Papers of M H F Wilkins: papers relating to Cambridge University in the 1930s

Contributors

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ONCE A MONTH

Science and Discovery: Edited by CHRISTOPHER COLUMBUS

THE TRUTH ABOUT MARS

Is there life on the Planets? In this article John J. Johns explains the secrets of the Stars.

MARS is a fascinating planet. The surface can be seen clearly; part is red like a desert, the rest is blue green, and there are white polar caps at the North and South poles. As the seasons change the polar caps disappear by melting and round the cap, which has melted, the green areas become darker. Many astronomers claim to see dark straight lines crossing the red areas. These lines were called canals, and there was much speculation and excitement about them for they looked artificial and made by intelligent beings.

The canals were said to darken near a melting polar cap. An astronomer, Lowell, made an elaborate theory about canals. He said intelligent people lived on Mars. The green areas were vegetation, but Mars was steadily drying up, water was short, and the red desert was eating up the vegetation. To preserve a water and food supply the Martians joined the ice polar caps to the green area by canals. When the ice melted water flowed along the canals and brought water to the green areas, and vegetation grew at the side of the canals and made them visible where they crossed the desert. Thus Martian vegetation grew and Martians fed. Lowell wrote a book about his theory. It was very clever. And someone suggested that a great bonfire be lit in the Sahara desert as a sign of goodwill and sympathy to Martians short of water.

But astronomers now say that the speculation about the Martians was nonsense. First the line markings or canals are very difficult to see. They are probably not straight and thin but wide and irregular, and do not look at all artificial. The important thing however remains, that Mars cannot be photographed well so those who still insist that they see straight canals cannot be proved wrong. The whole theory however raises the question: can human beings, as we know them, live on planets such as Mars? What is the truth about the "Men from the other Worlds"?

* * *

ONE quarter of our earth is dry land the rest is sea. People live on land where it is hot or cold and dry or wet. They can adapt themselves to the different climates; but there must be oxygen in the air to breath and water to drink.

What are the chances of life on Mars? It is quite warm. It has an atmosphere in which there is oxygen and water vapour; and sometimes a cloud or dust-storm can be seen. The polar caps are probably ice, that melts. The green areas and faint line markings do change with the seasons. It is quite possible that the green areas are vegetation and become darker green when supplied with moisture.

That is all we know. In the minds of many astronomers it is possible for life to exist but it is as hard for us to check up on them as it would be for the Martians (if they exist) to check up on us. They would not be able to see that there were men on the earth!

To their naked eyes the earth would appear just a bright star. Through a telescope they could see a small disc half covered with white clouds. The sun would be brilliantly reflected in the sea. The North and South poles, and other parts covered with snow, would be white, deserts would be yellow, and sea and forest regions dull blue. Towns and canals would be too small to be seen. There would be no sign of man at all. They would never know at all that a human race existed.

* * *

ON this basis it is pretty well impossible to arrive at any definite conclusions though probability is that there is no life, as we recognise it, on this planet.

GUIDE to READING

WHY THE ENGINEERING STRIKE? John Gollan's great book "Youth in British Industry" (published by Lawrence & Wishart, in conjunction with Victor Gollancz, 6/-, L.B.C. extra 2/6.) Tells you all you want to know about youth's conditions in industry and shows the solution of their difficulties.

STRIKE! The story of the Clyde Apprentices' Strike. One Penny. Published by the Young Communist League it shows why the Clyde Apprentices came out and tells the story of that strike. Very useful at the moment. Copies from local Y.C.Ls or direct from Y.C.L., 127 East Road, London, N.1.

THE PEACE LIBRARY. Seven titles. Published by the Communist Party. Can now be obtained—all booklets for 2d. Really worth having!

SCIENCE! Man and his Universe by John Langdon-Davies. Thinker's Library No. 61, published by Watts. 1/-. The story of man's attempt to solve the mystery of creation.

WE AREN'T GOING TO DO NOTHING. Cecil Day Lewis, well known author, replies to the Pacifist line. Left Review, 6d.

While his time lasts he will gun for birds.

How the Ignorant were

... were Tricked

MIRACLES MADE EASY

ANYONE can perform a miracle nowadays. Once upon a time it used to be the prerogative of certain religious orders—particularly in Russia—who used simple chemical experiments by which to fool their ignorant congregations.

Typical of such a "miracle" is *Offering Fire*, when after fervent prayer flames come down from Heaven and ignite an offering placed on the altar. Simple permanganate of potash in a porcelain dish moistened with sulphuric acid, concealed in the altar, is the explanation of this.

Over the dish are placed wood shavings carefully laid in a wire triangle. The "Miracle maker" prays, passes his hand over the offering squeezing an alcohol saturated rag. The drops fall on the chemicals, the reaction of the sulphuric acid and the permanganate releases oxygen which sets the alcohol alight which in turn kindles the shavings.

Very simple—and profitable. For each miracle the congregation willingly pays.

Then there's the *Self-igniting Candles*. A tallow candle is moistened, with a suspension of yellow phosphorous in carbon disulphide. In a few moments the candle ignites.

The explanation is that as the carbon disulphide evaporates it deposits finely divided particles of yellow phosphorous which combine with the air, at room temperature igniting in the process.

IT WOULD BE ABSURD IF TO-DAY THE REACTIONARIES OF THE CHURCH TRIED TO FORBID THE STUDY OF SCIENCE OR THE PURSUIT OF KNOWLEDGE.

YET YEARS AGO THEY TRIED TO DO THAT. THEY CONDEMNED ANY SCIENTIFIC EDUCATION OR EXPLANATION AS HERESY.

IT IS EASY TO SEE WHY THEY DID THIS WHEN YOU READ THE FOLLOWING ARTICLE.

From time to time from the Medicine Man onwards, the unscrupulous have used such methods to exploit the credulity of the innocent. The atmosphere of superstition and fear which such miracles were able to create can easily be imagined.

The mixture of phosphorous and carbon disulphide was a very good friend and the modern chemist can explain many miracles with the aid of these simple chemical substances.

Take, for example, the celebrated *"writing on the wall"* by which, in the Old Testament the prophet Daniel shook up the Babylonian king, Belshazzar.

Paint the letters on a wall with the above mixture. Turn out the lights and in the dark the phosphorous sticks out in a vivid manner.

(Continued in next column)

How they Photograph the Sky

SKY mapping is an expensive business. It also needs a large and complicated apparatus. The most powerful telescope owned by an amateur is in America. It is a 28½ inch telescope.

Owned by Dr. Gastavus Wynne Cook of Philadelphia (who by the way is a very big business man) this hobby costs him some £20,000.

To photograph the heavens he uses a star-camera weighing more than 2 tons. It has a 6½ inch lens takes pictures 20 by 24 inches and covers 1/40 of the visible sky at each exposure and shows about 400,000 stars on one plate.

It will take Dr. Cook some forty plates to photograph the entire northern sky. He intends to make a star atlas with the results.

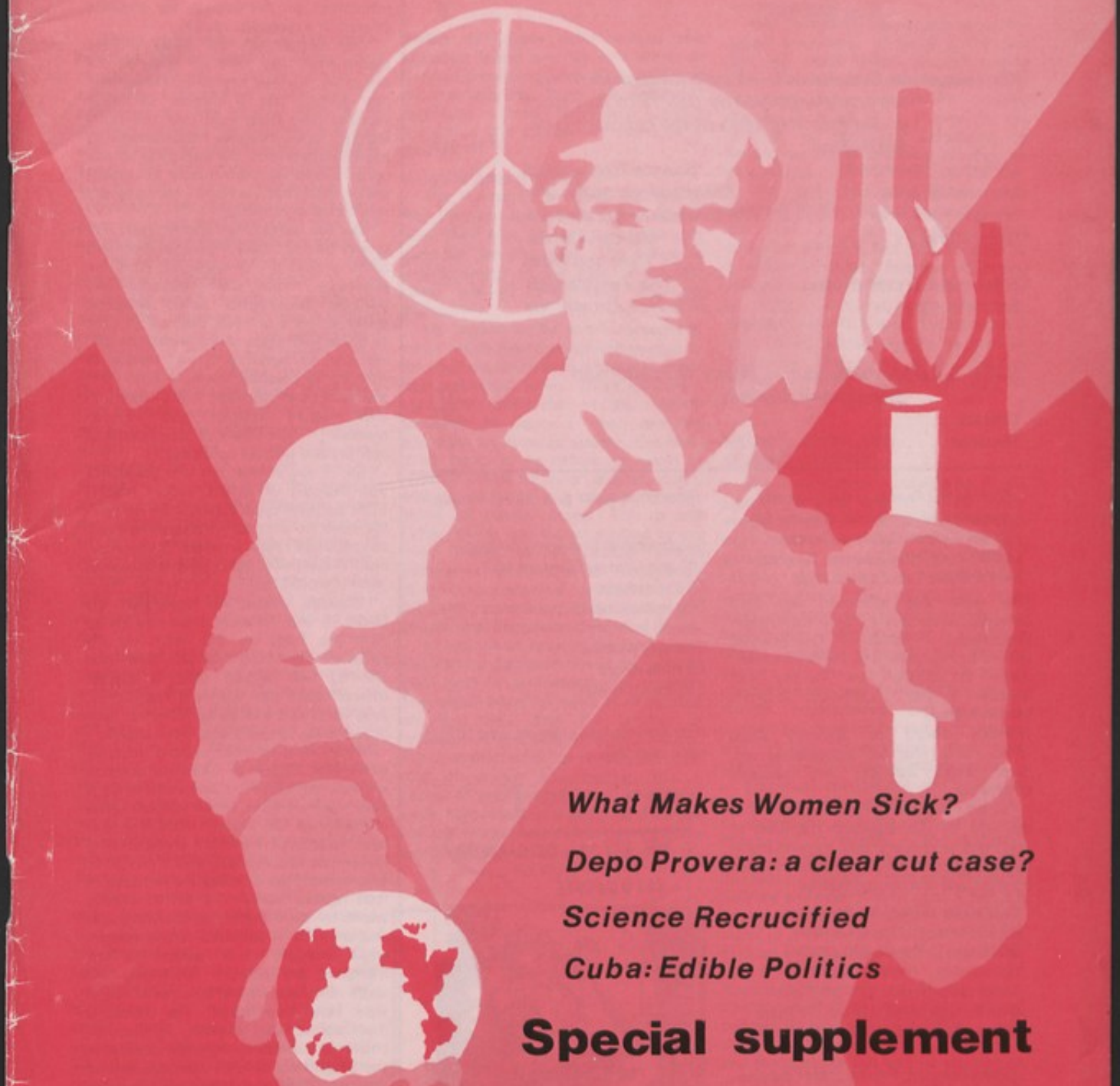
(Continued from previous column)

The hundreds of other tricks which mystified the peasants and work-people of the middle ages (and even still do in some parts of Eastern Europe) are readily explained by modern science. No wonder the reactionary church tried to ban the pursuit of knowledge.

Science at the Crossroads

SCIENCE for PEOPLE

A Socialist View of Science, Technology and Medicine



What Makes Women Sick?

Depo Provera: a clear cut case?

Science Recrucified

Cuba: Edible Politics

Special supplement

Science at the Crossroads

51

70p

contents

spring '82

Science for People **is the quarterly magazine of the British Society for Social Responsibility in Science BSSRS , 9 Poland Street, London W1V 3DG. Tel: 01-437 2728.**

This issue was produced by: Sara Barefoot, Dave Boyd, Philip Boys, John Bradbrook, Robin Collingwood (artwork), Margaret Jee, Danielle Lesser, Tony O'Connell, Jon Turney and Michele Whyte. Additional artwork Martin Ingley.

Humble thanks to Beverley Miles (B.A. Hons.) for her scintillating wit, razor sharp intellect and valuable comments.

Finally you will have noticed that the cost of the magazine has risen to 70p, this is entirely due to production costs, in fact we have been losing money on the trade distributed copies!

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A Note From The Collective

Science for People 51 looks back over 50 years of radical science. Our special 20 page supplement published with the support of the Science and Society Trust, presents a series of acute reflections on the struggle for socialist science in a capitalist society.

Our enthusiasm for these articles means that they take up more of the magazine than we first planned and this has meant that a number of other features have been held over and some letters and reviews shortened to accommodate this. We apologise to any contributors we were unable to consult about this! And we hope you agree the results are worthwhile.

On the subject of contributors, we would like to make a general comment on what goes in the magazine. While we obviously make both editorial and political judgements when selecting material, we don't feel that we all have to agree with every word printed in SfP. We try to cover as many shades of opinion as possible, within a broad left spectrum, and we hope it is understood that signed articles and reviews represent the views of the individual authors and not the collective.

So, if you've ever felt that the article you thought of writing would not fit SfP's political perspective, have no fear! We have no interest in squashing every contribution into a single framework, even if we could agree among ourselves what it would be.

Looking ahead, we hope that the boost to our finances from the tie-up with the Trust for this issue and the stock of copy held over will mean that SfP 52 will appear after a shorter interval than has been the case lately. And we would like to ask for your help in two areas to make sure this happens.

First, send us your articles, reviews, news and letters, or ideas for subjects we should cover. And when you've done that, give us your time! Several active members of the present collective have been overtaken by other commitments in the last few months, and those of us who remain are finding the production load quite heavy. The SfP collective works in an informal, co-operative way and all that is required is enthusiasm, good humour and patience. New members are always welcome, and some new faces would be an even better sight now than usual. The collective meet about once a week, in London, and a phone call to John Bradbrook in the BSSRS office is all you need to find out the time and place of the next meeting. Why not do it now?

We would like to thank the Science And Society Trust for their help in producing this supplement on the Science at the Crossroads conference.

WHAT MAKES WOMEN SICK ?

Three of us were among the several hundred who attended the Women and Health Conference held in London at the end of November which was co-ordinated by the **Women and Work Hazards, Women in Medical Practice,** and **Women in the Politics of Health** groups. We were impressed and heartened by the warm, cooperative and stimulating atmosphere of the conference, and the obvious work and care which had gone into organising the day.

This inviting environment and thoughful planning extended to the variety of health topics covered by the workshops together with the informed talks of the speakers, the well-stocked bookstalls, the (delicious) catering, the lively lunchtime cabaret presented by the **Spare Tyre** theatre group, the creche (run by men), and a poster display highlighting, by witty additions, the implicit sexism of most health education campaigns. Not to say the *obvious* sexism – one poster from The Scottish Health Education Unit showed, under the caption "You don't need to smoke to be a success...", photographs of twenty male 'stars' who had successfully kicked the habit, somebody supplied the perhaps obvious but pertinent rejoinder "... but you do need to be a man."

However, it was unfortunate that the planning groups had not been more aware of the basic requirements of disabled women if they are to participate fully. It was pointed out forcefully that the facilities (in particular the choice of building which tended to segregate the disabled), were completely inadequate for the disabled women who made up a strong section of the conference participants. It became clear to all of us who were not disabled that our thoughtlessness was politically retrogressive.

When we were discussing how to write about the conference we decided that we could not adequately convey the importance and relevance of the topics discussed if we tried to give (secondhand) summaries of all the workshops. We therefore decided we could most usefully give detailed accounts of the ones we had attended. We also decided against writing long reports of the talks presented during the plenary session. Although much that was said by the three speakers was spirited and rousing we feel that in the main the issue of practical strategies for change was barely tackled (probably because there was so little opportunity for discussion and participation). Nevertheless we believe other women who attended found the day as a whole as thought-provoking and stimulating as we did and that from this optimistic beginning a strong movement pressing for action and change will continue to grow.

WOMEN AND STRESS

The workshop on stress was led by Ro Clayton and Sheila MacKechnie, who gave very informative talks as a basis for the discussion. The following is a summary of the main points that came out of their talks and the discussion.

We looked firstly at the concepts of stress which have predisposed women to see their experiences in individual rather than collective terms. There is often a significant difference in the medical approach to stress in men and the approach to stress in women. For men, stress tends to be defined in terms of physical illness e.g. heart disease, ulcers; while for women it is often defined in terms of mental illness. This is one factor which will induce women to see their illness as a problem within themselves, while men are more likely to see it as a physical reaction to stressful work, something which carries less stigma than depression or mental illness.

In looking at stress in the workplace, there was a particular focus on the way shift work is problematic for women. The organisation of work is structured on the basis that the worker is 'available' and is able to work in shifts. For a woman who has to play the dual role of worker and mother, this is bound to produce conflicts. While shift work may result in a stressful situation for any worker, women are particularly vulnerable. Unfortunately, though, most studies on shift-work have looked at its effect on men, and more research is needed on how it can affect women.

Another area of work which was considered in relation to stress, was work involving new technology, where an increasing proportion of women are employed. The mechanisation and control of jobs, and short cycle repetitive work can again make women prone to stress.

But militating against the move for better conditions of work is the fact that the Trade Unions are still largely dominated by masculine values. They can only be a weapon for positive change if there is a change in the attitude to collective bargaining to encompass the social effects of work, on all workers.

The two important points that emerged in the workshop were, firstly, the need to challenge the present structure of work, and look forward to a different system (perhaps following Sweden's example with moves to get rid of shift work wherever possible). Equally there is a need for consciousness raising among women, to gain an awareness that stress is a collective problem, which could then lay the foundations for self-help.

WOMEN, WORK AND UNEMPLOYMENT

The workshop considered two aspects of work and its effects upon women's health – unemployment and work hazards. Ann Spendiff spoke first giving us a general account of the existing literature on health and unemployment. She emphasised how more and more people are beginning to believe that unemployment is a serious health hazard, although the precise nature of the connection between unemployment and ill health is unclear. Recent governmental and academic interest has produced a spate of reports and studies investigating the links between unemployment and ill health.

Statistical correlations can be drawn between periods of economic stability or instablility and the rates of various indices of ill health – for example, cardio-vascular disease, general mortality, suicide, homicide, mental hospital and prison admissions – but it is not possible to separate out unemployment as such from other variables such as class, income, housing etc., and thereby illustrate a causal connection. Recorded incidence of all the above conditions of ill health is higher during periods of economic instablility – but this refers to booms as well as slumps – and the statistics can take no account of ill health which is unreported and unrecorded.

Despite these problems, what did become clear from Ann's talk and the subsequent discussion was the essentially sexist nature of virtually all studies about health and unemployment. Almost no studies of the effects of

unemplyment upon health mention women at all. (Though epidemiological data clearly suggest, for example, that perinatal and maternal mortality rise as unemployment increases.)

In the main, women feature neither within the statistical studies which correlate numbers of unemployed by class, age, previous occupation, region etc. with subsequent reported physical and mental health, nor are their opinions and feelings considered in studies which have interviewed unemployed men. Such blindness on the part of social researchers seems particularly ironic as women are becoming unemployed at a faster rate than men (the figures for the percentage increase from September 1975-81 show that men's unemployment increased by 169% and women's by 329%).

One researcher has gone as far as to suggest that women who are unemployed do not suffer the same degree of stress as men because they do not have the same expectations of the working role. In other words, unemployment does not matter to women because loss of job does not threaten a similar 'loss' of their own person and role identity because their self-images and self-esteem are neither formed by nor dependent upon their work roles.

Some recent trade union activity also supports the view that women's jobs are less important than men's and the belief that they do not 'need' to work to support themselves or their families. There have been cases where trade unions have pressed for redundancy for women in order to 'save' men's jobs.

What is particularly interesting in view of such assumptions about the significance of work for women is that two of the most commonly reported symtoms amongst unemployed men are lethargy and depression. These are precisely those conditions from which many women confined to the home with young children and no outside employment and no 'work role' suffer, and which have most frequently been attributed to their neurotic tendencies and incipient mental instability.

The second half of the workshop started with an outline from Jude Connor of hazards at work experienced by, or particularly likely to affect, women. Amongst her list were the following:

1. There are particular hazards attached to some jobs where women predominate numerically; for example, textile workers, nurses and hairdressers are all exposed to dust, gases or sprays which are harmful to health.
2. Most industrial and manufacturing machinery is not designed for women to use. The difference in shape and size between women and men makes the use of male-oriented machinery by women uncomfortable and dangerous.
3. Protective clothing is designed for men not women.
4. Pregnant women are exposed to the dangers of machinery and substances potentially harmful to a foetus. Management response is to remove pregnant or fertile women from such work, not to make work conditions safer.
5. Women are exposed to sexual harassment.

The point was also raised that women are at risk from their husband's work – potentially harmful substances are brought into the home on the husband's body and clothing affecting both his wife and children.

Threshold Limit Values, which supposedly control the levels of dangerous substances, were a point of concern for several women. Again the TLVs are calculated on the basis of an 'average' man's body weight not that of women which is lower. It seems likely therefore that women would be more likely to be affected by exposure – yet most mass outbreaks of illness amongst women in factories are attributed to psychogenic causes or 'hysteria'; again encouraging a view of women's innate weakness and vulnerability rather than allowing the possibility of an external and removable cause.

Discussion turned to what could be done about such hazards. Sue Barlow pointed out the inadequacies of existing factory legislation, protective practices and inspection procedures, and emphasised the need for more trade union organisation.

From the discussion that ensued it became obvious that many women were less than satisfied with their trade union's activities in relation to work hazards, and had little confidence that unions would campaign on their behalf. Women, speaking from direct experience, openly criticised trade unions for their lack of serious concern and generally unhelpful or even obstructive attitude towards problems experienced by them as a result of using dangerous machinery or harmful substances in their work places.

Clearly the workshop raised some very worrying questions. I shall attempt to summarise what, in my opinion, seemed to be the major issues:

1. Why are women largely ignored when the effects of unemployment upon health are studied?
2. Why aren't trade unions waging a more active struggle to protect women's jobs and to ensure better protection against work hazards?
3. Why is depression in men attributed to an external cause, such as unemployment, whereas in women it is more likely to be attributed to individual 'personality' factors, and in the case of mass illness amongst women even to 'hysteria'?
4. Why doesn't the similarity between the aimlessness and sense of 'loss' experienced by unemployed men, and that of women confined to the home, lead more people to question whether home is the best place for wives and mothers?

We came up with few answers but much spirited discussion. The obvious statement that we live in and are subjected to a patriarchal society does not provide a strategy for action. Perhaps a recommendation made during the discussion about work hazards was most optimistic – women were urged to conduct their own surveys on health hazards in their place of work for use both within union struggles and against employers. Such surveys in themselves generate interest and provide evidence on which to press for action.

WOMEN AND CANCER

The workshop on cancer also took the form of two speakers presenting introductory talks as a basis for discussion. The workshop focussed on cancer in general and on the two types of cancer most likely to affect women – cancer of the cervix (not the most common type of female cancer by any means, but on the increase, and by definition confined to women), and cancer of the breast (extremely rare amongst men, but showing the highest rate of registration and mortality of all cancers amongst women).

Jean Robinson spoke about cancer of the cervix. She stressed the general point that vast sums of money are spent on research into cures and treatments for cancers but that virtually no research is financed to investigate the causes of cancer. This is especially alarming in view of the fact that it is suspected that as many as 80% of cancers may be attributable to carcinogens in the environment, which are potentially detectable and removable.

The causes of cervical cancer are known to be multifactorial. The disease is unknown in virgins. It is unfortunate that its incidence correlates closely with, amongst other things, the age at which a woman first has intercourse, and the number of her sexual partners, which makes it all too easy to lay the blame for its appearance on the

tipoffs & ripoffs . . . . . .

LUCAS THREAT TO BURNLEY WORKERS

It is no co-incidence that the Lucas Aerospace management, should have chosen Burnley as the place to announce over 1,000 redundancies.

It is not because the company has not been making enough money. Last year they declared a profit of 21.1 millions. It is more likely that it is because since Mike Cooley's sacking last year, the centre of resistance to management's plans for restructuring and redundancies has shifted to Burnley. It has been in Burnley that there are the best organised plants and the strongest support for the workers' plan for socially useful production as an alternative to an insecure future working on the means of death and destruction.

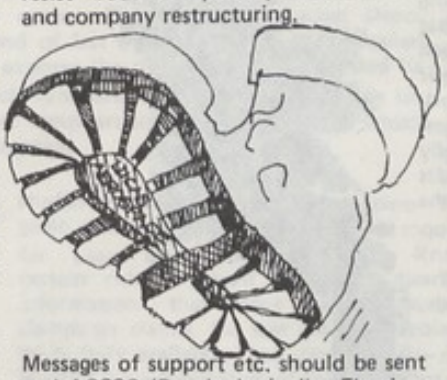
A packed meeting was held at Burnley's football ground, Turf Moor, on February 11th to discuss the threat of redundancies and what the workers' response should be.

The meeting passed a resolution condemning the company though many of the products are now being developed elsewhere.

The company's intransigence is compatible with the position declared in the mid-seventies by the chairperson Sir Bernard Scott that,

> "Lucas will move overseas with capital, investment and jobs and retain its UK operation on a care and maintenance basis."

The committee intend to conduct a campaign to both protect and create jobs, based on the alternative products outlined in the Corporate Plan and to resist both compulsory redundancies and company restructuring.



Messages of support etc. should be sent to LACSSC (Burnley) c/o Jim Fleming, 28 Ennismore Street, Burnley, Lancs.

MULTINATIONALS CAN BE HAZARDOUS TO YOUR HEALTH ... ESPECIALLY IN IRELAND

In early March over a hundred people attended a national public conference in Ireland on 'Hazards at Work & Poisons in the Environment.' The major theme underlying all the topical workshops was the threat to health posed by the multinationals attracted to Ireland by tax-free holidays, subsidies and weak health and safety requirements. In the 32 Counties as a whole, Britain's traditional role as the major foreign investor has been taken over by the USA. Many multinationals choose to relocate in Ireland (and other dominated countries) rather than meet the more stringent American safety standards won through workers' and community struggles.

The conference, organised by the recently formed Alliance for Safety and Health (A.S.H.), was appropriately held at University College, Cork, a major Irish centre of research for the multinationals and site of Ireland's experimental nuclear reactor. (A.S.H. grew

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behaviour of its victims. Cervical cancer also presents a steep social class gradient with women in the working classes most often afflicted; again allowing the opponents of women's sexual liberation and the so-called moral guardians of society to assume that the lower-class woman pays the price for her "promiscuity."

It is always the woman who is blamed and never the other way round: the men who comprise those sexual partners remain free of criticism. Only one study, carried out in Israel in the 1970s, has looked at whether the incidence of cancer of the cervix is higher among women whose *husbands* have had several partners. The study did indeed find a positive correlation.

Other studies suggest that incidence of cancer of the cervix in married women correlates with their husband's occupation. The highest rates are found amongst the wives of coalminers, metal workers, fishermen, and other manual workers whose work entails frequent contact with dust, coaltar or asbestos. This provides an alternative explanation for the particularly high rates amongst women of social classes 4 and 5.

The sudden increase in cervical cancer mortality is another factor which counters the common assumption that women are themselves the cause of their disease through their promiscuous behaviour: sexual behaviour patterns change slowly.

The moral condemnation which often accompanies this disease serves to detract attention from any further investigation of those causes suggested above, and makes for an easy slogan, "the only guaranteed cure is prevention through abstinence." Jean suggested that women must counter such blatant sexism not by fearing and avoiding sexual relationships but by themselves promoting barrier contraception for the clear protection which it affords. They must also press for the control and elimination of known carcinogens in their own and their partners' work places. In addition she stressed how women must not be lulled into a false sense of security by the relative simplicity and accuracy of cervical cancer screening. Protection, she said, must ultimately mean prevention.

Lesley Doyle spoke to many of the points raised by Jean and other women during the subsequent discussion. She emphasised that despite the vast sums spent on the search for cancer cures the survival rates for most cancers have improved little over the past twenty years. This fact, she suggested, means that cancer must be treated as a preventable disease with a refocussing on *causes* rather than cures.

Cancer death rates for both women and men have increased overall in this century, but arguments abound as to whether this is an actual rise, a statistical artefact, or a 'natural' result of the larger numbers of old people in the population. Such disputes detract from the need to research environmental causes, although social class differences in cancer rates support an environmental explanation. However, occupational and 'lifestyle' factors (smoking, diet etc.) present two particular problems for the explanation of women's cancer. First, much less is known about occupational hazards among women than among men (mortality statistics register married women according to their husband's occupation), and little is known about the effects of a man's occupation on his partner. Secondly, the 'lifestyle' concept can be used to 'blame the victim' for her/his moral weakness, thereby individualising the problem.

The argument about individual or social determination of behaviour in any case provides few guides for action. More information is required about carcinogenic substances. In Britiain, where there is no "freedom of information" act, such knowledge is hard to come by. The precise substances involved in many production processes remain closely and legally-guarded 'industrial secrets.' Lesley urged that women must collectively start to look for, and protest about exposure to, known carcinogens in their places of work, in consumer products, and circulating freely through general environmental pollution.

Sara Barefoot
Margaret Jee
Danielle Lesser

tip offs & rip offs . . .

out of Ireland's successful anti-nuclear movement, formed in 1978 to oppose the government's plans to construct Ireland's first nuclear power-generating reactor at Carnsore Point, County Wexford). Cork was also an appropriate venue because the embryonic movement against toxic industries won its first major victory there when Raybestos Manhattan finally announced its departure in Autumn 1980 after months of residents' resistance to asbestos dumps and open revolts by the plant's workers.

The conference reflected the working class character of the revolt which has been provoked by the location of such multinationals in Ireland and their blatent disregard for safety in transporting and disposing of toxic substances. The conference heard Seamus Connolly (of the Cork ITGWU and Trades Council) speak on hazards at the workplace and Jim Maher (shop steward of the Electrical Trade Union) explained the difficulties of using the existing law against toxic hazards. Members of the Finglas Anti-Toxic Action Group described their battles – in the courts and the streets – to oppose a toxic waste dump in their North Dublin neighbourhood; their new video film pointed out that there would have been an immediate outcry from the established 'environmentalist' organisations if the government had attempted to locate such a dump in a middle class area. The presence of Irish Special Branch men (monitoring the chartered coach from Dublin) indicated one of many ways in which this movement is by no means an Irish counterpart of the respectable conservationist organisations in other European countries.

Martin Ingley

The volatility of these issues (as well as of the substances!) was shown by a front-page lead article in the *Cork Examiner* which reported the claim by a former chairman of the Chief Fire Officers Association that toxic substance transport in Cork could easily start a major fire which might kill 100,000 people, because the fire service would be powerless to deal with it. These are the risks which the pro-imperialist Irish government is asking the Irish people to accept in the name of 'development' by using as blackmail threats of unemployment – even though many of the multinationals leave soon anyway, after they've made their 30% annual profits but before they are hit with mass demands to compensate their workers and communities for the health and environmental damage

The movement has so far published one major document, *Toxic Ireland*, which analyses all the known toxic hazards in Ireland and locates them within the changing cycles of international capital. Copies are available @ £1 (one-third discount on bulk orders) from:

A.S.H.
Box 1240,
Hamilton Street,
DUBLIN 8

DEPO PROVERA

a clear cut case?

Depo-Provera (DP) is an injectable contraceptive which has recently caused a lot of controversy. It is banned in the USA but is widely used in 3rd World countries, and increasingly in Britain too, writes Sara Barefoot. It is to be reconsidered for licensing in Britain by the Committee on Safety of Medicines in the near future, and is likely to be passed despite numerous reports of uncomfortable and dangerous side-effects, and a possible link with cancer. The *Campaign Against Depo-Provera* brought out a report at the end of last year which provides detailed information on these hazards, and also exposes the politics which lie behind its promotion and continued use. This article summarises the report, which calls for the immediate withdrawal of DP; but also considers some of the questions which have been raised about the aims of the Campaign.

