Letter from Norman Steenrod to R.L. Wilder

(February 28, 1937)

R. L. Wilder's Correspondence with Norman Steenrod. Center for American History (Wilder, R.L. Papers), University of Texas at Austin.

Norman Steenrod studied topology as an undergraduate student at the University of Michigan, under R. L. Wilder, who was instrumental in getting support for Steenrod to do graduate work at Princeton. There he wrote a Ph.D. thesis in 1936 under the direction of S. Lefschertz. He then taught at Chicago and Michigan, before returning to Princeton for the remainder of his academic career. He directed 13 Ph.D. students, one at Chicago, one at Michigan, and 11 at Princeton. He wrote jointly with Samuel Eilenberg the classic treatise *Foundations of Algebraic Topology.*

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Princeton N.J.
February 28, 1937

Dear Sir:

I am perplexed by a problem, maybe you can give me the correct steer. You see, it is this way. Beginning in January last, I've been running a seminar in topology; and doing it in the spirit of the R.L. Moore school. So far I've been following the notes to the course you gave at Michigan. The class contains about six willing workers. They have proved already 19 theorems and 16 lemmas. There are still 10 lemmas left before it is necessary to introduce Axiom VI. I hesitate to do this. I've been stalling them off by giving them some propositions on the theory of sets to prove.

The thing I've noticed about Princeton is that the students who come here without having done research work have considerable difficulty in learning how to do it. They have to pick it up for themselves. So I talked up the virtues of the Moore system to Lefschetz. He finally agreed that it was worth trying. However he complained bitterly about the material I proposed using. So far he has been satisfied. He attended several of the meetings and was very pleased with the way things were going. He was so pleased in fact that he is starting a seminar in algebraic geometry to be conducted in as nearly the same fashion as possible.

Perhaps now you see my difficulty with Axiom VI. It is just the kind of thing Lefschetz would find objectionable. In a way I sympathize. The axiom is hard to remember. Couldn't it be replaced by merely requiring that the boundary of a region is a simple closed curve? Perhaps you know of some other set up which would be more satisfactory. I hope to have the class go completely through the characterization of the plane.

The business of teaching is an eye-opener……

[Sections of the original letter have been omitted. Full copy of original letter is attached.]

Hurewicz gave us a series of lectures of his work on homotopy. It is beautiful stuff and plenty of it.

It looks as though I will be able to remain at Princeton for another year in my present position.

Sincerely yours

Norman E. Steenrod

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The business of teaching is an eye-opener. I have two freshmen classes. The course consists of analytical geometry (first semester) and differential calculus (second). I didn't do too badly the first semester. There are 250 freshmen taking the course so they have fifteen classes, and throw uniform examinations for them. They have preferred sections for the better students. My students do about average among the non-preferred. Right now we are trying to teach the notions of limit and continuity in a rigorous fashion. The boys don't like it. We are using a mimeotyped set of notes (22 pages); this will last for a month when we will transfer to Fine's Calculus.

Right now I'm pretty much in the dumps. Have been working on one problem since last November. I made much progress and on several occasions thought I had a proof. But each time a slip appeared. In order to get around each slip I had to double the complications in the construction. At the moment I have such a morass of complications that I can't move. Besides, the sickening feeling is creeping up on me that I will wind up by trying to prove something obviously equivalent to the original theorem. The problem is this: M = connected manifold (compact or not). Topologize in obvious manner the group G of homeomorphisms of M into itself. Prove that G does not contain arbitrarily small compact subgroups. Newman has proved that G does not contains arbitrarily small periodic subgroups (Quart. Journal 1931). I have so much good argument that failure to push it through would convince me that there is no god in heaven. Maybe I need a course in how to do research work.

A young fellow named Dowker who is working here on his thesis has been getting some interesting results. He has shown that the number of elements in the 1-dimensional Čech homology group of a straight line is equal to the power of the continuum. He gets these result by generalizing the theorems on the numbering of homotopy classes of mappings of compact metric spaces into spheres. He generalizes to normal spaces and requires that homotopies be uniform. Then in an n-space the dual homology group of dimension n defined in the Čech manner numbers the classes of uniformly homotopic mappings into an n-sphere. It is easy to construct mappings of a line on a circle which are not uniformly homotopic to a point. Dowker likewise generalizes the ~~same theory~~ mapping theorems to locally-compact separable spaces using ordinary homotopy and finds that the dual homology group based on locally-compact infinite cycles gives the numbering of the homotopy classes.

Hurewicz gave us a series of lectures on his work on homotopy. It is beautiful stuff and plenty of it.

It looks as though I will be able to remain at Princeton for another year in my present position.

Sincerely yours

Norman E. Steenrod

P.S. My mother, father, sister and dog are living with me in Princeton this year. We are renting at 126 Jefferson Rd. Dad is the only one who likes Princeton — he goes to all the lectures.