The original of this portrait, by R. G. Matthews, occupies a position of honour over the fireplace of McGill University's Rutherford Museum; it is reproduced by kind permission of the Director, Professor F. R. Terroux.

(Facing p. 303)
The last Rutherford Lecture to be given in Australia was in 1968. It may well be another seven years before there is another, and it is quite likely that your next Rutherford lecturer will not have known him personally, for he died in 1937. That is my excuse for including a few reminiscences as part of my tribute.

Many of you will have heard or read about Rutherford from the Governor of South Australia, Sir Mark Oliphant, F.R.S., whose knowledge was that of a close friend and a direct scientific colleague, as an assistant director of research in the Cavendish Laboratory during the 1930s. My much smaller stock of stories comes largely because I was under Oliphant’s immediate charge from 1928 until 1931 when he was a senior research student in the Cavendish Laboratory.

I well remember being called into Rutherford’s room and being introduced. After telling Oliphant that I had been through the Cambridge Part II Tripos class and ‘should be all right’, Rutherford turned to me and said ‘Oliphant will tell you what to do. He’s a very fast worker’. That was all. The remark was highly characteristic, not only because hard work and fast work were to Rutherford among the highest virtues, but because he never hesitated to say something good about a man in his own presence, always in a crisp way that gave no hint of praise for the sake of praise. That Oliphant was a fast worker was just something I ought to know, and to heed, and though technically Rutherford was my supervisor, I was actually Oliphant’s pupil. ‘How’s your satellite?’ was the usual query when Rutherford looked in, usually on a Saturday morning, to see how things were going.

He was himself a ‘very fast worker’, and his top year for sheer output of scientific results was from McGill in 1903 when, mostly in collaboration with Frederick Soddy, he published no less than seven articles in the Philosophical Magazine within six months. The least of these would have represented a good year’s work for most physicists. In later years, when the main papers from the Cavendish Laboratory were sent to the Proceedings of the Royal Society, Rutherford said ‘If one of my boys publishes one good Royal Society paper a year, he’s doing well’.
Among these seven papers in 1903, two or three are great classics, for it was in the first years of this century that Rutherford, with the expert chemist Soddy, transformed radioactivity from a mystery into a vital new science. The complex sequence of $\alpha$- and $\beta$-radiations, starting with the radioactive gas thoron, had been disentangled sufficiently to show that each of them was emitted from a different kind of atom, and that the activity of each decayed exponentially with time. The conclusion had been drawn that atoms were spontaneously changing; yet the chemists, whose belief in atomic immutability was thus destroyed, were assured that each of these new types of atom had perfectly respectable chemical properties, even though standard chemical techniques such as precipitation could not be applied in the ordinary way because of the extremely small amounts of material involved, and the balance and the burette had to be replaced by the electrometer. Chemistry was enlarged, not undermined, by their discovery of the transmutation of matter; it henceforth had a new branch, which we now call radiochemistry.

These same papers included the first measurement of the charge-to-mass ratio of $\alpha$-rays (which made it highly probable that they were doubly charged helium atoms) and of their velocities (which showed that they carried very great kinetic energy).

There is more in a classic, whether literary or scientific, than the main story and the memorable incidents, and there is more in these papers than the magnificent discoveries they contain. They show Rutherford as a scientific writer of great character and power. Here are some of the striking sentences in which he and Soddy put their main points:

'All cases of radioactive change that have been studied can be resolved into the production of one substance by another... when several changes occur together they are not simultaneous but successive. Thus thorium produces thorium X, the thorium X produces the thorium emanation, and the latter produces the excited activity.'

'Both the radioactivity and the emanating power of thorium X decay according to the same law and at the same rate... Hence it is not possible to regard radioactivity as the consequence of changes that have already taken place. The rays emitted must be an accompaniment of the change of the radiating system into the next one produced.'

'There is every reason to suppose not merely that the expulsion of a charged particle accompanies the change, but that this expulsion actually is the change.'

