O.R. Frisch, 22 Worts Causeway, Cambridge, U.K.

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Prof. Otto Stern
759 Cragmont Avenue
BERKELEY 8
California
U.S.A.

b. 19.XI.1968

22 WORTS CAUSEWAY CAMBRIDGE PHONE 47207

3.11.1968

Lieber Herr Stern!

wie Sie wohl schon in der Zeitung gelesen haben, ist Lise Meitner am 27.10. gestorben. Sie hat nicht viel gelitten, wurde bloß in den letzten Monaten immer schwächer in Körper und Geist, verbrachte die letzten zwei Monate in sehr guter Pflege in einem Nursing Home und verschied im Schlafe.

Anfang Februar hoffe ich auf paar Tage nach Berkeley zu kommen; ob ich Sie da wohl vorfinde?

Mit herzlichen Grüßen

Ihr

Robert Frisch

Professor Dr Lise Meitner FMRS

Born 7th November 1878

Died 27th October 1968

A great scientist who never lost her humanity or humility

O.R.Frisch, 22 Worts Causeway, Cambridge, U.K.

1/6

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Prof. Otto Stern
759 Cragmont Avenue
BERKELEY 8
California,
U.S.A.

6.9.XII.68

O. R. FRISCH
22 WORTS CAUSEWAY CAMBRIDGE PHONE 47207

24. 11. 1968.

Lieber Herr Stern,

es tut mir sehr leid zu hören, daß es Ihnen so schlecht geht. Dabei wollte ich Sie sogar um einen Gefallen bitten. Man drängt mich jetzt, eine Biographie von Lise Meitner zu schreiben, und da versuche ich einmal zunächst Material zu sammeln, vorallem über die frühen Jahre, von denen ich wenig weiß.

Ob Sie sich wohl kräftig genug fühlen, um gelegentlich irgendwelche Erinnerungen zu notieren? Wann und wie Sie die Lise kennengelernt haben? Wo, mit wem und worüber man damals diskutiert hat? Lustige Anekdoten sind natürlich besonders willkommen. Es wäre wohl eine Anstrengung für Sie, aber vielleicht auch ein Zeitvertreib!

Meine Reise liegt noch nicht ganz fest, aber wahrscheinlich bin ich in der dritten Januarwoche in Kalifornien und freue mich darauf Sie dann zu sehen.

Herzlichst Ihr Robert Frisch

~~Liebe Käte~~

dear Käte,

I changed my ~~New York~~ arrival to Thursday June 25 so I wont accept your kind offer thousand thanks anyway stop shall stay in London June 23 to 25 probably Hotel Mayfair Love

Otto

Tiefenau, Zürich

Lieber Herr Frisch, d. 16 VI

vielen Dank für Ihre prompte Besorgung des Zimmers im Mayfair.

Leider gibts wieder eine Komplication, ich muss ~~2 Tage~~ einen Tag länger in London bleiben, vom 23 bis 25 VI. Ich hörte, dass Estermann schon [illegible] wäre. Wenn er wirklich noch da ist, kann er sicher beim Zimmer finden helfen, die U.S. Gesandschaft hat "pull". ~~Lise Meitner Brief~~ Ich möchte ihn natürlich auch sehr. Lise Meitners Brief erhielt ich gestern.

Da sie anscheinend Schwie-
rigkeiten am 22. mit

22 WORTS CAUSEWAY CAMBRIDGE PHONE 47207

14. Juni 1964.

Lieber Herr Stern,

im Mount Royal Hotel war leider kein Zimmer mehr frei und die andern Hotels waren auch sehr voll, weil es in der Woche die Pferderennen in Ascot gibt. Aber im Mayfair Hotel hab ich endlich ein Zimmer für Sie bekommen und kann nur hoffen, dass es ruhig ist. Das Mayfair Hotel gilt für gut.

Jetzt hoffe ich noch, dass Sie eine gute Reise haben und freue mich darauf Sie Dienstag nachmittag zusammen mit der Lise Meitner zu besuchen!

Mit herzlichen Grüsse

Ihr

O R Frisch

PS. Würden Sie gerne auch den Estermann sehen, oder wird Ihnen das zuviel? Er arbeitet am Office of Naval Research, Keysign House, Oxford Street, LONDON W.1.