DP was developed in the 1960's by the American company Upjohn, and is a contraceptive based on the hormone progesterone, usually injected in doses to last 3 months. Initially it must have appeared a very attractive alternative to the other contraceptives then available. Its appeal lies in its simplicity: only one injection and protection is guaranteed for a long period. It would seem to free women from a lot of the trouble that other methods involve.

But there are no 'miracle' drugs. Side effects of DP range from head-aches to weight gain, with the most common being menstrual disruption. Many women experience heavy and prolonged bleeding, or possibly amenorrhoea (absence of periods) later on. There is often a delay in the return to fertility after taking DP, and some suggest that permanent infertility could result. And, women taking DP are more prone to diabetes, may have less resistance to infections, and may risk harming both unborn and breast-fed babies. But the most alarming aspect of DP is the association that has been revealed in animal studies between the drug and cancer. Breast cancer has been indicated by experiments on beagles, and cancer of the endometrium (the lining of the womb) by experiments on monkeys.

The research evidence on beagles was one of the main factors which led the American Food and Drug Administration (FDA) to reject Upjohn's application to market DP in the US in 1978. However this did not affect the International Planned Parenthood Federation, the main distributor of DP, or the World Health Organisation, neither of whom saw the research as a contraindication to the use of DP.

Shortly after the FDA had rejected the application to licence DP both the other organisations issued statements re-affirming their confidence in DP as a safe and reliable contraceptive.

While it is true that the animal studies do not prove that DP is carcinogenic among women, the case has not so far been *disproved*, and clearly a certain risk is shown. Yet even the information that does exist, the Campaign claims, has not always been made fully available. They show in their booklet how publication of the research findings was controlled by the company, allowing the drug to become well established first. Drug companies performed much of the research on their own drugs, and they obviously stand to lose money if problems with their product are revealed.

But Upjohn have certainly not suffered this fate. The FDA, while they prohibited the general use of DP in America, declared that it might be an appropriate drug in countries:

> "where the alternative methods of contraception may be less available and less acceptable and where the physician/patient ratio is lower".

They were referring to countries with an overall poor standard of health, and a high infant mortality and disease rate.

Following their statement, the United States Agency for International Development (USAID) took steps to press for a change in legislation so that Depo-Provera could be legally exported despite the ban in the US. The double standards had become officially instituted, and although there has been questioning among international family planning agencies, DP continues to be agressively promoted throughout the world. To date, an estimated 3-5 million women, in 76 countries, have received injections of DP.

These double standards reflect the fact that population control by the state has taken precedence over birth control by women, and their health care. Population control can be seen as a means by which the West can control the destabilising effect of population growth. The Campaign's report quotes Robert MacNamara, former US Secretary of State for Defence, head of the World Bank and Managing Director of Ford Cars, who is quite explicit about the aims of the promotion of "family planning":

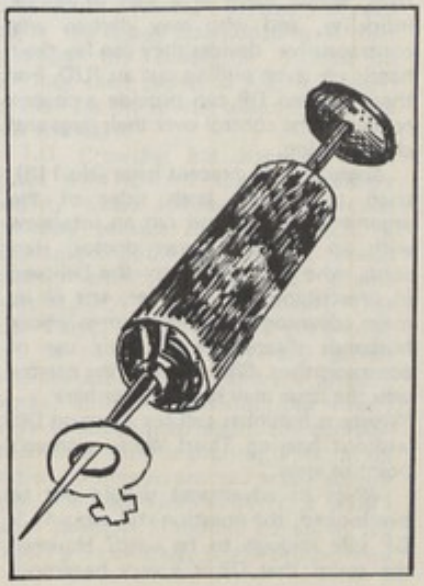
> "Family planning programmes are less costly than conventional development projects ... (they can) yield very high economic returns."

With DP's obvious advantages in its ease and cheapness of administration, it's not hard to see why it has been promoted so widely.

This convenience of administration has also made its abuse in 3rd World countries much more likely, for it is easy to give the drug without a full explanation of what could be involved in taking it. And DP is essentially targetted on illiterate women in developing countries, who are highly unlikely to have been fully informed about what they are being injected with. For example, in New Zealand it is thought that the majority of the 10,000 women involved in monitored trials on DP were Polynesian or Maori women who had not been provided with an interpreter.

The greater part of the 'market' for DP consists of women who will have little access to information on the drug, and whose often-cited belief that 'good medicine comes through the needle' has clearly been exploited. Added to this the risks of lowered resistance to infection, and anaemia resulting from heavy bleeding, seem particularly hazardous for women who live in communities where the standards of health and nutrition are low.

The abuse of DP is not restricted to 3rd World countries. There are sections of the population in this country who also appear very vulnerable to being given DP without much effort to inform them about the drug. DP is not licenced in this country for general use as a contraceptive, though women who have just had a rubella immunisation, or whose partners have just had a vasectomy, are excluded.



It is hard to gauge actual numbers, but it is known that DP has in fact been used fairly frequently for women outside these categories. The report says,

> "There is no doubt that those who are being offered Depo-Provera in Britain tend to be women who are very poor and who, overwhelmingly, are non-English speaking and/or black. The Depo-Provera injection is being given by doctors on social grounds, to women considered by them to be inadequate in some way."

As a one-off injection its efficiency is confirmed for months, an excellent way of dealing with those who don't 'manage' with other contraceptives. And once again it is easy for the drug to be given without informed consent. The Campaign's report cites the case of a 14 year-old West Indian girl given a DP injection while not fully recovered from an abortion — the doctor apparently justifying this later by claiming that the girl was 'Educationally Sub-Normal '.

It is clear, from many of these cases, that DP has been used in very alarming ways. But there are women, nevertheless, who have made definite requests to be given DP. Reports, for instance, that an increasing proportion of white/middle classs women in the UK, presumably with some knowledge of the drug, are asking for DP, suggest that its dangers are not viewed the same way by everyone.

In the 3rd World also, there have been many claims that DP is a very popular contraceptive, and women may travel long distances to get it. Of course, the fact that the women may be largely ill-informed about its risks explains this in part. But arguments supporting the use of DP should not be totally dismissed on this basis: DP does provide some benefits for women in certain societies which other contraceptives cannot offer.

There are women whose husbands are opposed to them using contraceptives, seeing them as a sign of sexual infidelity, and who may destroy any contraceptive devices they can lay their hands on, even pulling out an IUD. For these women DP can provide a chance to have some control over their lives and child-bearing.

Spare Rib, in a recent issue (No.116), tried to present both sides of the argument on DP, and ran an interview with an Indian woman doctor, Hari John, who explained why she believed in prescribing DP. For her, one of its main advantages was for women whose husbands disapprove of their use of contraceptives. She attacked the narrow way the issue may be seen over here:
"Western feminists call for a ban on DP without hearing Third World women's point of view."

While its advantages should not be overlooked, the question still remains, is DP safe enough to be used? However the claim that DP is a very hazardous drug also needs to be seen in a wider perspective.

What has to be considered is how (as yet mostly unproven) hazards of DP stand in relation to the risks of the Pill (which has been actually *known* to cause deaths) and the IUD (actually *known* to cause infertility). Jill Rakusen raises the question in a recent article which conveys the complexity of the subject very well. She suggests that

> "to single out DP as dangerous can lead to confusion. It tends to result in DP being labelled the arch-villain ... Focusing solely on the drawbacks of DP can lull women into a false sense of security about the other non-barrier methods of contraception."

An obvious difficulty with the whole issue is that the real extent of DP risks is just not known; certainly more research is required. Yet as things stand it is often hard for women to know of even the possible risks they may be taking. Wendy Savage, a consultant at the London Hospital, conducted a survey which revealed that only a small proportion of DP users had been properly informed about the drug. She advocates that

> "the method should be used with caution and only with the woman's full and informed consent."

Banning DP could be seen as further denying women a certain contraceptive choice, but so far it has represented a travesty of choice for most women. The crucial question seems to be one of how a genuine informed choice can be guaranteed, and whether it is possible at the present.

The Campaign's stand on this is quite clear; for them DP does not represent a real choice for women. It is essential to their argument that DP is basically a 'passive' drug: control lies largely in the hands of the administrator, with very little active participation from the woman. The very nature of DP, as a long – term injectable contraceptive, lies at the heart of the abuse to which it is wide open. This is something which needs to be recognised, and not only in relation to DP. New generations of injectable progesteron contraceptives are being developed. These have received very little attention, but they are at the moment being given to women at Kings College Hospital, for example. Their existence does show that, while the future of DP may not yet have been settled, the form of injectable contraceptives is becoming far more established.

Further information can be found in:
The Report by the Campaign Against Depo-Provera, available from
Campaign Against Depo-Provera,
c/o ICAS,
374 Grays Inn Road,
London W.C.1

Jill Rakusen, "Depo- Provera,"
Women, Health and Reproduction,
ed. Helen Roberts.

Wendy Savage, 'Fertility and Contraception. Vol. 2 No. 3.

Spare Rib Issue No.116.

I would also like to acknowledge the help of John Holliday.

Sara Barefoot

Science at the Crossroads

Looking back on 50 years of Radical Science

Science Recrucified

The origin of the papers printed in this collection lies in a crowded meeting held by the British Society for Social Responsibility in Science in November 1981. An audience of 150 came to celebrate the 50th anniversary of the Second International Congress of the History of Science and Technology, which took place in the Science Museum in London in the summer of 1931. Clearly no ordinary academic gathering. Indeed the Congress, or rather the special session on 4th July at which the Soviet delegation presented their papers, proved to be a crucial formative occasion, both for theory and practice concerning science and society.

The articles which follow provide insights into the social and cultural background of Britain in the 1930's which contributed to the reception of the Soviet papers, and analysis of the achievements, strengths, and weaknesses of the intellectual and political tradition to which they gave rise. The purpose of this introduction is to provide a few key facts about the Congress itself and its aftermath. (More detailed accounts can be found in J.G. Crowther *50 Years With Science*, Barrie and Jenkins, London 1970; and in P.G. Werskey *The Visible College*, Allen Lane, London 1978. The papers of the Soviet delegation, together with additional material by Joseph Needham and Gary Werskey, are republished in *Science at the Cross Roads*, Frank Cass, London, 1971).

The 1931 London Congress was the second in a series still proceeding. (The 16th was held in August 1981 in Bucharest, and paid perfunctory tribute to its more illustrious predecessor). What lifted the 1931 Congress into history was the last minute decision of the Soviet government to send an eight-strong heavy-weight delegation headed by Nikolai Bukharin. Only one Russian, Zavadovsky, had been expected. But on 23rd June there was a change of policy by Stalin on the attitude to the intelligentsia. After three days of frenzied activity the delegation flew off for London, where the Congress was due to start three days later. In the rush Bukharin discovered that he had left his speech behind, and the delegation was further delayed as the plane had to return to retrieve it.

On arrival in London they found, not surprisingly, that there was no time available in the Congress programme for presentation of their lengthy papers. After tense negotiations, an extra session was arranged for Saturday morning 4th July; but there would be time only to present a summary of each paper. To ensure dissemination of their message, the delegation decided to produce their papers in full, as a book, in time for the special session. They had just five days.

J.G. Crowther has described in *50 Years With Science* the extraordinary activity which followed: "The Soviet Embassy became the headquarters of this unique publishing venture. Several expert translators worked almost continuously, turning the Russian texts into English, while printers' boys stood by, to rush with copy and proofs between the Embassy and the printing works. Authors, translators and readers worked through the night, and compositors came into the printing works at six in the morning to proceed with composition". In the end, however, it wasn't quite possible. Unbound copies of the papers were available to the delegates on

"None of the amateur or professional students of the history of science could think of any comment for opening a discussion of the Russians' enthusiastic and exciting papers. After a pause, a twenty-year-old youth named David Guest drew attention to the significance of their views, stressing especially the historical element in all their philosophical and scientific concepts, and contrasting this with the non-historical concepts employed by Pearson and Russell in their philosophy of science. No other speaker could think of anything more to say. Guest subsequently graduated with first class honours in philosophy from Cambridge University, and was killed in Spain in 1938, fighting with the International Brigade in defence of the Republican Government."

J.G. Crowther, 1941

Dear Rosenhead,

Thank you very much for your letter of 9th October. I am no longer able to undertake long journeys, but I am deeply touched by your concern for me, and all those who have offered such kind help. I am very sorry that I shall not be able to be there.

With regard to the 1931 Congress, I have written my view in *50 Years with Science* and elsewhere, and I think what was written about the time is more exact and penetrating than recollections in one's latter years, when the faculties are less efficient.

Looking back on the history of the development stimulated by the Congress, my strongest feeling is the fundamental importance of pursuing a deep intellectual line steadily, but always within the mass of working people. It is striking how it can mature even through the most unpromising periods.

Yours sincerely,
J. G. Crowther

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that Saturday morning. The book, titled *Science at the Cross Roads*, was published a week or two later.

The ideas contained in the papers delivered by the Soviet delegation, especially that of Boris Hessen on "The Social and Economic Roots of Newton's *"Principia"*, were to start a ferment in the minds of a number of British participants who were to be key figures in the radical science movement of the 1930's and 40's. Joseph Needham was one of the conference organisers, as was Lancelot Hogben. Hyman Levy was there, and J.G. Crowther; and J.D. Bernal, who was to develop those ideas most cogently and originally in the coming decades.

The British left scientists were in no doubt as to their intellectual debt to the Russian papers, and acknowledged it handsomely. Thus Levy in 1939 described how they had crystallised his realisation of the impossibility of using science fruitfully within the framework of a chaotic capitalism. Bernal himself called the Congress "the starting point of a new evaluation of the history of science", and "the most important meeting of ideas that has occurred since the Revolution".

Yet the debt was not repaid in practical terms. The members of the Soviet delegation suffered the fate of many intellectuals during Stalin's purge, but without effective protest from their British comrades. Abram Ioffe survived to an honoured old age as the doyen of Soviet physics; others were less fortunate. Boris Hessen engaged in active controversies over the status of relativity theory until 1934, and then there was silence. He is presumed dead in the purges of the mid 30's. Ernst Kol'man survived years of imprisonment – he slipped across the border into Finland as late as the 1970's. N.I. Vavilov, Russia's internationally best-known biologist, a victim of the early advance of Lysenkoism, died after release from prison camp in 1942. Bukharin himself, a close associate of Lenin's, was already out of favour with Stalin by the time of the Congress – he was expelled from the Politburu in 1929. However, his final disgrace (or triumph) did not occur till 1938, when he was the defiant victim of Stalin's most spectacular show trial. His execution ended an era.

Ideas outlive their authors in curious ways. But it is important to emphasise that the impact in Britain and round the world of *Science at the Cross Roads* was not just intellectual. British scientists organised the Cambridge Scientists Anti-War Group, the revitalisation of the Association of Scientific Workers, the establishment of a new division of the British Association concerned with the social relations of science were all manifestations of a broad-based influence on science, albeit one which was to be stunted by the advent of the Cold War. Internationally this movement was to bear fruit in the World Federation of Scientific Workers, and (at least in part) Pugwash.

In a sense the British Society for Social Responsibility in Science, organiser of the commemorative gathering, is itself part of this heritage. I can remember hearing Hyman Levy speaking with vigour at the inaugual meeting of B.S.S.R.S. in 1969, and meeting J.G. Crowther in the crowded hall. J.D. Bernal too made a brief appearance, tragically confined to a wheel-chair. And Joseph Needham was to address our tenth anniversary meeting at the age of 80. As we celebrated the 50th anniversary of *Science at the Cross Roads*, the history which we were discussing was our own. There is much to learn from the weaknesses of that earlier movement, but also from its strengths.

When holding a commemorative meeting was first mooted, it appeared obvious that the most suitable location for it would be the Science Museum itself, site of the original Congress. The Museum authorities were approached, first informally and then formally. The request became subject to inexplicable delays, and eventually reached the level of the Director, where it lodged. A variety of bureaucratic evasions ensued in which the Museum authorities declined to say 'yes', but were evidently unwilling to say 'no' in print, which would have involved the embarrassment of giving a reason for the refusal. Eventually with time running out the meeting was transferred to another location.

Why was the Museum so uneager to be associated with this celebration of one of the most seminal events ever held within its walls? The subject was surely

Dear Jonathan Rosenhead,

It was very kind of you to invite me to participate in the meeting on the 20th November commemorating the 50th anniversary of the 2nd International Congress of the History of Science in London. I have just returned from the 16th of these International Congresses, held at Bucharest in Rumania, and I feel sure I was the oldest inhabitant there. No one else, I think, had been at the 1931 Congress also. In this Library we have, I am glad to say, a complete set of the Reports of all these Congresses.

I much regret that an engagement in Cambridge will prevent me from attending your meeting, but you may like to read this letter to the company. Please allow me to wish it every success. You may like also to mention the current issue of *Isis*, which contains valuations of the 1931 Congress by Jerome Ravetz and others.

There can be no doubt at all that this Congress, and particularly the contributions of the Soviet delegation to it, were seminally important for the Science and Society Movement of the thirties, in which I had the honour of participating. The analysis of the social background of Newton's *Principia* by Boris Hessen, even though "plain, blunt and Cromwellian", was a great inspiration to the younger socially conscious British scientists of the time. To the older British historians of science it seemed almost like sacrilege, or at least lese-majeste. It remains, however, a striking statement of historical materialism.

During the past forty years I have, as you know, been occupied with the history of science and technology in Chinese civilisation. The first question which led to our engagement in it was of course why had modern science taken its origin in Europe alone? But as we came to know more, a second question presented itself, why during the fourteen previous centuries had the Chinese been more effective in acquiring knowledge of Nature, and applying it for human benefit? Today we are still in the midst of the "Science and Civilisation in China" project, with eleven volumes published, and nine more to be written, finalised and issued. At the same time we are seeking support for the establishment of this Library and Research Centre upon a permanent basis, for the use of scholars from all over the world in decades, perhaps centuries, to come.

We do not like to attach to ourselves labels of orthodoxy of any kind. But it might well be that the understanding of why modern science developed only in the European context could be elucidated by a careful study of the Asian civilisations in which it did not arise. We see the European changes as a kind of package deal, in which the rise of modern science was associated also with the rise of capitalism, and the Protestant Reformation. All these were highly progressive in their day and age, though not destined always to be so.

In considering the absence of the spontaneous development of modern science in China, there are two sorts of factors to consider, the "internal" and the "external". We do not intend to under-play the differences in linguistics, philosophy and theology, but we feel that these alone could never carry the weight of the explanation. We believe that only a careful comparison of the social and economic structures of the civilisations will explain the historical phenomena – in so far as history can ever have explanations. So it might conceivably be that historical materialism will do what concentration on intellectual factors alone could never do, and show how it was that Europe was the focal point for the rise of modern science, rather than the civilisations of Asia, great and more successful though they had been before.

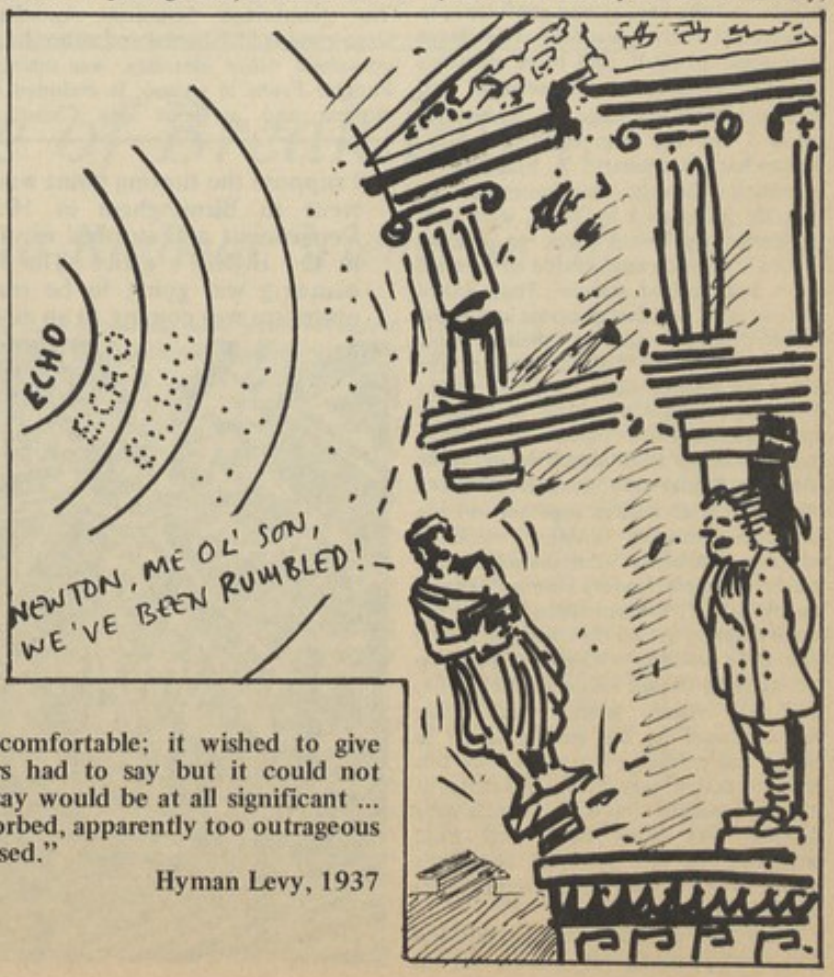
Lastly, I sincerely hope that you will be able to let me have a transcript of the addresses given on your important occasion, so that this Library may preserve them.

Yours very sincerely,
Joseph Needham 1981
(East Asian History of Science Library)

relevant – not only the history of science, but the *history* of the history of science. One can only conclude that the ideas launched 50 years ago by those Russian cuckoos in the nest are still seen as too dangerous by the British establishment.

The 50th anniversary meeting was actually held in a chemistry lecture theatre at Imperial College. The building abuts onto an unprotected flank of the Science Museum, perhaps 20 yards away across a courtyard. We must hope that the reverberating echoes of that historic session in July 1931, promulgated from such close proximity, have not undermined the Museum's foundations, nor done irreparable damage to its priceless exhibits.

Jonathan Rosenhead



"... the audience was a little uncomfortable; it wished to give attention to what these foreigners had to say but it could not believe that anything they could say would be at all significant ... The ideas were too novel to be absorbed, apparently too outrageous to be seriously considered or discussed."

Hyman Levy, 1937

Science at the Crossroads

Recollections of the 1930s

The purpose of my talk is to give a brief impression of what it was like to be part of the radical scientists' movement in the late 30's. I saw this as an undergraduate in Cambridge studying Physics from 1935-38. The background was the great Popular Front movement against Fascism and war. The scientists' movement was based in the Association of Scientific Workers and in Cambridge in the Scientist Anti-War Group.

First let us consider the scientific scene as a whole in Cambridge. There was the most extraordinary impressive atmosphere, awe, respect and enthusiasm for science. Young people looked up at the wonderful edifice of science, and the great success of scientific ideas, the romantic laboratories and the remarkable figures of the great scientists themselves. Rutherford lectured to 1st year students (including me), Cockcroft, my College Bursar, was splitting the atom with Walton, Dirac, inventing anti-matter, could be seen driving his sports car, Eddington would give a talk in the evening in a small college room where students could ask questions (as I did), Bernal was in crystallography, Gowland Hopkins was discovering vitamins, and Needham was prominent too. All this reinforced the very strong confidence in science. Clearly scientific knowledge was good in itself; and most scientists extolled the idea of pure science. But, although these scientists were clearly very good I had the impression they were somewhat reactionary. A minority of scientists directly or somewhat indirectly influenced by Marx, were very different and went back to Francis Bacon's position and refused to separate pure and applied science. They linked the ideas of political progress and social benefit. As I saw it these people expressed to the full the idea of the value of science. Also, Marxism, being holistic, linked these ideas of progress with Art and all aspects of culture. Architects like Corbusier, Picasso (in the Popular Front) even the surrealists and Freud were all pushed together to form a forward-looking Socialist vision of culture which was illuminated by the Soviet example. Isadora Duncan dancing in the USSR, Russian films, and all the experimental art of the early years after the 1918 revolution helped to build up an exciting overall vision of the future. In this vision science was quite unquestioned as the leading factor in social and cultural progress (we see this view of course very clearly in Science at the Crossroads). The only troubles were science was being held back from achieving its full potential and, even more sadly, it was being misused in its application to war.

The key to achieving the full and peaceful potential of science lay in Socialist planning. If one removed the economic contradictions in Capitalism the economy would expand, if one got rid of the contradictions of the class struggle the innate good in human beings would express itself. For science one needed a plan both for fundamental and for applied research. It was all quite straightforward and we seemed unaware of the possible contradictions within state bureaucracy, or the environmental complications which might arise from large-scale application of science. Bernal, of course, was a central figure in all this new thinking. His opponents like Polanyi seemed misguided fuddy-duddies who refused to recognise the full potential of science.

But we were quite right in seeing the biggest contradiction in science was war. The Cambridge Scientists Anti-War Group was led by Bernal and although it contained other Marxists, was mainly Popular Front in nature. It included 4 women and at least one Christian Pacifist. I think I was the only undergraduate, having been introduced by John Fremlin who was then a research student in the Cavendish. Meetings were in a basement under a cafe in Kings parade during lunchtime sandwiches. Our main programme was one of opposition to the government's plans for air-raid precautions A.R.P. This was part of the Popular Front's opposition to Baldwin's rearmament policy. Much of our time was spent considering the government's plans for civilian defence against gas attack. We were not satisfied with the low-cost civilian gas masks which were, of course, later given to every man, woman and child (infants had special devices) during the war. One thing we discovered was that the masks removed gases but not arsenical smoke. Also we spent much time on the problem of gas-proof rooms. We showed that gas would leak into these rooms. To measure the leak-rate someone had the clever idea of liberating a harmless gas inside a room and measuring the rate it leaked *out*. But J.B.S. Haldane, who was in London and only on the fringe of the group, pointed out that the gas leaked out through all six surfaces of a room but leaking in would be mainly through only two or three. However the

"I suppose the turning point was about the time I left Cambridge and went to Birmingham in 1939 to do a Ph.D in the Physics Department and stubbed my foot against a sack of uranium oxide in the H.O.D.'s office. The fission bomb was beginning, science planning was going to be really big and 300 years of Baconian optimism was coming to an end."

data enabled some worthwhile criticism of the ARP schemes and pamphlets were written and public meetings held in many parts of the country.

I was put in charge of experiments to test a report from the Spanish War that incendiary bombs landing on the top of a high building would burn through the floors and set light to the whole building. Experiments took place in Wooster's back garden and appeared a clear failure. Wooster took me on one side and said, whatever I did after graduating I should not take up research. Actually I got the right result: bombing during the war showed the report from Spain was quite wrong.

The Anti-War group always tried direct experimental approaches but when considering shelters against high explosive that was not possible. We opposed the cheap government surface shelters and advocated deep concrete shelters like the plan worked out by the Labour Council for the borough of Finsbury.

What sort of lesson does all this have for us now? The materialist interpretation of history certainly led us correctly to oppose the pure science idea. But our ideas about planning science were simplistic and scientistic. Of course Bernal was right at one level. Although science in the 30's was very small scale and much more dominated by outstanding individuals than are the armies of research workers today, Bernal saw the potential advantages of organising research. His ideas were taken up with much success during the war and afterwards. But, in a way, Polanyi was right and he is now read sympathetically by some Marxists. The problems of science are more subtle and complex than the Marxists of the 30's had realised. They did not foresee the environmental crisis and the growth of anti-science attitudes. I suppose the turning point was about the time I left Cambridge and went to Birmingham in 1939 to do a Ph.D in the Physics Department and stubbed my foot against a sack of uranium oxide in the H.O.D. office. The fission bomb was beginning, science planning was going to be really big and 300 years of Baconian optimism was coming to an end.

The most positive element in the 30's movement was its Popular Front aspect, where a common enemy, Fascism and war, was identified clearly. The problem was seen to transcend political differences and join Socialists, Liberals and even Conservatives in a broad movement. We got that both in A.S.W. and in the C.S.A.W.G. But because of the nature of Fascism, the anti-war movement of the 30's could not succeed, and the radical scientists ended up in the war against Hitler. What we see now is a new Popular Front emerging in the Nuclear Disarmament Movement, and it is of course essential that this Movement succeeds. It has been commented that the recent Hyde Park Demo was bigger than the Popular Front Demo's against Fascism in the 30's. Socialists see, of course, that Socialist ideals are not likely to rise unscathed from a nuclear war.

It is good to see the CSAWG is reborn in SANA (Scientists Against Nuclear Armaments), the Medical groups against nuclear war and the Russell Campaign against Chemical and Biological war.

Maurice Wilkins

"... to a great majority of those present the standpoint consistently adopted by the speakers was a novel one, not easily grasped by a British audience of confused philosophical outlook."

Hyman Levy, 1937

The Climate of Breakdown:

The Background to Radical Science in the Thirties

The radical movement of the thirties was born out of the most severe economic crisis the world had yet seen, a depression which led inescapably to political crisis and the threat of war. The slump that began with the New York stock-market crash in 1929 cut production in the United States by more than half, and soon involved every capitalist and colonial country in the world. In 1931 came Japan's invasion of Manchuria, in 1933 Hitler's rise to power. As one who was not a scientist, but was involved in the left-wing movement of the time and has done some work on its history since, I want to try to suggest something of that context, which may help in understanding the nature of the movement itself.

The *Science at the Crossroads* conference took place in London in mid-July 1931. Within a month the Labour government had resigned, and Labour's Prime Minister Ramsay Macdonald was forming a Government with Conservative and Liberal leaders. The resulting demoralisation and disillusion with official Labour lasted for years, as I remember from my own experience, affecting not only its traditional working class supporters, but also those intellectuals who had hoped for some kind of new policy to counter the world capitalist crisis. Instead, reductions in employment payments and real wages, demanded by the employers to put the economy back on course, led on, after the return of the National Government, to further all-round cuts in social services, the family means test on the unemployed, cuts in education, teachers' salaries, scholarships, grants to scientists and expenditure on research. DSIR grants to junior research workers were cut from £140 to £120 in 1931, and had still not been restored in 1937. Meanwhile unemployment went on rising, reaching 3 million in 1933, and never fell below one million throughout the decade.

The issues seemed in some ways clearer and simpler in the early thirties than they do now. "Poverty in the midst of plenty" was a literal and visible fact. For this was plainly man-made disaster, the planned and organised destruction of production and productive capacity on a larger scale than in any previous crisis. Wheat and coffee were burned to keep up prices, potatoes ploughed into the ground, oranges dumped in the sea, milk poured down the drains, while children sickened for lack of vitamins which new research had just shown to be essential. Two-fifths of British shipbuilding was shut down to allow the rest to make a profit; steelworks, cotton-mills and pits were closed. The old industrial centres became Distressed Areas, full of ghost towns and (literally) hungry people – for the dole was too low, by any scientific standard, for a family living on it to be adequately nourished. And in many industries wages for those in work were so low that half the nation was living below the nutritional level

necessary for full health, as Sir John Boyd Orr demonstrated in his famous survey *Food, Health and Income* (1936).

The relative deprivation and poverty of the British working class was far more severe, and the contrast in life styles between middle and working class much sharper than it is now, with no national health service, little free education beyond 14 years, no child allowances, no cheap milk in schools, no student maintenance grants except for a tiny minority of scholarship holders.

