A Conversation with Charles Stein

Morris H. DeGroot

Charles Stein was born in Brooklyn, New York, on March 22, 1920, and received a B.S. in mathematics from the University of Chicago in 1940. His graduate studies at Chicago were interrupted by the Second World War, and he served in the U. S. Army Air Force from 1942–1946, attaining the rank of Captain. He received a Ph.D. in mathematical statistics from Columbia University in 1947 and joined the faculty of the Statistical Laboratory of the University of California, Berkeley, where he remained for the following two years. He was a National Research Council Fellow at the Institut Henri Poincaré in Paris during 1949–1950, and an Associate Professor of Statistics at the University of Chicago from 1951–1953. Since 1953 he has been a member of the faculty of the Department of Statistics at Stanford University, where he is now Professor of Statistics.

The following conversation took place in his office one afternoon in October 1984.

"I HAD ALWAYS INTENDED TO BE A MATHEMATICIAN"

DeGroot: How did you get interested in statistics and come into the field of statistics?

Stein: Well, I first took a couple of courses in probability and statistics at the University of Chicago, probably as an undergraduate, from Walter Bartky. And then I took the first three actuarial exams when I was unemployed after graduating in 1940. I failed the third one on probability and statistics, partly out of discouragement from thinking that I had done very badly on the first two. Then when I came back to the University of Chicago, I took more courses in probability and statistics.

DeGroot: You went back as a graduate student?

Stein: As a graduate student for two quarters in mathematics.

DeGroot: How long was the interim? You said you were unemployed.

Stein: I was unemployed for a year, and then I was a graduate student in mathematics for two quarters. At that time I wrote something that was intended to be a master's thesis, if I ever got around to getting a master's degree. It was on the distribution of the ω^2 criterion of von Mises in the case of two samples. I just took the paper of Smirnov and almost line by line extended it from the one-sample case to the two-sample case. Very unimaginative work and, fortunately, it was never published. [Laughs] Later, Murray Rosenblatt did it under proper supervision and did it right. Then I was in the Air Force during the Second World War, mostly in the headquarters at the Pentagon in meteorology. There I did a lot of statistical

work. I was in a group with Kenneth Arrow, Gil Hunt, George Forsyth, Murray Geisler, and several others who are probably quite good mathematicians but somehow I haven't kept up with them.

DeGroot: Did the interest that developed from working on those problems lead you back to graduate school in statistics?

Stein: Yes. Of course, I had always intended to be a mathematician but I found that if I had gotten my degree in pure mathematics I would have had to accept guidance in my choice of topic; whereas I had already, just as a result of casual conversation with Kenneth Arrow and scanning some work of Wald, published the paper on the two-sample test for Student's hypothesis with power independent of the variance. [*Ann. Math. Statist.* **16** (1945) 243–258]

DeGroot: So your intention was to become a doctoral student in statistics and to use this work as the basis of a thesis?

Stein: Yes, and I did. I went to Columbia University as soon as I was discharged from the Air Force in February 1946, and I got my degree in a year and a half. Then Neyman offered me a job at Berkeley, so I was definitely in statistics.

DeGroot: Was Wald at Columbia when you were there?

Stein: Yes, Wald was there; Hotelling had just left; Wolfowitz was there. P. L. Hsu was visiting; Doob was visiting; B. O. Koopman was there, in another department. Ted Anderson came the following year.

DeGroot: And you had contact with them even though you came with your thesis already finished?

Stein: Yes.

DeGroot: Let me just go back a little bit. What
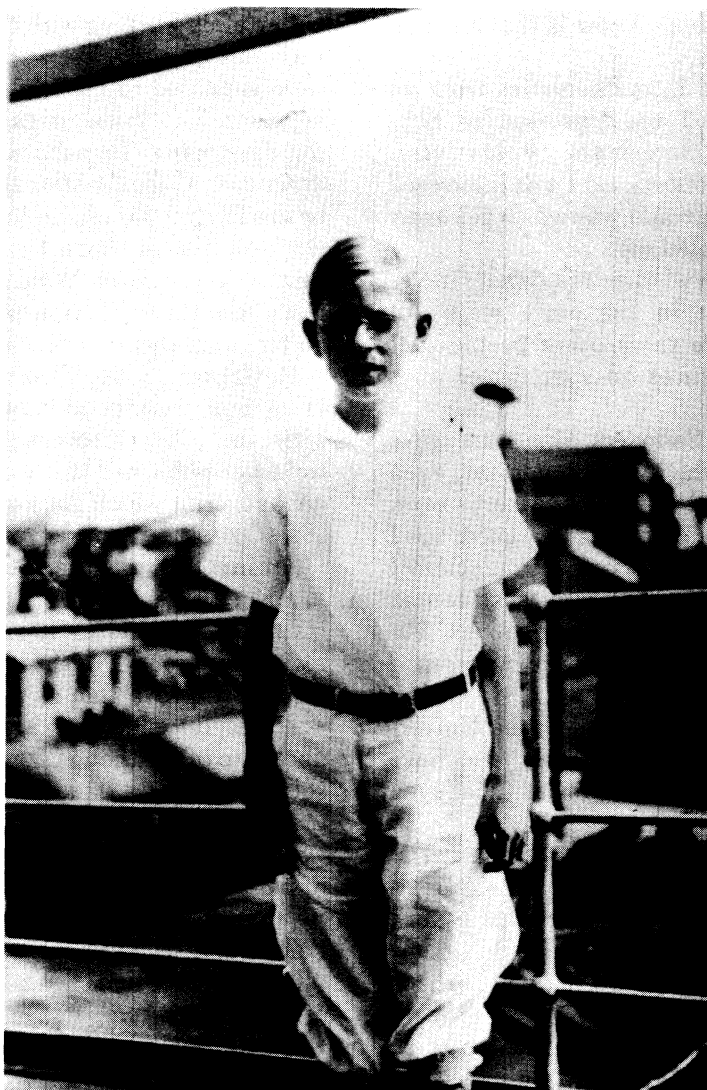
Charles Stein, 1931

had you studied in your bachelor's program at Chicago? Was this the Hutchins era, when there was a general education program? Was that why you were unemployed?