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O.R. Frisch, 22 Worts Causeway, Cambridge

Herrn
Prof. Otto Stern,
Pension Tiefenau,
Steinwiesstrasse 8,
ZÜRICH

Switzerland

b. 16.VI.64

ZÜRICH 32
16.VI.64-11
NEUMÜNSTER
VIII

ZÜRICH 1
16.VI.64-10
E[illegible]ZUSTELLUNG
VIII

Bky, 31. Mai 1965

Lieber Fritz,

besten Dank für die sorgsame Durch-
arbeitung meines Briefs vom 23.
Vor einigen Tagen erhielt ich einen
Brief von Born, der über Ihren Artikel
schreibt: „Seit langem habe ich mich nicht so
gefreut. ... [illegible] Folgerungen [illegible]
[illegible] im Zusammenhang dargestellt. ... etc."
~~Ganz meine Meinung!~~
Es tut mir sehr leid, daß Lise Meitner
ernstlich krank war. ~~Ich glaube [illegible]~~
~~[illegible] mir immer [illegible]~~
~~[illegible]~~ [illegible] † Ich schreibe ihr [illegible]
Ich selbst habe es auch seit [illegible]
mit dem Herzen zu tun, jetzt [illegible] ich
[illegible] die [illegible] und [illegible]
mir [illegible] abgesehen [illegible]. [illegible]
besonders, [illegible] können [illegible] nicht
mehr [illegible]. ~~Übrigens [illegible]~~
~~[illegible]!~~

† Schön, daß [illegible] jetzt [illegible] gibt.

Nach diesen [illegible] ist es mir eine ~~besondere~~ große
Freude, zu hören, daß es Ihnen und Ihrer Familie
so gut geht, und ich wünsche von Herzen, daß es
recht lange so bleibt. Bitte grüßen Sie Ihre Frau
und Kinder bestens von mir und Sie selbst recht
herzlich gegrüßt

von Ihrem alten

Adolf [illegible]

To open slit along here

Sender's name and address:

O.R. Frisch
22 Worts Causeway
Cambridge, Cambs.
England

To open slit along here

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CAMBRIDGE
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23 MAY
1965

b, 31.V, 65

Prof. Otto Stern
759 Cragmont Avenue
BERKELEY 8
California
U.S.A.

O R Frisch, 22 Worts Causeway, Cambridge.

22.5.1965.

Lieber Herr Stern,

vielen Dank für Ihren freundlichen Brief, den ich gestern erhalten habe. Dazu muss ich noch sagen, dass der Editor vom Scientific American mein Manuskript sehr verbessert hat!

Lise Meitner, der es gut geht, war mit mir auf 3 Wochen in U.S.A., u. zwar in Boston*, in New York, wo sie eine Schwester hat (und ausserdem Miss Wu getroffen und Brookhaven besucht hat) und Washington DC, wo wir mit meinem Kousin Weihnachten gefeiert haben; da war sie in sehr guter Form. Aber bald nachdem sie am 28.12. mit mir zurückgeflogen war, klagte sie über starke Schmerzen in der Schulter; ein Herzspezialist wurde zugezogen, diagnostizierte einen kleinen Infarct und verordnete Bettruhe im Sanatorium. Dort ging es ihr eine Woche lang ziemlich schlecht, aber dann besserte sich ihr Zustand rapide. Bald war sie wieder aus dem Bett, musste aber noch einige Wochen im Sanatorium bleiben, bis es uns nach viel Arbeit gelungen war, ihr eine deutsche Haushälterin zu finden.

Jetzt ist sie wieder zuhause und sehr zufrieden. Die Krankheit hat sie hergenommen und sie ist nicht mehr so energisch wie früher. Aber sie ist vergnügt und hat keine Schmerzen.

Meine Frau und Kinder und ich selbst sind alle gesund und guter Laune.