It was widely felt, and not only on the left, that the capitalist system of production had broken down and could perhaps never be restored, at any rate in its old form. One could quote writers as varied as Harold Macmillan, T.S. Eliot and G. Lowes Dickenson in support of this view. In this situation Soviet ideas of economic planning, based on production for use and not for profit, made a powerful appeal to at least some intellectuals – especially to the younger scientists who saw no prospect that capitalism would ever allow science to realise its potential for increasing human happiness. Thus radical scientists, some of whom had attended the 1931 congress, among them J.D. Bernal, J.B.S. Haldane, Joseph Needham, Hyman Levy, P.M.S. Blackett and others, became active campaigners for a socialist way out of the crisis and the socialist organisation of science. Discussion of planning indeed went on much more widely, but Liberal, Labour or American New Deal schemes for a planned economy *without* serious encroachments on the power and wealth of the capitalists did not impress this group, and like the Wellsian dream of "technocracy", were denounced as illusion or even "shamefaced Fascism"![1]

For the Left, both in the Labour movement and among intellectuals, the Soviet Union represented the first great practical break-through to a better economic and social order, which it was the task of socialists elsewhere to support and defend. It was not then, a great power, let alone a super-power, but an industrially weak and under-developed state, impoverished by war, whose very survival against capitalist pressure was still in question. Yet its early constructive achievements against terrible odds, the importance attached there to science and education, and the optimism of its vision compelled respect, as did its internationalism when it was the only country prepared to sell arms to the Spanish Republic. Not surprisingly, then, there was a tendency to equate the Soviet Union with all that socialists hoped from a victorious revolution. The shortages, low living standards and intellectual restrictions were not ignored, but seen as the result of technical backwardness arising from Tsardom and from the devastating wars of intervention backed by the Allies. Continued encirclement by more or less unfriendly capitalist powers made the shortages of skilled manpower and materials difficult to overcome. Bernal, for instance, noted in *The Social Function of Science* that it was hard for Soviet science, despite its originality and the resources currently devoted to it, to develop the necessary critical spirit, partly because of the international isolation created by the long state of siege, and the political and financial barriers which still kept it to some extent cut off. But it was generally assumed that given a period of peace and freedom from outside threats (which as we know never materialised) defects of this kind would be overcome as the economy grew stronger and more secure. And because the capitalist press had always been hostile to the U.S.S.R., ignoring its successes and exaggerating

its failures, reports of the extent of Stalinist repression in the later thirties were simply not believed on the Left, though individual cases of injustice were known about and sometimes contested. This helps to explain, though not to justify, an element of idealisation and unwillingness to question Soviet policy which may now seem difficult to understand.

It was the Nazi take-over, however, which began to change political attitudes among much wider circles of scientists and intellectuals. It's difficult to convey now the shock and horror that followed Hitler's advent to power in 1933. Germany, after all, was considered the most advanced European country in terms of science and culture; most research workers had some kind of contact with it. Yet here they were burning the books, driving out Jewish and liberal scientists and artists, jailing and killing Communists and Socialists, and threatening aggressive war to conquer "living space" in Europe for the master race. The arrival of thousands of refugees, homeless and jobless, brought home to relatively sheltered British scientists and academics that the future of science, indeed their own personal future, was in danger. This didn't lead uniformly to politicisation – some leading figures arguing that science could best be safeguarded by ever stronger denials that it had anything to do with politics. But many others, whatever their political or social outlook, felt the urgent need to do what they could to resist the advance of Fascism and the drift to war.

On the Left, and among Communists especially, Hitler's success and his brutal smashing of the working-class movement forced an agonising reappraisal of past sectarian divisions. Was it really true that Fascism was just one more inevitable stage of capitalism, which could not be defeated except by socialist revolution? Unity of all anti-Fascists in Germany – Social Democrats, Communists, Liberals – could probably have kept Hitler out: why had unity not been created? It was now argued that Fascism had to be and could be stopped, and existing democratic rights defended, even where there was no basis for advancing directly to socialism. Moreover it was in the fight against Fascism that people would learn to oppose the system whose breakdown gave rise to it. This change in thinking, towards an anti-Fascist united front, was already developing on the Left in Britain and in France in 1934; it was pressed in the international Communist movement by the Bulgarian Dimitrov (not as is often suggested, by Stalin, who in fact argued against it), and was fully publicised at the Seventh Congress of the Communist International in 1935. For students and intellectuals, it meant working to bring not just a small revolutionary socialist minority, but thousands, perhaps the majority of their own kind of people into action for their interests and democratic rights against Fascism and alongside the working class.

Meanwhile it was the danger of world war that began to dominate people's minds. Fear and hatred of war was very strong in the British Labour movement, many of whose leaders had refused military service in 1914-18. Younger people had read the war memoirs of Graves, Sassoon and Remarque while still at school, and were determined not to be involved in another slaughter for nationalist and imperialist ends, as witness the Oxford union vote in February 1933 that "this House refuses to fight for King and Country." But after Hitler's take-over, many felt that traditional pacifism and war-resistance would not be enough. Significantly, the 1935 Peace Ballot, a voluntary poll in which over 11 million people in Britain registered their opinions, showed a majority for collective security through the League of Nations, supporting economic and even military sanctions to deter an aggressor. And after 1935, when Mussolini invaded Abyssinia, although pacifist feeling (organised nationally in Dick Sheppard's Peace Pledge Union) remained strong, the Labour movement was committed to international collective security as the way to prevent world war. This would of course have entailed a defensive alliance of Britain, France and the USSR.

However, as Hitler and Mussolini broke one treaty after another without any determined opposition from the British and French governments, the feeling grew that British ruling circles were not seriously concerned to resist Fascism, and were indeed to some degree in collusion with it. Some of the extreme Tory Right, such as Lords Londonderry and Astor, publicly sympathised with Hitler's anti-Bolshevik crusade, were not outraged by his anti-semitism, and looked on the Nazi regime as a valuable bulwark against Communism in Europe. Even his expansionist aims need not be discouraged as long as they could be directed to the East rather than the West. At the time it was not clear how widespread such attitudes were, and the Left (Claud Cockburn's *The Week*, for instance) was accused of exaggerating them without proof. But our access now to information that was then secret, in Cabinet minutes and diplomatic dispatches, ministerial diaries and letters, shows conclusively that the suspicions were more than justified. British government acquiescence in Fascist aggression against Spain, Austria, Czechoslovakia (leading to the debacle of Munich), was not the result simply of military weakness or inefficiency, or pressure of pacifist opinion in the country, but represented a conscious, deliberate establishment and class policy.

To quote but one example among many, Foreign Secretary Halifax, visiting Germany in 1937, thought Hitler "dangeous but sincere" and the regime "absolutely fantastic." He "liked all the Nazi leaders very much," and could not doubt they were "genuine haters of Communism." It was "essential for us to get along with them" and on his return to London, he proposed to ask the newspaper proprietors to stop Low and Dyson drawing such cruel cartoons of Hitler, and persuade English Christian leaders not to protest about his harassment of the German Church.[2]

I wish I had the space to cite more examples of this kind of thinking. Those interested should consult Maurice Cowling's *The Impact of Hitler* (1975) which documents the attitudes prevalent in our German and French Embassies, among the owners and editors of such papers as *The Observer* and *Times*, *Express* and *Mail*, as well as in the Chamberlain Cabinet itself. It was no wonder if anti-Fascists felt they were living in a climate of betrayal by these "guilty men." No doubt Stalin too concluded that an Anglo-Soviet alliance was simply not on.

Because of this, the responsibility for arousing resistance to Fascism and exposing its real nature fell on the Left – on the labour movement, the intellectuals, the students. Little enlightenment about the barbarities of Hitlerism was given by official or right-wing sources, even during the war, so that to many people the films from Belsen in 1945 came as a complete shock. However, the left in the thirties did much to provide this enlightenment, for example through the documentary *Brown Book of the Hitler Terror*, Left Book Club books like Robert Brady's *Spirit and Structure of German Fascism*, Bernal's exposure of racialist Nazi science in *The Social Function of Science* (pp. 210-221), the showing of films and pictorial exhibitions. Many scientists were involved in campaigns to help refugees (who were mostly prevented from working at their professions); to refuse academic contacts with the universities that had persecuted their colleagues; and later to provide medical aid, food and arms for the Spanish Republicans.

Scientists were more conscious than most of the horrors a second world war would involve. "The millions who suffered in the last war are aware that to a large extent their sufferings were directly due to scientific developments and that science, far from having brought benefits to mankind, is in fact its worst enemy," wrote Bernal in 1939. Younger radical scientists increasingly regarded the application of science to war as the worst prostitution of their profession. Yet a complete boycott of war research, even if it could have been organised (which it couldn't), would merely have left democratic countries at a disadvantage compared with fascist ones. Scientists disagreed as to what they should do: participate in war

research on a 'non-political' basis, boycott it on pacifist grounds, or – probably the most widespread attitude – decide whether to participate according to the policy the arms were designed to serve. Both pacifist and non-pacifist scientists could however agree in warning of the nature of modern war and the need for more research and expenditure on protecting the civilian population. Hence the work of the Cambridge Scientists' Anti War Group, exposing the inadequacy of official air raid precautions, which incidentally helped to spread the idea that defence was too important to be left to the high-ups.

The left-wing movement among scientists and intellectuals in the thirties had above all a practical, urgent character. There was as yet no basis for seminars and debates between different schools of academic Marxists – indeed it was a fight to get a hearing for Marxist ideas in the universities at all, since they were more or less excluded from official courses in history or economics.

Scientists felt the need both to organise themselves and to make contact and join with the 'forces outside science' which were campaigning for change and would be decisive in carrying out the ideas of science into action.[3] On the practical side, I think of the work of F. Legros Clark on the Committee against Malnutrition, done in close co-operation with the National Unemployed Workers' Movement; of scientists at the Cavendish Laboratory inviting railway trade unionists in to discuss their work; of the medical students who dressed the feet of the hunger marchers passing through Cambridge in 1934, some of whom later helped to form the first British medical units with the Spanish Republican forces. I think too of contemporaries and friends who joined the International Brigade, like John Cornford or David Bill Alexander, and Len Crome; and Guest, a young mathematician who had spoken in the discussion at the original 1931 conference, and was killed fighting on the Ebro; and other scientifically-trained volunteers who survived to carry on the fight, for instance Frank Lesser, Haldane on the Madrid front advising on air-raid defence.

The urgency is reflected too in the kinds of books and theoretical writings produced by radical intellectuals in those years, reaching out to non-specialists and to thinking workers on a remarkably wide scale. The Left Book Club at its peak in 1938 was selling 60,000 copies of its monthly choices; *Fact*, a left-Labour publication, some 8,000 a month (not to speak of articles in the popular press).[4] Left Book Club books were discussed at meetings of its local groups, often with authors as speakers. The titles included not only economics and politics, but popular science, both theoretical and applied – Haldane on air-raid precautions, Hyman Levy on Marxist philosophy, Doctors McGonigle and Kirby on Poverty and Public Health, A.J. Clark on Patent Medicines, Bill Beck and Harry Collier on the foundations of chemistry and biology. Here and there special LBC Science discussion groups were set up in working-class areas such as the East End of London.[5] A particularly successful fusion of trade union, economic and scientific expertise was *Britain without Capitalists*, produced by anonymous authors for the C.P. in 1936. Reading these writings now, one is impressed by the directness of language, style and tone, even where difficult theoretical or technical questions are involved, and by the weight of factual evidence assembled to support the arguments for the ordinary reader. Taken as a whole, they brought both confidence and vision to the wider movement. It's an approach from which we, with our more complex problems but with much greater resources of scientific skill and knowledge on the Left to draw on, could still learn something.

There was much, certainly, that the radical intellectuals of the thirties failed to understand or foresee. The capitalist economic system turned out to have far greater powers of recovery and adaptation than they expected, allowing a massive expansion of science and technology and even long spells of full employment. On the other side, not only capitalism but socialism under duress proved capable of terrible repression.

Moreover, their writings of the thirties naturally emphasise the immediate issues that seemed most urgent then – inadequate expenditure on medical and biological science, for instance, rather than the problems of population, ecology and pollution arising from its rapid commercial application after the war. Nevertheless they seem to me far less simplistic and 'techno-economist' than they've sometimes been made out. Anyone who takes the trouble actually to *read* crucial texts like the *Social Function*, rather than rely on a few quotations taken out of context, will find radical scientists already deeply concerned about the perversion of science to damage humanity and the need to democratise the workings of science at laboratory level. It is consistent with their whole record that after the war radical scientists of the 1930's were among the foremost in campaigning against the use of nuclear weapons. What matters now, however, is not to prove the pioneers were always right, but to tackle the problems they left unsolved with something of their energy and determination. For the real achievement of that generation was to begin to organise intellectuals against reaction and inspire them to place their knowledge and imagination at the service of working people.

Margot Heinemann

1. The phrase is Bernal's from the early thirties (article in *Cambridge Left*, Winter 1933, reprinted in *Freedom of Necessity*, 1949). See also broadcast by Blackett, reprinted in *Frustration of Science*, cited in *Social Function of Science*, 1939, pp.395-396.

2. From Halifax letter cited in Maurice Cowling, *The Impact of Hitler*, 1975, p.274.

3. *Social Function of Science*, p.385.

4. Haldane, Ritchie Calder, J.G. Crowther and others wrote brilliant journalism for the New Statesman, Discovery, Manchester Guardian, Reynolds', Daily Worker and many other papers.

5. See John Lewis, *The Left Book Club*, for further details.

The Political Agenda: Then and Now

We are here tonight to celebrate and reflect upon the beliefs and achievements of an earlier generation of scientifically-minded socialists. Could B.S.S.R.S. have picked a worse moment for this exercise? At first glance, I would doubt it; not if the Society were hoping to salve our collective political pride. For the contrast between the pre-war movement's achievements – whatever criticisms we might have to make of it – and our own, however positively we might rate them – could not appear starker. My job in this talk is to rub our noses in that difference and, paradoxically, make us feel better about it.

Let me begin, though, with an unheroic confession. I feel politically quite ineffectual at the moment, not to say unhinged. Many other socialists, particularly those of 'a certain age', undoubtedly feel this way, too, and for very good and all too familiar reasons. For a start we have witnessed in recent years a worsening in East-West relations and in Britain, a deterioration in the collective power and domestic living standards of working class men and women. Over the last ten years the number of industrial workers has shrunk drastically. Over the next ten years we are expected to cope with massive levels of youth unemployment. Yet despite the increasing unpopularity of the most reactionary Government since the 1930's, we find the labour movement's resources significantly reduced and/or dissipated. Meanwhile, 'the Left', however broadly or narrowly you define that political tendency, has already been culturally marginalized to the point where a Labourist form of McCarthyism could soon take hold. That leaves us with the great grey hope of my class Fraction, namely the headless, gutless and policyless SDP-Liberal Alliance. In short, as a young middle aged 'old Lefty', I've never felt less trendy. On the contrary, I find the socialist opposition's limited influence within the labour movement, here in 1981, distinctly uninspiring.

But in any of these respects were our predecessors that much better off fifty years ago? The answer is: 'undoubtedly No'. In 1931 international and domestic crises were at least as severe, the labour movement was just as weakened and divided and the extra-Parliamentary Left was, to say the least, in the doldrums. Indeed it was in the 1931 General Election that a breakaway group of right-wing Labour MP's made common cause with the Tories to form a National Government. The verdict of one historian on their campaign is worth recalling here:[1]

> Lacking any common programme, lacking any programme at all except a belief in their own indispensability, the various supporters of the National Government followed two courses: first, they confused the voter with their divided voices, proclaiming rival programmes and contradicting their allies; then they united in abuse of their Labour opponents and appeals to the voters' fears if Labour were victorious.

The coalition, it should be added, won a huge Parliamentary majority, which it then used to bring in a deflationary budget, including cuts in unemployment benefit, university finance and State-controlled programmes in science and technology. Is it any wonder, then, that J.D. Bernal – fresh from his exciting contact with the Soviet delegation to the History of Science Congress – should have felt so frustrated in his isolation and powerlessness as a socialist intellectual? The twenty-nine year old Bernal in fact described himself in July, 1931, as "intellectually free but socially totally ineffective". That is a phrase which still has a lot of resonance left in it. Accompanying Bernal's lament was his unfulfilled hope of becoming one day "a component part of a system where knowledge and action are joined for one common social purpose".[2]

Though Bernal's colleagues and comrades never attained such a state of socialist bliss, they did succeed in rebuilding and widening the constituency committed in the short run to modernizing and humanizing British capitalism and, in the longer term to achieving socialism. Their success was the product of four forces: 1) hard, patient and creative political work; 2) the existence of severe social and economic problems, which could be ameliorated by scientific and other means but without recourse to full-blooded socialism; 3) fifteen years of depression and war-time sacrifice which strengthened and focused both the working class's sense of its own political power and the legitimacy of asserting its needs over and against those of capital; and, finally, 4) a persistent and growing belief – very much underpinned by developments in the Soviet Union – that socialism was in the long run, economically at least, as viable and socially far more progressive than an increasingly barbarous capitalist mode of production. Unfortunately there is not time here to weight and inter-relate those factors. All I can say, very briefly, is how they bore upon the circumstances of British scientists in this period.

You have already heard something tonight about the political experiences and struggles of our predecessors. These have also been fully and, I believe, fairly summarized in my book *The Visible College*.[3] Of all their achievements and endeavours, I would say that the most significant and enduring one was their rejuvenation and expansion of the Association of Scientific Workers. The attempted unionization of technicians, scientists and engineers – within the university system as well as industry – was an audacious undertaking in the 1930's. That the attempt came off in the 1940's under the auspices of a T.U.C.-affiliated union, committed to leftist notions of a science for the people and led by Fellows of the Royal Society, has still to be explained adequately. Nevertheless that legacy of the pre-war movement's committment to scientific socialism is still very much alive within the A.Sc.W's successor, which of course is the 480,000 strong A.S.T.M.S.

The second and third factors making for the success of the pre-war Left also had their own scientific dimensions. In retrospect, the spaces for political initiative on the part of socialists, whether scientists or not, seem very great indeed. Unionizing new areas of work, not least research and development, was one example. Others included the fight against poverty and malnutrition, the exposure of State propaganda concerning civil defence, a variety of anti-fascist campaigns and opposition to rearmament. Progressive scientific workers were active on all fronts. In fact the weight of these and other initiatives by liberal as well as socialist scientists helped to prevent, during the Second World War, any widespread identification of science with destruction. On the contrary, the notion that scientific research could play a positive role in post-war reconstruction was both an ideological resource appropriated by the labour movement and a constituent part, albeit a subsidiary one, in the wartime rise of working class morale and self-confidence.

One final, indeed essential fillip to the progress of the post-war Left was a working model of socialism; somewhere to 'centre' and fill out their identity as socialists. In these ways, and many others besides, the Soviet Union played a major and positive role in enlarging the theory, practice and, perhaps most important of all, the energies of left-wing activists – especially if they were scientists.

Our predecessors' introduction to scientific socialism Soviet-style could

not have been more dramatic or positive. Here was a socialism that literally fell out of the skies when in July, 1931, a Soviet delegation led by Bukharin was dispatched by plane from Moscow to London.

"His arrival was unheralded. He left Moscow on Wednesday and flew here via Berlin and Amsterdam ... no one could look less like a revolutionary ..." *News Chronicle*, **July 6th 1931**

"Moscow Hater of Britain Now in London Stalin's notorious agent Bukharin Shows His Colours ... sneered at religion ... extolled the Five Year Plan ... *Daily Mail*, **July 4th 1931**

What happened at the Congress – the introduction of dialectical materialism into polite scholarly discourse; the shocking arguments of Boris Hessen connecting Newtonian physics with grubby technologies and Christian apologetics; the delivery to dazed antiquarians of detailed reports about the progress of Soviet electrification – has been described elsewhere.[4] However, whatever the intellectual merits and influence of the Soviet speakers may have been, they were all notable for the passion and idealism which they brought to bear upon the theme of science's unity with socialism. Just listen to Nikolai Bukharin's opening address in the Science Museum:[5]

> One can feel with one's hands, as it were, how the requirements of the rapid and intensive growth of the U.S.S.R. imperiously dictate the solution of a number of technical problems, how the solution of these problems, in its turn, dictates the posing of the greatest theoretical problems ... One can feel with one's hands how the development of socialist agriculture pushes forward the development of genetics, biology generally, and so on ... And all the poverty of the idea that the 'utility' of science means its degradation, the narrowing of its great scope etc., becomes crystal clear and apparent. Great practice requires great theory ... The relative disconnection between theory and practice characteristic of captalism is being eliminated. The fetishism of science is being abolished. Science is reaching the summit of its social self-recognition.

(They don't make Socialist Industry Secretary's like that any more, either here or in the Soviet Union).

A still more moving form of this same message cropped up at the end of Boris Hessen's seminal paper on Newton, when he declared[6] that

> The teachings of Marx and Lenin have been incarnated in life. The socialist reconstruction of society is not a distant prospect, not an abstract theory, but a definite plan of great work being accomplished by the population of one-sixth of the world's globe. And as in all epochs, in reconstructing social relationships we are reconstructing science.

I have quoted at length from the Soviet papers to give ourselves a taste of how and why the inspiration of "an actually existing socialist society" was so important to the confidence and elan of the pre-war Left. This was certainly true of many radicalized scientists who came to believe, as did Hessen, "only in socialist society will science become the genuine possession of all mankind. New paths of its development are opening before it, and there is no limit to its victorious advance, either in infinite space or in everlasting time".

Who among us would dare to proffer such hopes in 1981? Why should there be such a gap in expectations between then and now? Part of the answer is simply that we lack some of the crucial ingredients which gave the pre-war Left its momentum and put "history on its side". Neither we nor open-minded non-socialists, for example, can find either in the Soviet Union or anywhere else a model of socialism that both 'works', in its own terms, and embodies in everyday practice principles of mass democracy and worker self-management. In consequence our credibility and our hopes as advocates of a societal transformation have been diminished in roughly equal measure. The constituency for socialism has also been reduced, not least in the working class which has detached from the labour movement. Likewise the political spaces for making reformist demands as part of the onward march towards socialism have been closed up. For many of these demands have either been met or become dissociated from a specifically socialist political agenda. Hence whatever we as socialists conceive to be'the next step' is bound to appear that much more revolutionary, difficult and utopian. Finally, even the ultimate object of our struggle has, for many, been rendered more cloudy and complex as a result of issues posed by the women's movement. For these reasons alone, I do not see how we could expect our movement today to be politically effectual in ways similar to those of our predecessors. The context and agenda of socialist politics have changed that much.

What, however, we do share with our pre-war counterparts is a capacity for hard, patient and sometimes creative work. And while it is true that – for the moment – history does not appear to be on our side, we may be able to claim as a mobilising force a growing desperation arising either from the bomb or unemployment or both. Of course many people in the 1930's felt aggrieved if not desperate about the prospect of war and the misery of the dole queue. Nevertheless the cutting edge of these respective issues in the 1980's will, I believe, sharpen ever more quickly and to an unprecedented degree. On the one hand the spread of nuclear weapons is posing a near-universal species threat. On the other, the expectations and culture of today's unemployed, particularly those under the age of thirty-five, has already produced threats to 'society' of quite a different order to those posed in the pre-war period.

So while today's Left has in certain ways been marginalised, and some of our demands been deemed outrageous, the increasing desperation fostered by the nuclear arms race and localized mass unemployment may create spaces for new political alliances and novel forms of explicitly socialist politics. In the process we may recapitulate, albeit on a different level and in different contexts, the progress of the 1930's scientists' movement. This can already be seen in the formation of groups like Scientists Against Nuclear Armaments. In the area of unemployment, by contrast, we will have to go on devising anew such initiatives as alternative workers' plans, socialist technology strategies, new technology centres for the unemployed, etc.

To my mind, however, the most significant question is the extent to which, like our predecessors, we attempt to develop these politics within our natural constituency, i.e. among scientists, technicians and engineers, especially those in industry. Unlike the Thirties, any attempt to reach scientific workers will be mediated to a significant degree by well established trade unions representing sectional/professional interests. Though there are numerous possibilities for collaboration – e.g. the design of alternative products and processes, campaigns against the restructuring of R & D and its associated labour processes, etc – there are also difficulties in how we negotiate differences between our accumulated theories/practices and theirs. But should the radical science movement succeed in working out a new relationship with organized scientific workers we shall have some grounds for hoping that history is again on the side of socialists, and socialists are again committed to the creation of a new science for the people.

Gary Werskey

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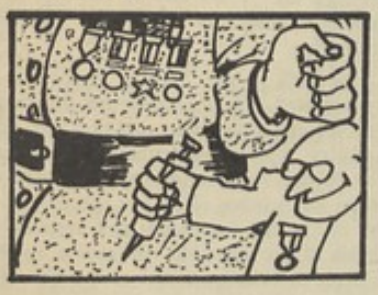
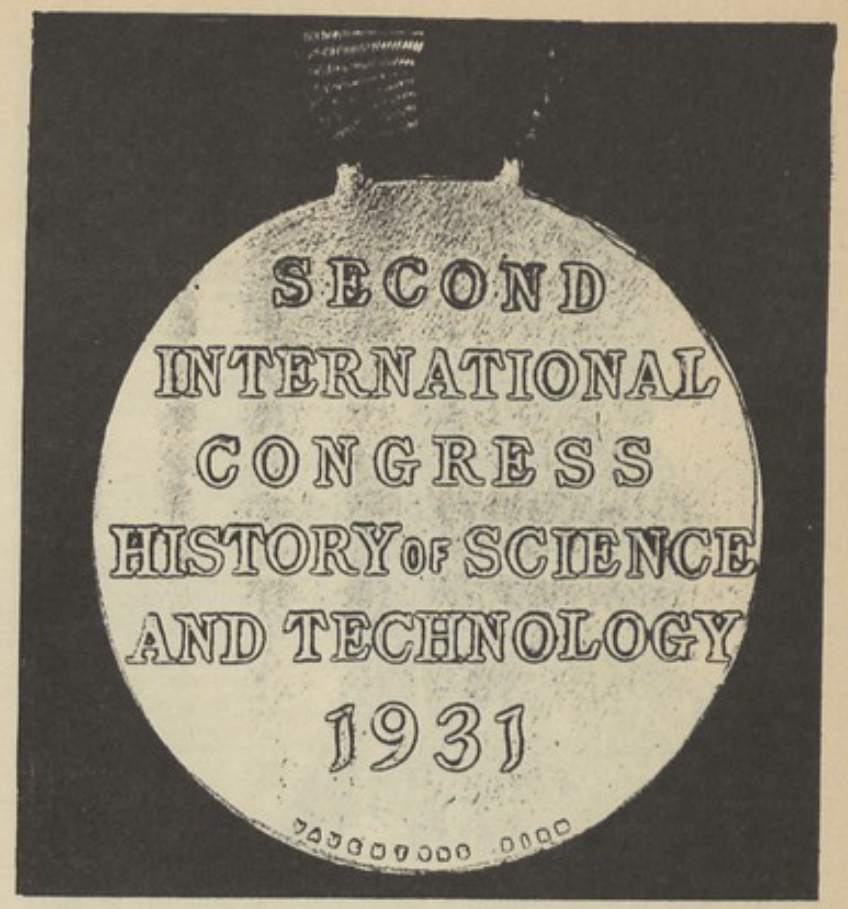




The Two Bernals:
Revolutionary and Revisionist in Science

Desmond Bernal emerged as one of the central figures among the left scientists of the 1930's, to exert a major theoretical influence on the socialist conception of science. He joined the Communist Party in the 1920's, but the papers of the Soviet delegation to the 1931 Conference were undoubtedly a key contribution to the development of his thoughts on the social relations of science. Our intention here is to examine Bernal's role in the politicisation of science and scientists – the social function of science movement, or the social responsibility in science movement, call it what you will, but effectively the 1930's antecedent of today's radical science movement – in both writing and political practice. The thesis which we will advance is that there were two Bernals, the one a revolutionary and direct forerunner of our current movement, the other hopelessly compromised by his loyalty to the Soviet Union and to an increasingly oppressive techno-economism.

If one re-reads today the corpus of Bernal's writing, from *The World the Flesh and the Devil*[1] of 1929 through to the valedictory interviews and the essay *After 25 Years*[2] of the late 1960's, there is a remarkable homogeneity and coherence of themes: a continuing, unshakable optimism about what science could do for human liberation once free from the shackles of capitalism; the belief in the essential possibility of a rational world, a world planned for the benefit of humanity and a certainty that scientists as one category of intellectuals have a task in the creation of this world, which if only they could cast off the constraints on their thinking imposed by a fragmented capitalist educational system, they would see and embrace.

To today's radical science movement none of these propositions is so self-evident; its doubt and concern about the very nature of the scientific enterprise inhibits such optimism. Where Bernal saw communism as an inevitable consequence of science, today's movement sees sociobiology; where Bernal lauded rationality, today's movement sees the instrumental rationality of capitalism with science as its servitor; where Bernal saw scientists as intellectuals, today's movement sees them as a divided stratum of elite managers on the one hand, and proletarianised detail labourers on the other. Part of our purpose here must be to examine this disjuncture between old and new left and its significance.

The radicalization of the 30's scientists

In a very real sense Bernal's optimism and enthusiasm, which must be an essential ingredient of any successful revolutionary change, seems now a product of a particular positivistic tradition of early 20th century English socialism. As Benjamin Farrington has remarked, Bernal's strengths were not those of the philosopher; his marxism was almost an extension of his natural science:

> My impression was that at least half of the Marxists whom I met (in the 1930's) were scientists. But I had the impression also that their Marxism was a peculiar brand. They seemed to be under the impression that Marxism had originated from scientific sources – I mean the physical sciences – and not to be so much aware of the social and philosophical background ... In England chiefly also among scientists, I found a

Monument to the Third International in Moscow, 1920.
Tatlin's Tower, which was to have been made of 'iron, glass and revolution' - in the words of a contemporary admirer - would have loomed a hundred metres above the Eiffel Tower.
His model, painted red and made of wood, metal fittings and paper, was never built. Inside a conical area, there were to be four chambers for various government functions. These chambers would turn at rates varying from one revolution a year to one a day. The meeting areas were to be subject to constant agitation, rather than for fixed functions or passive deliberation. A chamber at the top of the tower would have turned out news for the proletariat, with facilities for movie projection and sky writing.

complete optimism about Marxism and science. It seemed to them, and I heard the actual words from them, that Marxism was the theory which gave science its opportunity. And it seemed as if science and Marxism had absolutely been married to one another ... [3]

Yet is was precisely this which was Bernal's strength; the strength of a positivistic tradition taken and made over in the interest of working people and of socialism. And above all the understanding that making a revolution was not merely a business of getting one's theory right, or influencing the '2000 who really mattered' in the elite Fabian conception, but of practice, of struggle at all levels, in the streets and factories as well as the laboratories and committees.

Bernal's crucial role among left scientists of the 1930's must be seen in the context both of his own development over the period and the general political and social situation. The global background is well known: the depression and massive unemployment in all the capitalist countries, the emergence of Nazism in Germany and fascism in Britain, and the then apparently clearly shining beacon that the Soviet Union represented. In this context, and with the most recent of a long line of capitulations of the Labour Party leadership (Ramsey Macdonald) there for all to see, it felt to many that the Communist Party was the only place for socially aware intellectuals, as well as workers, to be. Bernal himself saw this plainly. As Perry Anderson [4] points out, for some of the artists and literary figures their affiliation to the Communist Party was to prove a transient, romantic attachment – an attachment that admittedly led many to their deaths in Spain. But Anderson fails to recognise that for many of the left scientists, the commitment was to last much longer, even if their socialism was to become translated into more uncritical enthusiasm for the Soviet Union. Why? Perhaps Farrington is right, and 'scientific socialism' spoke particularly to them; perhaps the influence of the leading figures was more powerful. But also, socialism and communism seemed to offer something concrete to scientists which it could not so straightforwardly provide, other intellectuals. The enormous growth of and respect for science in the Soviet Union, its full incorporation into the political structure of the state (even now there is a high proportion of scientists and engineers on the CPSU Central Committee), enthusiastically proclaimed by Bernal in article after article throught the 1930's (and of course, long beyond) seemed to be a glimpse of the possibility of a promised land.