'There are thus strong reasons for the belief that the $\alpha$-rays generally are projections, and that the mass of the particle is of the same order as that of the hydrogen atom and very large compared with... the $\beta$ or easily deviable ray from the same element. With regard to the part played in radioactivity by the two types of radiation, there can be no doubt that the $\alpha$-rays are by far the more important.'

In the extraction of simple and fundamental facts from an extremely complex experimental situation, Rutherford and Soddy's work has few parallels in the history.
of physical science. The nearest, I think, is Faraday's ionic interpretation of the varied phenomena observed when electric currents are passed through conducting solutions.

Rutherford and Soddy, like Faraday, worked at the bench more than in the study; they found many of the problems as well as the answers, and the true nature of radioactivity – the spontaneous transformation of individual atoms of matter – was laid open by them only seven years after Becquerel's first discovery and only five years after Rutherford had turned from experiments in signalling by radio (in which he once held the long-distance record) to experiments on ions in gases.

Rutherford's most famous discovery, the atomic nucleus, links him to Newton. It was his study of the orbital theory in Newton's Principia that enabled him to interpret Geiger and Marsden's large-angle scattering of α-rays. Rutherford was, of course, a competent mathematician even though he professed a disdain for theoretical physicists.

The discovery of the nucleus, like that of the first nuclear reaction, the emission of protons from nitrogen bombarded with α-particles, show how prophetic for Rutherford's future work were the words quoted earlier 'There can be no doubt that the α-rays are the more important'.

Throughout his life, the alphas were Rutherford's special favourites. He discovered them, named them, and made nearly all his discoveries with them. And when, for the first time in his life, he was persuaded to pay a big bet on a purely theoretical prediction, it was α-rays that gave the triumphant result. I refer, of course, to his support of Cockcroft and Walton in building their proton accelerator, after Gamow had told them that protons should theoretically be able to interact with nuclei at unexpectedly low bombarding energies. Cockcroft and Walton, not unreasonably, looked first for gamma-radiation, but Rutherford pressed them to use a scintillation screen and there, at once, were the alphas from the disintegration of lithium.

From that day on, accelerators became increasingly dominant in the study of nuclear structure and, in due course, of sub-nuclear particles.

Now, having paid my tribute to the great scientist in whose memory this lecture is given, I shall turn to a small branch of physics that I have found interesting: the use of swift rotation. It has, I am afraid, nothing to do with Rutherford, though at one or two points it connects with people who worked under his wing at Cambridge, and there is, perhaps, a connection by way of analogy. Accelerators of atoms and molecules are now becoming as important in chemistry as accelerators of electrons and nuclei are in physics, and the first 'chemical accelerator' was a rotary one.

It may be wondered why at this point, as indeed in the title of the lecture, I use the old-fashioned word 'swift'. Why not fast, or rapid, or high-speed? It was one of Rutherford's favourite words: men worked fast, but α-rays were swift. The word carries a splendid sense of flight, and it has another meaning appropriate to the title of the lecture – a rotating frame for yarn-spinning.

The most familiar use of swift mechanical motion in physics is to chop a beam...
of particles into short pulses, so that their time of flight to a detector can be measured, thus making a velocity-spectrometer. If the detector is behind a second shutter, phased with the first shutter so as to transmit only particles that have a particular time of transit, then of course we have the equivalent of a monochromator.

The rotating-shutter principle goes back nearly 200 years, to Fizeau and his measurement of the speed of light. It began to be used in atomic and molecular physics in the 1920s, when the thermal velocity distribution of gas molecules was experimentally demonstrated. In the 1930s it was applied in neutron physics, first by Dunning and Pegram with a pair of disks rotating on the same shaft. Each disk was covered with cadmium, apart from one or more ‘open’ sectors and the method thus depended on the strong absorption of cadmium for neutrons in and just above the thermal region of energy. At this point it is useful to remember what speeds of neutrons are involved; the mean thermal speed of a neutron at room temperature is about 2 km s\(^{-1}\) and the peak absorption of cadmium is at \(\approx 6 \text{ km s}^{-1}\). At higher neutron velocities, the absorption falls and the disk needs to be thick. A suitable chopper is then a rotating stack of cadmium and aluminium plates.