Mit herzlichen Grüssen und allen guten Wünschen

Ihr O R Frisch

* bei J. Franck's Tochter Lisa Lisco

THE PHOTON SPLITTERS

(Variations on an old Danish theme)

A play in one scene to celebrate the
70th birthday of

Oskar Klein

A. Take a photon ...

B. How?

A. Well, take a weak light source and open a shutter long enough to let out one photon.

B. But how do you know? You may get two, or none!

C. Is it single photons of visible light you want?

A. Yes.

C. Then I can help you; I have a generator for single photons of the sodium resonance line. A beam of slow sodium atoms is crossed by a beam of yellow sodium light, which excites some of them. Those which emit a photon toward you are deflected onto a hot tungsten wire by their recoil, ionized and recorded by an electron multiplier. The output pulse tells us that a photon is on the way.

B. Won't it be gone before we observe the pulse?

C. There are lenses and mirrors to send the photon on a detour of 300 km, which gives you one millisecond notice.

A. Fine. So we take a photon ...

B. Can you make sure you have one?

C. There is no need; my generator is quite reliable.

B. Still, one ought to be able to make sure there is a photon.

A. One could record it with a photomultiplier ..

B. Not with any certainty!

C. True; the best photocathodes have only about 30°/o efficiency. But with some semiconductors one can get close on 100°/o. There is some noise, but with deep cooling...

B. Alright; let us say we have a perfect photon detector. So we can make a photon, know when it will come, and verify that it has come. But in verifying we kill it!

A. I know. Still, it seems I may at last say 'take a photon'. It behaves essentially like a particle: it starts from a point, it travels along a line, it ...

D. Surely not! Light consists of waves; you can at best create a wave packet! And after travelling 300 km ...

C. Let me give you the scale of my apparatus. My lens has a diameter of 1 meter; over 300 km the wave spreads only a few cms; the second lens is a little larger to allow for that, and it forms an image just as small as the original source, only a few wavelengths in size. The spread is ...

D. Essentially nil, I agree. But where does the photon pass through those large lenses of yours?

A. I don't care; somewhere. Just let me take my photon from the focus of C's second lens. We can consider this as our photon source, and we know-we have 1 millisecond warning-when the photon is coming.

B. Alright, we'll let you take a photon. What will you do with it?

A. I shall split it with a half-silvered mirror.

D. But that doesn't split the photon; it is either reflected or transmitted, the chances being half and half.

A. So the photon travels either in the direction 1 or 2?

D. Sure. If you were to place photon detectors in both beams, either one or the other would record the photon.

A. Good. Now please note that I have provided angle mirrors. (Fig.1) which cause both beams to return and to be recombined.

D. I see. You have built an interferometer similar to Michelson's. If your two distances are exactly alike then the re-united beam will go to the detector 3, not 4, it I've got the phase shifts right.

A. Correct. From that I conclude that the photon has been split, that it is present in both beams 1 and 2. There is no element of chance. We need both parts of the photon to be sure it will be recorded at 3 and not at 4.

D. But how do you account for the fact that of two photon detectors placed in 1 and 2, only one - at random - will record the photon?

A. It must be the detectors that introduce the randomness.

D. You mean that when one detector happens to report the photon, the other one is precluded from doing so?

A. Yes.

D. Even though the other half of the photon passes through it?

A. No. Surely the other half no longer exists; it must have been destroyed when the photon was spotted in the first beam. Only one photon was produced by C's source, and a photon can only be absorbed and detected once.

B. Isn't that the 'reduction of the wave packet'?

C. Yes, but what does it mean? How can the observation of a photon in one place destroy the other half of the photon?

D. There is no split photon; there is only a wave.

C. I must protest; my generator surely makes photons, one at a time. We agreed to that at the beginning.

F. Let me try to remember what I was taught. The wave associated with one photon is not real; it is merely a mathematical tool which allows us to compute the probability that a photon will be observed at a given place. It is a description of our knowledge that a single photon has entered our interferometer and passed through a half-silvered mirror. Once we know that the photon has been observed in one of the beams the probability that it should be found in the other becomes nil.

A. Just as the chance for a horse to win a race becomes nil when another horse has won it?

F. A bit like that.

B. But if it is only a matter of probability that the photon is observed, might it not be missed by both detectors?

F. Let us assume the detector in beam 1 is nearer the splitting mirror than the one in beam 2. Then if it records the photon it modifies the wave - which only represents our knowledge - so that 2 has nothing to detect. But if detector 1 remains silent at the critical time, then the wave in beam 2 gets strengthened so that the photon is sure to be recorded there.