For the situation of science in Britain was truly impoverished. Jobs were scarce and poorly paid (£150 -450 p.a. for a researcher or lecturer) at least until the professorial level (£1000-2000 p.a.). Research funds were scarce in universities, almost non-existent in industry. In a non-scientific society, science education was correspondingly poor, with anachronistic syllabuses (Bernal was to point out that science syllabuses in schools mainly concentrated on work that had been done prior to 1810, with a brief excursion in chemistry towards the 1880's) and was, apart from biology, then appropriately called 'Nature Study', virtually non-existent for girls.

It was against this background that critical voices in science had begun to be heard from the 1914-18 war on, demanding the professional recognition of scientists. Hence the emergence, in 1917, of the National Union of Scientific Workers (which was to backtrack and change its name in 1927 to the Association of Scientific Workers and critical concerns firmly at its masthead. It was to provide a natural focus for part of the work of the left scientists.[5] Bernal was active in A.Sc.W. through the 1930's and by 1938-39 was himself chairman of the Union Executive.

As well as the A.Sc.W. there was a determined effort to capture the main ground of scientific opinion in the country, and in particular to insist that the British Association for the Advancement of Science (BA) put the question of its social functions and responsibilities of science onto its agenda. In the battle to win over the BA., the left scientists had a major ally in Richard Gregory, who as editor of *Nature* used its columns to open up the science and society debate. Gregory was no marxist, but for much of this period there was a relatively easy alliance between liberal reformists – such as Gregory and the biologist Julian Huxley – and the left. It was this combination which resulted, eventually, in a characteristic concession by the B.A. It opened a special section, Section X, for the discussion of the social context of science, thereby neatly encapsulating and potentially incorporating the radicals.

A crucial mobilizing issue for the left scientists was the growing threat of fascism. The tasks confronting them ranged from the ideological to the practical. Within the ideological there was an increasing necessity, as the 1930's wore on, to expose and demolish the claims of Nazi pseudoscience and its Aryan racist genetics. Practically, there was the struggle to rescue scientific victims of Nazi persecution. From 1932, these and related issues were the concern of the Cambridge Scientists Anti-War Group (CSAWG), with which Bernal was actively involved.

The Social Function of Science

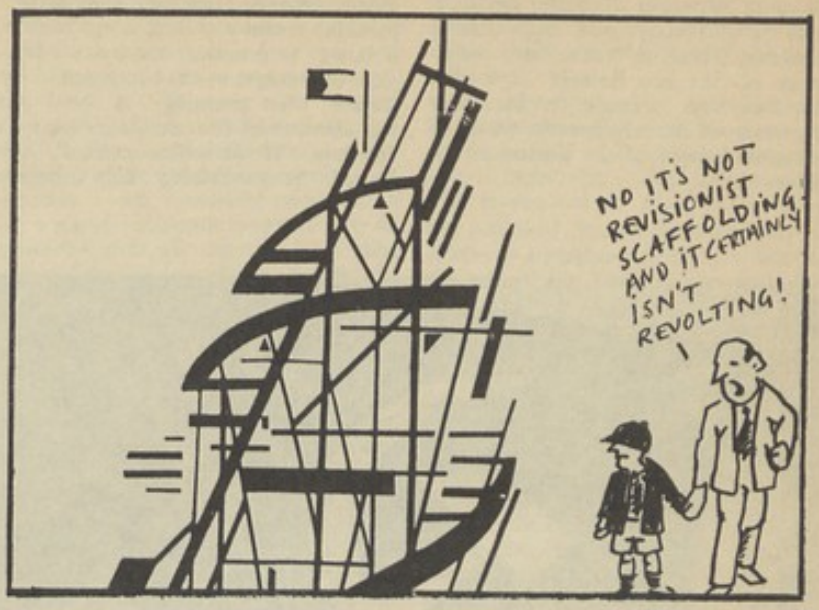
These concerns then, of the unionization and politicization of scientists and consciousness raising about the context and consequences of their scientific work, formed the major element of Bernal's political practice over the decade. But Bernal's outstand-

ing achievement during this period was theoretical; the summary statement of the problematic state of science under capitalism and a prophetic attempt to describe the goals of a science for the people, represented, in its final form, by *The Social Function of Science.*

The Social Function of Science seems to have taken the best part of the decade of the thirties to mature. During this period there were two major additional theoretical inputs into the thinking of left scientists. The first was the new perspective on the theory of the growth of science introduced to Western scholars at the International Congress of the History of Science of 1931. Hessen's path-breaking paper on the *'Social and economic roots of Newton's Principia'* made a profound impact on the left scientists attending the meeting. Bernal was at the conference, and acted as Bukharin's London guide during the meeting. Bernal's subsequent writing on science was to show the influence of both Bukharin and Hessen. Hessen's method of relating scientific development of the economic and social order and the demands of the rising bourgeoisie was one which Bernal was to draw on in all his later writings. The Bukharinist influence was to outlive Bukharin, as he was the first leading Communist to become, after his trial and execution in the purges, a non-person for Bernal.

The second theoretical input was the rediscovery of dialectics. Engel's major attempt to describe the meaning of dialectical materialism, *The Dialectics of Nature*, was not available in English until 1940. But the earlier *Feuerbach* and *Anti-Duhring* were available, as of course was the much cruder *Materialism and Empiriocriticism* of Lenin. Bukharin's contribution to the 1931 conference emphasised the importance of dialectical materialism and Western marxists were soon to respond. The French biologist Prenant, the Scottish mathematician Levy, and J.B.S. Haldane were to write consciously "dialectical" accounts of science and the scientific method.[6] Bernal's essays in this direction were fewer – Farrington's comment quoted earlier applies – and he seems to have been happier in the positivistic English tradition to which we earlier alluded.

These theoretical inputs: of Hessen's social determinants of science; and of dialectics of nature; together with the practical experience gained through the political organizing of the 1930's, crystallised the earlier drafts into *The Social Functions of Science*[7]. In the introduction, 'The Challenge to Science' Bernal emphasises the critique of science; its implication in technological unemployment, in weaponry, in the sense of loss of individual security both in a psychic and a practical sense. Faced with these threats from science, there has been a revolt from reason, a retreat into irrationality and idealism of which



"I can say that the inspiration for my own work and that of many others in science, notably Haldane and Hogben, can be traced definitely to the visit of the Marxist scientists to the History of Science Congress in 1931. We did not understand all they said, in fact, I now suspect they did not understand it entirely themselves, but we did recognise that there was something new and with immense possibilities of thought, and that, as it were, the whole range of our understanding could be multiplied by working out the suggestions they offered."

J.D. Bernal, 1939

fascism is but one example. Yet this idealism is no substitute for the creative use of science; science is power, the scientists as workers must use it in the interest of humanity. The emphasis on technological unemployment is even more relevant now, in the face of the microchip in the context of capital's restructuring of the economy than when S.F.S. was written.

We don't want to push the parallels too far. Largely missing from Bernal's account are today's knowledge of the threats of a science which generates industrial pollution, health hazards, nuclear weaponry. His enemy was too little science; for the new left it is often too much capitalist science. Above all S.F.S. is silent on the ideological role of science under capital, on the implied continuation of the hierarchy of expertise even into a post-revolutionary science, on the sexism of science. These silences are no more than a reflection of Bernal as a man of his time, they imply and are of piece with, his own lived practice and his explicit acceptance of the theory of the party with its vanguard theoretical (scientific in Althusser's sense) relationship to the masses. For the new left in general these issues have moved to the centre of the agenda – they are not mere peripherals to be dealt with 'after the revolution'.

The first section of S.F.S. is a pioneering attempt to describe the existing state and frustrations of science in Britain and throughout the developed world. It was this strand within S.F.S. that was to provide the basis for the technoeconomist interpretation of Bernal in the 1950's and 1960's. The second section of S.F.S. is programmatic – what science could do. It calls, even under capitalism, for the tenfold expansion of spending on science to one per cent of GNP (by the 1960's developed countries were spending 2-3 per cent of GNP on science). It calls for the restructuring of the science curriculum at all levels, for the reorganisation of the structure of research, including the reorganisation of laboratory practice itself. Laboratories should neither be autocratically run by a director nor left to anarchy, but by a regularly meeting laboratory council involving all the scientific staff, including technicians. Hierarchization of science should be prevented not by denying expertise but by making progress through the ranks (from "lab boys" (sic) upwards) much easier and more flexible.

The rest of the book is a more euphoric account of what science could achieve for humanity once the shackles of capitalism had been thrown off. It concludes with the claim that science is communism, communism is science: a claim which can be read as simultaneously as incorrigibly romantic as the young Marx and as bureaucratically

heavy as Althusser. It is the simultaneous revolutionary and authoritarian intent of this claim that enables one to speak of 'the two Bernals'. If in this paper we wish to rescue the liberatory dimension of Bernal's agenda, we do so conscious always of the shadow of the other.

No account of S.F.S. would be complete without reference to the response to the publication of the book. It was of course widely seen as the manifesto of the left scientists, the most worked out statement yet of their position. The issue of 'planning in science' became a battleground between right and left. In particular the cytologist J.R. Baker (who re-emerged in the 1970's from relative Oxford obscurity with a book entitled *Race* which threw its weight behind the hereditarian views of Eysenck, Jensen and the most reactionary voices within the new right) issued what he called a "counterblast to Bernalism" in which he attacked the idea of any planning or 'dictation' in science. His understanding of what Bernal was arguing was primitive: 'I do know what he's after: he's going to tell me that I dare not ever again use gentian violet as a stain for my section, but from now on I shall have to restrict myself to methylene blue'. Bernal was able to respond merely by denying that planning meant any such thing; Science, if it was to progress, demanded a freedom of thought at the bench and in the library; what planning was about was the creation of the conditions for such freedom. 'If Bernalites existed', said Bernal, 'they certainly didn't believe what Baker claimed for them'.

Bernal's practical activity gave little time for reflection and development of the theoretical position of S.F.S., whilst the scientific mobilisation that the war involved transformed the actual situation of science in Britain and rendered many of the themes of the frustration of science raised in the book temporarily irrelevent.

Science in Two Camps

By the time the Second World War ended the ten-fold increase in science spending which he had called for in 1939 had been achieved; science was on the political agenda as never before, and the time should have been right to move both the theoretical analysis and political struggles to a higher level. What happened, as we know, was rather different.

To understand the fate of the left scientists' movement in the post-war world we have to have a clear recognition of the historical context in which the ideology of the Iron Curtain was deliberately constructed, and the world was split into what Stalin was to refer to as 'two camps'.

For left scientists this meant that political space was denied to them. Section X of the British Association, which in a less ideologically split world had provided an arena for left/right debate, became closed to the Marxists. The social democratic scientists who had hitherto quite often worked with the left, such as Huxley, were forced to choose between being silent or leaving the left and joining the constrained discourse of the right. Bernal himself, was excluded from the Council of the BA following reports of a speech he made in Moscow attacking the restriction on political freedom in the capitalist west. The plain truth is that it was no longer possible even for a natural scientist, FRS., to be a revolutionary and keep a toehold in the British establishment. Despite the potential of people to make history, they do so, as Marx reminds us, in circumstances not of their own choosing. And the circumstances of the post-war world were indeed singularly unpropitious for the optimism of the thirties scientists.

A key factor in the isolation of socialist scientists, and especially socialist biologists, was the Lysenko affair, which came to a head in 1948. Lysenko claimed that the theories of conventional genetics were bourgeois and idealist, and hence self-evidently reactionary, anti-Darwinist and anti-social. Despite the inadequacy of his scientific position, Lysenko was able to crush all oppositon by revealing that Stalin had administratively ruled in favour of 'proletarian science'. Soviet geneticists, faced with the administrative decision, enforced by terror, tht genes have no materi
enforced by terror, that genes have no material existence, found themselves either committed to acquiescence or to persecution.

There is no space here to examine the ramifications of this affair in detail. But the issues raised in the debates over Lysenko are not of mere historic interest; they speak to the core of the present concern over the relations of ideology and science, the making of a socialist science and indeed of the race/IQ and sociobiology controversies. Legitimite points could be made on both sides. The problem was that the closure of the debate by administrative means and terror in the Soviet Union and by exclusion by the cold war mechanisms in the West has done the gravest damage to marxist thought, irrespective of the twists and turns of the career of Lysenko himself from the 1930's to his death in the 1970's. The long night of revolutionary thought in the West had begun.

Science, the Peace Movement and Technoeconomist Revisionism

Politically restrained at home, Bernal turned his attention to the building of bridges abroad and circumventing the constraints and threats of the cold war. His work in the A.Sc.W. became directed towards developing the international World Federation of Scientific Workers. The successor to the CSAW Group was Science for Peace, founded in 1949, and many of whose activists were, in turn, to become involved in the educational work of the Campaign for Nuclear Disarmament in the mid 1950's. What was to be called the 'international peace movement' began to be built. In the aftermath of Lysenko and of the distancing from Stalin of the mid-1950's, grappling with the theoretical issues of the nature of a socialist science was discarded. Bernal's response was essentially the pragmatic one of leaving the past to bury the past. From the 1950's on, he never refers again to Lysenko, except in the context of *Science in History*,[8] whose successive editions in the 1950's and 60's gradually reduce the prominence given to Lysenko's claims. Bernal's uncritical eulogy of 'Stalin the scientist' which appeared as his part of the Stalin obituries of 1953, is the last reference to what he then described as 'the greatest figure of contemporary history', who was 'at the same time a great scientist' who 'combined as no man had before his time a deep theoretical understanding with unfailing mastery of practice ... his wonderful combination of a deeply scientific approach to all problems with his capacity for feeling and expressing himself in simple and direct human terms.'[9]

Of course Bernal should not have abdicated critical thought – anymore than those of us who more recently spoke of the Chinese or Vietnamese revolutions in similar, if less grandiloquent, terms, should have done. But if failing to appreciate at the time what was happening was understandable, failure to speak subsequently *at all* was not. Such silences and inconsistencies meant that the critical questions of science and ideology, of whether there can be a socialist science, was to be neglected. It may well be that the barbarous constraints imposed by the context of the cold war made it impossible for such a difficult and potentially threatening theoretical work to be realised. Left scientists turned to issues of world peace and the dangers of nuclear holocaust as both a necessary and in some ways a less problematic activity. Bernal himself abandoned wrestling with these ideological questions to go on to the writing of the optimistic *World Without War*[10] and the massive project of *Science in History*. For the 1960's radical science movement, the closure of these questions and the long silence over Lysenko meant that the theory has to be rediscovered rather than developed from an ongoing debate. Only recently has the discussion, for example, of science and ideology been thoroughly reopened amongst the radical science movement.[11]

Back in the immediate post-war period of the late '40's however, the positivistic strand of Bernal's pre-war writings was to remain and flourish whatever the theoretical and political difficulties that the other strand encountered. He had revealed the inefficiencies of British science and had inspired a generation of younger scientists to think in the same vein. The war absorbed all their talents and by the end many had tasted the delights of power. Had a Tory government been returned immediately, many might have gone back into opposition, but Labour's mandate for change in the 1945 election and its continuing belief in the need to reconstruct British science attracted them. Any rate, progressive scientists like the physicist Blackett and the biologist Zuckerman moved closer to the scientific advisory apparatus, carrying with them many of Bernal's views on how to organize, but without his commitment to revolutionary change – which meant that he himself was soon excluded.

Bernal's thesis that capitalism was incompatible with the full development of science and technology was about to be put to the test – and found wanting. The subsequent 35 years was to show the unabated growth of science in all advanced capitalist countries, at least at the rate of the Soviet Union, and with little sign of let-up in its innovative dynamic. It is ironic but not surprising therefore that the reborn radical science movement of the late 1960's was to criticise not so much the inadequacy of support for science under capital, but the science itself, which had become revealed as oppressive and anti-human.

In Britain the Labour party was open to the technoeconomic argument – historically it has always favoured industrial captial, unlike the Tories, who have favoured finance capital – and the period of "13 wasted years" of Conservative power between 1951 and 1964, briefly invested the technoeconomic argument with some of the glamour of opposition. We have shown elsewhere[12] how technoeconomism was adopted by Harold Wilson in the run-up to the 1964 election and given an almost messianic quality, of the building of socialism in 'the white heat of the scientific and technological revolution'. Bernal himself was involved in some of the many science policy debates which the Labour party held in the run-up to 1964 – again, we have discussed these in detail elsewhere.[13] The failure of the Wilsonian promise – the last time the Labour party has fought an election with a manifesto promising radical social change – was also the failure of a revisionist Bernalism, a failure which no British government in the post-64 period has been able to overcome. Bernalism, without the social change on which it was premised, could never be a panacea for social democracy.

Bernal, Marx and Science

Had Bernal died before the birth of the radical science movement, it would have been as if the revolutionary flames he had helped to light in the 1930's had burned down indeed to a very dull ember. It was for a new generation to attempt to capture that spark and fan it to life.

What should the new movement seek as its legacy from Bernal and the old? The recognition of the social function of science for sure. The recognition of the material nature of the world, which humanity, through scientific practice, seeks to know and change; the understanding that obscurantism and idealism are the enemies of social progress, certainly. Nowhere in the corpus of Bernal's writings are these ideas

expressed better than in his 'Marx and Science'.[14] Farrington's introduction is brief: 'I do not think Bernal has ever written better than here', he says, 'and to those who know only the Bernal of S.F.S. it will prove a startling experience'. He is right, for in describing Marx's approach to science and dialectical materialism, Bernal addresses virtually all the themes which the radical movement has come to make its own: ideology, expertise and the nature of science under capital.

The task of making a new science, he concludes

> 'is not a simple or an easy one. It involves very great struggles and contradictions, because the whole ideology of science itself, an ideology implicit in all scientific theory, is derived from that of capitalism'.

Science must become

> 'the property of the whole people, firstly by assuring that most scientists are drawn from the working people, and then by directly involving working people in scientific research relevant to their own problems ... the philosopher has finally started to change the world, and what we have seen is but a small foretaste of things to come. The struggle is still in front of us but we can be confident of the future'.

A future, in the phrase of Bernal we must continue to cherish, of 'a science for the people'.

Hilary Rose and Steven Rose

* The written version of this talk is a compressed version of a fuller account to be published under the same title in *Fundamenta Scientia.*

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Radical Science at a New Crossroads - Which way to go?

I would like to stretch back our perspective on this event three times further than the fifty years which we're commemorating, because this year is also the 150th anniversary of the founding of the British Association for the Advancement of Science, and I believe that there is a certain continuity between the ethos of that body and some problems which I perceive in the radical science movement. Among the reasons given for setting up the Association were 'to raise scientific and literary men to their just place in society', to 'advance the cause of science', to form 'a parliament of science', and to establish science as a recognised profession. One of the ways in which there is a continuity between those notions and our own movement is that kind of professional self-consciousness and self-confidence, a *noblesse oblige* approach to one's relationship with the rest of society.

There have been intersections between our movement and the Association, as, for example, when in 1938 a division for Social and International Relations of Science was set up within the Association. This was, of course, wound up in the Cold War, at the height of the Lysenko affair. Later on, in 1970, an attempt was made to intervene in the British Association in ways which were appropriate in the anti Vietnam War movement to the struggles against chemical and biological warfare – an intervention which was very unwelcome indeed.

I came along a couple of years after BSSRS was founded. At that time BSSRS was very clear that it was a political movement, but it was nonetheless an elitist one. It had moved from 'responsible' to 'radical', but its message to the general public was, 'You are in good hands'. The Social Impact of Modern Biology conference (1970) had as its keynote, 'First catch your Nobel Laureate'; there were eighteen there, although seventeen have decamped and only one, Maurice Wilkins, is still with us. There were four young radicals who spoke at that meeting; three of them are now professors and one is a television series editor. I wonder if we still qualify as what Margot Heinemann called 'good students' and 'good socialists'?

The idea that an expertocracy is what the radical science movement has to offer the rest of society is in deep conflict with the libertarian politics-of-

the Left, and I believe this conflict has something to do with the kinds of debates which we've been conducting. It goes back to the equating of progress with scientific development, and that equating means that our aim as scientists is to be experts in rationality, and the better an expert in rationality you are, the more you have to offer society. We are therefore, according to this opinion, engaged in searching for the history, philosophy and social relations of 'truth', which we then hand down to the working class.

Earlier versions of this position can be seen in two quotations from British Association lectures. John Lubbock, President of the Association in 1882, spoke on 'Fifty Years of Science';'To science we owe the idea of progess. It is not, I think, going too far to say that the true test of the civilisation must now be measured by its progress in science'. Three years later, in his address to the same body, Lyon Playfair completed the equation of science with progress and civilisation. 'Human progress is so identified with scientific thought both in its conception and in its realisation, that it seems as if they were alternative terms in the history of civilisation'.

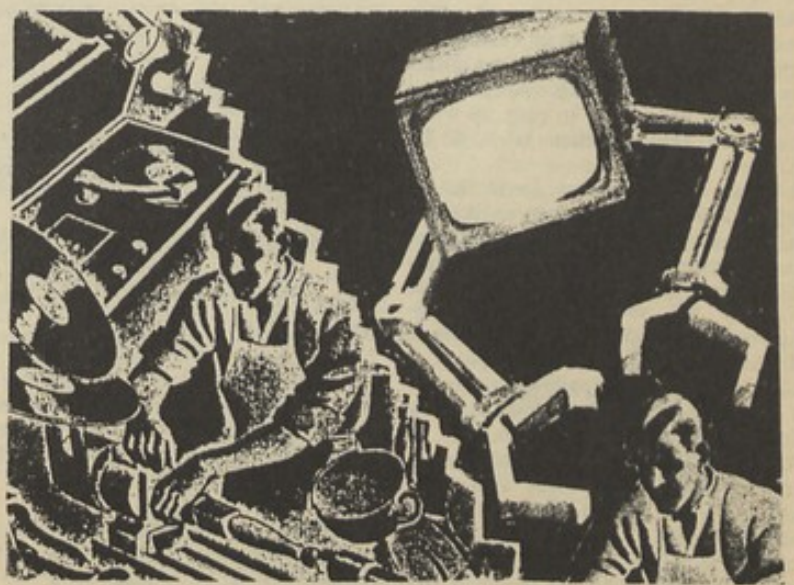
Moving forward to the 1930's, the radical science movement of that period was concerned with the notion that science was vulnerable to distortion by the capitalist mode of production. The aim of the movement at that time was to make truth responsible. That impulse was shared by Bernal's generation, the people who founded BSSRS, and the people who left BSSRS and founded the Council for Science and Society. They held three different political positions, though each associated with the belief that the answer lies in the responsibility of the scientist, which can be roughly summed up as 'you could be in good hands', 'we will put you in good hands', 'you are in good hands'.

I think that there is another message which comes from the essay in *Science at the Crossroads*. Bukharin, in his intervention, said that the idea of the self-sufficient character of science is naive. It confuses the subjective point of view of the scientist with the objective practical role of science. The connection between the social function of science and the way scientists see their own work is hidden by the division of labour in society. In other words, he is defining the false consciousness of the scientist. He went on to say that priorities in research and development, as well as the resulting scientific theories, are, however unobviously, expressions of the prevailing balance of social and economic forces in a given society. The practice of science is the practice of material labour continued in a particular form, the form of natural science.

There is a very long tradition, predating even the British Association, of concern about scientists' self-conception and their contribution to society. Think of Marlowe's Dr Faustus; think of *Frankenstein*, in which Mary Shelley was explicitly concerning herself with the contemporary physiological theories of Erasumus Darwin and Johannes Mueller. Then there have been Jules Verne, H.G. Wells' *The Island of Dr Moreau* and more recent science fiction, e.g. Kurt Vonnegut's writing about Taylorism in *Player Piano*. In the 1940's and 1950's, the bomb, the Pugwash Conference, and Sputnik all demonstrated a close relationship between science and society. In the 60's the same issues emerged with chemical and biological warfare, napalm, the defoliant 'agent orange', and so on. Therefore, the idea of science as above all a progressive force, which is after all the view that Marx took of the entire bourgeois revolution, especially including science, always had its critics and in the latter part of this century began to be more seriously questioned. A need began to emerge for a deeper kind of political analysis.

It seems to me that in our own period there is an intersection between the agenda of our movement – as it has moved towards the full integration of science, technology and medicine, and the critique of them with the critique of capitalism – and the agenda of society as a whole. It is that intersection which provides us with a new opportunity. If we set about thinking of the ways in which science impinges on us and our society, the concept of impinging ceases to matter; they are inextricably intertwined. To convey this better we might say they are mutually constitutive. We have the role of high technology in fertilisation and gestation; the role of automated procedures, robotics and microprocessors in the factory, the office, the home, the school, and in communications; and the whole revolution around information technology which knits all these things together with the introduction of the home terminal; then there are cerebral implants; genetic transplants in the offing; the dramatic developments in birth control. We are no longer in a position to see science and technology happening in the universities and producing spinoffs, and then another domain called our lives. In thinking about these things, we need to move away from a model which is about the history, sociology and social relations of 'truth', to a model which is more consistently radical and certainly marxist and which looks at the varieties of human industry, the transformative relationship between human practice and nature, and puts these things into one melting pot (not to say 'crucible').

The problem here is that, on the whole, we in BSSRS still think of ourselves primarily as academics who are doing research. It is difficult for us to think of ourselves under that heading of 'industry', but I believe we must break out from that form of self-conception. If we are to build a new strategy for our movement, it depends on building solidarity around a new kind of coalition, and the first thing we have to do in that coalition is to acknowledge that each one of us is what has been called a 'techno-peasant' in every sphere except our own little niche in the division of labour. We can no longer dub ourselves experts in rationality. We can only say, here are our practices, and here's the industry of our labour process, but in all other things we do and which impinge on us and reconstitute us (which of course can become quite literally true in the next few decades) we are peasants – in the sense that a peasant may not understand the high technology of so-called advanced societies.



If we are going to think of a techno-peasant's coalition, if we are going to think about the production and reproduction of nature (by which I mean looking after the husbandry of nature),

we are going to have to make connections in organisations which involve new initiatives. They all involve digging where we stand in traditional areas of scientific teaching and research. We should learn to see these as 'industry', but also to go further and to treat as 'industry' the activities around pacing, surveillance and control at work, at home, in leisure and in education. Below are some suggestions as to what these organisations may be.

1 *Producer Initiatives.* Unless we can agitate and organise among the originators of labour processes, among people who prioritise R & D, we are in dead trouble. The hopes, successes and failures of the Lucas Combine Corporate Plan are widely known. It is now time for a very radical reappraisal of what that initiative was and of how easy a target it was, either when there was re-employment or when there was very grave unemployment. That is an analytical and educational task which I think we haven't yet begun (although I know that a book on the Lucas Plan by Hilary Wainwright is shortly to be published).

2 *Consumer Initiatives.* There is a model for consumer initiatives which the Dutch have developed in their Science Shop movement, from which we could learn a lot. In Holland, they are based in shop fronts, so that anyone can walk in off the street, or come from a neighbourhood group or trade union and say, 'We want this science, rather than that science, to be done'. This process is of course full of contradictions, but the idea of consumer initiatives is a way of raising consciousness about who has a right to propose what should be the priorities in R & D.

3 *'Come back General Ludd, all is forgiven'.* We have to try to re-theorise our struggles around the introduction of new technologies and systems of pacing, surveillance and control, so that our stand is not seen as merely anti-progressive and destructive. This is a difficult issue, and one to which I believe we have not applied ourselves at all seriously. But we must avoid a situation where we are unable to articulate the real issues involved in these struggles because we are afraid being labelled as Luddites, as being against progress. Luddism as anti-progress is a spectre which is always hurled at those who are resisting the introduction of new technologies, and I believe that we should apply ourselves to the task of laying that spectre finally to rest.

4 *Green Parties.* Why is the British left so very ignorant about what is happening in Europe in the Green Parties? It seems that many people who held the same kind of politics as the people who created BSSRS are now setting about re-theorisation and re-organisation in the Green Parties. The ecology movement in this country has come nowhere near the kind of serious politics that is going on in the rest of Europe on these questions. If as serious a revolutionary as Rudi Dutschke could have died while advocating the Green Parties, I think we should want to know more about it. I confess that I am ignorant about it myself, but I believe that it is a task in self-education which we should embark upon, because while we have European Nuclear Disarmament, we do not have any other kind of serious subversive coalition around what Continental Green Parties are about.

5 *The relationship between Nature and Gender.* By this I refer to the work of, among others, Brian Easlea, Carolyn Merchant, and Donna Haraway. They have begun to question how scientific priorities have arisen (not in the global sense, but in the quite precise sense of influence in research programmes, conceptions, theories, ways of conceiving nature), using in their analysis some of the insights which have arisen from the feminist movement and studies on gender. This is a relatively new approach, but it is one from which I believe we can learn a great deal.

It seems to me that these questions are what we should be concerned with in the 80's – not as a professional elite in search of truths to be handed down but as a coalition of workers and techno-peasants labouring to transform the labour processes of production and reproduction.

Bob Young

Some Resources

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The Bride and the Funeral Cart

"Those of us who recall our first encounters with historical materialism," the historian Eric Hobsbawm has testified, "may still bear witness to the immense liberating force of such simple discoveries." For left-wing scientists in the 1930's, such as Needham, Crowther, Levy, Haldane, Hogben, Bernal, that initiation can be dated very precisely. Radicals and reactionaries alike reckon the demise of the ideology (or "ideal") of the "freedom of pure science" from Boris Hessen's address at the Second Congress of the History of Science on "The Economics, Physics and Technology of Newton's *Principia*." According to some, the momentum he imparted has not diminished down to today.

As Joseph Needham declared in his "New Forward" to the 1971 reprint of *Science at the Crossroads,*

"Perhaps the outstanding Russian contribution was that of Boris Hessen , who made a long and classical statement of the Marxist Historiography of science, taking as his subject of analysis Isaac Newton. It was a veritable manifesto of the Marxist form of externalism in the history of science... This essay, with all its unsophisticated bluntness, had a great influence during the subsequent forty years, an influence still perhaps not yet exausted..."

The "mainsprings and hindrances" to science, according to Hessen, are found *outside* of science, in the *economic base* of society. For Hessen, a simple one-to-one causal relation obtains between the economic and technological needs of an ascendant social class on the one hand and scientific ideas on the other. The former evokes the latter.

"In opposition to the tradition which represents Newton as an Olympian standing high above all the earthly technical and economic interests of his time, and soaring only in the empyrean of abstract thought", Hessen's Newton is nothing if not practical. He runs the Royal Mint, analyses the construction of fortresses, investigates mechanisms for steering ships and seeks to transmute the elements. The apparently abstract mathematical character of Newton's most important work, the *Principia*, conceals its base roots. In brief, the bourgeoisie had to solve a great variety of tricky problems if the expansion of profits – and hence its own power as a class – was to be guaranteed (problems, for example, of transport, industry and warfare). Many of these problems were ultimately problems of mechanics; and Newton solved them.

The following is a series of extracts from Hessen's original text as published in *Science at the Crossroads*. We include them, together with brief summaries of some characteristic arguments, to help readers get the flavour of the Russian contribution to the conference. In fact Hessen devotes only a few hundred words (out of more than 8000) to the *Principia* itself. By far the greater part of his essay is a survey of "Transport", "Industry", "War and War Industry" and "The New Science" towards the end of the middle ages and up to the middle of the Seventeenth Century.