Stein: No, there was not a compulsory general education program yet. In the first two years we were required to take four broad survey courses plus English and a foreign language and some mathematics. But other than that we were free to do as we wished. Professor L. M. Graves encouraged me a great deal. I had him as my teacher in analytic geometry my first quarter there. Then he encouraged me to go into the undergraduate analysis course the second quarter. Simultaneously, I did very poorly in the second quarter of calculus, for which he also urged the professor, Sanger, to accept me. I did very poorly there and well in the undergraduate real analysis. So that was the start of a very mixed career in mathematics. The second year Saunders Mac Lane was visiting and he gave the first quarter of what was then the beginning graduate algebra course but it was at only a slightly higher level than the Birkhoff and Mac Lane book. The second and third quarters were given by Albert, I believe, using his book *Modern Higher Algebra.*

DeGroot: Had you had any experience in the Birkhoff and Mac Lane type of material?

Stein: No. I didn't know much algebra, but then of course you don't really have to. That's the nice thing about pure mathematics. It has a tendency to be self-contained.

"I WAS DOING GROUP THEORY WITHOUT REALIZING IT"

DeGroot: How did a boy from Brooklyn, New York, come to go to the University of Chicago?

Stein: Well, actually we moved to Queens when I was a small child.

DeGroot: [Laughs] I don't know if that's a step in the right direction or not.

Stein: I was interested in mathematics from an early age and, in particular, I was impressed by Dickson's book on elementary number theory, his *Introduction to the Theory of Numbers.* So I was impressed by the University of Chicago and wanted to get away from home. And they accepted me. . .

DeGroot: I gather from what you said that Graves was an influence on you in the early stages of your college education. Are there other people that you regard as major influences on your career as it developed?

Stein: Well, certainly Saunders Mac Lane gave me a feeling for algebra which was important, even though I never really learned very much. Albert's work was more technical and was certainly very good, but I was terribly lazy as an undergraduate and somehow I was readier than most students to accept abstraction. So I did fine in the first quarter of any subject, but then I would tend to get lost from not having worked as hard as I should.

DeGroot: What about after you left the University of Chicago? Are there other people who have influenced your career subsequent to that time, even up to the present time?

Stein: Well, certainly Neyman and Wald. I first became familiar with Neyman's work in statistics when I was in my first two quarters of graduate work in mathematics at Chicago. At that time I didn't know of the work of Wald, but Murray Geisler and perhaps Kenneth Arrow knew Wald at Columbia because Wald would sit in Hotelling's classes and he seemed to know what was going on. [Laughs] And then I got interested in Wald's work in sequential analysis. He was very helpful to me. He gave me encouragement, and in those days I wrote letters occasionally, so he gave me a lot of encouragement in correspondence.

DeGroot: This was while you were in the Air Force?

Stein: Yes.

DeGroot: And was that how you got interested in working on the two-stage procedure?

Stein: Yes. I got interested in that because Arrow lent me Wald's restricted monograph on sequential analysis and then asked me what I thought of it and what was in it. And he expressed disappointment at the fact that Wald didn't deal with the question of getting a sequential version of the t test for which the power depended only on the difference between the means rather than on the variance. So actually Kenneth Arrow suggested the problem to me in that sense; I mean, suggested that at least it was a serious problem. And then I started thinking about it and realized how to do it. It took me about a week to convince myself that my solution was right. But it was very easy to think of.

Certainly working with that group of people, though I am not sure that we were all successful in what we were supposed to be doing, gave me a lot of encouragement. You know, it gave me a lot of possibilities for developing. In particular, when we were sitting around in Asheville after the war was over, waiting to be discharged, we ran an informal seminar. I did some work on that in which I extended in rather unimportant ways some of Wald's work on most stringent tests. And then Gil Hunt pointed out to me that I was doing group theory without realizing it.

DeGroot: I see. That was the start of the famous Hunt-Stein results on invariance.

Stein: Yes. Of course it was very unfortunate that we never published this work. There was at one time a manuscript which got lost.

DeGroot: What do you mean, it got lost?

Stein: I think it had been mislaid. Let's see. What happened was this. For simple problems where all invariant procedures have constant risk and it is meaningful to talk of the best invariant procedure, we set out to prove that it is necessarily minimax. We were careful enough so that we found that we were unable to prove that. [Laughs] So Gil and I eventually showed that it was true for abelian groups and trivially true for compact groups. Actually, the possibility of averaging over abelian groups had been done earlier, in particular by Banach and numerous other people. So anyway, it's true for abelian groups and compact groups and, therefore, by successive reductions, for solvable groups and then things that are combinations of abelian and compact groups.

We did realize that we were unable to prove it for the full linear group in two dimensions, and eventually I got a counterexample for that group. But there was a long delay in getting it and that's what led to the long delay in the paper, and it never got done. Shortly later, Peisakoff at Princeton was working on it under the direction of Tukey, I think. That was a little surprising since it was so abstract a topic, but Tukey *was* interested in mathematical statistics. And Peisakoff got a counterexample for the free group with two generators. Of course, all this was being done by other mathematicians as well, at roughly the same time. Perhaps even earlier. You know, *essentially* the same things. But Peisakoff thought he had a proof for the full linear group, I think.

DeGroot: He thought he had a proof and you had a counterexample.

Stein: Yes. Well, the counterexample is very easy.