Choppers of ‘sandwich’ geometry were introduced by Fermi, and it is these that have been developed into the splendid examples of mechanical construction that have since been used in many neutron laboratories. The plates of the sandwich are curved to fit the paths of the neutrons as seen in the rotating frame of reference, and the speed of rotation has been pushed to the limit set by the strengths of materials, approaching 1 km s\(^{-1}\).

Velocity-selectors for molecules are much easier to make, because very thin sheets are perfectly opaque to them. Rotating disks, with many narrow slits, are standard for this purpose; even for the lightest and therefore fastest molecules, the linear speed does not need to be particularly high until one wants to select velocities at the top end of the Maxwell distribution. However, this is the most interesting end of the velocity-spectrum. Remember that the average kinetic energy of a molecule at room temperature is around \(1/30 \text{ eV}\), but that the activation energies of chemical reactions are typically a few electron-volts, corresponding to speeds of several kilometres per second even for a molecular mass of, say, 100. So for studying molecular collisions in the chemically interesting region, the velocity-selectors have to be well engineered, though on a much smaller scale of size than for neutrons. Obviously the disks should be made of strong but light material and should be thinner at the edge than towards the centre; the ideal profile, which makes the stress the same throughout the material, is that of a Gauss error curve.

With beam-choppers as for other rotary devices, high speeds call for more than good design and strong material for the actual rotor. It has to be driven and it must run about some sort of axis. Its speed must be controlled; indeed as we have seen, it is often necessary to control and to vary, quite precisely, the relative phases of two or more rotors.

Problems of drive, of bearings and of stability have, of course, been met and conquered by engineers over many decades. The obvious example is the steam
turbine, where they have been overcome in the presence of torques and thrusts, azimuthal and axial, of high temperatures and of corrosion. The most remarkable advance in this technology was the discovery about eighty years ago by de Laval that by making the shaft of a turbine flexible, so that the main resonance of the assembly about its bearings occurs at a comparatively low frequency, one can pass through the resonance into a stable régime. To permit this passage, de Laval turbines are provided with special rings which take up enough vibrational energy to limit the amplitude at resonance to a tolerable amount.

A great contribution to the understanding of such systems was made by a friend and protégé of Rutherford’s, Academician Peter Kapitza, F.R.S., and some loose ends can be tied up by giving an outline of it. Kapitza, it will be remembered, was prevented from returning from a holiday in the U.S.S.R. to the Royal Society Mond Laboratory at Cambridge which had been created around him – one might almost say for him – at Rutherford’s instigation, and where he was applying his remarkable gifts as a physicist and as an engineer to the study of low-temperature phenomena. Rutherford generously recommended, and the Society equally generously agreed, that some of the laboratory’s equipment should be allowed to go to Moscow, and Kapitza started to rebuild his scientific career on Russian soil. He published in 1939, conveniently enough in English though in a journal of the U.S.S.R. Academy, a pair of outstanding papers. In the first, he commented upon the thermodynamic inefficiency of the high-pressure piston machines previously used for liquefying gases and described an expansion turbine, working at relatively low pressure, which was mechanically simpler and also more efficient. In the second paper he explained in detail how he had met, understood and overcome the instabilities of fast rotation, in the particular circumstances of a rotor that had to turn in a gas with only small clearance in its housing. He showed, among other things, that as soon as the rotor becomes slightly excentric, the difference of gap on its two sides will produce viscous forces that inevitably tend to increase, not decrease, the amplitude of vibration.

Physicists will readily see how they arise from figure 1a, b and will understand how the equations of motion of the system become

\[
\begin{align*}
M\ddot{x} + H\dot{x} + Kx - Ny &= 0, \\
M\ddot{y} + H\dot{y} + Ky + Nx &= 0
\end{align*}
\]

where \(M\) is the mass, \(K\) the elastic force-constant, \(H\) the constant representing the viscous drag for to-and-fro motion, while \(N\) depends both on the viscosity of the medium between the rotating cylinder and its housing and on the angular velocity of rotation, \(\omega\). The variables \(x\) and \(y\) are the horizontal and vertical displacements of the rotor from its central position and it will be noticed that, for the anticlockwise rotation I happen to have chosen, the final term has a negative sign in the upper equation but a positive sign in the lower one.