OBSTACLES TO PROGRESS: TRANSPORT

By the beginning of the middle ages trade had already achieved considerable development. Nevertheless, the land ways of communication were in a miserable state. The roads were so narrow that even two horses could not pass. The ideal road was one on which three horses could travel side-by-side, where, in the expression of the time (14th century) "A bride could ride by without touching the funeral cart".

Commonly, commodities were carried in packs. Road construction was almost non-existent. The self-centred nature of feudal economy gave no impulse whatever to the development of road construction. On the contrary, both the feudal barons and the inhabitants of places through which commercial transport passed were interested in maintaining the poor condition of the roads, because they had the right of ownership to anything which fell on their land from the cart or pack.

The speed of land transport in the fourteenth century did not exceed five to seven miles in the day.

Naturally maritime and water transport played a great part, both in consequence of the great load-capacity of the vessels and also of the greater speed of transit: the largest of two-wheeled carts drawn by ten to twelve oxen hardly carried two tons of goods, whereas an average sized vessel carried upwards of 600 tons. During the fourteenth century the journey from Constantinople to Venice took three times as long by land as by sea.

Nevertheless even the sea transport of this period was very imperfect: as sound methods of establishing the ship's position in the open sea had not yet been invented, they sailed close to the shores, which greatly retarded the speed of transit".

From these and a great many more observations, Hessen finds that "merchant capital ... set the following technical problems in the realm of water transport":

(1) An increase in the tonnage capacity of vessels and their speed.

(2) An improvement in the vessel's floating qualities: their reliability, sea-worthiness, their lesser tendency to rock, response to direction and manoeuvring, which was especially important for war-vessels.

(3) Convenient and reliable means of determining position at sea: means of determining the latitude and longitude, magnetic deviation, times of tides.

(4) The perfecting of the internal waterways and their linking up with the sea; the construction of canals and locks.

Such problems, he argues, could only be solved after fundamental laws governing the floatation of bodies in liquids (hydrostatics), their movement through resistant mediums (hydrodynamics), and the movements of the heavenly bodies (celestial mechanics) had first been established.

INDUSTRY AND MINING

Using the same style of argument, Hessen moves on to consider the problems of physics underlying the technical limitations of production, particularly of metals. Gold and silver production had to be vastly expanded to satisfy the currency needs of developed exchange; iron and copper were in enormous demand for the war industry. For example, Hessen writes,

In the months of March and April 1652 alone, Cromwell required 335 cannon and in December a further 1,500 guns of an aggregate weight of 2230 tons, with 117,000 balls and 5000 hand bombs in addition.

Ores had to be raised from considerable depths, which required mines to be ventilated (aerostatics) and drained (hydrostatics). Water pumps, blast furnaces, rolling and cutting machines, all evoked multiple developments in mechanics, for example in the mathematics of gearing and pulleys, and of friction.

WARFARE

Many other developments in mechanics followed from military needs. Thus Hessen explains why the laws of ballistics were being established at this time – laws governing the behaviour of gases (during explosions), action and reaction (in the recoil of guns), air resistance and the trajectories of balls. Hessen concludes that "the problems of physics raised by the development of transport, industry and mining" and warfare at this time were "purely problems of mechanics":

(1) The problem of simple machines, sloping surfaces and general problems of statics...

(2) The free fall of bodies and the trajectory of thrown bodies...

(3) The laws of hydro- and aerostatics and atmosperic pressure. The pump, the movement of bodies through a resistant medium...

(4) Problems of the mechanics of the heavens, the theory of the tides...

We come to the conclusion that the scheme of physics was mainly determined by the economic and technical tasks which the rising bourgeoisie needed to accomplish.

THE UNIVERSITIES

Hessen continues with a vitriolic assault on religion and the universities of the day, which were themselves a further obstacle to the ascent of the bourgeoisie. They "constituted a closed system of scholasticism ... Official science, the centres of which were the medieval universities, not only made no attempt to accomplish these tasks, but actively opposed the development of natural science":

In Paris in 1355 it was decided to teach Euclid only on holidays.

The chief "natural-science" manuals were Aristotle's books, from which all the vital content had been removed. Even medicine was taught as a logical science. Nobody was allowed to study medicine unless he had studied logic for three years previously. It is true that when sitting for the medical examination the student had to face a question of a non-logical character (testimony to his being the child of a lawful marriage) but obviously this one illogical question was hardly sufficient for a knowledge of medicine, and the famous chirugien Arnold Villeneuve of Montpellier complained that even professors in the medical faculty were not only unable to cure sufferers from most ordinary illnesses, but even unable to apply a leech.

The feudal universities struggled against the new science with a strength equal to that exerted by the dying feudal relationships against the new progressive methods of production.

Whatever was not to be found in Aristotle for them simply did not exist.

When Kircher (the beginning of the 17th century) suggested to a certain provincial Jesuit professor that he should gaze through the telescope at the newly discovered sun-spots, the latter replied "It is useless, my son. I have read Aristotle through twice and have not found anything about spots on the sun in him. There are no spots on the sun, they arise either from the imperfections of your telescope or from the defects of your own eye".

He also shows the church's role "as an instrument of feudal hegemony", and concludes:

The bourgeoisie had need of science and science arose together with the bourgeoisie despite the church (Engels). Thus the bourgeoisie came into conflict with the feudal church.

"Hessen gave the first concrete example of how science should be interpreted as a product of the life and tendencies of society ... Hessen's demonstration of the depth and range of Newton's dependence on the ideas promulgated by the epoch in which he appeared, made a profound impression on some of the younger members of the congress. It transformed the study of the history of science, and out-moded the former conceptions of the subject, which treated it as governed only by the laws of its internal logical development. Henceforth, no satisfactory history of science could be written without giving adequate attention to the dependence of science on social factors ... It showed that a knowledge of the history of science was not only of entertaining antiquarian interest, but was essential for the solution of contemporary social problems due to the unorganized growth of the technological society."
J.G. Crowther, 1941

THE NEW SCIENCE

Only then does Newton himself appear. After briefly outlining the formation of European scientific organisations outside of the universities, Hessen notes the formation in 1661 of the Royal Society, which brought together, among others, Robert Boyle, Christopher Wren, Edmund Halley, Robert Hooke and Newton.

The necessity arose of not merely empirically resolving isolated problems, but of synthetically surveying and laying a stable theoretical basis for the solution by general methods of all the aggregate of physical problems, set for immediate solution by the development of the new technique.

And since ... the basic complex of problems was that of mechanics this encyclopaedic survey of the physical problems was equivalent to the creation of a harmonious structure of theoretical mechanics which would supply general methods of resolving the tasks of the mechanics of earth and sky".

The explanation of this work fell to Newton to supply. The very name of his most important work indicates that Newton set himself this particular synthetic task...

The remainder of Hessen's essay is an outline of the contents of the *Principia* – the laws of motion of bodies under the influence of central forces and in resistant mediums, the fundamentals of hydrostatics, the compression of gases, and liquids, pendulums, the flow of liquids through and out of tubes, trajectories, tides, the mechanics of the heavenly bodies and so on. Taken section by section, Newton is seen as laying the foundation for the solution of specific practical problems; but when taken together, the *Principia* is understood as the intellectual bedrock of industrial capitalism.

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EDIBLE POLITICS: food and agriculture in Cuba

Nancy Worcester and Tim Johnson-Newell are both involved in the Politics of Health Group and Agricapital. They visited Cuba in Spring 1981, on the Health Study Tour. Tim is now in Cuba for a month leading the British contingent of the Jose Marti Work Brigade.

Food is a politcal issue. Who decides what is grown, who grows it, who owns the land, the way it is grown, how it is processed and distributed, its domestic preparation, and who gets to consume it are political decisions and reflect the priorities of those making the decisions. In this article Nancy Worcester and Tim Johnson-Newall look at what Cuba has done about these issues.

Sugar was crucial to pre-revolutionary Cuba's development. Her role as sugar producer resulted in the introduction of slavery, "colonisation" by both Spain and the USA, and the vulnerabilities of monoculture.

Changing the politics of food, particulary land ownership, was a fundamental commitment of the Cuban revolution. Steps towards revolutionary changes in food production were among the initial achievements which made Socialists throughout the world look seriously to this revolution in the Caribbean. Yet, ironically, food production and availability has been one of the areas of the revolution most criticised by both Western Cuba-watchers and the people within Cuba who are dissatisfied with the quality of their diet after 20 years of revolutionary promises.

> "Our only problem is the shortage of food, which has turned many against the government. But they're the kind who reason with their bellies... Although nobody starves here in Cuba, hunger is the state's worst enemy."
>
> Gracia Rivera Herrera(1)

"I found in Cuba the traces of a magnificent struggle, and a people caught up in the joy of liberation, but I also found great economic disorder...Were Brazil and Mexico, respectively ten and six times more populous than Cuba, to repeat similar errors and make the same faulty economic improvisations, the Soviet Union could not possibly bail them out to the same extent it has done with Cuba."

Rene Dumont (2)

Bread! Peace! Land!

The Peasantry has been the main power in the Cuban revolution and high priority was given to meeting their demands; an end to hunger, an end to Batista's savage killings, and agrarian reform. Land reform was the first action taken by the Castro government and is considered the most important single action taken by the revolution.

The first Agrarian Reform Law (1959) prescribed 900 acres as the maximum amount of land which could be privately owned. Some individuals had owned up to 330,000 acres. The U.S. owned Cuban-Atlantic Sugar Corporation had owned 630,000 acres. Land was expropriated without compensation to the landowners, and this was the initial cause of the break between liberal reformers and revolutionary socialists. In keeping with revolutionary promises, over 100,000 farmers were given their own land. Large cultivated sugar estates, other intensively cultivated units, and pasture lands were maintained intact and run as state farms.

Land owners had been allowed to keep 900 acres, but they promoted counter-revolutionary ideas and actions necessitating the Second Agrarian Reform Law in 1963. This law limited privately owned land to a maximum of 165 acres, but, unlike the first, compensated landlords for the land expropriated.

Cuban agriculture is now in two sectors, state and private. The private sector, accounting for approximately 20% of the land, is composed of non-state owned farms, some of which are run on a collective basis, others are small private farms which grow enough for their own needs and sell any surplus directly through peasant markets or through state retail outlets. Private farms continue to grow most of the labour intensive traditional crops – 80% of tobacco, 80% of coffee, and 60% of vegetables purchased by the state is grown on private farms. In contrast, state farms produce the export and status crops – meat, milk, and sugar.

Agriculture FOR the People

The other 80% of the land is now in the state sector. Working for the state instead of private landlords, the peasants have a new stability of employment and income, but they often have no more say in decision-making. The state sector is centrally planned, and this has given rise to vast criticism. It is ironic that a government who placed so much (too much?) value on "the role of the expert" in building the nation's health service, concentrating on doctors instead of paramedical workers, should have minimised the role of technicians and peasant experience in the development of agricultural policies. Centrally made decisions were often not relevant to local conditions and small scale experiments should have been carried out before large scale plans were made.

However, Cuba has agricultural achievements to be proud of in spite of hurricanes, CIA induced and other crop and animal diseases, and a lack of mechanisation. Between 1960 and 1975, citrus fruit production tripled, and egg and fish production quadrupled. But Cuba's agricultural achievements would have been much greater if local people had had more say in decision making and if central planning were done by people who were both technically competent and revolutionary!

> "...it is better to have a revolutionary with no technical competence than someone who is technically competent but not a revolutionary."
>
> Fidel Castro, 1963

Bittersweet Monoculture

Pre-revolutionary Cuba was totally dependent upon sugar. Revolutionaries considered this monoculture one of many evils for which the US investors were mainly to blame and the revolution was committed to making Cuba less dependent on sugar. But in 1981, sugar is as important as ever to the Cuban economy, providing more than 80% of her foreign exchange.

The revolutionary attack on monoculture was not well thought-out. With much land under their control, state leaders made great efforts at diversifiaction in the early 60's. These effects,

Infant mortality, deaths per 1000 births.

UK				
	class I	class V	all	ratio(class V/I)
males	13.60	34.75	19.91	2.5
females	9.60	26.67	15.57	2.7
Cuba				
	Villa Clara	Isle of Youth	all	ratio (high/low)
	17.0	29.9	22.3	1.8

poorly organised and not based on a knowledge of local conditions, complicated by bad weather, resulted in a reduced sugar crop and shortages of most foods. Cuba had to admit she was dependent upon sugar for foreign exchange and, with a complete turnabout, huge efforts were made, to make a 10 million ton sugar harvest by 1970 at the expense of other food production.

Cuba did not make this 10 million ton goal and Castro himself took the blame for the problems it had caused. For the government and the people, there was a renewed realisation of the problems they faced as a developing country.

The contribution of sugar production to the internal economy has decreased, but sugar production has increased in absolute terms. "Sugar cane is what we can produce most, what brings in the most income, what costs less to produce, is the industry in which we can recuperate investment outlay most rapidly...so are we to give up sugar just so as not to be labelled a mono-exporting country?" (3)

Over three quarters of Cuban imports come from the socialist camp, so fluctuations in world sugar prices (64¢/lb. in 1975, 6¢/ lb. in 1979) affect Cuba less than other sugar-exporting countries because of her agreements with other socialist countries, particularly the Soviet Union. It is estimated that the Soviet Union puts $2 million per day into Cuba. (Puerto Rica, with 1/3 of Cuba's population, receives $8 million per day in US Federal Funds.)

Revolutionary Distribution

Food distribution in Cuba is more revolutionary than food production. The emphasis on equal distribution of basic necessities (including free health services, and free education) has meant that Cuba has consistently seen that food was equally available to all regardless of income. Contrast this to the UK where income differences are reflected in a different food consumption pattern dependent upon class, and income dependent life styles (type of storage facilities, car ownership, stores within shopping distance, shopping time, money available for bulk buying) result in rich people getting better value per pound spent on food than do poor people.

Rationing was introduced in Cuba in the early 1960's due to food shortages aggrevated by hoarding on the part of well-to-do bourgeois families and the economic blockade. "We had to come up with an equitable method of distribution that would give everybody equal access to products, so that low income families, for example, could buy meat every week. Because we must bear in mind that under capitalism those families were subjected to fierce rationing: they couldn't buy the meat or shoes they needed." (3) A survey in 1956 indicated that only 4% of the population ate meat, 1% ate fish, 2% ate eggs, 11% drank milk, and 3% ate bread.

One of the first achievements of the revolutionary government was a "downward shift" of income distribution. An enormous improvement in the income share of the lowest 40% took place in the first year after the revolution. In later years, the middle groups have benefited more than the lowest group from income distribution. As differences between lower and higher paid were narrowed, there was a big increase in demand for certain foods which people had not been able to consume before. An elaborate rationing system has enabled *all* Cubans to have a basic, though meagre, diet at prices they can afford. When foods are scarce, the per capita quantity available, price, and frequency of purchase are strictly controlled. Less popular and more abundant foods are not rationed.

Most Cubans participate in mass organisations and thus have their role to play in food production and distribution as in other aspects of building a revolutionary Cuba. The Committee for the Defence of the Revolution (the block-by-block organisations initially set up for self defence at the time of the Bay of Pigs invasion) have been responsible for the supervision of food distribution. A major early CDR activity

was the lard and margarine census in 1960 when the US economic blockade meant these goods were in short supply. CDR's then set up and supervised the rationing system. Women's organisations have been important in recruiting volunteer groups for harvesting.

Certain nonwage policies explicitly favour people on low incomes. Some goods are free only to low income earners. For example, there is no charge for canteen meals at work for the poorest paid, but workers who earn above a certain level are expected to make a contribution towards canteen meals. The state has kept basic food prices stable. Beans still sell at the 1962 price because highly priced luxury goods (i.e. television sets and alcohol) help cover the inflationary costs of the 20% of goods imported from capitalist countries.

Nutrition

Improvements in agricultural production and a more equitable distribution system are reflected in Cuba's improved nutritional status. Per capita food consumption in Cuba is the highest in Latin America. Improved nutrition, reflecting basic socio-economic changes, has been a major factor in the better health and increased life expectancy (50 years, 1958 – 70 years, today) of Cubans. The tragic poverty-malnutrition– infection-death cycle, still common in most Third World countries, has been eliminated in Cuba. Infant mortality, a key indication of maternal health and nutritional and socio-economic conditions, is now the lowest in Latin America at 22.3/1000 live births – a figure comparable or better than many parts of North America and Britain. More telling, infant mortality figures are more consistent in different groups in Cuba than in the UK.

Children Are The Revolution

The Cubans are investing generously in the next generation, with children emerging as the new elite. This is reflected in nutrition policies. Breast feeding campaigns throughout the country are trying to reverse the bottle feeding patterns previously influenced by artificial milk companies. The new born baby not only receives the full adult ration, but is also given extra cereals, juices, and milk supplements. Standards for baby foods and foods consumed in large quantities by children (soft drinks and ice cream!) are particularly strict about allowed additives.

Children in day care centres are given, free of charge, three full meals, snacks, and vitamins. This is not taken away from their ration at home.

Learning about food production is a central part of Cuban education. Agricultural production carried out by many secondary school children is not only essential in developing a revolutionary consciousness towards manual labour, but the food produced is needed for Cuban markets and helps pay for the expensive educational programmes.

The Future

There are still areas where parasite-caused malnutrition remain a problem, but basically Cuba has succeeded in providing her population with a frugal basic diet and minimised malnutrition. Improving the nutritional status of the population has been an impressive part of the preventative, rather than curative, health programme of the last 20 years. But, the diseases of poverty are now being replaced by the diseases of affluence. We watch with interest to see how Cuba can use her revolutionary health principles emphasizing prevention, the responsibility of the state, and the role of mass participation in dealing with the health problems of modern society.

Cuba has had to work hard to feed her population so quantity of food has necessarily received more attention than the quality of the diet. Old fashioned concerns (increasing protein and energy intakes) are no longer necessary, and in the case of energy can be positively dangerous as obesity is now a common problem. Much responsibility for choosing a healthy diet is left to the individual rather than the state. "If you are overweight, see your doctor!" Fruits and vegetables are still not popular so health education and agricultural planning must continue to stress their importance in a balanced diet. Cuba buys whole grain from Canada and the Soviet Union, and faces the same problem as in the UK in that mills are designed to produce white flour. There seems to be no government policy to try and increase fibre consumption although a special "fibre added" bread is now available. Low sugar juices were once available, but "people did not like them". Sugar is believed to be a contributing factor in the diseases of affluence – obesity, heart disease, diabetes, etc. – which are increasingly common in Cuba, but Cubans seem totally unaware of dangers of sugar and there is no campaign to encourage people to reduce sugar intake unless they are trying to reduce weight. (Visitors to Cuba always comment on the high sugar consumption, so it is worth noting that per capita sugar consumption in Cuba and the UK are practically identical!)

Mass organisations have been involved in past health campaigns and have played a crucial role in the victory over infectious diseases. They are yet to be involved in health campaigns related to the new disease patterns. Popular Power (the new system of electing representatives, introduced in 1976) could have an important role to play in making food production *by* the people as well as *for* the people. P.P. controls the production and distribution of food through local industries. People have quickly responded to this channel as a way to complain about the quality of their food and the distribution of non-rationed foods. Hopefully, as people gain experience in voicing their complaints through P.P., they will choose to think, debate, and influence more fundamental issues of food politics.

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(4) "Income Distribution and Consumption in Postrevolutionary Cuba" by Susan Eckstein in CUBA STUDIES 10:1, January 1980.

If this article whets your appetite... write to BSSRS for details of the pamphlet "Looking at Cuba's Revolution through her Health Services" soon to be published by the Health Study Group.

letters

Dear SFP,

Best thanks for sending me a copy of *Science for People.* I found its content most interesting and useful, especially for safety delegates and others interested in problems of occupational health.

It may sound like nit-picking, making some criticism. I believe, however, a jounal like the above named cannot afford even minor mistakes or inaccuracies without the risk of being discredited. Therefore, I want to point out a serious mis-statement contained in the 50th issue, winter '81/82. The second paragraph on page 11, under the subtitle "cover up", reads: "... in the list of 18 notifiable diseases, the word cancer is not mentioned once". Yet looking at the leaflet N12, Sept. 1968 of the DHSS (the last at hand for me) the list of prescibed diseases are numbered up to 44. To this number at least one more has been added since 1969. While it is correct that up to No. 18 cancer is not mentioned, subsequently there are several cancers listed as qualified "occupational" in certain prescribed occupations:

No. 25: ulceration or malignant disease of the skin...
No. 37: a) carcinoma of the mucous membranes of the nose...
b) primary carcinoma of bronchus or lung ... (in nickel workers).
No. 39: primary neoplasm of the epithelium of the urinary bladder ... (in quite a number of occupations where certain chemicals are being handled).
No.44: primary malignant neoplasm of the mesothelium ... (in asbestos workers).

Since 1968 neoplasm of the nasal sinuses in woodworkers in furniture factories has been added to the listed carcinomas. There may well be some more, of which I have no record (having retired some years ago and given up lecturing, I have not kept up to date with my records).

If I remember correctly, in 1969, the Chief Factory Inspector complained that carcinoma of the scrotum is not properly notified to his department because hospital M.O.'s and G.P.'s rarely take note of the occupation of their patients in treating them and filling in death certificates, where needed. In his Annual Report of this year he therefore stresses that notification is well below the actual incidence, as can be found out by looking at the Registrar General's Occupational Death list (Decennial list).

I have made the above observations because I feel that claims for benefits or notification may be omitted if readers of the journal accept the false statement quoted above. May I therefore suggest that a correction is published in the next issue of your journal.

With all good wishes for success in your important and most useful work.

Yours sincerely,
Dr. Hermann Grunwald

Dear SFP,

I was not impressed by the empty-headed rhetoric of the pro-EEC Agri-capital article featured in the last issue. The EEC's Common Agricultural Policy (CAP) is implied to be the golden goose of Socialism because it compares favourably (indeed healthily) with the state of food supply at the turn of the century.

SFP members concerned with identifying and constructing an imaginative land – use policy should not *begin* by advocating the CAP. What we need *first* is a proper analysis of how to equate supply with demand so as to satisfy socialist objectives/radical ideas about nutrition and health, working conditions, land division/ownership, liberty and free access to the countryside, recreation and conservation, and fertility.

Do we really want to advocate the maintenance of an existing expensive super-structure which can't be forced to adapt even the minutest part at summit meetings, so full is it of conventional 'commonsense' conservatives. To do so is to wrap ouselves up in a Gordian knot of our own tying.

BSSRS is supposed to disentangle the science – in this case agriculture – from the great thrust of contemporary politico-economic values and structures and assumptions, analyse it as if people mattered individually, and develop science-related policies in a radical, humanitarian socialism. We should not start out within an existing conservative framework such as the CAP and then attempt to hack at it, and chip away the bits we don't like – we'd end up with a policy for agriculture about as cohesive and complete as a roofbeam infested with tunnelling beetles.

Besides the verbose rhetoric buttressing the very foundations of the EEC, relatively little was said about what we might want to correct about existing agricultural policy. Other areas needing attention include: how to stem rural depopulation; how to re-enfranchise people (particularly in view of micro-chip – age chronic unemployment); how to ensure free access for all people to the countryside; how to protect non-economic uses of the countryside (recreation, tourism, conservation and the fabric of the countryside itself) from the power interests of the farming lobby.

To develop just some of these points; as Marion Shoard points out (in her new book *The Theft of The Countryside),* the state subsidy to private agriculture is vast – easily equalling the subsidy per worker in the Tory's pet hate, British Steel.

The same 'forgotten subsidy' creates advantages for juggernauts in state Transport planning (see John Wardroper *Juggernaut)* whilst British Rail, and now London Underground, are Tory targets because their finance, as a wholly-owned public body, is more easily revealed. Marion Shoard finds many subsidies to be wholly uneconomic, simply the result of excess power of an industrial lobby of farmers who have thier own direct line to the Cabinet (an entire Ministry!). Other land-interests, however, though involving more people and often more jobs (e.g. tourism), can't even influence farm pressures on their interests because the heady days of post-war socialism gave farmers carte-blanche freedom from planning laws – a unique position.

All detailed analyses looking at the question of 'Can Can Britain Feed Itself? (such as K. Mellanby's book of that tittle) conclude that less land than currently used would be needed. It is only possible, however, by giving more people landplots/large allotments (which are the most productive form of agriculture) rather like the old 'commonland' and 'strip' system. This orients agriculture towards market gardening (always at least ten times more productive than 'secondary food-chain' farming of animals, since animals lose more of their fodder's energy than they keep. There may yet be a place for animals since they would form a floating population to cope with crop gluts and would diversify the flavours and textures of food.)

By devolving farming thus, we could cut out the current loss of a staggering 25% of agricultural production which occurs during collection, distribution, wholesale, retailing, processing, etc. (see Robin Roy, *Wastage in the UK Food System).*

The system under which farming operates can thus be shown to be one of the most important factors governing the amount of food produced. Centralised control and mass-production are not the most efficient in increasing nutritional value – but they are the best for creating monetary value. Current farming 'increases in output' are normally pursued and recorded in terms of financial pounds or 'greenpounds,' occasionally in terms of 'gross weight' so as to ignore distribution losses, and *never* in terms of 'nutritional value.' Readers should therefore be very suspicious of figures such as those quoted as a table in the Agricapital article of SfP 50.

What is needed is a more responsive agricultrual system, which takes more

Reviews

The Nuclear Barons

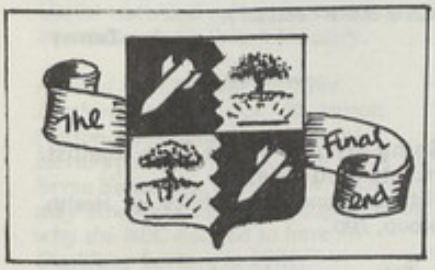
Peter Pringle and James Spigelman

Michael Joseph 1982. 578pp. £12.95

To quote the inside cover, the authors, "using a worldwide network of sources, trace with cool objectivity the tissue of interests, individuals and turning points that have brought us to our present folly ... It is an eyeopening and ultimately devastating account."

Well, yes, the book does contain many shocking revelations, some of which I have not seen elsewhere. But it is insufficient simply to "devastate" your reader if the intention is to remove the threat of both nuclear bombs and nuclear power.

The Barons of feudal Britain were more than just a loose-knit group of individuals, they were a class with a clear class interest. *The Nuclear Barons*, however, does not reveal that the individuals which comprise this modern branch of royalty have a collective or class interest in promoting nuclear technology.



The authors present a detailed history of the international development of nuclear energy but they do not understand that history in a political way. Having read the book one is full of events but starving for an interpretation.

The Nuclear Barons are defined as those who are most committed to nuclear energy at any phase in its development. For example in the 1930s, to have been enthusiastic to produce nuclear fission lead to ennoblement – thus both scientists and military were Barons. During the construction phase, after World War Two, membership was extended to the enthusiastic engineer or programme supervisor.

Politicians are also to be found among the Barons, but only the most tenuous of links between them and the rest are demonstrated; the authors exert a subtle pressure toward conspiracy theory when they declare that various individuals were "lifelong friends" or "went to school together."

Whilst the enormous cost of nuclear technology is well illustrated (at one time 10% of US electricity production was consumed by the bomb programme), very little is said of the role of big business in securing and carrying out the contracts nuclear development provides.

In the end this book sees nuclear technology as an aberration not as a strategy.

Tony O'Connell

account of interests other than the powerful farming/agribusiness lobby. We are not in need of farmers taking their collective influence to the EEC. The exisiting agricultural administration is a profit-enhancer for farmers, not a Socialist model.

David Solman

Popular Planning for Social Need

An Alternative to Monetarism, from Shops Stewards Committees and Trades Councils. 20p from Colin Lindsay, Coventry Trades Council, 31 Stepney Road, Coventry, Warks.

Jobs for a Change

Alternative Production on Tyneside. 60p from Days of Hope Bookshop, 115 Westgate Road, Newcastle upon Tyne, NE1 4AG.

These two publications are both important contributions to the debate that is going on around the 'Alternative Economic Strategy', as to whether the AES could become more than an attempt to revive British capitalism on an insular basis.

They are based on a searching critique, not only of the policies of the current Conservative government, who only approve of public expenditure when it is on bigger and better ways of killing or repressing people, but also of the inadequacies and failures of Wilson/Callaghan style interventionism.

'Jobs for a Change' concretely relates this to the experience of Tyneside, showing how the Tories, with their enthusiasm for new technology, 'defence' expenditure and nuclear power are in fact destroying jobs by concentrating scarce resources and capital in capital-intensive automated industry.

There is also a warning against expecting an incoming Labour Government to miraculously solve all our problems.

"Even if a radical AES were adopted, with a commitment to socially useful production at its core, we cannot rely on a future Labour Government to do the whole job for us. Such an economic strategy would face massive institutional opposition, and could only be carried through with genuine and widespread popular support. In other words, we are not contemplating a new round of "nationalisations" from the top down, but the creation of a democratically-planned economy from the bottom up".

"Popular Planning for Social Need" is the best thing so far from the shop floor workers point of view, putting the position of critical support of the AES. We are certainly are going to need *an* alternative economic strategy when the time comes and this is certainly a great improvement on the versions previously on offer.

Both these publications should be available from the BSSRS office at cost price, subject to the constraints of supply side economics.

John Bradbrook

Transnational Corporations in Food and Beverage Processing.

ST/CTC/19 United Nations, New York 1981

This is the book for all those people who want to know what is the biggest food processing company in the world (Unilever), how many people are engaged in food processing in various countries (22 million throughout the world), or what proportion of sales goes on advertising (8% for breakfast cereals in the USA). It is a bit like the Guinness book of hit food processors, and it is the first time that you can eyeball the size and extent of the trade. We've always thought it was like this, but didn't have the facts and figures to back it up.

But there is more to this book than just facts and figures. For academic marxists or budding fruit sellers it is a harvest of insights into the strategies behind these major companies. Most of the food processing companies have interests in other areas – for example Unilever with soaps and Imperial with tobacco. They can move in and out of food, or diversify away from food: for them, food is just another commodity.

Of more importance to developing companies is the priority that these companies attach to processing and marketing all sorts of different products, rather than getting involved with the primary production of raw materials. The latter industries are too great a risk; and contracts with primary producers means that these large companies need not be bothered. It is also reckoned that the expansion of these companies has been enhanced by their penetration into developing countries. But, the book suggests, it is unlikely that there will be any more entrants, as most of them were already in position by the beginning of the seventies.

This gives a brief taste of the book's approach, although it deals with nine particular flavours – including sugar, coffee, and dairy produce. In total, it's quite a feast.

Charlie Clutterbuck

Reviews

From Chance to Purpose
An Appraisal of External Human Fertilisation
Clifford Grobstein

Addison-Wesley, 1981. pp 207, £11.60
ISBN 0-201-04585-0

As the debate about 'test-tube' baby research belatedly takes off in Britain, Grobstein's book provides a timely reminder that there has already been extensive, if inconclusive, public discussion of such work in the US.

In this country, the medical profession and the State treated the early development of *in vitro* fertilisation with benign indifference. The Medical Research Council refused to fund Robert Edwards' and Patrick Steptoe's research, but they were able to proceed with Ford Foundation money.

In the US, by contrast, would-be researchers in this field faced an effective prohibition on work with human subjects until a Board set up by President Carter's Health Secretary, Joseph Califano, had appraised some of the issues the work raised.