"I CERTAINLY TRIED TO PROVE ADMISSIBILITY FIRST"

DeGroot: It sounds as though that may have been the start of two Charles Stein traditions that I know of. One is the development of counterexamples to
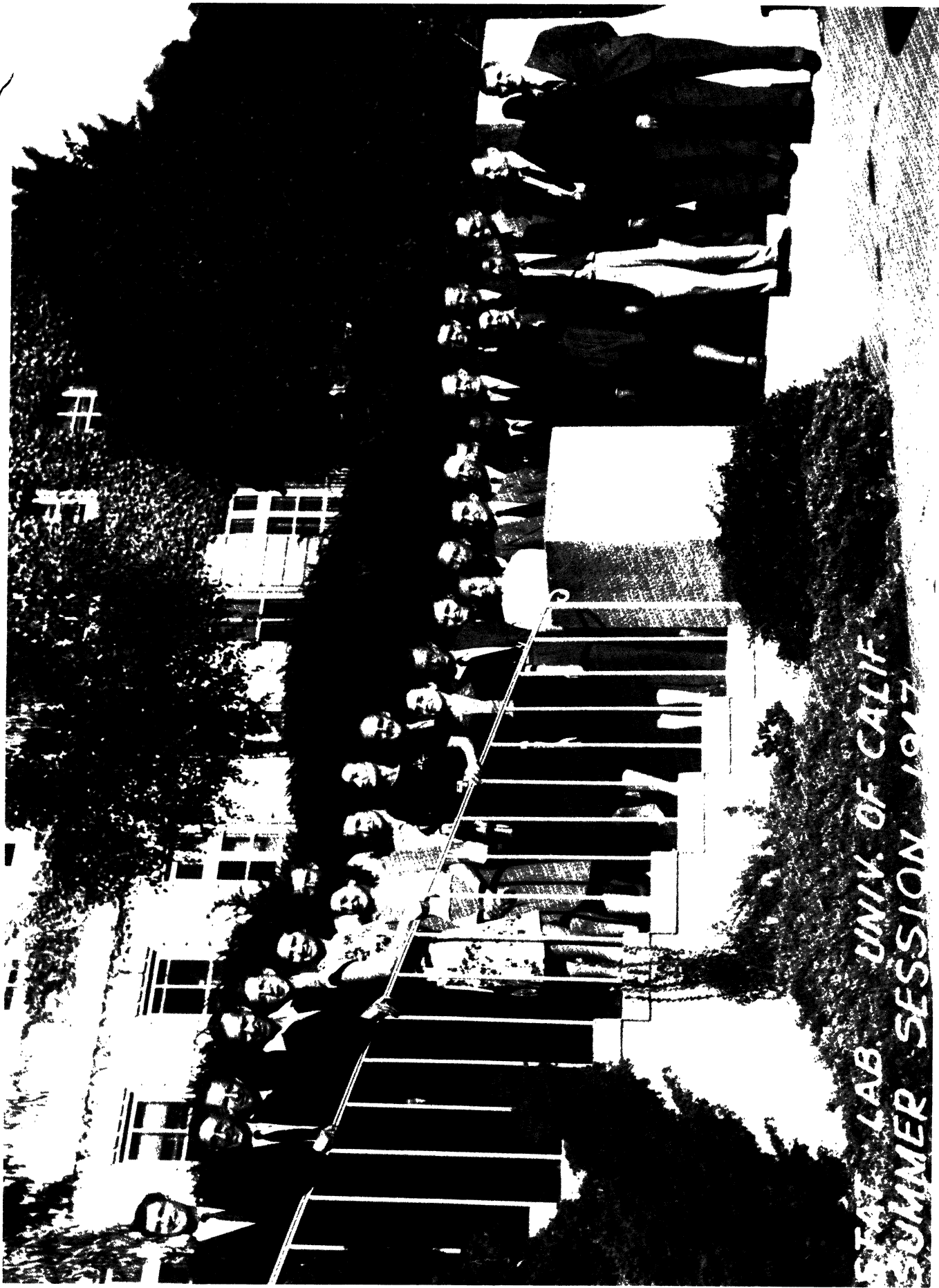
Statistical Laboratory, University of California, Berkeley, Summer Session 1947. 1. E. L. Crow; 2. E. S. Keeping; 3. V. Lenzen; 4. D. G. Chapman; 5. C. Stein; 6. E. Crow; 7. E. Fix; 8. S. W. Nash; 9. E. L. Scott; 10. D. Cruden (Lowery); 11. J. Gurland; 12. L. Elvebach; 13. E. L. Lehmann; 14. T. A. Jeeves; 15. E. Seiden; 16. E. A. Fay; 17. B. Epstein; 18. T. Birge; 19. I. Blumen; 20. ?; 21. B. M. Bennett; 22. J. L. Hodges; 23. T. Hodges; 24. H. Cramér; 25. G. R. Seth; 26. J. Neyman; 27. H. Hotelling; 28. Z. Szatrowski.

things everybody else believes true. The other is not publishing a lot of interesting results that you have developed. Do you feel those are accurate descriptions of at least certain aspects of your career?

Stein: Yes. That results from a certain laziness and perfectionism in the bad sense. Inability to get things done because they are not quite satisfactory.

DeGroot: What about the other aspect, the creativity involved in the development of counterexamples that are very enlightening?

Stein: Yes, it's always useful to speak in terms of examples. And naturally it's better if these examples are striking.

DeGroot: In your work do you tend to think first about examples, or do you attempt to prove some result first and then look for counterexamples when you see that certain aspects don't seem to go through?

Stein: Yes, I certainly tried very hard to prove most of these things.

DeGroot: What about the inadmissibility results, for example?