The solution of these equations is difficult. Kapitza deals with it in two stages, first by omitting the damping terms \(H\dot{x}\) and \(H\dot{y}\) and showing that the antisymmetry
of sign of the terms involving $N$ makes the motion inherently unstable; the rotor will spiral out until it hits the casing.

To determine what value of $H$, the 'ordinary' damping coefficient, will counterbalance the antidamping effect of the 'peripheral friction', he equates the energy lost in one cycle by ordinary fluid damping (i.e. through $H$) to that gained from the peripheral antidamping (i.e. through $N$, which is itself proportional to the angular velocity of rotation). The required ratio of $H$ to $N$ naturally involves $K$ and $M$ but, as it happens, only through their ratio. In this way, Kapitza obtains the extremely simple condition $H \geq N/p$, where $p = \sqrt{(K/M)}$, and is of course $2\pi$ times the frequency of free vibration of the system. The analysis shows that the lower the natural frequency of vibration of the rotor about its shaft, the larger must $H$ be in relation to $N$ if instability due to the azimuthal forces $-Nx$ and $Ny$ is to be prevented. He concludes that 'other conditions being equal, in turbines with a flexible de Laval shaft, where $p$ is low, instability of this kind appears more readily'. One must not, however, overlook the fact that $N$ increases with $\omega$, the angular velocity of the rotor; in Kapitza's rotary liquefier, he gives reason to believe that it is proportional to $\omega^2$, so one might expect trouble at high speeds, even though they are far above the critical frequency of the de Laval system.

![Figure 1. Forces (broken lines) arising from off-axis displacements.](http://rspa.royalsocietypublishing.org/)

The question therefore arises why do de Laval turbines run smoothly above their critical frequency, in spite of the terms involving $N$? Kapitza's belief is that damping in the bearings saves the situation; an oil-lubricated journal bearing will have an $H$-term as well as an $N$-term. For this explanation to hold, $H$ must also increase rapidly with $\omega$. Kapitza mentions that ball and roller bearings, in spite of their economy, are seldom used by turbine engineers.

I have started with Kapitza's study of the effects of rotation with small clearances within a viscous fluid, partly because these were apparently what first aroused his interest; partly because they involve an important point of physics, illustrated by the cross terms in the equations and comparatively simple to explain; partly because of an analogous antidamping effect with magnetically suspended rotors which I shall mention later.

However, the bulk of Kapitza's paper is concerned with the effect of lack of exact balance of the rotor on its shaft and with the complications introduced by the fact...
that the shaft, however stiff or flexible, will have its own resonant modes of natural vibration, additional to the main mode where the rotor vibrates as a single mass subject to the elastic restoring force due to the bending of the shaft. I shall say nothing about the vibrations of the shaft itself, beyond the obvious comments that if their frequencies can be made very low, they are not likely to cause much damage, and if they are all above the highest frequency of rotation that is to be used, they will not be excited and will cause no trouble at all.

What I do want to discuss is the behaviour of the main vibration when the shaft is flexible. The principles are the same whether the shaft has bearings at both ends or at one end only. Rankine, as long ago as 1869, realized that an unbalanced rotor would vibrate with increasing and ultimately disastrous amplitude as the rotational frequency came up to the natural vibration frequency. De Laval in the early 1890s found experimentally that, if this resonance could be survived, the rotation became stable again at higher frequencies. Pöppl in the late nineties gave the basic explanation of this stability, which is the change of phase of response of the driven system to the driving force as the frequency of the latter passes through and above the natural frequency of the former.

Far above resonance, the deflexion of the shaft becomes equal in magnitude, but opposite in sign, to the displacement of the centre of mass of the rotor from the axis of the shaft, and what Kapitza did was to develop the theory quantitatively in a simpler form than had been used by various mathematical engineers between Pöppl’s time and his own.