B. Which detector affects the wave if they are the same distance from the mirror, to within the length of the wavetrain? And anyhow, if the wave represents our knowledge it can only become modified by something that we come to know. What if we don't look at the first detector but merely arrange for its signal to be recorded?

F. There will still be an even chance - just as if the first detector wasn't there - that the second detector will report the photon.

B. Yes; but it will not report the photon in those cases where a later inspection of the first detector shows that it had, unknown to us at the time, recorded the photon. Does the present behaviour of the second detector then depend on the future state of our knowledge about the first one?

F. No. We must interpret knowledge in a wider way. When one of the detectors records the photon, then the way it went is 'known' though you and I may not know it.

B. This is getting ever more implausible: the knowledge stored in one box of electronics is said to affect a wave elsewhere - without signalling! - and so the behaviour of another box. Why not admit that the photon, on meeting the half-silvered mirror, takes a snap decision, at random, whether to go through or be reflected?

A. Because then you can't account for the interference. If you were sure that half the photons travel along one path you could block up the other path and merely halve your intensity. But you know that if you block one path you destroy the interference: you then observe as many photons in detector 4 as in 3, whereas with both paths open all the photons arrive at 3.

D. Wouldn't the wave account for the interference? There is both the wave and the photon. The wave gets split while the photon is either transmitted or reflected.

A. But if we block one path with a detector and find the photon has gone that way, then you still have a wave travelling along the other path; a futile little wave without a photon! Unless you "reduce it", and then we are back to where we were.

B. Can't one spot the photon without absorbing it?

C. Certainly. For instance, a transparent block of mass M, thickness a and refractive index n will move forward by the amount $s = a(n - 1)(h/Mc\lambda)$ when a photon of wavelength λ passes through it. That displacement is small, but ...

A. I know. However, such a block causes a phase shift of $2\pi(n - 1)a/\lambda$. If this is to be less than 1, so that the interference is not destroyed, we must make $a(n - 1)$ less than $\lambda/2\pi$, and the displacement s becomes less than h/Mc, the "Compton wavelength" of the block. Heisenberg won't let you measure to that accuracy!

C. But must the phase shift be small? Couldn't we make it $2\pi N$ where N is a large integer? That would not affect the interference, and the displacement would be $s = nh/Mc$, which can be measured.

A. True. But we still cannot hope to know the velocity of the block to better than $\Delta p/M = h/sM = c/N$, and the associated Doppler effect gives again an uncertainty of 1 in the phase shift; so we are no better off.

D. Look, all this has been threshed out in Copenhagen, in the 'thirties. If you spot the photon you ruin the phase.

C. I think I see a way around that. Let me suspend the half-silvered mirror so that we can measure its momentum ..

D. That has all been disposed of. If you measure the momentum of the mirror to within h/λ, the momentum of a single photon, its position is uncertain by λ, and that ruins the interference.

C. No, wait, I have a new trick. I propose to suspend my mirror with a half-period equal to the time the photon takes to go out and back again along an arm of the interferometer. If it deviated by +d from its equilibrium when the photon first arrived it will deviate by -d when the photon returns. Thus d has no effect on the phase, and the interference will not be ruined: it will happen just as if the mirror had been fixed in its equilibrium position!

D. Ingenious. How will you measure the momentum transfer?

C. Well, I just measure the momentum before the photon enters and again after it has been recorded at 3. The mirror will have been pushed one way if the photon was reflected at first and transmitted on the way back, and the other way if it followed the other path.

D. But in the latter case the push comes half a period later than in the first; and the outcome is the same whether you push a pendulum now in one direction or half a period later in the opposite. So your measurement does not tell you which path the photon has taken!

C. Ingenious. I fear you are right. So I must do one of my momentum measurements while the photon is in the interferometer, and that spoils the interference.

B. Still, you have designed a means of observing where the photon is, without doing anything to it; does that not prove that the photon really is in one beam, and not split?