Stein: Oh yes, I certainly tried to prove admissibility first. In several forms. One was the concrete form that in the case where you have the finite-dimensional translation group operating transitively in the multidimensional location-parameter estimation problem, I tried for a long time to prove that the best invariant procedure—Pitman's estimator—was admissible. Possibly at the suggestion of Jimmie Savage, I finally gave up and tried to prove the opposite, and that turned out to be very simple. That is, using the same method that Lehmann and Hodges had used in the one-dimensional case and combining that with orthogonal invariance considerations, it was easy to prove the admissibility in two dimensions in the normal case. Trying to apply the same method in three or more dimensions, it just became clear that the same method wouldn't work. And then possibly it took a long time and perhaps even some prodding from Jimmie Savage to realize that since that method clearly failed and it was not possible to make it work, inadmissibility was perhaps true. And then there's the other more abstract form: If you have two completely independent decision problems and you add up the losses, and you have an admissible procedure for the first one and an admissible procedure for the second one, the conjecture was that if you combine them the resulting procedure is admissible for the combined problem. And of course that doesn't work because if you combine the admissible procedures for the two-dimensional and the one-dimensional estimation problems the result is not admissible for the combined problem. So that was a counterexample to that conjecture. I don't know if anyone else had obtained a counterexample earlier for that case.

DeGroot: While we are talking about your re-

search, are there papers of yours, or even unpublished papers of yours, that you particularly like or regard as particularly important or influential in the field?

Stein: Well, it's hard to say. I'm hoping that in the long run the ideas that I started in my Sixth Berkeley Symposium paper on normal approximation will turn out to be more important than any of the others. ["A bound for the error in the normal approximation to the distribution of a sum of dependent random variables," *Proc. Sixth Berkeley Symp. Math. Statist. Probab.* **2** (1972) 583–602.] I'm now finishing a monograph in the form of a set of lecture notes on this. That certainly will not justify my belief, but at least it will set out the ideas in such a way that perhaps people will be able to apply them. A number of people have taken up this collection of ideas. Louis Chen, my student, gave a very good treatment of the Poisson analog and some other work on the normal case. ["Poisson approximation for dependent trials," *Ann. Probab.* **3** (1975) 534–545.] Barbour and Eagleson wrote a paper a couple of years ago on applications to graph theory. [Barbour, A. D. and Eagleson, G. K. "Poisson approximation for some statistics based on exchangeable trials," *Adv. Appl. Probab.* **15** (1983) 585–600.] And then Hall and Barbour wrote one. [Hall, P. and Barbour, A. D. "Reversing the Berry-Esseen inequality," *Proc. Amer. Math. Soc.* **90** (1984) 107–110.]

DeGroot: Has this monograph grown out of a course that you taught?

Stein: Well, I've given lectures on it several times. And it hasn't worked out as well as I had hoped. But I now think I understand the basic idea and can expound it, even though I haven't done very well in applying it. And the applications tend to be messy. There are several other people who have applied it— Erickson [Erickson, R. V. "L_1 bounds for asymptotic normality of m-dependent sums using Stein's technique," *Ann. Probab.* **2** (1974) 522–529.] and some people in France, and others I can't remember.

DeGroot: And you feel that potentially it's an idea of wide applicability?

Stein: Yes. But that's in probability theory, not statistics.

"THE BAYESIAN POINT OF VIEW IS OFTEN ACCOMPANIED BY AN INSISTENCE THAT PEOPLE OUGHT TO AGREE TO A CERTAIN DOCTRINE, EVEN WITHOUT REALLY KNOWING WHAT THAT DOCTRINE IS"

DeGroot: Do you see any clear distinction between probability theory and statistics?

Stein: Yes. My work in statistics has been in mathematical statistics interpreted rather narrowly. That is, mostly thinking of problems in a decision-

Charles Stein in Yosemite National Park, August 1985.

theoretic way. So I think of probability theory as different, although a lot of work in statistics is straightforward application of probability theory and you can apply these considerations to the distribution of things statisticians are interested in. But certainly this method that grew. out of my work in the Sixth Berkeley Symposium isn't the only way to think about probability. In particular, in statistical problems there are frequently methods related to the parametric structure which, because of the decision-theoretic aspects and for other reasons, use prior distributions in order to derive results even though not in the Bayesian philosophical framework.

DeGroot: Since you brought up the subject, what is your view of the Bayesian philosophical framework?

Stein: Well, it's rather negative. Of course a lot of good work has been done by people from that point of view. But basically the Bayesian point of view is often accompanied by an insistence that people ought to agree to a certain doctrine, even without really knowing what that doctrine is. For example, would you be able to specify to me what you would consider an authoritative presentation of the Bayesian view that we are so often approached to accept?

DeGroot: Well, I'm not being interviewed. [Laughs] I could put in a plug for my book on Bayesian

decision theory that gives an axiomatic system for probability and utility theory which together imply the entire decision-making process. I mean, normatively anyway.

Stein: Yes, but of course that is the thing. One is asked to accept something normatively before one knows what that thing really is, rather than the attitude that we have toward other systems where we set out axioms or definitions and use them for the purpose of developing a system, and *then* if the system turns out to be interesting we pursue this. But we never ask whether those axioms are true or not; rather, we ask if we can find instances in which this axiomatic development is useful. If so, we accept it. In particular, we try to judge the consequences. Whereas, as you know, there are grave difficulties in trying to apply the Bayesian notions to interesting problems because of the difficulty of choosing a prior distribution. There is one point of view specified by Jeffreys who seems to be saying that there is a prescription, which he did not invent but which he seems to endorse, for choosing a (usually improper) prior distribution, and *that* simply does not work in general. The alternative is that the choice of a prior distribution must be made subjectively, and that I find completely inappropriate. Well, what can one say? Of course, statistical decision theory gives us, within a certain class of problems, an indication of how prior distributions do enter statistics from another point of view. And so in some ways the difference between Wald's decision-theoretic approach and the Bayesian approach is small.