The Ethics Advisory Board on Research Involving Human In Vitro Fertilisation and Embryo Transfer took evidence from a variety of academics and held public hearings around the US, with TV and radio coverage.

This book is basically an expansion of their report, which is republished as an appendix, and so provides a useful guide to the present position of public debate.

Grobstein is Professor of Biology and Public Policy at the University of California, San Diego, an appointment unlikely to be paralleled in this country in the foreseeable future, and is just the sort of liberal-establishment academic who might have been chosen to serve on the Ethics Advisory Board.

His discussion shares the main deficiencies of the Board's report. To begin with, he is stronger on the biology than the social policy, and gives a clear account of what is possible and which of the developments like surrogate motherhood or embryo modification are unlikely to come to fruition.

However, the discussion of costs and benefits is limited by Grobstein (and the Board's) failure to pay any heed to the likely influence of patriarchy on the future development of reproductive technology.

The majority of workers in this field are men, who see themselves as simply helping (married) couples overcome the distress caused by infertility. No doubt this distress is often deeply felt, but nowhere is there any consideration of why the urge to have children is so strong.

When considering the risks of *in vitro* fertilisation, Grobstein appears content with his conclusion that the 'most highly motivated' subjects will bear the risk in the early stages, and clarify the dangers for later subjects.

But will their motivation be that of one of Steptoe's early patients, who told a *Daily Mirror* reporter in 1970 that; 'You don't feel a complete mother until you've had a child of your own. That is what a woman is for'.

Grobstein even discusses possibilities such as selection of the sex of the embryo without mention of the relations of power between the sexes.

Martin Ingley

Instead, he retreats into bland generalisation;

> 'Sex selection could have significant demographic and social consequences, and possibly unfortunate ones. Clearly the relationships between individual free choice and broader social purpose would need careful consideration in establishing policy'.

This refusal to be concrete is characteristic of a great deal of comment on this embryonic technology. Perhaps more people realise the force of Jalna Hanmer's assertion that 'reproductive engineering ... offers a vehicle for the final working out of the antagonism between women and men' than will admit it, even to themselves.

Nevertheless, Grobstein's book offers a workable account of the field, of technical possibilities, and of formal public debate thus far. It will be useful preliminary reading for the further work which the characteristic evasions of that debate make necessary.

Jon Turney

Going Private — The Case Against Private Medicine.
Fightback and the Politics of Health Group, 70p.

This collaboration between **Fightback** and **POHG** has resulted in an excellent report which combines the current facts on the growth of private medicine in Britain with an informed and coherent political analysis, as well as providing a guide to tactics to prevent the further encroachment of private medicine.

The argument is not left at the level of the National Health Service is good and private medicine bad. The inadequacies of the NHS in the past are shown to have contributed to the ability of the private medicine to penetrate the public sector. The long-term decline in the NHS is shown to have fuelled people's fears and encouraged even trade unions to join private schemes.

In the section on the extent of the problem it is argued that the issue of 'pay beds' within the Health Service should be broadened to include private hospitals and clinics. Even as I was reading the pamphlet I heard that planning permission has been granted to an American company which wishes to build a private hospital on land adjacent to the public hospital in Southampton. For the implications of this and other developments read this report.

Tony O'Connell

Pamphlets and Books received

Entry in this listing does not preclude a future review.

Against Biological Determinism, Dialectics of Biology Group, Steven Rose (General ed.) (Allison & Busby) pp 184 pb £4.50. "The last decades have seen a resurgence of attempts to define 'human nature' as the product of biological inevitability, to assert that biology is destiny, determining capitalist competition, sex roles, racial and national antagonisms – the list is endless. The ideological purpose that these types of explanation serve for a society in crisis are clear and they have been enthusiastically embraced by the New Right in Britain, the USA and elsewhere. This book, the product of a unique international meeting of biologists, psychologists, philosophers and sociologists, is a powerful rebuttal of such biologically determinist, reductionist views." This is the companion volume to *Towards a Liberatory Biology,* see below.

The Arms Drain: Job Risk and Industrial Decline. A Trade Union Analysis, Tim Webb (CND) pp 43 pb £0.50 + 20p from CND, 11 Goodwin St, London N4. Useful statistical information on impact of militariam on British industry.

Beyond the Cold War. Not the Dimbleby Lecture, E.P. Thompson (European Nuclear Disarmament & Merlin) pp 36 pb £0.60; from END, 227 Seven Sisters Rd, LondonN4. "Readers may now form their own judgement on why the BBC decided to have no Dimbleby Lecture in 1981."

CALIP Newsletter 13, The Conservation Society Campaign Against Lead in Petrol, pp 20, minimum £2 for a year's subscription for 4 newsletters; from CALIP, 68 Dora Rd, London SW19 7HH. A must for anybody interested in the Campaign.

The Destruction of Nature in the Soviet Union, "Boris Komarov" (Pluto) pp 149 pb £2.95.
"Smuggled out of Moscow by the author, a high soviet official in close touch with the scientific and political establishment, this book breaks through the Russian equivalent of the Official Secrets Act to reveal a problem which has now reached crisis proportions, affecting the very stability of the state."

Ecology for Beginners, Stephen Croall & William Rankin (Writers and Readers Publishing Cooperative) pp 174 pb £1.95.
Another title in the "For Beginners" series, bringing together plenty of information about the ways ecology affects our day to day life. Lively presentation – lots of pictures! – which simplifies without being simplistic and doesn't forget the politics.

Radical Science Journal 11, pp 154 pb £2.00; individual copies from BSSRS. Includes eagerly awaited RSJ Collective statement about their proposed "reorientation of the analysis of science in an agitational direction concerned with struggles around the process of origination of scientific products, the labour process, the social relations and articulations of scientific work." Strongly criticised by Hilary and Steven Rose in the *Socialist Register* and elsewhere "for going too far with concepts of ideology, fetishism, reification, social relations, labour process and reflexivity," the collective contend that their perspective is more amenable to making subversive politics in science, technology and medicine than the traditional leftist views, "including thos of the Roses. They argue that a concern with the "objectivity" and "scientificity" of science forces would-be radical scientists to represent reality in a way which limits what it is plausible or possible to contest."

The Nuclear Numbers Game: Understanding the Statistics Behind the Bombs, Radical Statistics Nuclear Disarmament Group (BSSRS) pp 95 pb £1.50 + 35p; from BSSRS. Amongst a million other scary things, find out how to calculate your own area's devastation rate. Far and away the most detailed and thorough account.

CLASSIFIEDS

Classified ad. rates are 5p per word and 10p for headings (in bold).

TRAINING POSTS IN COMMUNITY HEALTH CARE

YEMEN – 2 midwives & a health worker trainer are needed to join a project training local midwives & primary health workers in mountain villages, in preventative measures & improved clinical techniques.

ZIMBABWE – community nurses, health visitors & midwives are needed to contribute to the Zimbabwe government's primary health programme. They will train community health workers, & be based in rural health centres & district hospitals.

NICARAGUA – nutritionist to contribute to training of local health workers & planning policy, as part of a government primary health programme.

TOWN PLANNER, NICARAGUA
to work on urban development schemes with Nicaragua's housing ministry.

PHOTOGRAPHER, ECUADOR
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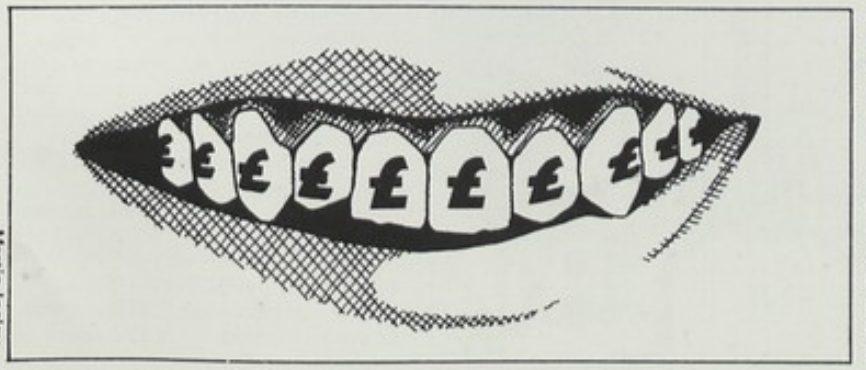
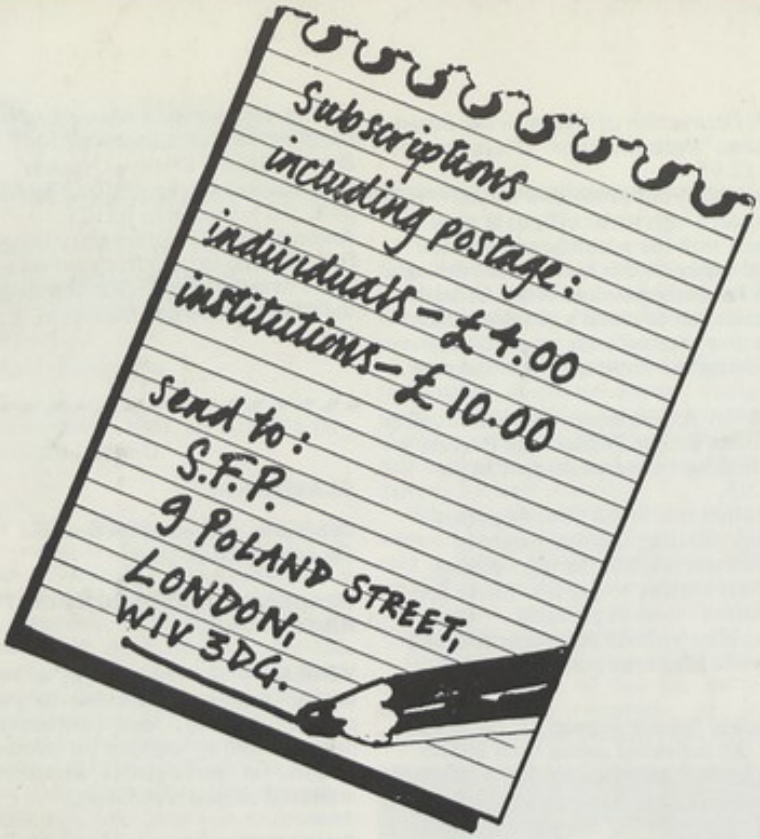
PERSONS WANTED to join AT group negotiating to buy/lease, live/work space Galway area. Capital, energy, commitment in appropriate mix. Practical experience in solar/wind/water power an asset but common sense more important. Please write, at length, to: Dave Calvert, 3 Hilton Place, Leeds 8

The Dental 'Drill and Fill' Racket.

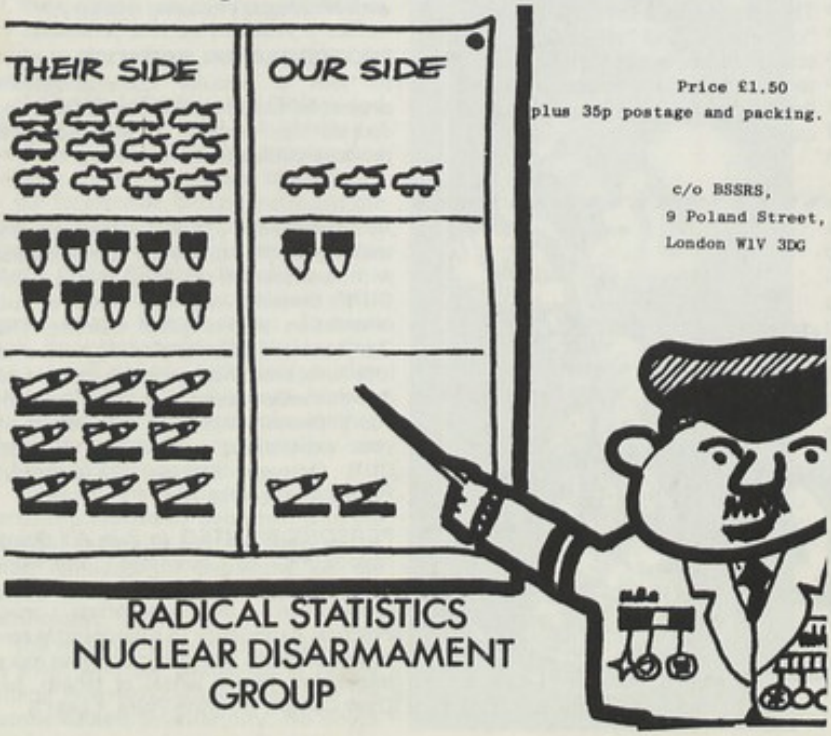


Martin Ingley



The Nuclear Numbers Game

Understanding the statistics behind the bombs



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London W1V 3DG

RADICAL STATISTICS
NUCLEAR DISARMAMENT
GROUP

BSSRS

BSSRS is a group of scientific and technical workers in industry, hospitals, education and research establishments.

We believe that science is not neutral. It cannot be separated from politics. It both reflects and helps determine the values of society. Hence to change the social role of science it is necessary to change society.

We are committed to fighting for the use of science and technology by and for the benefit of working people, to demonstrating the political content of science and to furthering the links between scientific workers and the rest of the labour movement.

- local groups exist in a number of towns;
- the Work Hazards Group provides information and advice to trade unionists and community groups; it publishes the *Hazards Bulletin* bi-monthly and a series of booklets: Noise, Oil Mists, Vibration and Asbestos;
- other specialist groups are listed below; additional areas in which BSSRS has intervened are, riot control techniques in Britain and Northern Ireland; women and science; military research in colleges; unionisation of scientists; sexism in science; an ecology counter course; science education.

BSSRS attempts to do this in several ways:

- A quarterly magazine – *'Science for People'*,

BSSRS GROUPS

Agricapital
Politics of Energy
Sociobiology
Work Hazards Groups (several around the country)
Hospital Hazards Group
Radiation Hazards Group
Women and Work Hazards Group
Women's Caucus
Unemployment among Scientists
Science Teachers' Group
Local Groups in Birmingham, Brighton Cambridge, Durham, Edinburgh, Kingston, London – University College, Manchester, Pennines, Sheffield.

AFFILIATED GROUPS

Politics of Health Group
Radical Statistics Group
Radical Statistics Health Group
Radical Science Journal

JOIN BSSRS!

Membership includes Science for People and the Internal Bulletin

Ordinary Membership	£7.00 per annum
Student/Unwaged	£3.50 per annum

Pg 1 19/20 reinforcing in an almost religious manner the intellectual...

Pg 2 5 Possibly Arthur believed in the same principle.

6 I can remember little of the detail of these discussions but there is little doubt that they centred on one greater theme...

15 Though the revolution would be violent?

Pg 4 and with it came the disappointment

The Times
Saturday 23rd June 1973

OBITUARY

KEITH GILBERT

Keeper at the Science Museum

Margaret Weston, Director of the Science Museum, writes:—

Keith Gilbert, who has died at his Dorking home, had been Keeper of the Department of Mechanical and Civil Engineering at the Science Museum since 1962. He was 58.

He was well-known in our circle for his long association with the Newcomen Society, first as its secretary and then as its current president. Educated at King Edward VI School, Birmingham, and St John's College, Cambridge, he first became research assistant to Professor B. W. Holman at the Royal School of Mines, and then became a flight lieutenant in the RAF during the Second World War. On demobilization in 1945 he worked in the research laboratory of CAV Ltd, joining the staff of the Science Museum in 1948 and subsequently having charge of the collections of Textile Machinery, Hand and Machine Tools and Fire Fighting Appliances. The strength of these collections testify his scholarly work and his application, as do the books and catalogues he wrote.

His other great interests in life were music (especially opera) and mountaineering. He was of quiet, gentle disposition and he reserved his friendship for a few close companions. But once he had made up his mind that a course of action was right he pursued it quite unremittingly and even relentlessly.

What I best recall is how his face used to light up when he spoke of something which obviously gave him joy. His happy married life with Lucy Auerbach lasted for 30 years. After her death he married Electra Michelaidou, who survives him.

PONGAROA HONOURS A NATIVE SON

In the November 1996 issue of *Historic Places* an article by Bernadine Meech about Maurice Wilkins, one of only two New Zealanders to be awarded a Nobel Prize (the other was Ernest Rutherford), drew attention to the link between Wilkins and Pongaroa, the Wairarapa township where he was born in 1916. Wilkins spent only the first six or seven years of his life in New Zealand. His family returned to Britain in 1923 and it was there Wilkins pursued his distinguished scientific career. In March 2001 Pongaroa gave further recognition to its connection with Wilkins when it unveiled a monument on the Village Green. The initiative for the project came from Bernadine Meech. It was taken up eagerly by a local organisation, Pongaroa, the way to go. Pongaroa is proud to have the Wilkins family part of its past and to have a monument to the achievement of its distinguished "native son". The picture shows Bernadine Meech by the monument.

MEMBERSHIP SURVEY RESPONSE

The Historic Places Trust is very grateful to all those members who responded to the membership survey which was sent out in the last issue of *Historic Places*. We have been overwhelmed by the response from members, which at last count exceeded 1,500 replies. Replies are still coming in. We particularly appreciate the effort many of you made to offer your thoughts and opinions on several of the Trust's activities. Many valuable suggestions were made, and we will take these into account as we evaluate the data. At the time of going to press, staff at the Trust have started gathering the data from the survey so that future marketing and membership strategies may be developed. Once the survey evaluation has been completed, we will provide an overview of the results in this magazine. Thank you all for your assistance.

AWARD TO MEMBER

A NEW CORPORATE SPONSOR

New Zealand **Historic Places Trust, number 81**, May 2001, page 4

Immediately to the left of Bernadine Meech is George Jones (President, RSNZ Wellington Branch; several other hats) and to the left of him Gregor Yeates (President, RSNZ Manawatu Branch). Each of these two Branches made contributions to the cost of the monument.

the Blind buildings in Auckland, the surname of Clutha Nantes Mackenzie, who played a leading role in the life of the Institute, was mistakenly spelled "McKenzie". The error is regretted.

membership card. To find out more about this offer, please call Cooper's Restoration on 0800 678 678. The Trust looks forward to a rewarding working relationship with Cooper's.

historically significant for the parts congregations and clergy of successive generations have played in the city's life.

G Barnard 'A fragment of memory' TV. tape.
he was at Princeton in 37–39.
~~middle of Sept~~ 7 Sept back in London
(Student - T. 1908 Gossett)
when right & wrong? ? predictability

Margaret Gardiner. 25 Dec 88 Ch
aged 84. Father egyptologist. strict nurse. anarchic mother. Frobel school. Art
Vintage friendship (Auden) Bernal Science makes money, ovl spends it
Gabo said art can change society. All art is abstraction
staircases are becoming & change.

Art & Culture. Bob Cooper ~~very~~ opposite of me. iconoclastic
Eliture Joyce . Wordsworth & Coleridge at O level Pre-Raphaelites
Music & my father . gramophone. Ellington no concerts? Films. Cosmo
Marx Bros (first time) Potemkin Citizen Kane /. Arts Soc. Pargeter. Herb. Read
on Surrealism. Botanical Gdns. & Fitzwilliam. ~~Gogs~~ → Empty! Gogs
I did some painting. Morris Cunningham (Patrick Heron) Tillotsons
Ely Cathedral. Sky Welsh hills. the black dog. Charles Morgan
Mass Observation.
Manchester

Personal Life. Great big buildings like Citadels. myopic eyes after exams
First term. nose & sinuses. (Moved into lodgings. marmalade & bread & milk)
Back at Christmas. Now I am grown up! Morris & Irish trip.
Corn Exchange Str. names & indicator In or Out. Lonely. | Many cleverer people clear from beginning!
Grand tour. Andrew Barlow. 1937. tetchy.
Physics Pt 2. breakdown. Supervisor Carl Michaeli complained I wrote Pop. Science for Left wing Youth paper.
Tutor sent me to see Barrister Anxiety about my father B'ham to Coventry in winter
My sisters expenses in Oxford. Flew to ~~Germany~~ Hamburg. I was the good, sensible dependable son)
Life all in the head. I had friends but lonely. Arthur Hone. Cotswold Holiday.
Bumping into girl at corner. Was I ever drunk ?? no dancing singing or loving swimming.
10:1 ratio. Margaret Ramsay.

EHW.
Politics Keith G. Wells & Shaw. CP member? Soc Soc.
Gas chamber? Pact to prevent Nazis & all others attacking
Spain. Socialism without revol & Democrat. Epitomised
Why S.U. Kept out.
King & Country 1932

my politics Gilbert. Wells Shaw CP?

Very strong Popular Front Anti Fascist + Progressive Positive (largely Pro Soviet). very Hopeful.

Spain rallying point. Democratically elected. Epitomised anti fascism

The CP was the main leadership, intellectual analysis
Working class friends. Bthw. George Barnard

Very extensive scientists movement ASW. edit. in Nature.
Scientists refugees from Nazism. Academics, quakers etc helping.
Very extensive medical movement. Poverty + Malnutrition EHW.
McGonigle + Boyd Orr.

Marching in London. Scholarships not Battleships.
Baldwin wants war. (?) - Daily Worker + Haldane.

The problem of the Trials. newspaper propaganda? Confessions?

Dialectical materialism - not understood? Hegel now
(a few ~~some~~ addicts)

my doubts re Soviet help for Spain + non-intervention

Polemics after reactions. Authority of many fine people + fine minds
Failed to see autocracy + bureaucracy.

Bringham. Finnish War. Philip Morples.
Soviet Nazi pact. ~~left~~ broke away CP. but explained in terms of avoiding combined attack Britain France + Germany on S.U.

Political movement in Cambridge 1935-38.

Most significant aspect of my Cambridge experience. Important to rebutt the current claim ① Cambridge Com. were spies ② The left were appeasers of fascism.

1. Blunt et al were ~5 years earlier in arts & took jobs in security

In the late 30s the movement was large-scale & open. public campaigns. none of the people I knew were in a panic to spy, they were mainly scientists rather than arts.

2. The left had a very clear view of Fascism as 1. barbarity 2. war-making & ~~its dependence on war~~

Rex strongly opposed appeasement. Much evidence that 'respectable' Tories were interested in doing a deal with Hitler & arranging an Anti Communist war against the Soviet Union. The French cleared The Left led the anti war movement & joined Christian pacifists in that work. They also opposed ~~the~~ British rearmament & described the horrors of war but they saw the Fascists as the war-makers. The 1932 King & Country resolution was before Hitler came to power & may not have been simple pacifism but literally not accepting King & Country as a war aim

The general background was ① Gen. Strike 1926 & Depression. Contradictions of capitalism. Poverty in the midst of Plenty. (EHW)

② The rise of the Soviet Union. Webbs Russian films etc.
Malcolm Muggeridge was an exception.

Remarkable era of progressive thinking Unity of Art Medicine Science Films Anti-Fascist & Pro Left. Margaret Gardiner & Freud Picasso. Henry Moore, writers, poets even surrealists & earlier Isadora Duncan. (Experimental Art & writing went out of favor in the Soviet U)

Science was just being held back by contradictions of capitalism & when unleashed by Socialism would bring untold benefits.

Bernal: contradictions enormously stimulating to me.
remarkable rhetoric ⇒ value of science. Ex Catholic he had simplicity of Faith was scientistic (but very imaginative). Tho' very broad cultural interests art ~~etc~~ history ... failed to see complexities of human dimension of science. Failed to see environmental crisis (pollution obvious then in '30s) Saw the power, but, apart from war, failed to see the destructive side.

Time / 89 Rose interview. Cambridge life

Contacts with research workers. Redman. Frenlin, before I went up. Reflectors. Burch. Armagh visit. Ellison + his book
Redman's advice to do Physics

Driven to Cambridge. Resentful of rich son whose father went to Cambridge
Fust time away from house. Large buildings. Sense of vocation. Fright + illness But

John Frenlin's 'Holy lab'
Science for human benefit. Science library at School. A.V. Hill
Living machinery.
Science Choice of College. Physics. big. Keith Gibert
Privilege + access. Wordie + bear rug. repelled by Softers.

The Earth Harold Jeffreys. Oliphant 1st year 1:1 Cockroft. Eddington meeting.
Dirac, Rutherford's lecture / The friendly staff. Dee's hello. Ratcliffe locking door
Bullard Wells sat + talked / Cosmic ray man Carmichael not reading the maths.
rather like an apprenticeship G.F.C. Searle's deadly practicals. visit
to Cambridge Sci Instr Co. in some ways a tiresome interval
Wordie said broaden
Palaeontology + mineralogy + the room in B'ham museum.
Lyells. Geology. Natural Science Club. Andrew Huxley Synge etc
Pt 2 Crystal Physics. Jack Allan in Mond
My breakdown (see later). 2.2 // not nuclear physics. Cockcroft in
Mond Lab. Electrons were alive. how right I was. Bernals TMV. (fixed
(And lost interest in astronomy).
Braggs appointment to Cav. Chair.
Had to leave.! Other Universities Rang Oliphant.

CSAWG. Thro' Frenlin. Bernal, the Woosters → A.S.W. Christian Pacifist. Yates Needham? Maasdorp. [King Parade]
J. Humphrey. Dick Synge

Frenlin in Lyons (with typewriter) Began as educ. + propp. to Peace Groups. W.I.s, Unions
horrors of war + danger of Fascism. Air show leaflets. Nature correspondence. Use science to stop war!
Baldwin + bombers. Gas masks + gas proof rooms. Arsenical smokes. Sealing room. liberating CO_2 experiment
hot + smelly. Incendiary bombs. Deep shelters. Finsbury Labour Borough. concrete.

Was right about level of damage except in early war.
Were right Bernal that science needed large-scale planning.
Were partly right on shelters.
—
Were not right?? on opposing rearmament. But did not
Know who was going to fight who. Big question to John Frenlin.

Bernal.

King's College London (KQC)
UNIVERSITY OF LONDON

STRAND, LONDON WC2R 2LS TELEPHONE: 01-836 5454
Ext. 2426

M.H.F. Wilkins
Emeritus Professor of Biophysics

19 May 89

Dear George,

I enclose a rush mash of correspondence with the Fremlins (They were very happy that ~~they~~ you should see what they had written). Also I enclose a draft about Arthur Hove & yourself. I hope this is not too much to plough through. The matter of political affiliations could be a sensitive area & I would be glad if you could be frank about anything I have written. If you could let me have any reaction ~~by~~ in time on time! for the next Steven Rose interview it would be good but I realise you may be too busy for that.

All best wishes

Yours

Maurice

King's College London (KQC)
UNIVERSITY OF LONDON

STRAND, LONDON WC2R 2LS TELEPHONE 01-836 5454
Ext. 2426

M.H.F. Wilkins
Emeritus Professor of Biophysics

13 June 89

Dear George,

Your letter came in time for the Rose interview. I enclose a copy of the document about the interviews. Your letter & the interview you enclosed were most helpful & I shall come back to you in Sept when I look forward to meeting you. You are quite right that 'hypnotic' must be got rid of. Your point about judgements about whether one did right or wrong in the past is a very good one — I wonder what St Peter at the gate of heaven would think about that?

best wishes for the summer

Maurice

Obituary: Margot Heinemann

To the aid of the Party

FOR the young Cambridge Communists of the 1930s Margot Heinemann, who has died at the age of 76, was primarily the lover of our hero John Cornford, killed in the Spanish Civil War. Since the moving, untitled poem *(below right)* which was one of the last that Cornford sent to Margot and which has become an anthology piece, her name will most likely continue to be tied to his, and to Spain: rightly, for she defended the cause of the Spanish republic until the end. But it is absurd to think of this brilliantly talented, powerful and independent personality as an adjunct to someone else.

She devoted her life to the cause of communism and the intellect with a tenacity of will which, until the end, defeated persistent ill-health ("Margot is either at death's door or writing a pamphlet"). She came to both from a Frankfurt-born merchant-banking household which transferred to London before the Great War — but which was not sufficiently anglicised to know that one sent a daughter either to Roedean or to King Alfred School, but not to both. This experience no doubt helped to give her that sense of the ludicrous which made her company so enjoyable, even in her most formidably bolshevik period.

At Newnham she acquired a love for literary scholarship but after a postgraduate year she postponed it for half a lifetime and, as the hunger-marchers passed through Cambridge, nourished a passion for the British working class. She joined the Communist Party in 1932 and after a spell of teaching at the Cadburys' Bournville, served the trade unions in the Labour Research Department, which compensated for the virtual absence of full-time union research officers before the second world war. She stayed there until 1948.

The British labour movement then was not much used to intellectuals, still less to high-powered women. This was especially true of the miners, in whose affairs Margot specialised. Her first book, Britain's Coal (1944) was written for them. The experience did nothing to diminish her admiration for the British proletariat, especially when it came from South Wales, and even more when it came from Fife and was called Abe Moffatt. That the miners actually offered her a full-time post in the union (which she refused on the advice of Harry Pollitt, whom she helped with his speeches) speaks highly for both sides. No woman has ever won more respect in that very macho union, or known more about it.

She left the LRD to work for the Party at King Street, but soon left full-time work when she had a child by the great crystallographer J. D. Bernal, with whom she began to share her life at this time, insofar as the Sage's complicated life-pattern permitted monogamy. Her semi-autobiographical novel The Adventurers (1953) dates from this period. Though she continued to be profoundly, but critically, engaged in the Party, not least as a longtime member of its London District Committee, she never returned to full-time political work. The events of 1956, about which she was to write an illuminating memoir, made even the most monolithic reconsider the equation: Party = life.

She became a teacher of English literature, first at Camden School for Girls, later at Goldsmiths College, finally at New Hall Cambridge. An academic at last, she could write what she had planned at Newnham, a study of culture in another period of revolution. Her Puritanism And The Theatre was outstanding. She also, not quite so successfully, wrote a study of Britain in the 1930s with a former colleague from the LRD, Noreen Branson.

She went on teaching, researching, writing and, when possible, walking on mountains until the end of 1991, a low-slung, ravaged, passionate figure sustained, among increasingly frequent bouts of hospital, by the sheer will to live and work, by wit and a sense of the absurd. But perhaps the end of the Communist Party diminished her determination to resist death.

She leaves behind a daughter, Dr Jane Bernal, at least two books which will last, and friends who know that she was one of the most remarkable people of our time and a testimony to its indestructible hopes.

Eric Hobsbawm

Margot Heinemann, born November 18, 1913; died June 10, 1992.

Defender of communism and of the Spanish republic . . . Margot Heinemann, of whom John Cornford wrote:

Heart of the heartless world,
Dear heart, the thought of you
Is the pain at my side,
The shadow that chills my view.

The wind rises in the evening,
Reminds that autumn's near.
I am afraid to lose you,
I am afraid of my fear.

On the last mile to Huesca,
The last fence for our pride,
Think so kindly, dear, that I
Sense you at my side.

And if bad luck should lay my strength
Into the shallow grave,
Remember all the good you can;
Don't forget my love.

Birthdays

(right), who is 53 ... Mother, 71; **Sandy Barclay**, jockey, 44; **Nick Brown MP**, ... Treasury spokesman, 42; ... 'Doc' Cheatham, jazz ... **David Curry MP**, ...

Ma... Goodman

...g a marvel of Captain ...

... was ousting the text-heavy pulps on the newsstands — comic books.

In 1929 Tarzan and Buck Rog... had burst from the pulps ... colour newspaper ... were soon being ... comic books, 64 ... or a dime" ... horror comics that ... strict controls ad... the Comic Cod... Goodman's ... by cutting ... own di... sa...

Eagle 1991

EMMERICK, Ronald Eric (BA 1961), Professor of Iranian Philology at the University of Hamburg, has been made a corresponding member of the Istituto Italiano per il Medio ed Estremo Oriente, and of the British Academy.