He shows that, for passing safely through the resonance, the equation of motion must include a damping term at least half of what is needed to give critical damping (aperiodic vibration) of the system when it is not under rotation, even if the rotor is perfectly balanced. He also calculates how much extra damping is needed to deal with a given amount of imbalance of the rotor about its shaft.

These calculations of Kapitza’s, which he extended to cover some of the problems of shaft resonances and of long shafts with multiple bearings, as well as the effects of external shocks, led him to the logical and quantitative design of a low-pressure rotary liquefier, dynamically extremely stable and thermodynamically very efficient. The dynamical problems are not easy, and I think any physicist who reads Kapitza’s unifying account must admire the early turbine engineers who overcame them partly by experiment and partly by calculations of their particular examples.

Another very satisfying explanation of engineering practice is contained in Kapitza’s paper. He shows how a loose disk embracing the flexible de Laval shaft, which will fling into contact with the casing when resonance is approached, will first slide and then roll against the casing. At the instant when sliding changes to rolling, the angular velocity of whipping of the shaft must reverse, so the shaft must then straighten.

Kapitza’s paper is firmly based on physics, and its mathematics is plain and convincing, even though there are 92 equations in the whole paper!

At this point in the discussion we see the advantage, which the physicist can
exploit more easily than the engineer, of a rotor that has no mechanical bearings but is levitated freely so that it will automatically rotate about an axis passing through its centre of mass, and such as to make the moment of inertia a maximum or a minimum. At about the same time as Kapitza's work, but quite independently, levitated rotors were beautifully developed by Jesse Beams and his colleagues at the University of Virginia, the levitation being magnetic, with a sensor of vertical position that may be either a beam of light or a coil whose inductance is altered by the proximity of the steel rotor; in either case, a servo system, including suitable damping derived from $\frac{dh}{dt}$ where $h$ is the height, enables the rotor to ride freely. It can be driven by an external rotating magnetic field.

Now from my simple statement that a freely suspended rotor will rotate about its centre of mass, it might be thought that there are no problems of instability in the horizontal plane. Experiment soon shows the contrary. When the rotor is set spinning, it develops an orbital motion about the axis of symmetry of the lifting field, the orbital frequency being about the same as the frequency of horizontal vibration determined by the mass of the rotor and the horizontal force called into play by any displacement from that axis.

Unless damping is provided, the radius of the orbit will increase until all horizontal control is lost. The behaviour is very similar to that which Kapitza explained by an off-axis interaction between his rotor and the gas that separates it from the housing, though the physical cause is quite different, lying presumably in an off-axis electromagnetic interaction between the rotating steel and the magnetic field.

The Virginia group, at one stage, literally manipulated their rotors into obedience by moving the lifting magnet by hand; later they introduced damping by a so-called 'corn-stalk', a permanent magnet on a flexible wire in a dashpot beneath the rotor.

In 1947 I wanted a simple way of reaching high peripheral speeds, and thought of combining magnetic lift with the action of an ordinary spinning top; the magnet took about 90% of the weight and a tiny ball of hardened steel protruding from the base of the rotor put the remainder on a horizontal glass plate.

Besides avoiding the need for a servo system, I had hoped that the friction of the ball sliding on the glass would suppress the orbital motion, but it did not. My colleagues and I decided to allow the lifting magnet itself to swing, either directly in a dashpot or attached to a light framework which dipped into oil. Both worked pretty well, though in recent years we have gone over to the Virginia corn-stalk system of damping.

One can have a great deal of fun, and indeed do some very interesting experiments, with levitated rotors or with the peg-top type, and they have the great advantage that, apart from possible internal resonances of the rotor, there is only one mode of vibration to be damped out. But they have severe limitations: rotation can be only about a vertical axis; drive can be applied only weakly; and though they can all too easily be heated, they cannot be effectively cooled. So for most purposes, flexible-shaft systems are used in high-speed work.

Now I should like to discuss a few other uses of swift rotation in physics and
chemistry. Beam-choppers, already mentioned, have contributed the most to Rutherford's own subject, nuclear physics, but I think he would have been interested in an application that I had the good fortune to make about twenty years ago, not least because its origin lay in a topic that was discussed at a conference on β- and γ-rays that he chaired in the Cavendish Laboratory in 1928.