A. Einstein said something like that.

D. You have not really observed where the photon is, merely by suspending the mirror; you must measure its momentum.

C. But the momentum is in the mirror, and I can measure it or not, as I wish; surely that does nothing to the photon!

D. Yes, it does; it spoils the interference. All you have done is to share the information about the position of the photon between the photon and the mirror so that it can be extracted from either of them. Your particular suspension insures that the information is automatically lost again, the moment the photon, by interference between the two paths, gets directed to detector 3.

A. If the mirror had not been suspended in that way but just left floating, then the momentum transfer would have measured the photon's position?

C. Not necessarily. I think I could construct a mechanism by which you could make the mirror go to where the suspension would have taken it, if you so decide before the photon returns. All I need is ...

A. We'll believe you. But once I have reflected a low-energy photon from the mirror so as to determine its velocity by Doppler shift, then the measurement is done?

C. Not necessarily. I might construct a system of mirrors to send back the low-energy photon and return its momentum to the mirror.

A. But when is the measurement done?

C. I think any reversible process can be reversed. But I would regard myself as beaten if you have let the system interact irreversibly with say, a semiconductor, a photographic grain, or a retina.

B. That makes sense. After all, to observe is to create information; and information is a state - in a machine or an organism - which extends from a certain time into the future. Irreversibility is the very essence of information. (I must read up what Brillouin wrote about that.)

A. But don't we sometimes obtain information without irreversible interaction? For instance, when the detector in beam 1 reports nothing we know that the photon is in beam 2.

B. Yes; but the detector has to be there, in beam 1; the presence of an irreversible system is essential.

A. What if I place a piece of black paper into one beam? Then we destroy the interference without getting any information in return.

C. Not necessarily; you could measure the temperature of the paper before and after.

A. But what if I don't?

C. Oh well, information can get lost if you are careless.

A. But, information aside, what does the photon do in my interferometer; does it get split, or doesn't it?

F. You mustn't say "information aside". Quantum theory is about information. All it does is to tell you how to use available information to make the best possible predictions about future information.

A. You mean, about what is going to happen?

C. If you agree to use the word "to happen" only for irreversible processes.

B. Surely something happens to the photon inside the interferometer; so quantum theory must be incomplete.

F. I don't feel that. Quantum theory is logically consistent, and it allows you to make all the predictions that you can test. Photons and waves are models that allow you to use your imagination instead of using the full theory, but they cannot completely replace it.

