September 19, 1982

Dear Stephanie,

My reply to your letter of Sept 10 has been delayed by a number of factors; in particular, by Ann's telling me that you really wanted to know more than you asked and urging me to compose a more elaborate letter than a literal interpretation of yours would require. Also, when I met your mother at Woods Hole in August she suggested that you were torn between physics and mathematics ??

Since I myself hesitated between physics and mathematics at one time and have come back to it part time and since you want to know what my "field" is I think I might begin by telling you something about how I started out and how I was led to my present interests — in fact my field cannot easily be determined by a label. This would make it easier for you to assess my bias and for me to describe what I see pure mathematics to be all about.

Up until I was about fifteen I expected to be a business man like my father. Then, at his suggestion (because of what turned out to be a quite temporary interest of his in a new chemical process) I got a book on chemistry out of the public library and read it. I was absolutely fascinated and from then until I entered Rice University chemistry was my all absorbing hobby. I had a crude laboratory in our garage, got bottles of sulfuric and hydrochloric acids as Christmas presents and I also did a lot of reading. In fact when I actually took chemistry my senior year in high school, I found that I knew more than my teacher. I had a bit of a struggle reconciling this new interest with my long standing career plans and finally compromised by signing up for the chemical engineering program at Rice. I went through the first two years of that program but by the beginning of my sophomore year I had already decided that I wanted to be a pure chemist. I had always been interested in mathematics and done well in it. In fact, I was interested enough to get a book and teach myself calculus[1] during the summer between my second year in high school and my first year at Rice. In any event, while I was considering moving into pure chemistry my teachers began to suggest mathematics to me. I seemed to take undue interest in the mathematical aspects of my chemistry and physics courses and was disappointed to find

[1]I should explain that back in the 1930's calculus courses in high schools were very rare if they existed at all and at Rice as in many colleges one did not get to it until the sophomore year at best.

that mathematics played a rather minor role in chemistry. On the other hand at the time pure mathematics seemed to me to be relatively dry and devoid of content compared to physics and chemistry and after another soul struggle decided to compromise by majoring in physics rather than either chemistry or mathematics. Actually I took in all just as many mathematics courses as physics courses and in effect had a double major. Officially however I majored in physics. Specifically I took two courses in mathematics and two in physics during each of the four terms of my junior and senior years.

When I went to Harvard to work toward a Ph.D. I meant to go with physics eventually but applied to the mathematics department for admission. My intention was to learn some more mathematics and then come back and do physics "right". I had found physics extremely interesting — especially because of the rather advanced mathematical tools that it used. On the other hand I was quite disturbed by the loose way theory was defined in physics and by the sloppy "hand waving" proofs. I wanted somehow to combine the logical precision of mathematics with the (apparently) richer content of physics. However as my mathematical studies progressed at Harvard I gradually came to realize that pure mathematics had just as rich a content as physics and quite happily dropped physics and became a full fledged pure mathematician. Indeed I took no physics courses after leaving Rice. On the other hand, as it turned out, my involvement with physics was far from over. For over a decade, though, I thought it was. My "field" at first was the theory of infinite dimensional vector spaces. At the end of my first graduate year I encountered a thick book in the mathematics library entitled "Linear transformations in Hilbert space and their applications to analysis" by M. H. Stone. Stone was then a young (35 year old) associate professor who taught me the second semester of the standard real variable course. I found the material in the book quite fascinating and ended up reading 60 or 70% of it during the summer and asking Stone to be my thesis advisor. He agreed but somehow I got away from Hilbert spaces and wrote a thesis on much more general infinite dimensional vector spaces. It was well received but it bored me and shortly after World War II I changed my field rather drastically to one combining Hilbert space, group theory and measure theory. The stimulus was another book. This one was French and was entitled "L'integration dans les groupes topologique et les applications". The author was a now very celebrated mathematician named Andre Weil. I first heard about the book in 1942 and still remember being very excited by one thing I learned by looking at it in 1944. However, unlike Stone's book which is clearly and beautifully written it is nearly impenetrable and

I would probably have never learned to appreciate it if I had not (in a rash moment) agreed to teach a course out of it in the spring of 1947. Thus pressured I forced myself through it in the summer of 1946. In fact in order to understand it I more or less rewrote it and then changed it considerably when I presented the material to the class. It was well worth the effort, however.