Rutherford was bemoaning the lack of a source of monochromatic γ-rays and said 'If anybody can tell me of one, I'll erect a gold statue to him in this laboratory'. Chadwick, in his usual quiet way, said 'What about ThC''?', but Rutherford would have none of it. 'Why, there's at least 10% of soft stuff in that!' Then Chadwick said, just a little gleefully, and to the huge delight of us all (including Rutherford) 'Isn't it some time, Professor, since you did an experiment to 10%?'

Now this 2.76 MeV γ-ray from ThC'' was the subject of a paper by a German physicist, W. Kuhn who was at that 1928 conference. It had recently become clear that γ-rays are emitted after an α- or β-transformation that has left the daughter nucleus in an excited state, and in this instance the daughter is 208Pb, a stable isotope of ordinary lead.

Kuhn had looked for the resonant absorption of this γ-ray in lead — the nuclear analogue of the resonant absorption of, say, the sodium D line in sodium vapour — realizing that, according to the then new wave-mechanics, the effective cross section should be of the order of the square of the wavelength, enormously greater than the geometrical cross section of the nucleus.

He had not found it, and he explained why. When the excited nucleus emits the γ-ray, it must recoil with momentum \( \frac{h \nu}{c} \) and therefore with kinetic energy \( (h \nu)^2/2Mc^2 \) where \( M \) is the nuclear mass and the other symbols have their usual meanings; the energy of the γ-ray is reduced by that amount. Similarly, if the γ-ray is to excite a ground-state nucleus to resonance, it must provide \( (h \nu)^2/2Mc^2 \) of kinetic energy, over and above the actual energy of excitation. Thus exact resonance does not occur, the width of the excited level being, in this instance, much less than \( (h \nu)^2/Mc^2 \). In the optical example I mentioned, the recoil losses are much less than the line width.

Over the years, other attempts were made to observe γ-ray resonance, but they did not succeed until 1950, when γ-ray sources were placed on the tips of rotors so that the forward-emitted radiation was given enough additional energy, by the Doppler effect, to replace the recoil losses. The experiment was done by observing resonant scattering rather than resonant absorption, and the nucleus was 198Hg, the source being radioactive 198Au and the scatterer being liquid mercury.

It had at first to be done to much better than the 10% of Chadwick's joke with Rutherford, because the γ-rays had to be counted with Geiger–Muller counters, which have virtually no selectivity against the large background of Compton scattering. Figure 2 shows a later measurement with a sodium iodide scintillation detector. The curve is not the profile of the nuclear resonance itself, but the very much broader one representing thermal motions of emitting and scattering nuclei.
Such measurements, made quantitatively, permit the lifetime of the excited state to be deduced, and have been most useful for lifetimes of the order of $10^{-11}$ to $10^{-12}$ s which are (or were) just too short to be measured by delayed-coincidence techniques.

![Figure 2. Variation of scattered intensity with velocity of $\gamma$-ray source.](http://rspa.royalsocietypublishing.org)

It was for measurements of this kind, with transitions that required higher speeds of motion, that the swiftest metal rotor was made at the Bartol Research Foundation in Pennsylvania. It was of titanium alloy, shaped like a four-pronged star, with each limb tapered approximately according to the Gaussian profile recipe which holds for whirling rods as for disks, and it was hung from a flexible wire and driven by a compressed-air turbine, according to one of the many successful patterns developed by Beams. A regular running speed of 1.3 km s$^{-1}$ was reported, and such a rotor would probably have reached $1\frac{1}{2}$ km s$^{-1}$ before breaking. With it, Dr Metzger and his colleagues were able to extend measurements of resonant scattering to nuclear levels more difficult than $^{198}$Hg.

The greatest importance of this type of experiment lay, however, in the revival of interest in the $\gamma$-ray resonant process, for it was while studying the effect of temperature upon the overlap of the resonance curves of source and scatterer (first observed by Malmfors in Sweden) that Mössbauer discovered the recoilless process which has given such spectacular results in many fields of physics. Although the basic technique of Mössbauer-effect measurements is to have an extremely small relative velocity between source and scatterer, there have been applications that involve rapid rotation and are concerned with tests of relativity.