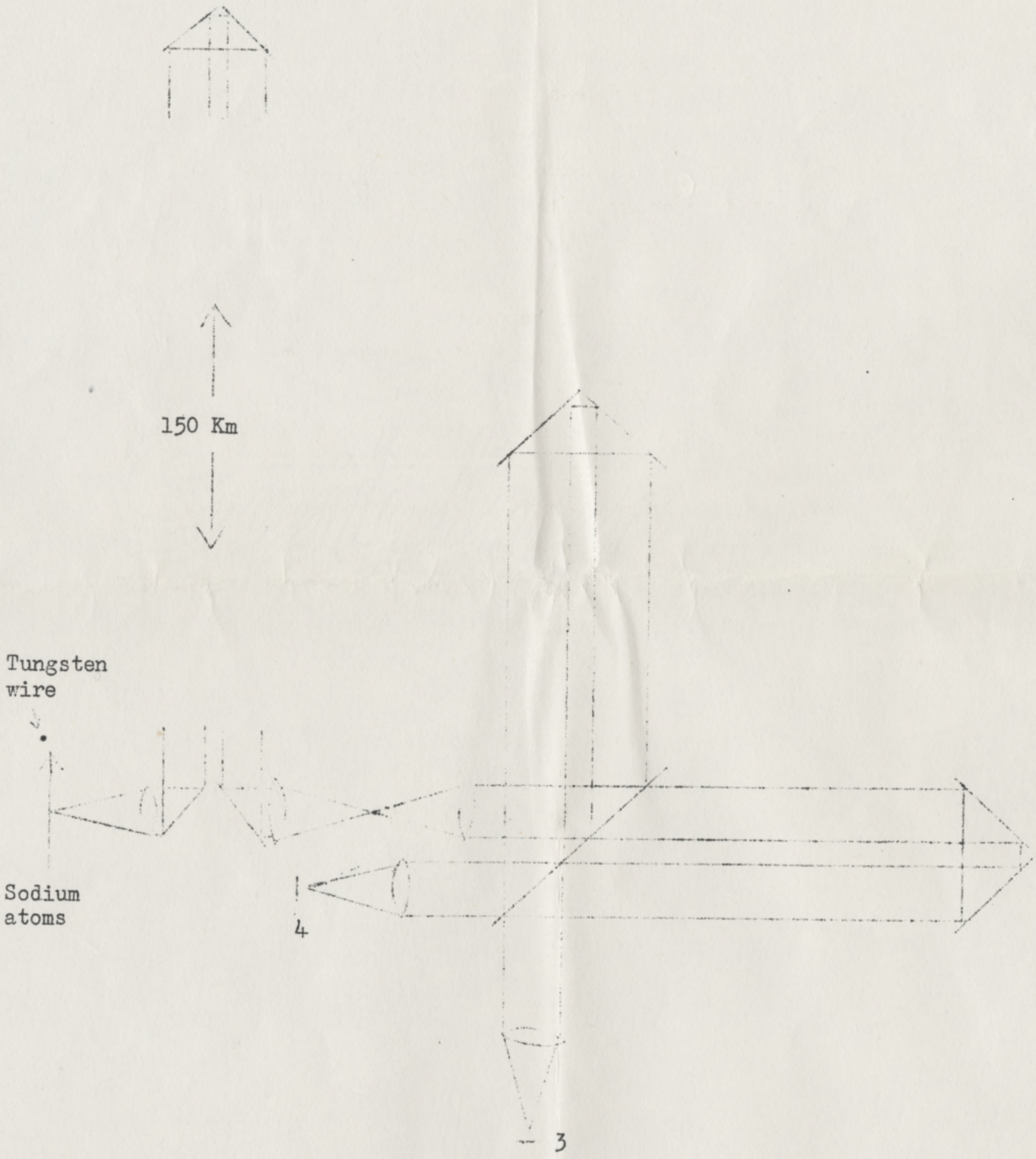
B. Couldn't one have a model that covers both photons and waves? Something more complex, perhaps multidimensional, of which our present concepts are merely flat projections?

F. Plato's cave. Well, produce such a model, and we'll discuss it next time.

A. But there are some even more awful difficulties which today we haven't even touched on. Take two photons ...

C U R T A I N

O.R. Frisch, June 1964.


150 Km
Tungsten
wire
Sodium
atoms
4
3

Experiment

The Magnetic Proton

By O. R. FRISCH, F.R.S.

Dr Frisch is Jacksonian Professor of Physics at the University of Cambridge, and the author of 'Meet the Atoms' and 'Atomic Physics Today'

IN 1930, when I was twenty-six, I went to Hamburg to work under Professor Otto Stern. My status was about that of a junior lecturer, but I gave no lectures; my German title was *Herr Assistent,* which made it clear that my job was to assist Stern in his researches. The work was planned by Stern; I had a knack for designing equipment, so he usually let me have my way in a good many details, although we carefully discussed it all beforehand. We had two good mechanics; under my supervision they built the various components, which I then tested and assembled with the help of a glassblower.

Perfecting the Apparatus

When all was ready, after six months or so, Stern would come into the room in high spirits, preceded by his cigar; he would sit down by the table with a large notebook, write down the date, and begin the first measurements to see if the apparatus performed as it should. Stern had always worked out exactly what to expect; so if there were any shortcomings, he could usually diagnose the reasons quickly. It might then take days or weeks to modify the apparatus, and in the meantime he worked with one of his other three assistants, or made plans for future experiments. After several months of repeated modifications the apparatus was finally considered good enough for the measurements it was meant for; Stern usually did most of the measurements himself, and all the calculations. Then the work was written up and published in the *Zeitschrift fuer Physik,* the apparatus was scrapped, the table scrubbed, and I began to assemble the next apparatus.