EPSTEIN, Steven Allen (BA 1976) has been an Associate Professor of History at the University of Colorado, Boulder, since 1988.

ESSAJEE, Shaffiq (BA 1986) BM, BChir (Oxon) was awarded the George Pickering Prize for the best performance in finals 1990.

FAIRHURST, Jack (BA 1947) has been Administrator, McDonald Institute for Archaeological Research, University of Cambridge since 1990.

FINDLAY, Peter Robert (BA 1963) has been an instructor in French and German at Erith College of Technology since 1990.

FISHPOOL, Geoffrey Mark (BA 1988) has been a post-graduate student at Imperial College London, studying for a PhD in plasma physics, since 1988.

FISHWICK, John Charles (BA 1982) has been Lecturer in Farm Animal Practice, Royal Veterinary College, University of London since 1990.

FLANDERS, Julia H. (BA 1989) has been a graduate student at Brown University, Rhode Island, since 1989.

FOGG, Professor Gordon Elliott (PhD 1943), Emeritus Professor of Marine Biology, University of Wales, became a Fellow of Queen Mary and Westfield College in 1990.

GALBRAITH, Ian Geoffrey (BA 1970) became Head of Upper School at Dulwich College in 1988.

GAMBLE, Paul William (BA 1983) has been Assistant Master in the Classics Department of Radley College since September 1990.

GARNETT, Dr George Stephen, Fellow 1983–87, was appointed to a Tutorship and University Lectureship at St Hugh's College, Oxford, in 1990.

GILL, Christopher John (BA 1967) was appointed Senior Lecturer in Classics at Exeter University in 1989.

GIULINI, Domenico J. W. (PhD 1990) has held an Assistant Professorship in the Institut für theoretische Physik at the University of Freiburg since 1990.

GOLBY, David Harold (BA 1961) has been Bursar of Handsworth Grammar School since 1990.

GREEN, David Mino Allen (BA 1973) has been Head of Economics at South Bank Polytechnic since 1989.

GREENWOOD, Roger Philip Garnett (BA 1979), has been Head of Classics at Rokeby School, Kingston, Surrey since 1988.

GUNN, John Michael Ferguson (PhD 1980) has been Professor of Theoretical Physics in the University of Birmingham since 1 January this year.

HADDEN, Michael John (matric 1978), has taught mathematics at Hills Road Sixth Form College, Cambridge since 1990.

HADLEY, Eric Julian (BA 1972) has been Head of Modern Languages at Warwick School since 1990.

HALL, Professor George (PhD 1951), former Fellow received the first ever degree of DEng (Honoris Causa) from Kyoto University in 1989.

HARE, Paul Gregory (BA 1967), Professor of Economics at Heriot-Watt University, is Visiting Professor at the Centre for Economic Performance, London School of Economics, for 1990–91.

B. KNOX

HAUNER, Milan (PhD 1973) has been Director of East European Studies, Woodrow Wilson International Centre for Scholars, Smithsonian Institute, Washington since 1990 and is Visiting Professor of History, University of California, Berkeley.

HAWKINS, Thomas Desmond (MPhil 1989), President of Hughes Hall, Cambridge, was President of the British Society of Neuroradiology 1986–89.

HAWTON, Keith (BA 1965), Consultant Psychiatrist and Clinical Lecturer at Oxford, was Visiting Boerhaave Professor at the Department of Psychiatry, Leiden University, from 1 October 1990 until 31 January 1991.

HELZLE, Martin (PhD 1988) was appointed Assistant Professor of Classics, Case Western Reserve University, Cleveland, Ohio in 1990.

HENNESSY, Dr Peter John (BA 1969), BBC Radio presenter, political journalist, and Visiting Professor of Government at Strathclyde University, received a PhD from Cambridge in 1990.

HOPE, The Rt Hon Lord (BA 1962), Lord Justice General of Scotland and Lord President of the Court of Session, has been elected Honorary Member of the Society of Public Teachers of Law and received an Honorary LLD from the University of Aberdeen in 1991.

JEFFERY, Keith John (BA 1974), Senior Lecturer in History at the University of Ulster, has been appointed Secretary of the Irish Committee of Historical Sciences, and was the principal Irish delegate to the 17th International Congress of Historical Sciences in Madrid in August 1990.

JOFFE, Michael (BA 1965) was appointed Senior Lecturer in Public Health at St Mary's Hospital Medical School, London, in 1989.

JOHNSON, Dr Matthew Harry (BA 1985), PhD 1990, has been a Research Fellow in the Department of Archaeology of St David's University College, Lampeter since 1990.

JONES, Michael Edward (BA 1986) has been a Research Associate in the Physics Department since 1990.

JONES, Richard Granville (BA 1947), Chairman of East Anglia District Methodist Church, received an honorary DD from Hull University in 1988.

JORDAN, Patrick John Francis (BA 1962) has been Headmaster of Packwood Haugh School since September 1988.

KENDLE, Professor John Edward, Commonwealth Fellow 1985, is Chairman in the Department of History at University of Manitoba, 1990–95.

KINDBERG, Tim Paul James (BA 1980) has been a Lecturer in the Department of Computer Science, University of London since 1990.

KING, Professor Edmund Joseph (BA 1963)has been Professor of History, University of Sheffield since 1989. He was Visiting Fellow at All Souls, Oxford, in Michaelmas 1990.

KINGS, Steven (BA 1984) who is training for ordination, was awarded the highest first in theology at Durham University in 1990.

KNOX, Bernard MacGregor (BA 1936), Director Emeritus of the Center for Hellenic Studies, Washington DC, was awarded the Frankel Prize from the National Endowment for the Humanities in 1990.

KONSTAM, Dominic (BA 1986) has been awarded an Oxford DPhil for a dissertation on 'Stock market efficiency and the overreaction hypothesis'.


BUILDING ON EXCELLENCE
ST JOHN'S COLLEGE CAMBRIDGE

I was the dreamer, they the dream; I roamed
Delighted through the motley spectacle:
Gowns grave or gaudy, doctors, students, streets,
Lamps, gateways, flocks of churches, courts and towers –
Strange transformation for a mountain youth,
A northern villager.

William Wordsworth, ***The Prelude***

ST JOHN'S has long been one of the largest Cambridge colleges. At present there are well over 500 undergraduates, nearly 250 graduate students and 125 Fellows. *It has grown, it has changed, yet it is still the same College.* This capacity for renewal in every generation is at the heart of the Johnian tradition.

Endowed with a unique site straddling the river, we have the finest range of College buildings in Cambridge, tied together for the last 150 years by the Bridge of Sighs. Little wonder that the young Wordsworth "roamed Delighted", just like any of us in our first term here. The fabric of the College is our heritage. Its growth over the centuries was not inevitable, it did not happen by accident, it was not without cost.

This is the College which has produced "generations of illustrious men" – reinforced by women too since their admission ten years ago. Our famous statesmen stretch back to the Cecils, father and son, ministers to Queen Elizabeth. Palmerston is one of our quartet of Johnian prime ministers. The abolition of the slave trade, which he helped achieve, completed the earlier work of Thomas Clarkson and William Wilberforce. Alfred Marshall was pre-eminent in establishing Economics as a University subject. The scientific achievements of the College defy easy summary: John Couch Adams's mathematical prediction of the existence of the planet Neptune; the development of the steam turbine by Sir Charles Parsons; Paul Dirac's formulation of Quantum Theory for which he received his Nobel Prize, now displayed in the College Library. And three of the ten Law Lords are currently Johnians. We are set to maintain this tradition. Among our Fellows today we have no fewer than twenty Fellows of the Royal Society, the British Academy or the Fellowship of Engineering to enhance our international reputation in research and scholarship. And we are recruiting students of the highest quality from schools of all types; their achievements, both academic and extra-curricular, are regularly among the best in Cambridge.

I could not print
Ground where the grass had yielded to the steps
Of generations of illustrious men,
Unmoved; I could not always lightly pass
Through the same gateways, sleep where they had slept,
Wake where they waked, range that enclosure old,
That garden of great intellects, undisturbed.

The Prelude

BENEFACTORS

The College's longstanding reputation for admitting students simply on ability has always depended on its endowments. Some of our most notable benefactors first entered the College through this door of meritocracy, centuries before that phrase came into use. It is right that the names of our benefactors – stretching back to the Lady Margaret Beaufort, whose legacy founded the College in 1511 – are read every year in Chapel.

In the 1770s, James Wood arrived as the son of a Lancashire weaver and lived in a garret, with his feet wrapped in straw, studying by a feeble rushlight, since candles or a fire were too expensive. Prospering in later life, he was able to leave the College £20,000, which became the nucleus of the fund for building the new chapel in the 1860s. Recently cleaned, its high Victorian style continues to win new admirers.

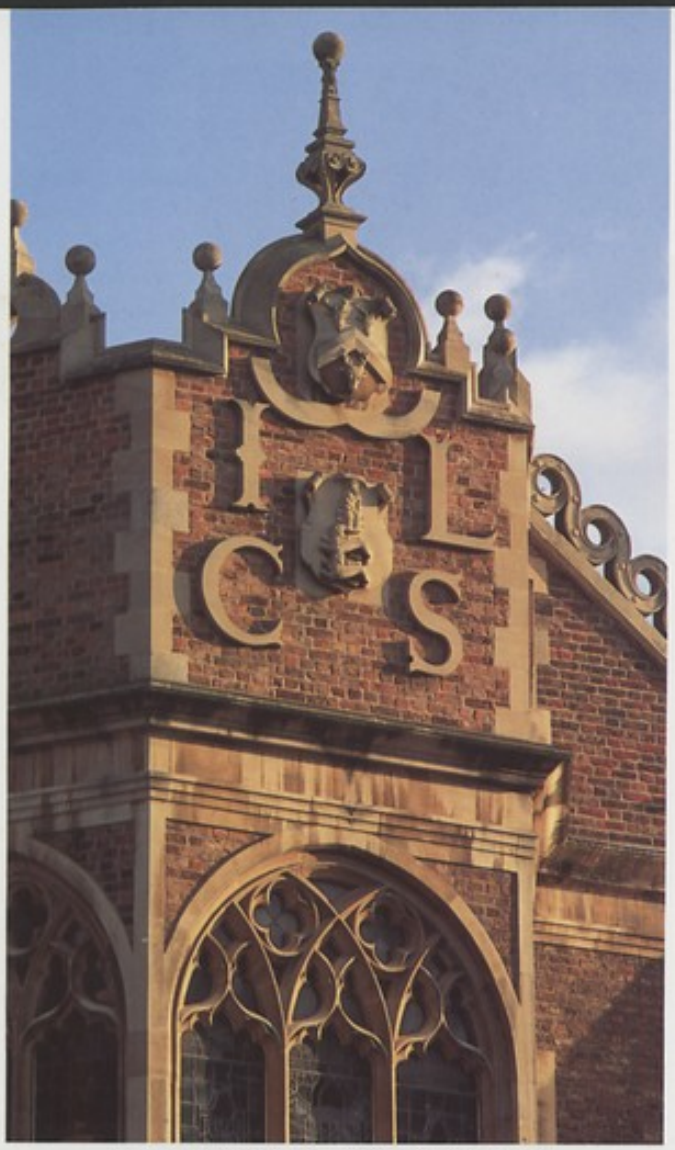
The College's greatest expansion was still to come. Its response to the need to make more university places available after the Second World War led to cramped living conditions which many older Johnians will remember. The construction of the Cripps Building in the 1960s solved this problem, and its name proclaims our debt to its donors.


Lady Margaret Beaufort


John Williams


James Wood


Sir Humphrey Cripps



The initials ILCS (Iohannes Lincolniensis Custos Sigilli), carved in stone on the river end of the library.

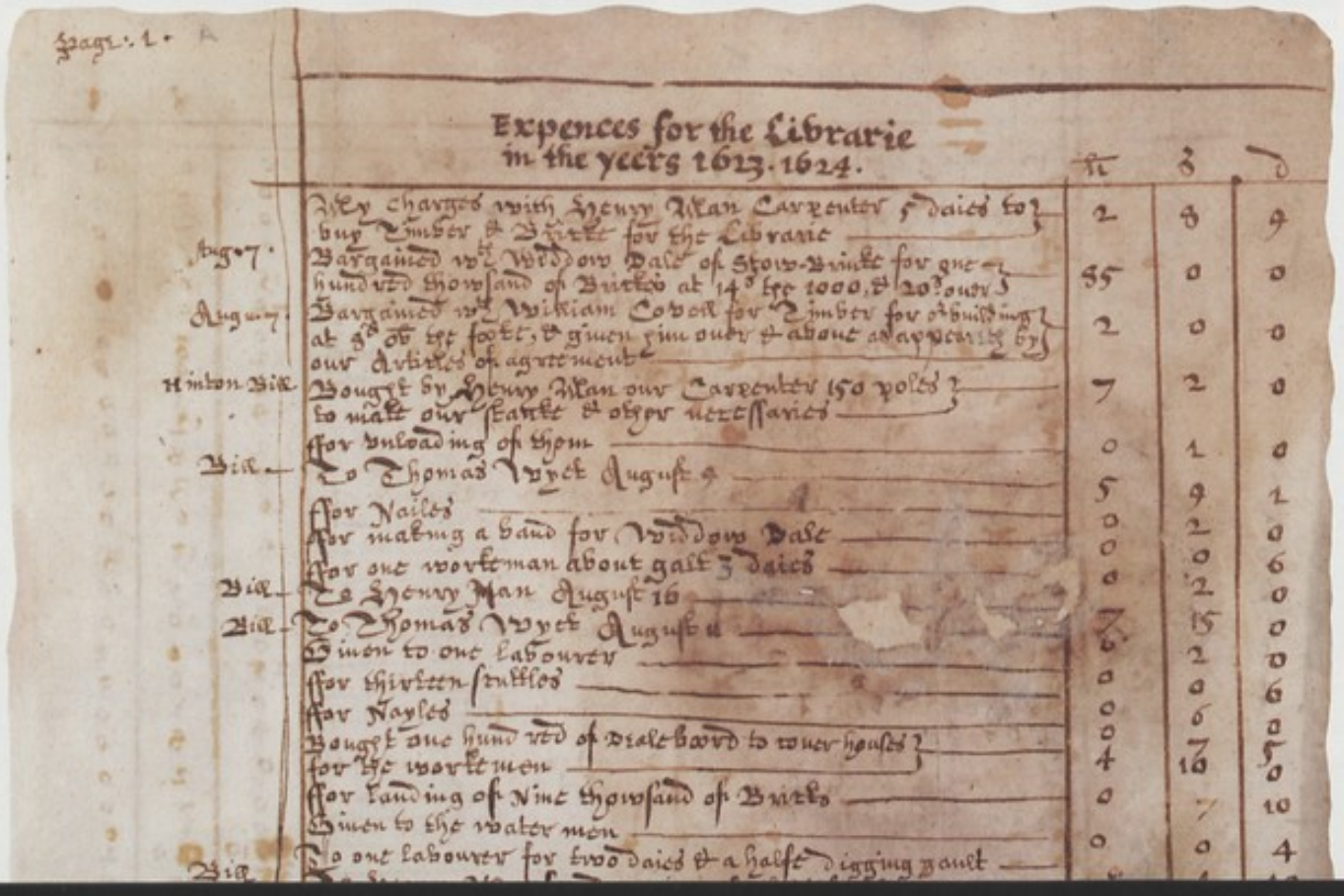
THE COLLEG
our key to "That garde

Right from the start, the buildings erected with the Lady Margaret's endowment contained a library – still identifiable in the First Court by a row of distinctive pointed windows – but, by the 1620s, this had become wholly inadequate. It was a bold decision to put a new library on what was then an undeveloped site next to the river, supported by gifts from John Williams, a former Fellow who had risen to be Bishop of Lincoln. This benefaction eventually totalled over £2,000 towards the total cost of some £3,000 – then enough to purchase a whole new purpose-built library. His initials are carved in stone on the fine building which he made possible.

THE NEW LIBRARY PROJECT

The Library has been on its present site since Bishop Williams's Library was completed in 1624–8. In the past century it was extended piecemeal to meet growing needs, creating the existing working library on the ground floor out of what used to be sets of rooms. This was disruptive and expensive but it did the trick – for a while.

Again we have reached an impasse. Not only has the size of the College increased but needs change, with students now more dependent on college libraries than they used to be. Space on the shelves is exhausted; places for readers are inadequate. Moreover, new technology is double-edged in its impact, increasing both opportunities and costs. St John's was the first Cambridge College to begin computerising its catalogue, and now that this is complete, automation of borrowing will follow. Perhaps more important, the use of electronic forms of information storage is rapidly growing, requiring comparable development of computer facilities.



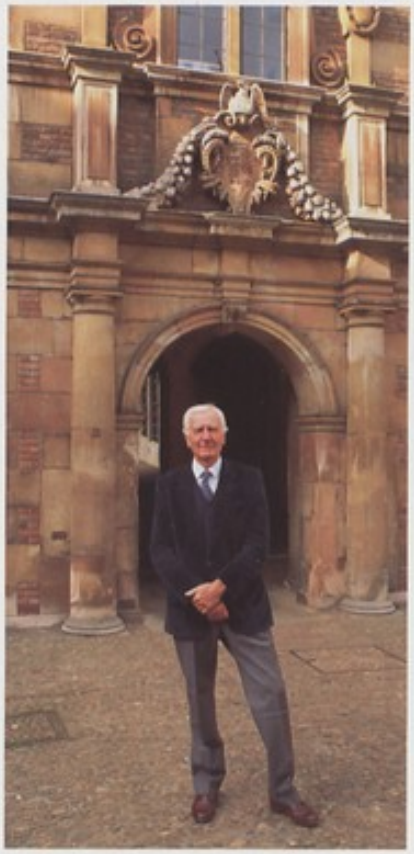
Page. 1.

Expences for the Librarie
in the yeers 1613.1624.

		li	s	d
	They charged with Henry Allan Carpenter 5 daies to buy Timber & Bricke for the Librarie	2	8	9
Aug. 7.	Bargained wt Widdow Dale of Stow-Wricke for one hundred thowsand of Bricks at 14s the 1000, & 20s over	55	0	0
August	Bargained wt William Cowell for Timber for ye building at 8s the foote, & given him over & above as appeareth by our Articles of agreement	2	0	0
Hinton Bill	Bought by Henry Allan our Carpenter 150 poles to make our Stanke & other necessaries	7	2	0
	for unloading of them	0	1	0
Bill	To Thomas Wyet August 9	5	9	1
	for Nailes	0	2	0
	for making a band for Widdow Dale	0	0	6
	for one workman about gaile 3 daies	0	2	0
Bill	To Henry Man August 16	7	15	0
Bill	To Thomas Wyet August 21	0	2	0
	Given to one Labourer	0	0	6
	for thirteen Strikles	0	0	6
	for Nayles	0	0	5
	Bought one hundred of Dale board to cover houses	4	16	0
	for the workemen	0	7	10
	for landing of Nine thowsand of Bricks	0	0	4
	Given to the water men			
Bill	To one Labourer for two daies & a halfe digging gault			

GE LIBRARY

en of great intellects."



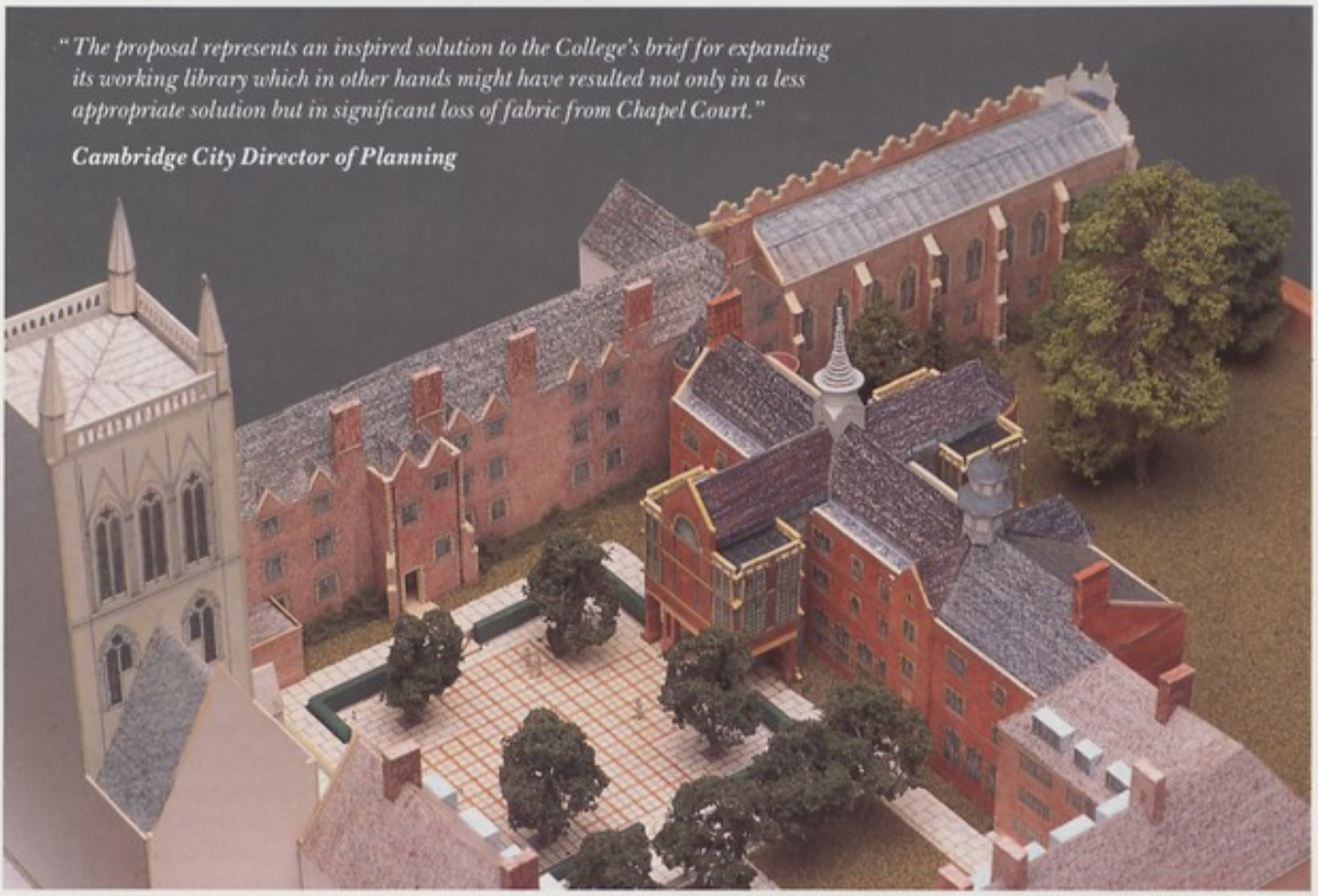
Where can suitable library facilities be put? After long consideration the College has decided to enlarge the Library by extending round the corner, into Chapel Court. The Old Library will be preserved, but now integrated with the adjacent new facilities. This means an ambitious scheme to reconstruct the Penrose Building, opposite the Chapel.

The College has commissioned Edward Cullinan Architects to work within these challenging constraints. They propose to build *across* the axis of Penrose – creating a modern Library with its facade in Chapel Court and an apse going back into the edge of the Master's garden.

Chapel Court will be enhanced in this plan. Its existing character will be retained but the Chapel tower will now face an equally impressive building. The planning authorities, charged with the duty of conservation, called this "an inspired solution" – at once traditional and innovative. The cost of the Library scheme is estimated at £5 million.

"Demands on College Libraries are increasing all the time. Triposes expand, facilities in the Departments and Faculties decline, book prices race ahead of student incomes. We had facilities which were good enough in our day as undergraduates. We must now offer students facilities fit for the twenty-first century."

Professor R.A. Hinde, CBE, FRS *Master*



THE NEED FOR STUDENTSHIPS

We must rely increasingly on our own efforts to keep the door open to talent, from whatever quarter. There is a broad spread of nationalities in the College. At present a third of our graduate students come from 30 overseas countries. A highly successful scheme for Benefactors' Studentships has helped home-based candidates as well as overcoming the obstacle of higher fees for overseas students. Depending on its terms, each award needs an endowment in the range £250,000 to £500,000 to maintain it in perpetuity.

THE NEED FOR STUDENT ACCOMMODATION

The College is nothing if not a residential community. It aims to offer housing to all its students – a policy made necessary by a dearth of rented accommodation. The country needs more graduates, so we must do more. The College's target is 50 additional students over the next five years, provided accommodation can be found. A new residential building in the College grounds is in the early planning stages, at a cost of over £1 million.

OUR FUNDRAISING CAMPAIGN

A WEALTHY COLLEGE?

St John's is often thought of as a well-endowed college. Its benefactions have been accumulated over the centuries and always managed with care. They have enabled the College to cope with heavy new commitments, notably in giving assistance to a series of University projects when support was needed and was not otherwise available. No appeal has been made to Old Johnians in the last thirty years. Savings from endowment income helped the College to undertake its last major building project – the Fisher Building linking New Court and the Cripps Building – within its own budget.

That the College could do this hitherto, however, does not mean that it can go on doing it. Times have changed. There is at present no pool of surplus cash and our income is fully stretched. For example, we have had to reduce the number of studentships we offer. Running down the College's capital assets would simply rob the future, and instead of raiding past endowments we now need to rebuild them by calling upon the generosity of new benefactors.

"It is clear to me that the College needs further resources for these projects and especially for the Library. I am equally clear that the College should not use up its own endowment to achieve these ends: the time has come for Old Johnians and others to contribute and thereby to participate actively in the College's development. As before in its history, the College seeks a new generation of benefactors to sustain its tradition of change and renewal."

Sir John Quinton,
Chairman of Barclays Bank
Chairman of the Campaign Finance Council

OUR SEVEN MILLION TARGET

To achieve our aims we need £7 million. After a careful assessment of what contribution it can make from its own resources, the College can pledge the first £2 million. It will also pay the total costs of fundraising, so that every penny raised will go towards the target. Old Johnians have a special debt which only they can know and only they can discharge. We are looking for benefactors who will make an investment in the future of a world-class college within a world-class university.

SCALE OF GIVING

The target is to raise £7 million for the Library, new Studentships and new Student Accommodation.

This will be achieved by seeking the following levels of gifts:

MAJOR GIFTS

Size of Annual Gift	Value of Gift over 5 years	No of donors Required	Totals
400,000	2,000,000	1	2,000,000
100,000	500,000	1	500,000
50,000	250,000	3	750,000
20,000	100,000	7	700,000
10,000	50,000	10	500,000
		22 gifts	4,450,000

GENERAL GIFTS

5,000	25,000	20	500,000
2,000	10,000	50	500,000
1,000	5,000	110	550,000
500	2,500	200	500,000
200	1,000	300	300,000
100	500	400	200,000
		1,080 gifts	2,550,000
TOTAL		1,102 gifts	£7,000,000

MAKE A DEDICATED GIFT TO ST JOHN'S COLLEGE

In making a contribution to St John's College, you may be interested in making a Dedicated Gift (Endowment) for a specific project. A full list of Dedicated Gifts (Endowments) is available from the Campaign Office.

MAKE A TAX EFFICIENT GIFT TO ST JOHN'S COLLEGE

There can be tax advantages both to you as a contributor and to the College campaign in the method of giving – for example, in the United Kingdom, covenanting. Details are available from the Campaign Office, explaining how you can make your money go further.

FUND RAISING CAMPAIGN OFFICE ST JOHN'S COLLEGE CAMBRIDGE CB2 1TP

Telephone 0223 338700 Facsimile 0223 337035

BUILDING ON EXCELLENCE

The Lady Margaret Boat Club – still an active reminder of our first benefactor

Produced by Tim Rawle Associates
CAMBRIDGE

Printed at The Cloister Press

Dear Dr Wilkins:

This should explain itself.

I hadn't realized that you, also, were at KES Birmingham. I suffered there under R. Cary Gilson a real stuffed shirt. But I had excellent education from Tommy Langley and Bubbles Walton. Also lots of lab. time to myself.

Good luck

Ernie Pollard

Issue 1, February 1990

science & people

THE WOODBURN PRESS

Recollections of Chadwick

Considering the importance of his discoveries and the eminence of his career, there has been relatively little written about Sir James Chadwick. As one of his former students, I kept in touch with him sporadically throughout his life. I would write him and perhaps a year later a reply would come. As he grew older, I began to worry about there being no biography of him, and I wrote to Sir John Cockcroft about the problem. He agreed that there was a problem but added that it had been possible to get Chadwick to dictate a two-hour tape.

From the tape, and presumably other sources, a good account of Chadwick was written in the biographies of the Royal Society of London. I also know of a short account by Maurice Goldhaber, and Oliphant wrote about him in Physics Today. Although there is probably more that I should be aware of, I don't know of it. Consequently, I am taking the liberty of writing my own memories of him.

A Sketch--and A Response

The accompanying sketch illustrates what I mean by "sporadic" correspondence. I am a total bust at producing art, but one day I read in a biophysical article that the eye sees contrasting light and shade and not outline. So I tried that principle on my glasses, which were in front of me on the table, and what I sketched looked like what I saw.

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EDITORIAL:

Science, People, and Survival

This is to be the first of a series of bulletins on the subject of "Science and People." The question immediately arises: "Why the concern, and why the bulletins?"

Understanding Nature Causes Change

Let's take the concern first. Since Galileo, about 300 years back, the understanding of Nature by a relatively small number of "Natural Philosophers," or, as we now call them, "scientists," has steadily increased. With the Industrial Revolution and continuing into the 19th Century, this understanding began to make profound differences in the way people lived.

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This led me to a year of sketching people at conferences, with varying success. Finally, aided by two pictures of Chadwick and my memory, I produced what you see and sent it off to Chadwick in a letter.

Chadwick must have been amused, for after signing the sketch, he commented wryly that my art probably would not earn me a living. The sketch is not a good likeness, but it does represent what Chadwick looked like to me, his neophytic research student. Perhaps his brow is too august and the line of his lips too tight, but there is the man who started me in research and who did so in such a way that I would not let go of it for nearly sixty years. Even more, my evident love of it became communicated to more than two score others who are not letting go of it either.

Chadwick at Cambridge

At that time, 1928, the European System was in full swing at Cambridge. The Cavendish Laboratory was the undisputed domain of the Cavendish Professor, Lord Rutherford, a strong and confident leader in the whole realm of Science.

Chadwick occupied an unusual role as a kind of Director of Research. Perhaps it would be more to the point to call him Rutherford's assistant. This was no mere follower of the boss's wishes: Chadwick had already made one very important discovery and had been associated with Rutherford in several of their pioneer papers on nuclear reactions, including the direct verification of the Einstein mass/energy relation.

Rutherford brought Chadwick with him from Manchester (in Chadwick's case via four years internment in Germany) in 1919, first as a collaborator and then, as the number of research students grew, in the role of advising about 20 highly selected individuals. I used to wonder how on earth he could do it, and I am sure that the tight lip that you see in my picture developed from the need to keep ahead of the group that Rutherford called "the boys" (even though there were young women there).

"The Professor"

Cambridge has a fine tradition of having no classes in June, July, August and September. Laboratories were legal but no lectures. So we neophytes, about seven in number, showed up in June and were herded into "the Nursery," a kind of loft room up a short flight of stairs. Here various modestly advanced experiments were set out, and Chadwick assigned us to do those he thought made sense.

Now time passed and a sort of scientific "Outward Bound" took place. Chadwick would come rapidly up the stairs, light shining on his glasses, and if the day were propitious, one of the groups would accompany him downstairs and be assigned to one of the faculty to get into real research.