DeGroot: Because Wald used priors as a technical device for determining optimal procedures.

Stein: Yes, and therefore we are considering the same procedures. Roughly speaking, the basic theorems of decision theory say that in some sense good procedures and Bayes procedures are very much the same thing. This is, of course, a gross oversimplification, but it does enable us to understand how prior distributions come in.

"IN NO SERIOUS WORK IN ANY SCIENCE DO WE ANSWER THE QUESTION, 'WHAT DOES THIS STATEMENT MEAN?'"

DeGroot: Let's talk about probability for a moment. You say that the notion of subjective probability is unacceptable to you. What definition of probability do you use?

Stein: Essentially Kolmogorov's. That it is a mathematical system.

DeGroot: Simply any set of numbers that satisfies the axioms of the calculus of probabilities.

Stein: Yes.

DeGroot: But what do these numbers represent in the real world?

Stein: Well, there is no unqiue interpretation. And of course I'm talking about Kolmogorov's old interpretation of probability and not the complexity interpretation. In his book he mentions briefly two aspects of the interpretation. The first is the traditional relative frequency of occurrence in the long run. And the second is that when one puts forward a probabilistic model that is to be taken completely seriously for a real world phenomenon, then one is asserting in principle that any single specified event having very small probability will not occur. This, of course, combined with the law of large numbers, weak or strong, really is a broader interpretation than the frequency notion. So, in fact, the frequency interpretation in that sense is redundant. This doesn't answer the question, "When I say the probability is 1/6 that this die will come up 6 on the next toss, what does that statement mean?" But then in no serious work in any science do we answer the question, "What does this statement mean?" It is an erroneous philosophical point of view that leads to this sort of question.

DeGroot: Do you feel that probability can only be applied to an event such as the die coming up 6?

Stein: No. One can make probabilistic models for anything.

DeGroot: Even though they are nonrepeatable...

Stein: Yes, yes.

DeGroot: But if you can interpret only very small probabilities or very large probabilities, how do you assign a medium-sized probability in a one-shot situation?

Stein: The setting up of a mathematical model for any phenomena is the result of a historical process, and that's true also for setting up probabilistic models. It's also certainly true that probabilistic models, in practice, frequently fall toward the lower end of acceptability and seriousness of anything that can be considered scientific work. They are applied much more casually and simply much more often than, say, physical models, at least perhaps in respectable parts of physics. But that doesn't make them completely inappropriate as models. The lack of uniqueness of probabilistic models may also bother people. For example, one may have two alternative probabilistic models for a phenomenon, in which case a Bayesian who has decided that these are two reasonable models will simply go ahead and attach probabilities to them, and mix them in that way. And if one is to judge empirically, one can't really distinguish these mixtures on the basis of a single realization of the world. Even after one has found that one of the two models is correct, one cannot argue that it is the absolutely correct model rather than the half-and-half mixture of the two. So people may find this disturbing, but that's just the way it is. Probabilistic models do not have the same sort of uniqueness as other physical models.

DeGroot: But surely that means that there is a subjective element entering into the development of the models and the numerical probabilities.

Stein: But, you see, that's something very different from saying that one is absolutely never permitted to consider a probabilistic model in which anything is unknown, and that is the strict interpretation of the Bayesian point of view. Some statisticians seem to try to accept this point of view as sort of a philosophical position but to deny it in practice, which is not reasonable.

"PERHAPS I DON'T HAVE THE RIGHT PERSONALITY TO BE A REAL STATISTICIAN"

DeGroot: Have you been involved in any applied problems since your Air Force days?

Stein: No. Unfortunately, I haven't done any serious applied work. That is, I find it hard to even think of *any* applied work I've done. I suppose it's a result of two things at least. One is that people tend to be asked to do the things they do best. As I say, I'm very lazy and so I just don't have time to do the things I *have* to do and, in addition, applied statistics. And that's very unfortunate. And then there is the additional fact that I can't do applied statistics in the sense of really putting forth a statistical analysis and asking people to accept it, because I just don't have enough self-confidence. Perhaps I don't have the right personality to be a real statistician.

DeGroot: Could part of that hesitancy also be a lack of belief in statistical models as being appropriate vehicles?

Stein: No. I don't share this universal skepticism that some people seem to have. [Laughs] It's clear that probabilistic and statistical models *are* useful, that is, are a serious and correct way of dealing with natural phenomena in many cases. It's just that for a variety of reasons, I've never been able to do it. Now what I *could* do is work on analysis of data in the literature and reanalyze things. My personality wouldn't interfere with that. But the time question is difficult. Then there is the difficulty that real problems are typically so complicated that you cannot give satisfactory theoretical treatments of them, just because of the complication. And so somehow one has to come to deal with that. Of course, with the aid of computers, people can now compute a great many things. I think that there ought to be more of the combination of theory and practice. I regret that I haven't done it; I'm probably too old now.

DeGroot: Do you see a trend in the field generally of a merging of what used to be called applied statistics and theoretical statistics?

Stein: No.

DeGroot: So you think the world of statistics is

still pretty much divided into theoretical statisticians and applied statisticians?

Stein: Yes, I think so—even more divided. There are so many more possibilities for computation, and some of them are clearly useful. People can find things by using somewhat arbitrary computational methods that could not be found by using traditional statistical methods. On the other hand, they can also find things that probably aren't really there.

DeGroot: In other words, you think that the development of computational power has fostered this separation because people can just use the computer to fish around to solve applied problems, so to speak, without having to appeal to any theory whatsoever.

Stein: Yes.

DeGroot: Do you see the computer as a tool in theoretical work?