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I have seldom enjoyed teaching a course as much as I did that one. In fact I sometimes felt that I should be paying Harvard rather than vice versa. The full beauty and significance of the subject slowly dawned on me as I prepared and delivered my lectures and I was often in an almost ecstatic state. Fortunately for me it turned out that the subject was quite incomplete. Weil's book dealt in detail with two special cases: (1) that in which the groups are compact but not commutative (2) that in which the groups are commutative but only locally compact. It was an obvious problem to try to unify them by extending the theory to general locally compact groups which are neither compact nor commutative. In fact several papers had just come out on the subject. I joined the fray and managed to be one of the "pioneers" in developing what turned out to be a very fruitful extension of the beautiful material in Weil's book. In fact what I had learned from Stone's book came in very handy and was responsible for my being able to make one of my main contributions.

For about ten years I worked on this subject from the point of view of a pure mathematician and as a sort of hobby kept an eye on physics. Mainly I tried to understand what quantum mechanics is all about. This as you may or may not know is a beautifully subtle refinement of classical mechanics which has to be used in dealing with electrons and other very small particles and which makes it possible to understand with mathematical precission just exactly how molecules form out of atoms. It was discovered in the late 1920's but my physics program at Rice didn't go quite that far and I felt frustrated in not understanding it. I was especially interested because there were connections with the theory of operators in Hilbert space which was one of my main interests. The physics books were much too vague and given to hand waving to be of much use to me. However I kept trying and was reading the more mathematically oriented books by Dirac, von Neumann and Weyl and [one] day in 1956 or so a great light dawned and I suddenly understood the main ideas. Strangely enough my mathematical studies not only helped but enabled me to make a sort of contribution. Ultimately I found that the mathematical theory I had been helping to develop was almost the ideal tool for understanding the whole structure of quantum mechanics. I started spending a lot of my time on physics, trying to reformulate it from this point of view and giving courses for quantum mechanics for mathematicians. Still later I became interested in the fact that this same mathematical theory — the theory of unitary group representations — has extensive applications to the theory of numbers and began to learn number theory (of which I was absolutely ignorant) and to develop its connections with unitary group representations. Now I am interested in this whole complex of ideas and for well over a decade have spent a lot more time studying physics and number theory than trying to prove theorems about unitary group representations. You see why I cannot tell you what my field is in a few words. Actually there is another facet involving "ergodic theory" and probability but enough is enough.

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This description of what my field is and how I got into it is rather longer than I expected it to be and I apologize if I have tried your patience. On the other hand it is probably a bit unintelligible without further explanantion. What are topological groups, Hilbert spaces and group representations and why do I find them so interesting? Actually when properly understood they are rather central topics and I think it will help me to give you a picture of what pure mathematics is all about and how it relates to physics if I give you a brief historical sketch including definitions of some related concepts.

Let me begin by recalling two central events of the 17th century. In the 1660's I. Newton invented calculus and differential equations and used them to give a mathematical theory of the motion of the planets around the sun. In 1670 the son of P. Fermat published the marginal notes in his father's copy of Bochel's translation into Latin of certain books of the Greek mathematician Diophantus. Thus two of the main branches of modern mathematics were respectively started and revived within a few years of one another. The theory of numbers is one of the oldest branches of mathematics and already Euclid in 300 B.C. could prove that every integer is *uniquely* a product of primes; that there are infinitely many primes and could find all possible triples of integers x, y, z such that $x^2 + y^2 = z^2$. Six hundred years later Diophantus in Alexandria studied many problems involving *integer* solutions of equations and wrote several books about them. One now refers to Diophantine equations. Little progress was made after Diophantus until the time of Fermat. But Fermat revived the subject by challenging mathematicians with statements of theorems he claimed to be able to prove. For example (1) every odd prime whose remainder is 1 when divided by 4 is a sum of two squares. (It is easy to prove that when remainder is 3 the equation $x^2 + y^2 = p$ is impossible (try it)). (2) Every integer is a sum of four squares (3) If n is a non-square integer then $x^2 - ny^2 = 1$ always has integer solutions (4) There are no integer solutions of $x^q + y^q = z^q$ when $q \geq 3$. None of these were proved until well into the next century and (4) is still a challenge.