In one particular experiment we measured the magnetism of the proton (the hydrogen nucleus). Basically it was simple. You take a stream of hydrogen molecules, all flying through a vacuum in the same direction; then you place a strong magnet nearby and measure how much the molecules are deflected; that tells you how strongly magnetic they are. About ten years before, Stern had shown that this kind of experiment could be done: he had measured the magnetism of silver atoms, working with Walter Gerlach. He realized that this technique of studying streams of atoms all flying in the same direction—or 'atomic beams' as we call them briefly—could be used for a variety of problems; so when Stern was given a professorship at Hamburg University in 1925 he decided to concentrate on developing the atomic-beam method. The school he built up has now spread to all the corners of the world; although Stern is now in retirement, in California, to his many pupils he is still the well-beloved grandfather.

Why the Electron Spins

At first, it was thought that the magnetism of the silver atom was caused by its outermost electron going round in a circle; an electron going in a circle would be an electric current and would turn the atom into a tiny electromagnet. But by 1924 it had become clear that that view was wrong; the magnetism of the silver atom came from the fact that the electron spins very fast about its own axis, like a spinning top. This raised the problem of why the electron should keep spinning—why did it not run down?—but four years later, in 1928, Paul Dirac showed, by a purely mathematical argument, that the electron had to spin if it was to fit in both with the quantum theory and with the relativity theory. What is more, he was able to work out the strength of its magnetism, and the figure he got was exactly what Stern and Gerlach had observed.

In those days, everybody thought that matter was made of just two building bricks: the negatively charged electrons, which in some manner form the outside of the atoms, and the positively charged protons inside the atomic nuclei. Protons can also be found by themselves; the nucleus of the hydrogen atom—the lightest of all—is just a single proton. The proton was known to be almost 2,000 times heavier than the electron, but it had the same amount of electric charge and the same spin. So it was reasonable to assume that it would obey Dirac's theory, just as the electron does. In that case its magnetism would be smaller than that of the electron, about 2,000 times smaller because it was 2,000 times heavier; that followed from Dirac's theory.

To Otto Stern that was a challenge. The method he had used on silver atoms would have to be made 1,000 times more sensitive if it was to measure the magnetism of a proton, but he felt it could be done. No other way of doing it seemed feasible, and so he set about developing the required techniques. When I joined him in 1930 he was almost ready; but first I spent over a year helping him in another experiment, in which he showed the wave properties of atomic beams by placing a crystal in their way and studying the reflections. Stern had planned to measure the magnetism of the proton with another assistant, and he had actually set him to work on a similar experiment to gain experience; but that experiment ran into unexpected difficulties and took much longer than he had planned. So in the end Stern decided it was to be me who should help him with the magnetism of the proton.

Increasing Sensitivity

It is not, as a rule, possible to make a method a thousand times more sensitive in a single step; rather you have to try to improve all the factors that matter one by one, and each by as much as you can. First, there was the magnet. It had to create a field with a strong gradient, weak on one side of the beam, strong on the other. In their experiment with the silver atoms, Stern and Gerlach had used a specially-made electromagnet with poles close together—horseshoe fashion—and one pole shaped as a knife edge. They had placed it with that knife edge along the atomic beam; then some atoms were pulled towards the knife edge, others were pushed away, depending on how they happened to be oriented as they went by.

The first thing we did was to build a magnet with poles much closer together, to get an even stronger gradient; and I spent many days building a microscopic spring balance for measuring that magnetic gradient, in a space hardly wide enough for a toothpick. We also made the knife edge longer so that each molecule should spend more time in the deflecting field; but there we were limited because we did not want to lose too many of our molecules by collisions with air molecules in our imperfect vacuum (for vacuum pumps were not as good in those days as they are now). But in spite of all those improvements, the deflection we expected was still extremely small, only about one or two thousandths of an inch. So we had to use very fine beams of hydrogen molecules. They entered the apparatus through a fine slit at one end, and then they had to pass through two more narrow slits: these stopped all of them except those few that happened to be travelling in just the right direction. Finally, we had to improve the instruments with which we detected those few molecules at the other end of our apparatus; we did this by letting the molecules pass into an enclosure, and measuring the change in pressure, using delicate manometers which I designed specially for that job.

Hydrogen gas—which you can buy in cylinders—consists of molecules, and each molecule is a pair of hydrogen atoms stuck together. We could have broken them up into single atoms, but single atoms would have been useless for our purpose: they