The 14 keV $\gamma$-ray of $^{57}$Fe emitted by the radioactive $^{57}$Co is the most advantageous for these, as indeed for most Mössbauer-type experiments. This line, both in emission and in absorption, has a fractional width of order $10^{-12}$ and the energy is remarkably unaffected by external conditions such as pressure and temperature, though I should mention in passing that its temperature-coefficient can be regarded
as due to the dilatation of time resulting from change of thermal velocities and agrees with relativity theory to within a small percentage.

Hay, Granshaw, Schiffer and Egelstaff placed a source and an absorber on the same rotating disk, at different distances $r_s$ and $r_a$ from the axis of rotation. Time dilatation predicts that the relative frequency-shift will be given by

$$\frac{(v_s - v_a)}{v} = -\frac{\omega^2}{2c^2} \left( r_s^2 - r_a^2 \right)$$

and the change in transmission, as observed by a fixed detector, was found to agree roughly with this prediction. Later experiments in various laboratories verified that there was no shift if source and absorber were placed diametrically opposite at equidistant distances from the centre, and improved the agreement with theory to within about 1%.

This type of measurement can be extended to what, in old-fashioned language, may be called a search for an 'aether drift'. If the source and absorber are collinear with the centre of the disk, and the system is moving through a fixed aether with velocity $V$ at an angle $\theta$ relative to the source-absorber line, the classically calculated time of transit of $\gamma$-rays from source to absorber is $\tau = (r_s - r_a)/(c - V \cos \theta)$ and as the disk rotates with angular velocity $\omega = d\theta/dt$ we calculate a variation of frequency with $\theta$ given by

$$\frac{\Delta v}{v} = -\frac{\partial \tau}{\partial t} = \frac{\omega V(r_s - r_a) \sin \theta}{c^2}.$$ 

Since $\theta$ is the instantaneous angle between the source-absorber line and the direction of $V$, the frequency will vary sinusoidally during each cycle of the disk's rotation. If $\gamma$-rays are observed at a particular azimuth of the rotation, their frequency will go through a 24 hour cycle.

In practice, it is best to put the source at the edge of a rotating disk and to observe the $\gamma$-rays by a counter near its centre, that is surrounded by a thin foil of iron enriched in the isotope $^{57}$Fe. Electronic gating circuits then permit observations at several azimuths during the same experiment, and neither the absorber nor the counter need rotate!

This arrangement was used by Isaak, Preikschat and Broadhurst in what is by far the most sensitive test of aether drift or, as one would now prefer to say, of the isotropy of the velocity of light. Since the effect to be sought is additional to that of time dilatation, and since Mössbauer sources can be adjusted in frequency by altering the matrix in which the $^{57}$Co nuclei are embedded, it is possible to arrange that, at zero velocity, the source is centred on the steepest part of the absorber resonance curve while at the highest rotor speed it is at the steepest part on the other side of the peak. This refinement, enhancing the extreme sensitivity of the Mössbauer method, and added to the fact that the effect to be sought depends on the first power of $V$ multiplied by the very high linear speed of motion of the source, enabled them to place a conservative upper limit of 5 cm s$^{-1}$ on any 'aether wind'.
It is at least 100 times smaller than limits set by other methods, from the Michelson-Morley experiments (several kilometres per second) onwards. It may be compared with the Earth’s rotational velocity (500 m s\(^{-1}\)), the Earth’s orbital velocity (30 km s\(^{-1}\)) and the velocity of the Sun relative to the microwave background radiation (200 km s\(^{-1}\)).

The last field of application of swift rotation to be discussed is one that provided a practical demonstration of possibilities that, like those of \(\gamma\)-resonances, have recently blossomed into a major field of research.