Stein: Sure, it will be. I regret also that I haven't learned how to compute properly. I have a small IBM, but all I know how to use is BASIC and I tend to do everything from scratch. And, of course, I want to be absolutely sure that I know what is being done, so I don't like packages. I don't have them for my computer and have never learned how to use these bigger ones. But I hope to next quarter when I have a bit more time.

DeGroot: I know that you are teaching a large class in introductory probability right now. How many students are in there?

Stein: About 180. I am not happy with my elementary teaching. I am not happy with any of the textbooks. Occasionally I think of writing one myself, but there is *so much* to writing an elementary textbook other than getting the ideas laid out in a systematic, simple, intelligible, and yet correct, way. That *should* be the aim, and that's where I think people have fallen down largely, but there is so much more to an elementary text. You also have to provide exercises, you have to give explanations, you have to give examples. Of course, that's all part of laying out the ideas.

DeGroot: When you are teaching a course like introductory probability do you have some novel ways of getting across some of the basic concepts?

Stein: No, I don't. I tend to be overwhelmed by the process of teaching, and the textbook always forces one in the wrong direction.

"SOMEHOW I DON'T SEEM TO THINK ALONG THE SAME LINES AS OTHER PEOPLE"

DeGroot: What things do you like to do when you are not doing statistics? Tell me a little bit about your life outside of statistics and your hobbies or other activities.

Stein: I have a wife and three grown children. When the children were young they occupied a fair amount of my energy. During the war in Vietnam I was active in the antiwar movement, and I'm still occasionally active in the peace movement. But unfortunately I have drifted away from it, partly because any sort of interaction with other people is not really very congenial to me. I got involved only because I felt I had to, not because I enjoyed it. Then, also unfortunately, recently I've been somewhat more frivolous and have gone in a lot for hiking. I also run ten miles a week—three times, ten miles altogether. Which is not much, but of course I'm very lazy. I hike with the local day-hiking group—the Loma Prieta chapter of the Sierra Club. We have hikes every weekend.

DeGroot: Do you think your political views influence the kind of problems you work on and your scientific philosophy at all, or are they separate?

Stein: I'd say they are largely separate. Of course, I don't do military work, not even nominally. That is, I haven't been on a military contract for 18 years. Actually, even before that it was distasteful but I allowed myself to be talked into it. But this is a hard question to answer. I would admit that my work is largely independent of my political attitudes. I don't agree with Lindley that a subjective approach to probability is a consequence of being a Marxist or socialist.

DeGroot: I notice you have some Dorothy Sayers and Edgar Allan Poe books mixed in with the statistics books on your shelves. I don't imagine that those are textbooks for your classes, so they must be outside reading.

Stein: Frivolity.

DeGroot: You must enjoy reading mysteries.

Stein: Not much anymore, but I did at one time.

DeGroot: Since you joined the Stanford faculty in 1953, you spent years or parts of years on leave in Seattle, Berkeley, Cornell, Leningrad, Oxford, various other parts of Europe... I would be interested, just generally, in hearing about what kinds of things you did on those leaves. When you are away, do you change your style of working or the topics on which you work?

Stein: No, unfortunately I do not. Occasionally I am influenced by casual outside influences, either other people around me or just some articles I happen to pick up. But these have tended to be rather minor things. Well, there were two recently. There's a paper I wrote on the Turán-Kubilius inequality in probabilistic number theory [Tech. Report No. 220, Dept. of Statistics, Stanford Univ., July 1984] that just grew out of a casual glance at a paper written by a Hungarian mathematician, Ruzsa, on that subject. Then when David Hinkley was here he gave a lecture in a seminar that led me to do some work on the confidence sets of Welch and Peers based on prior distributions. [Welch, B. L. and Peers, H. W. "On formulae for confidence points based on integrals of weighted likelihoods," *J. Roy. Statist. Soc. Ser. B* **25** (1963) 318–329.] Anyway, there's a technical report of mine on that topic. [Tech.

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Report No. 180, Dept. of Statistics, Stanford Univ., November 1981; to appear in the Proceedings of the Banach Center, Warsaw.] The idea is that by choosing the prior distribution properly one can get probability coverage differing from the nominal probability of coverage by order of $1/n$, whereas almost any prior distribution will give you $1/n^{1/2}$. In one dimension, it's the Jeffreys prior; in higher dimensions, it definitely isn't.

DeGroot: I see. But most of your problems, I take it, are just problems that are generated somehow internally and that appeal to you as interesting problems.

Stein: Yes. They tend to be problems that are generated internally or, if generated externally, where I work on them partly because my way of thinking has something to say about them. But these are cases that are unrelated to my other work.

DeGroot: What does the future hold for you?

Stein: I don't know. I would like to have more time to do my own work. I tend to be overwhelmed by teaching and by any agreements I make to give lectures, so I would like to retire. I'm not sure; let's say half-retire.

**DeGroot:** You mean six years from now?

Stein: No, one year from now. And perhaps have more time to travel, although that isn't really the best way for me to get work done. Actually, however, I did get a fair amount done the last time I was away.