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The works of both Fermat and Newton led in the next two or three hundred years to an enormous development which encompasses a large part of modern mathematics and one of the really exciting 20th century facts is the extent to which the two directions have merged — a merge which started in the mid-nineteenth century. I want to delineate a few high points but first I must say a bit more about the work of Newton in inventing differential equations and applying them to physics. Differential equations are what made modern physics possible and the most important thing about calculus is that it makes differential equations possible. Newton's work is epoch making in the strongest sense of the word and I personally find it deplorable that these facts are so little emphasized in modern teaching.

A differential equation is an equation involving an unknown function and its derivatives. Solving it (in the simplest sense) means finding *all* the functions which satisfy it. One case you do study in elementary calculus but you don't usually call it a differential equation. It is the equation of the form $\frac{dy}{dx} = f(x)$ where f is a known function and y is an unknown function. For example the general solution of $\frac{dy}{dx} = x^2$ is $y = \frac{x^3}{3} + C$ where C is any constant. A simple example *not* of this form is

$$\frac{dy}{dx} = y$$

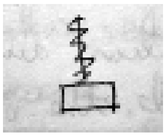
Here it is easy to guess *one* solution, i.e., $y = e^x$. To get the general solution we let y_1 and y_2 be any two solutions and consider $\frac{d}{dx}(\frac{y_1}{y_2})$. This is

$$\frac{y_2 \frac{dy_1}{dx} - y_1 \frac{dy_2}{dx}}{y_2^2} = \frac{y_2 y_1 - y_1 y_2}{y_2^2} = 0$$

Thus $\frac{y_1}{y_2}$ is a constant. Hence if y is any solution, then $\frac{y}{e^x}$ is a constant. The general solution is $y = Ce^x$ where C is any constant. Just as in integration there is an arbitrary constant but it enters in a different way. It isn't just added in. $e^x + 1$ is *not* a solution of $\frac{dy}{dx} = y$. A slightly harder case is the equation $\frac{d^2y}{dx^2} = -\lambda y$ where $\lambda > 0$. The general solution of this is

$$y = C_1 \sin \sqrt{\lambda} x + C_2 \cos \sqrt{\lambda} x$$

where C_1 and C_2 are possibly different arbitrary constants. It is easy enough to check that this is a solution for all C_1 and C_2 but rather harder to prove that there are no other solutions. See if you can do it!! The proof is not very long but it's tricky. Taking this for granted I can show you the essential reason why differential equations are important in physics. Consider a weight on a spring.



You learned in physics that when it is displaced from equilibrium it is accelerated back to its equilibrium position by an amount directly proportional to the displacement. Let f be the function telling you what the displacement is at time t. Then by the physics law $\frac{d^2f}{dt^2} = -\lambda f$ where λ is some constant which can be measured. This differential equation tells you everything about what happens. I have just told you that its general solution is

$$f(t) = C_1 \sin \sqrt{\lambda} t + C_2 \cos \sqrt{\lambda} t$$

Setting $t = 0$ we get $f(0) = C_2$. Differentiating we get $f'(t) = \sqrt{\lambda} C_1 \cos \sqrt{\lambda} t - \sqrt{\lambda} C_2 \sin \sqrt{\lambda} t$ and $f'(0) = \sqrt{\lambda} C_1$. Thus $C_1 = \frac{1}{\sqrt{\lambda}} f'(0)$, $C_2 = f(0)$. We have now a concrete formula for $f(t)$ at all times t

$$\frac{1}{\sqrt{\lambda}} f'(0) \sin \sqrt{\lambda} t + f(0) \cos \sqrt{\lambda}$$

Once we know the starting displacement $f(0)$ and the starting velocity $f'(0)$ we have a formula for the motion at all further times.

This is a simple example of the fundamental technique of mathematical physics: express the physical law as a differential equation and then study the solution of this differential equation. Its power and ramifications have to be seen to be believed.

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At this point I shall abruptly close. I have more to say and I am well aware that I have yet to answer your specific question. However I have a class to teach in 45 minutes and I am going to Princeton for two days this afternoon. I'll continue then next week but thought I might as well give you this much now. Give Ann my best.

Sincerely yours,
G.W.Mackey