that anybody is activated by evil motives.

To one who holds a liberal faith, but believes he is no dupe, the whole procedure was very disheartening, in spite of the fact that the resolution mentioned as having been passed was later tabled (upon motion and second of Hall and Strong) because the sponsors realized that a wire to President Truman, supported by a vote of 43 to 29, would really do their cause no good.

I am none too familiar with Unitarian affairs in California, but it seemed to me that the nucleus of this coordinated Communist effort sprang from the Unitarian Church of Los Angeles.

Writing this has been very unpleasant, but it has seemed to me to be a duty.

Sincerely yours,