DeGroot: I would say that you've gotten a fair amount of work done through the years. Indeed, I think that your claim that you are lazy is disproved by the amount of work you have gotten done. Just because not all of it has gotten written up, doesn't mean that it hasn't gotten done. The world of statistics recognizes that and appreciates what you have accomplished. I also think that *we've* covered a fair amount of ground in this conversation.
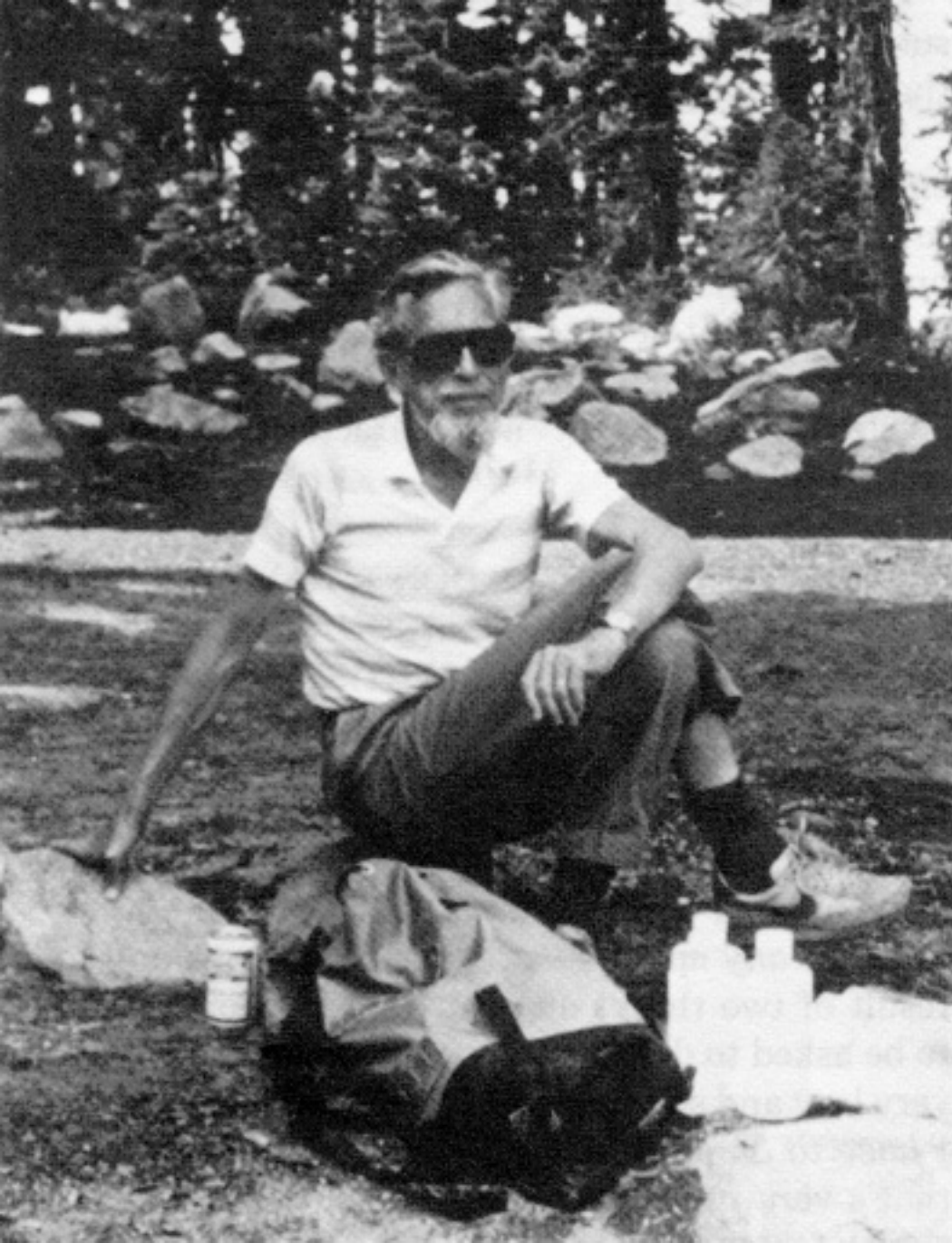
Stein: Yes. I may want to modify some of my answers when I see the transcript of this conversation. Somehow I don't seem to think along the same lines as other people, which is useful. It's good that different people think differently.