It is not difficult to make velocity-selectors that will chop out, from the Maxwellian velocities of gas molecules, fairly monoenergetic beams of molecules having speeds in the upper end of the range. Given good methods of detection, one can think of studying in detail the collisions of such beams with other molecules, including collisions that result in chemical change. When one comes to put numbers into a planned experiment, it does not look so easy; partly because the efficiency of detection of neutral atoms and molecules is usually low, partly because there are not many molecules at the upper end of the spectrum, so the primary beams are weak. There is one class of atoms – those of very low ionization potential, such as the alkali metals – where detection, by way of ionization at a hot surface of high electron work-function, is very efficient and virtually instantaneous. These alkali atoms are highly reactive and have very low activation energies for reaction with various molecules. So if an intense Cs or CCl\(_4\) beam could be produced at a speed substantially above thermal, such a reaction as \(\text{Cs} + \text{CCl}_4 \rightarrow \text{CsCl} + [\text{CCl}_3]\) should be easy to observe. By using a rotor as an impeller of molecules, instead of as a selector, this can indeed be done with the simplest of apparatus.

A rotor in the form of a rod, spinning about an axis perpendicular to its length and surrounded by gas at low pressure, will collect molecules and throw them off again. A simple collimator, looking tangentially to the circle described by the tips of the rod, will transmit a pulse of molecules each time a tip comes round; if the speed of the tip is well above the thermal velocities to which it is added, the pulses can be both reasonably well spaced and roughly monoenergetic.

By crossing such a swift pulsed beam of CCl\(_4\) molecules with a continuous beam of Cs atoms from an oven, and detecting CsCl by surface-ionization at a comparatively large distance from the collision volume, the time-of-arrival distribution and hence the velocity-distribution of this reaction-product was obtained. That is sufficient to determine completely the particle-dynamics and energetics of the reaction, since conservation of momentum connects the velocity and direction of each CCl\(_4\) with those of the corresponding CsCl.

These principles of analysis have of course long been standard in nuclear physics and, though detection of single particles was neither possible nor necessary, the familiar experimental features of accelerator, target, flight-path and detector were all there.

Though the results were by no means precise, they showed rather little energy from the strongly exothermic reaction to be appearing in the kinetic form, so most
of it must be taken up by vibrations and rotations of the product molecules. It is pleasant to recall that a Royal Society Government Grant of £350 in 1947 provided the basic equipment, which was also used for the γ-resonance experiment mentioned earlier.

Rotor-impelled beams remain to be exploited further, but in recent years there have been great advances in molecular-beam chemistry, with various sources of swift molecules and with improvements in ways of detecting reaction-products and measuring their velocities. Perhaps the most elegant molecular accelerators, which reach energies well into the chemically interesting region, are those that work by pumping a light gas through a nozzle to obtain a supersonic beam which can be seeded with heavy molecules that are carried along with it. Owing to their smaller rate of lateral diffusion, these are left in the core of the beam as it spreads out. There are no moving parts, the temperature of the beam may be varied downwards as well as upwards, and the intensity is so high that very good angular resolution can be afforded and choppers can be interposed to give sharp pulses for time-of-flight studies. The advent of tunable lasers has enabled the products of chemical reaction to be detected by resonance fluorescence, which is not only comparatively efficient but is selective; it tells whether the product molecule is in the ground state or in an electronically excited state. So now chemists are studying molecular reactions with something like the sophistication of the nuclear structure physicist.

Finally, an answer to a question that may be in your minds: how fast is it possible to make a rotor go? It depends, as I have indicated, on the strength-to-density ratio of its material and on the shape. I mentioned the Gaussian profile as the theoretical ideal; if you continue to lengthen it, the speed of the tip continues to increase, but the cross section decreases very much more rapidly, and for many purposes a conical taper suffices. We in Birmingham have run a rotor, with a conically tapered shaft of carbon-fibre composite (c.f.c.), to a tip speed of 2 km s\(^{-1}\). In relation to its density, c.f.c. is not quite the strongest material that is available, and I imagine that 2½ or 3 km s\(^{-1}\) could be reached, purely as a demonstration, with an outrageously thin rotor based on, say, boron fibres.

This is not the greatest speed attainable with mechanical objects in the laboratory; electromagnetic acceleration of metal slugs can reach around 10 km s\(^{-1}\), but there are not many experiments to be done with an object that flies off the bench in less than 1 ms.