DeGroot: Thank you, Charles.
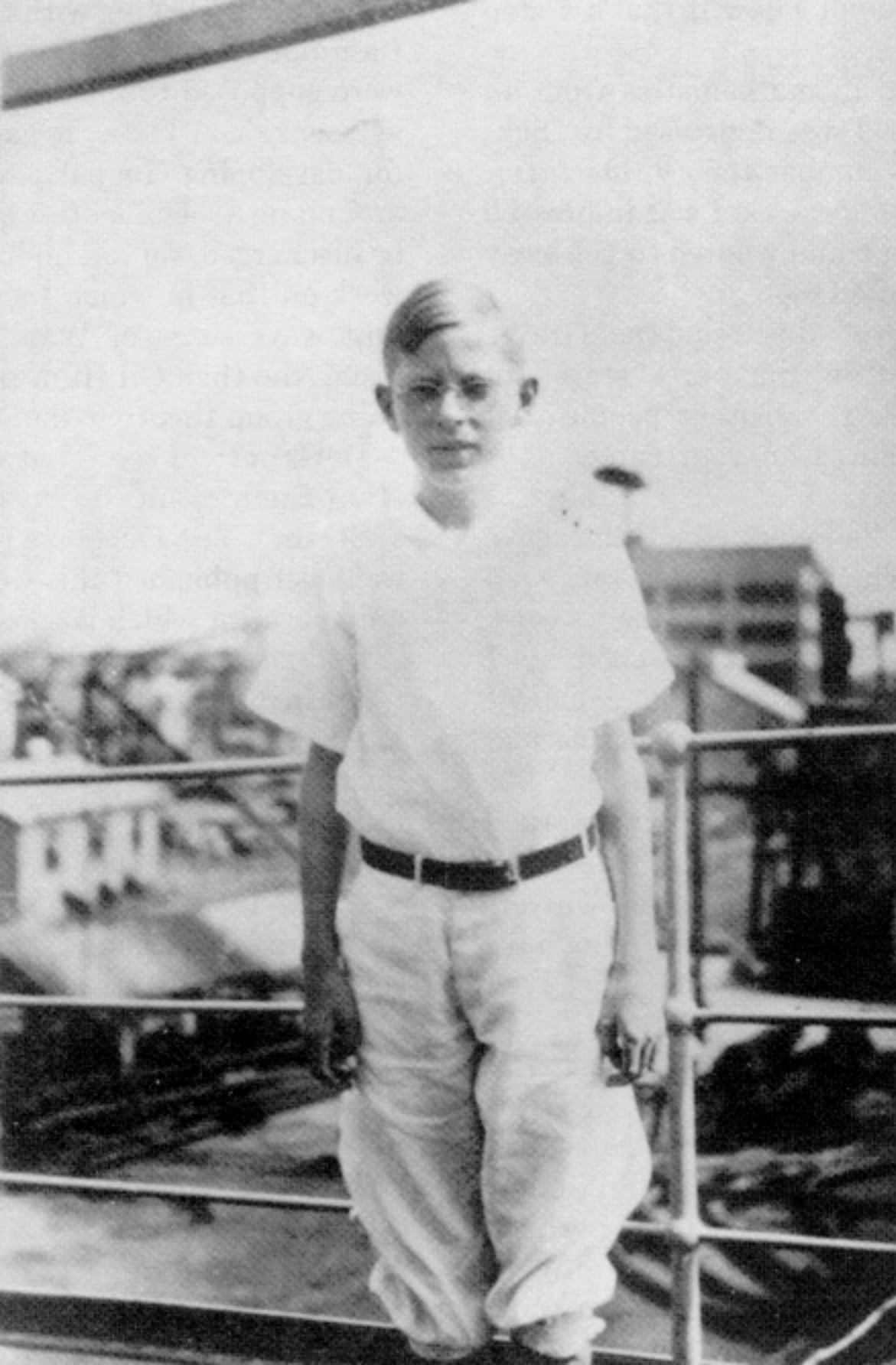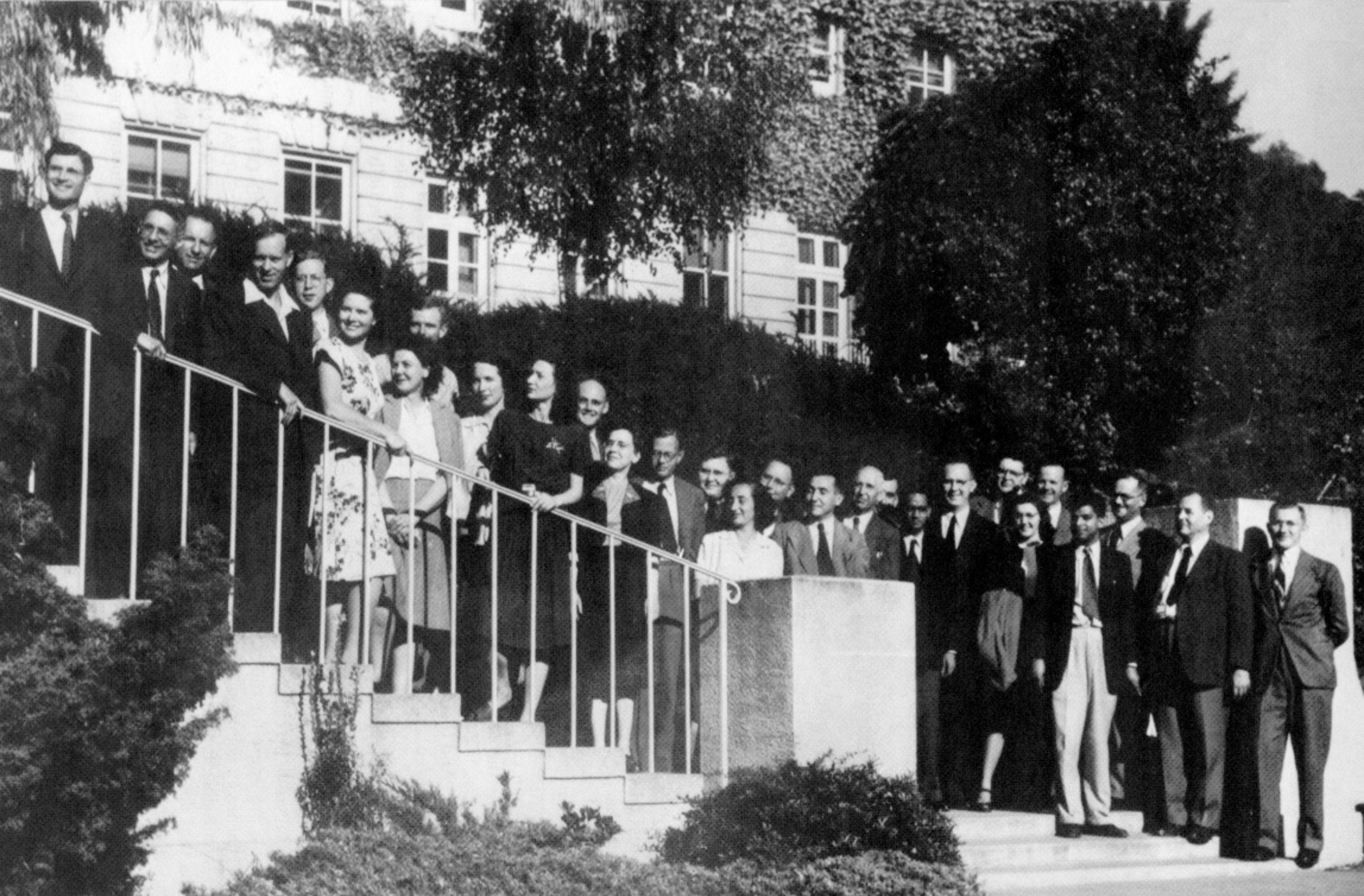
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