My partner and I were among the last to go--rather, we never went because we were assigned to Chadwick himself. Our task was to find a MUCH better way to detect nuclear particles. After a year with false starts and inadequate equipment, we gave up: the problem had been sort of solved by two students a year ahead of us, and we had to use their method. So that put us in a big room downstairs next to "the Professor." Consequently, I saw Chadwick every day and Rutherford nearly as often.

An Understanding Tutor

It was a difficult time for me: my eldest brother, to some extent a role model for me, had incurred a serious mental breakdown from which he only marginally recovered. I was put into a state of dreadful uncertainty: I simply could not let loose that single-minded drive that I knew was needed in research. I did some good teaching to earn my pay, but I have done a great deal better in the research lab in subsequent years than I did then.

So how did Chadwick deal with me? He soon caught on to my problem, and he may have noticed that in spite of my mental trauma, I couldn't resist the work in the lab. He also noticed that I kept the major problem in view and wasn't one to take up interesting side issues. So he let me make a start on what I wanted to do even with the equipment working imperfectly. He also set about getting me into a job where I could teach and find my own way. All this was very understanding, and while he had the reputation of being dour and humorless, I knew a lot better.

Chadwick's Commitment

Every day Chadwick toured the part of the lab under his care, carrying in his head the stages at which twenty different projects, all at the extreme of knowledge, were going along. He had to comment, advise, and perhaps set right all of these. Then he had to return to his own lab and begin his work.

Chadwick's schedule was demanding, but he did it. I've often wondered what he was like when he arrived home at the end of the day. I did once meet Mrs. Chadwick and his two daughters, but my partner and I were so green and inept at assessing the family that I can still make no comment. A great shame.

The Neutron--and Another Opportunity

I left Chadwick's laboratory in 1930. In 1932, using (I hope) an improved version of our apparatus, he made his major discovery, the neutron, and with it clarified almost at once the nature of the chemical elements. At the same time his subject began to require much more elaborate, engineered equipment. A chance to build this equipment at Liverpool University came his way, and he left the Cavendish Laboratory.

At Liverpool, Chadwick saw a young, promising physicist lose his life while building the new equipment. On top of this, the need to put intensive effort into understanding and perhaps making nuclear weapons moved him into a very different kind of work. Very few of the research leaders who entered that work were able to continue as laboratory leaders, and so it was with Chadwick, who wound up as the Master of my old College, Caius, in Cambridge.

Sir James Chadwick impinged closely on the lives of many, many of them young and starting their work. I know of none of these who did not hold him in high regard.

Ernest Pollard

Dear Chadwick...

After I wrote the above, I found a file which included two letters from Chadwick. One was in response to a letter I wrote and which I include in part. I also found some comments that I wrote in 1974, the year he died. I decided these were all worth looking at and also that to see Chadwick's writing was interesting.

Sir James Chadwick
August 5 1970.
Dear Sir James,

On my recent visit to England, I had a chance to talk to Dr. Powell in Caius College and you may by now realize that I have some concern about seeing that your biography is written up.

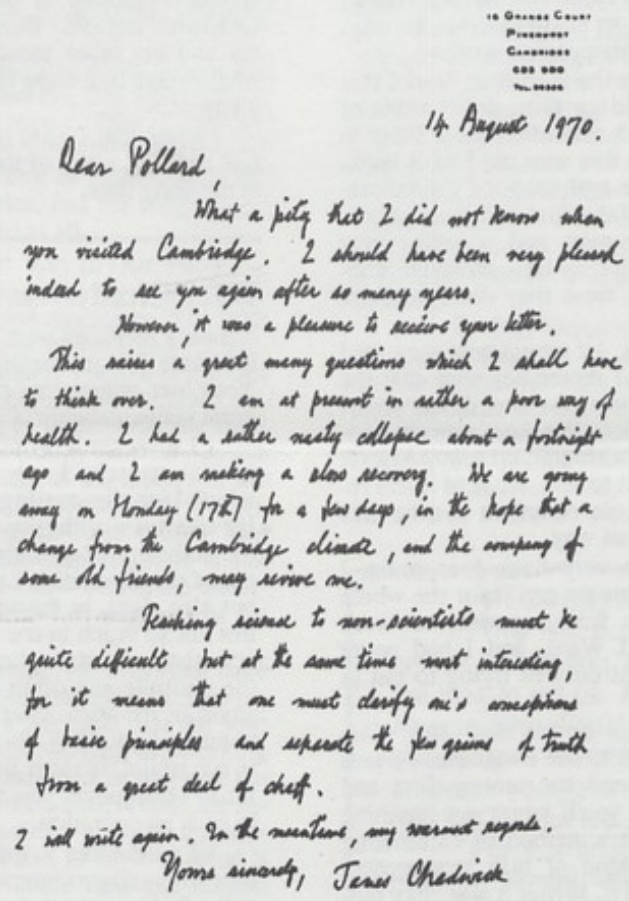
Although it may seem flattering to you that I show this concern and indeed there is no problem about my belief that your life should be well in the record, I have another concern and since you have a part in it I am writing to you.

One of my activities for the past ten years has been teaching science to nonscientists. What is clear is that a good deal of absolute nonsense gets into the record and this is then the kind of material which the majority of citizens believe about science. For example I have read a lot of nonsense about Oppenheimer, based on special presentations, mostly chosen to stress a point of view.

Unless I'm much mistaken, you represent the first example of what I will call a coordinator of a great laboratory. Your activities at the Cavendish lab when I was there were clearly those of an individual who understood the operations of a really fine laboratory and served as a major catalyst in their effectiveness. This is not something which is casually written up, but it actually requires good sensitivity in the recording and I am concerned that it may not properly be stated.

It would clearly be a help if there were available to some competent biographer, a simple statement by yourself on the nature of the problem of running a laboratory and in particular it would be very good if you could bring out clearly the two sides of the problem. You must, on the one hand, have had extremely good and under-

continued on page 4



16 Grange Court
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Cambridge
CB3 9BD
TEL.59326

14 August, 1970.

Dear Pollard,

What a pity that I did not know when you visited Cambridge. I should have been pleased indeed to see you again after so many years.

However, it was a pleasure to receive your letter. This raises a great many questions which I shall have to think over. I am at present in rather a poor way of health. I had a rather nasty collapse about a fortnight ago and I am making a slow recovery. We are going away on Monday (17th) for a few days, in a hope that a change from the Cambridge climate, and the company of some old friends, may revive me.

Teaching science to non-scientists must be quite difficult but at the same time most interesting, for it means that one must clarify one's conceptions of basic principles and separate the few grains of truth from a great deal of chaff. Just over a year ago I had a visit from Dr. Charles Weiner, the Director of the Center for History and Philosophy of physics set up by the American Institute of Physics. You must know more about this Center than I do.

I found him really interested in his subject, well informed, and most agreeable personally. We did some 12 or more hours of recording on tape. He sent me the transcript last autumn and I intended to make my corrections and emendations by Christmas. I did not do so for I had various troubles as well as other matters to attend to. I hope to start on it at the end of this month. I may find that I have covered some of the points you mention, certainly not all.

History is not what happened but what people think happened and what writers of books and articles tell them. No one has yet told the real story of the Manhattan Project, not even General Groves.

I will write again. In the meantime, my warmest regards.

Yours sincerely,
James Chadwick

Comments on Dr. J. Chadwick

(November 1974)

Reading the obituary by Mark Oliphant in Physics Today was my first indication that Dr. Chadwick, my major professor for my PhD at the Cavendish Laboratory, had died at the age of 82. Thinking about it, I began to realize that perhaps I am his only living student who has published a paper with him. (Maurice Goldhaber may be another.) also realized that only fourteen years separate us in age.

A Truly Great Man

Chadwick was a truly great man. He had a genuine in-depth understanding of the work of a large, leading laboratory. He was canny and close but thorough, and I must say I never caught him in anything like an error, particularly in judgment of other people's work.

I was most impressed by the work that he did that he DIDN'T publish. He would go through all kinds of leads from other people's work and often show them to be correct or wrong, and all this was used as a background on which to build the real work of the laboratory. I think that this is one of the first and most important signs of a truly great man and a truly great laboratory: the extent to which they have thorough confidence and understanding in areas they don't consider important enough to publish.

Chadwick didn't always let you know you had made an impression on him. I remember once that the problem of the resolution time for the detection of atomic particles was in question. We were already convinced that it should be short. I worked up a way to look at it one evening and showed it to him. He read it and returned it to me with the laconic comment that he was not sure you could look at it that way.

Three years later, by a very back-door route, I found that Chadwick had given me credit for the whole idea. In reality the idea came from experimental work done by Wynn-Williams and Ward, and I had never thought of claiming credit, but only of trying to put in some kind of analysis of it.

It is curious the phrases of his that stick. One which I'm afraid I've acted on to the irritation of deans and administrators was, "Spend the money first and think about it afterwards, or you'll never get anything done." I remember also when a method of eliminating background noise by some kind of bias arrangement came up. He vetoed our use of it, saying it was "like putting bathing drawers on it: I like to see what's to be seen."

Valuable Lessons

Chadwick was extremely good at giving me training. My association with him was at a time of great emotional stress and also of great ambivalence. Nevertheless, because he was a sufficiently colorful character with, as Oliphant says, a hard shell but great kindliness below, he produced a strong impression on me which lasted all my life.

Once I was able to straighten out my personal affairs, I applied his care and caution together with my own sanguine nature, and the combination has given me the enjoyment of a full lifetime of research. I learned from him never to have any illusions about rewards for research and always to be closely aware of the true value of what had been done. I learned that what mattered was the importance of the research to one's self and not necessarily what people said about you. I think this is of enormous use to a young person beginning a research career.

Chadwick's Fairness

Dr. Chadwick was always ready to criticize, and in reality criticism is the metier of science. At the same time, he was always ready to give credit to others. In fact, I very much miss today the scrupulous fairness with which credit was given to everyone whenever it was known they they had done the work. Today there is a certain technique in deliberately ignoring individuals. Chadwick not only didn't have that in him, he also left me and his other associates with a desire to build on what others had done fairly and to claim only what was justly ours.

I feel that I have been privileged to know a giant. I feel a strong sense of loss even at this great distance and at this long time.

Dear Chadwick, *continued from page 3*

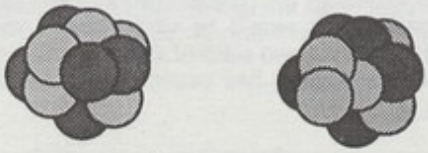
standing relations with Rutherford, and you had a considerable job in keeping the rest of the place going. Your own statement on this whole subject would be of great value.

Lest what I say seems to you to be somewhat of a special concern I would like to point out something which I am discovering the nonscientist is not aware of. He treats the authority of science as equal with that of the authority of an astrologer. The reason why the output of the Cavendish Laboratory was not only first-class but also right, in the sense that it developed real truth, lies not so much in the very fine collection of first class scientists who sit in the front row of the pictures, but in the multiple and often menial operations of those who stood in the back rows. The organization of such a laboratory into a first-class means of generating new truth is not trivial, is most important in world affairs and, as far as I can judge, you are the very first to have engaged in such an operation.

I would like to make my request that you take the time to set down some necessary facts and personal sensitive feelings about the time you spent as the coordinator in the Cavendish Lab.

The very best wishes.

Yours sincerely,
Ernest C. Pollard



Editorial, *continued from page 1*

These differences were great, changing rural to urban living and opening up vast, distant areas to colonization and life under different conditions. In spite of this, the amazing adaptability of homo sapiens showed. Nearly all of these changes took place with no acknowledged acceptance of the altered view of nature as understood by the minority, the small number of scientists.

With the 20th Century, there began to be an alteration. It wasn't only the expansion of scope, of comfort and the improvement of health that followed from the exploitation of the understanding of Nature, but new and deadly means of killing huge numbers of soldiers. World War I touched nearly every household in England, France and Germany with deep and lasting sadness. It altered the sex ratio and materially changed the planning and expectation of many women.

And so, in spite of the subsequent considerable improvement in accessible entertainment, this altered view of Nature took on a threatening aspect. That is where it stands today; the proportion of people who can speak intelligently about our understanding of Nature is lower than it has been in recent centuries, and the antipathy to that understanding has not relaxed at all.

Understanding Natural Law: Key to Our Future

Now the 19th Century witnessed the first of the great unifications, that of light and electricity. The 20th Century has seen the unification of physics and chemistry and of genetics and biochemistry. These unifications are a clear sign that there is, indeed, a basic Natural Law. Using this concept and seeking to understand the findings of astrophysics and of high-energy particle physics has led to an amazing development of the history of the universe which is intellectually satisfying and awe inspiring. That there is such a thing as "Natural Law" and that we should have the greatest respect for it is not questioned by any scientist that I know. Nor is the understanding of this Natural Law devoid of suggestions for ethical and moral conclusions.

This last winter I wrote out a set of essays that have been growing in my mind, and in them I came to face the idea that some agency gave us a start and that the start was followed by consequences dictated by the working out of Natural Law. Somehow, included in this was the human mind and what I call the human ETHOS, the internal agency that governs our actions and sets our values, moral and otherwise. I then had to say that what is going to take place will be by Natural Law and Human Action. Not all one OR the other but BOTH.

So, unless any proposed human action includes the understanding of Natural Law, the events that result may be disastrous. For minor things like governing a small family or even a county, this may not matter: the adaptability of humankind may overcome the disaster. But humankind has already begun to sense the control of very large forces, and without understanding may embark on courses so terrible that our very future will be at stake. This is the fear expressed by Carl Sagan in his TV series, COSMOS. It is a fear that I share.

My Concern

My fear is the exact reverse of that fear underlying the antipathy toward science shared by the great majority of people. Many of these people fear that a minority of knowledgeable people will perform some dreadful feat, make some inhuman but powerful monster, and so destroy peace and tranquility.

So there is my concern: that by looking away from what we know about Nature, we can go seriously wrong, perhaps irreversibly so. And by acting out of what can best be called ignorance, humanity may intervene to prevent some very desirable new understanding of Nature from taking place.

But Why the Bulletins?

Very well. Then why the bulletins? Clearly, one needed remedy is the opening up of the nature of science, scientists, and scientific endeavor to many more people. In recent years, interesting books such as Stephen Hawking's Brief History of Time have been widely read. Other deserving books, however, have barely justified their publication. In large measure this is for two reasons: (1) the material appears uninviting and (2) interested readers do not hear about them. Science & People will seek to improve that situation.

Each issue will have a mini-biography of an interesting scientist, 2-3 reviews of available books, and an editorial on some aspect of "science and people." We will invite the advertising of pertinent books at a reasonable rate.

The Woodburn Press is itself a publisher of the kind of material we seek to multiply. We don't pretend to ignore our own interests. Nevertheless, we have more in mind. So far, our attempt to spread information and understanding about science has made us little more than solvent--yet we persist.

REQUEST FOR FEEDBACK

This first bulletin is a trial issue. If it proves interesting and useful, please take the time to tell us. Also, if you are aware of anyone who would like to receive a copy, please give us that information also. Of course, if you have spotted a basic flaw, you may tell us that too .

For this first issue, I wrote all of the material except, of course, for Chadwick's letter. This is not the plan for the future. Very tentatively, we intend to write the mini-recollections about R.B. Roberts and Dean Cowie in the next issue. We are considering reviewing Weisskopf's book, The Privilege of Being A Physicist. We will consider any book suggested to us, and we invite the submission of promotional flyers.

Ernest Pollard

BOOKS

This part of the bulletin is to consider some books that we feel are relevant to SCIENCE AND PEOPLE. They are reviews, but there are some strict rules that must be followed in writing them. First we expect to buy the book and subsequently use it to further its purpose somehow. Second there will be no negative reviews. If we think a book should not be read in the interests of SCIENCE AND PEOPLE, we intend to leave it alone.

Negative reviews are a kind of censorship. We have experienced them in the past and they serve no useful purpose. So our accounts are of books we deem to be valuable. The accounts should be interesting but they also MUST address WHAT, WHO and WHY. What is in the book; who should read it and why should they. All this is an expression of opinion. Also we require the reviewer to have read the book start to finish

COSMIC JOY AND LOCAL PAIN. Musings of a mystical scientist. Harold J.Morowitz, Scribners, 866 Third Ave. New York, NY 10022. 321 pages, 4-6 hours moderately fast reading.

I found this book in my hands at a moment when I was a bit resentful and pressed for time. I opened it and began. It wasn't long before I had read it all and thoroughly enjoyed it. I have read it a second time with even more enjoyment.

I'm finding it very interesting that this is the first book review I've written under the rules printed above. So I must address WHAT. It isn't all that easy to say because there are very clearly two elements in that WHAT. The first is to be guessed from the table of contents: it is an account of our planet and its place in the universe, a statement of the relation of the planet, and indeed its very nature, to life. Then it goes, rather more uncertainly from life to mind and in all this there is apparent a design and so we can sense Cosmic Joy. That is not a bad impression of the first element in WHAT.

The second element is Harold Morowitz and he is there in every line and every page. Because I appear fleetingly, but perhaps causatively, in the book, I believe I am entitled to say something that will bear on the second element. He tells of entering my office to get signed up in the new program in biophysics. I can see him come through the door as if it was yesterday. He speaks of his trepidation: it was nothing to mine.

It took only seconds to elicit the information that he had completed a very difficult major in extremely short time.He was almost unbelievably young and he wanted to make a career in an unrecognized branch of biology about which I knew very little but which, as a tenured faculty member, I was undertaking to pursue while teaching physics. There were no textbooks, no journals, no faculty associates, hardly ten people in the world who could have an inkling of what was to be done.

Well, I couldn't talk him out of it and he went on to become about the most useful member of a remarkable group that assembled at Yale. His undergraduate major was a joint philosophy-physics program and that underlying interest has never left him and appears firmly in this book. So project that bright young man forward 43 years and remember that he is in every line of the book and now we have WHAT.

WHO should read it? Every thinking person; everyone who attends religious services; everyone who wonders if it all came about randomly and with no thought for humankind. Every young graduate student. Every retired old codger like me.

WHY should they read it? Because of the developing thrust of scientific knowledge. It is no longer simply related to comfort, convenience and health. The new telescopes, the great accelerators, the realization of the common properties shared by all living things, the returns from space exploration are all pointing toward an understanding of ourselves and our relation to the universe. Increasingly, projection from this knowledge to a sense of some kind of design can be very important in developing a personal adjustment.

I read what Harold Morowitz writes to mean that it is a factor in his own adjustment. His description of the present state of scientific knowledge in varied areas and his interesting thumbnails of biography and history render the book fascinating and worthwhile reading. My only complaint is that it seems to end too abruptly. The sabbatical ended too soon. Let's hope there is another before too long. -- ECP

KENNEDY, KRUSHCHEV AND THE TEST BAN. Glenn T. Seaborg (assisted by Benjamin S. Loeb). University of California Press, Berkeley. 320 pages. Reading time 15 - 25 hours.

This is a close account of the negotiations that preceded the signing of the limited test ban, prohibiting nuclear weapon testing in the atmosphere, the ocean and Outer Space. In 18 years following the first use of nuclear weapons this was the only agreed disarmament treaty between the U.S. and the U.S.S.R.

Dr. Seaborg was Chairman of the Atomic Energy Commission from 1961 to 1971 and in the first of those years was closely involved with President Kennedy's persistent attempt to negotiate a comprehensive test ban with Britain and Russia. He kept a diary which recorded events and impressions each day and with this he has managed to present all the events as they moved to failure and then partial success.

The pressure to stop testing nuclear weapons began with the testing of a hydrogen bomb at Bikini Atoll in 1954. The yield exceeded estimates: one Japanese fisherman lost his life, several had radiation sickness. Fallout reached Marshall Islanders, contaminated fish reached Japanese markets. This created a burst of adverse world opinion. President Kennedy wanted a comprehensive test ban. He associated this with the delay or deterrence of nuclear proliferation. There followed a rapid passage from the idea to intellectual and political thickets. One senses that the book is an account of trying to hack a way through these thickets.

There is a careful use of a well kept diary; full of the account of who became involved; careful accounts of the seemingly interminable studies and meetings but always with the sense of where the negotiations were go-

ing. Kennedy's team, including Seaborg, tried hard to achieve Kennedy's wish..

In the process, the U.S. followed the Russian lead and themselves conducted testing. Seaborg gives a clear account of how this came about, both in the atmosphere and underground . He shows clearly that things could, and did, go wrong.

In all this there is apparent today a fantastic ignoring of the right to know of the American people. At one stage it was actually suggested that we would allow the Soviets to examine the construction of obsolete weapons. To my knowledge such information about ANY weapon has never been declassified. So we would permit our putative enemy to know, but not the American public.

It is fairly clear to me that what BOTH sides were up to was not very creditable.

The elimination of testing in the atmosphere and the ocean WAS achieved, but with resistance from both Russian leaders and our own. Seaborg faithfully documents this part of the process.

So now you can form an opinion as to WHAT is in the book.

As for WHO should read it. All those interested in the international negotiation process; all those interested in slowly fading history. Those interested in observing a fine scientist mingling with statesmen and seeking to be one himself.

Now for WHY. First of all, any idea that a nation can undertake to negotiate with a single mind and purpose is clearly shown to be seeing through rose-colored glasses. Nations, whether under dictatorship, or supposedly democratic, or even working in a purely scientific arena, have multiple power sections which develop strong views and exert much influence. This shows clearly in the book. It gives an indication of the difficulty of negotiation even if the emotional climate is far better.

The book is full of history. The succinct account of the Cuban Missile Crisis alone is fascinating.

Seaborg, while trying to be objective, clearly reveals his own personality. He didn't like the news media calling him "shambling" and "craggy-faced" and the book makes it clear that he had reason to object. There are not many accounts of a scientist thrown into the diplomatic melee. This is a very good account of just that. The book dates from 1982. It may be that it can only be read in libraries. Never mind: read it. -- ECP

THE CREATIVE IMAGINATION OF AN EXPERIMENTAL PHYSICIST: W. M. FAIRBANK. By C. W. F. Everitt, in NEAR ZERO, Ed. J. D. Fairbank; B. S. Deaver, Jr; C. W. F. Everitt; D. F. Michelson. Pub. W. H. Freeman, New York, 1988. pp 19 - 64. Reading time 45 - 75 minutes.

This article is of interest because the description of a fine experimental physicist is tied into a variety of topics, each of which can be very formative in a scientific career: early training; marriage; wartime opportunity in a big laboratory; available advisers; choice of field of research; PhD project; exploitation of opportunities on the job; ability to milk theorists of relevant ideas; ability to talk on topics on his mind; possession of a truly kindly unabrasive personality; courage and a kind of faith about experimentation. All these are presented and discussed with Bill Fairbank as the personality and scientist in the spotlight. There are also brief accounts of Bill's remarkable achievements in the laboratory.

It is these last that limit WHO should read the article. It won't be easy for a non-scientist, but quite pleasant to a broad based physicist.

As for WHY. The skill in writing; the introduction of interesting comparisons; the ability to use examples from many and varied past scientific episodes; and the relative rarity of this kind of synthesis.

Since I appear briefly once or twice I can make a small correction. I never claimed in 1945 that nuclear physics would be "ancient news". But I DID point out the inadequacy of the only available machine for that kind of research at Yale in comparison with the low temperature option.

I do wonder why, in the midst of such excellent writing, he should refer to "the loud succession of trumpet flourishes....from the massed band of high energy theorists." I've read and reread the article. I thoroughly enjoyed it. -- ECP

more Books *on page 8*

Books, *continued from page 7*

THE DISCOVERY OF NUCLEAR FISSION. By Hans G. Graetzer and David L. Anderson. Van Nostrand Reinhold Co 1971. 120 pages. Reading time depends on the selections read. Perhaps 30 minutes.

The book is an account, with selections from original material, of the work that led up to the discovery of nuclear fission and then to its use as a weapon in World-War II. The descriptive material and the selections chosen are both very well done, but what makes the book very useful are two selections. The first is a paper by Mrs Ida Noddack: "On Element 93." Zs. fur Angewandte Chemie 47 653 1934. Here Mrs Noddack cogently questions Fermi's suggestion that the bombardment of Uranium by neutrons has developed a transuranic element. She argues well and had her suggestions been taken, nuclear fission would have been discovered five years earlier. This might have had frightening consequences had Hitler first developed nuclear weapons. The book goes on to give an account of the almost tortured work of Hahn, Meitner and Strassmann leading to the conclusion that the "drastic step of including Barium in the elements resulting be taken."

The other selection is a partial reprint of Secretary of War Henry L. Stimson's article on the decision to use the atom bomb. (Harper's Magazine, Vol. 194 p97 - 107 Feb. 1947.) This concludes with a personal summary which is very moving and well worth reading today.

WHO should read it: Anyone with thoughts on what must be the most revolutionary development of the century. Anyone who questions the morality of the use of the weapon as it was used.

WHY. Because of the clear, simple and effective use of text material and selections from original writings.

This is almost certainly something to find in the library. It will be well worth it. -- ECP

Science and People. E Pollard

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Dear Ernie Pollard,

I was very glad to hear from you and to receive your interesting Science & People. We need more of that kind of thing. When is Issue 2 coming out?

Some time ago I decided to write an autobiography with relevant family history: The Cambridge Left wing in the 1930's / Manhattan Project / Radar at Birmingham at Berkeley, Mol. biol & DNA: also British Soc. for Social Responsibility in Science etc.

About magnetrons I corresponded with Oliphant, who I greatly admired, when I had to write the Royal Society Memoir on Randall. I liked what you wrote about Chadwick. It relates to the whole matter of how scientists are motivated & how the attitude of scientists may help science to have a healthy influence on the future of the world. That is the main question I want to address in my autobiography, though I am very aware that a didactic do-good approach can easily wreck an autobiography. Your own writing is I believe one of the best I have seen in the whole area of 'public understanding of science' which is a very broad area indeed when we recognise how science affects our deep-down attitudes to the whole of life e.g what will we report to St Peter when we meet him at the Gates of Heaven (and that is nothing to do with being a Christian)

I would much appreciate comments on KES. (How did you live in Birmingham with its great background of Priestley?) I feel there was a certain basic democratic enlightenment in Birmingham & somehow that prevented KES having the horrible hierarchical bullying & snobbery which was very common in public schools. On the whole I had a marvellous time, no bullying, no sport, almost no gyms because the teacher preferred to discuss philosophy . . . . Possibly England was a better Head than Gibson who left in my first year. On the whole I respected the teachers. England seemed very starchy & stiff but gave me some encouragement.* Did you go to Cambridge from KES & afterwards to the States? The new Professor of Public Understanding of Science at Imperial College ⊛ James Durant should know of your Issue. If you care to send me half a dozen I could give them to friends & colleagues who should understand what you are doing. Would it be possible to write a book about Ida Noddack It sounds exciting

Best wishes
Maurice.

⊛ Prince Consort Rd London SW7

* but later called me a silly goose because I did not relish a second year in the upper 6th before going to Cambridge

Research on Embryos

Linzey · & Clark ·

Lecturer Crook Ac · 86 or 87

Dr Stephen Clark (in Gov[t])

Dr Hugh Gordon

458 5316

The J. D. Bernal Peace Library

Professor Dorothy Hodgkin, OM, FRS, Nobel Laureate:

One could write much about the many different sides of J. D. Bernal's life and work—in Physics, Chemistry and Biology, in campaigns in war and for peace. I like to remember one of his friends, from Weimar, who had lived through difficult and dangerous times, explaining how he had bought four copies of Bernal's book *World Without War*, for his four children, so that, if anything happened to him, they would know the kind of world in which their father believed.

There must be friends of Bernal who feel the same way in almost every country. Today this world without war may seem further away than ever. Yet everywhere there are people working towards it whose efforts would be strengthened by more knowledge of each other's problems and achievements. To help these efforts is the main purpose for which we have founded the Bernal Peace Library.

Dame Barbara Hepworth:

I remember Desmond Bernal with deep love as one of the most creative and the most extraordinary men of this (or any) country. He was, of course, unique as a scientist but I comment here on his quite extraordinary insight into the visual world as a whole and particularly of the sculptor and painter.

During the four decades that I knew him and cherished his friendship, he was the most inspiring person ever to come into my workshop as he had the amazing capacity for comprehending in an instant the nature (and even the formula) of every sculpture, and hours were spent in exciting discussion and drawing, and even drawings of the inter-relation of artist and scientist within the nature of the universe.

A centre for peace education

The J.D. Bernal Peace Library was founded in 1967 as a permanent centre to continue his efforts for disarmament, creative coexistence and a world without war—a world where the achievements of science could be used for the benefit of all mankind. Professor Bernal was present at the inaugural meeting and expressed his full support. During his long illness, and until his death in 1971, he took a keen interest in the work of the Library, which was granted the status of a charitable educational trust in 1968.

The Library has organised conferences on chemical and biological warfare, on aid to developing countries, and on an aspect of the nuclear fuel problem. A book, *Chemical and Biological Warfare*, based on the papers of the first conference has appeared in several languages.

Scientist for mankind

Bernal was a great scientist, but he was also active socially and politically. He considered that the science he left undone would be done by others, but unless the political work was done, particularly in relation to peace, there would be no science at all. That is why he devoted so much of his energies to the work of the World Peace Council, of which he was elected Chairman after the death of its founder, his life-long friend, Joliot-Curie.

Bernal was well aware of the dangers facing mankind, not only from war, but also from the underdevelopment of vast areas of the world, the waste of resources and the pollution of the environment. Nevertheless, he believed profoundly that the peoples of the world could and would tackle these problems.

Activities

It is this spirit of optimism that inspires the J. D. Bernal Peace Library. It plans lectures and conferences on such problems of peace as disarmament, coexistence, colonialism and the unequal distribution of the world's resources.

Contacts have been established with similar organisations in Britain and overseas.

The Library is assisting in a scheme for scholarships on aspects of Bernal's work for peace, initiated by the World Peace Council and financed by various national peace organisations.

The Library's books and source materials on the above themes are accommodated at the Marx Memorial Library, 37a Clerkenwell Green, London, E.C.1.

All friends of Professor Bernal and all who would wish to further the causes to which he dedicated his life, are invited to become associates of the Library by sending a donation or by signing a seven-year covenant.

You can contribute by

Filling in the enclosed Covenant form. A covenanted donation increases the amount ultimately received by the Library by more than 50 per cent.

Sending a donation. Cheques should be made out to the Bernal Peace Library and addressed to the Manager Lloyds Bank Ltd., Victoria House, Southampton Row, London, WC1B 5HR

Giving books and other material that will be useful to the Library.

Making a bequest under your will. This should be made out to the J. D. Bernal Peace Library for its general purposes and should specify that the receipt of the acting Treasurer of the Library should be accepted in full discharge.

honour: Gilbert Bernal spirit of
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P. Kapitza

PIOTR LEONIDOVICH KAPITZA

9 July 1894 — 8 April 1984

Elected F.R.S. 1929

By D. Shoenberg, F.R.S.

Russia, 1894–1921

Peter Kapitza* was a legendary figure both in Rutherford's Cambridge of the 1920s and 1930s and subsequently in Moscow to the end of his long life and the legends serve to illustrate his colourful personality. In his scientific work he showed great versatility and brought the skills of an engineer and mathematician to bear on important problems in physics and technology in an entirely original way. He also had broad cultural and social interests and his original ideas on scientific education and organization have had a profound influence, particularly on the development of Soviet physics.

He was born in Kronstadt, the island fortress on the Neva near St Petersburg,† into a family with strong intellectual traditions. His father, Colonel (later General) Leonid Petrovich Kapitza was a military engineer involved in modernizing the fortifications of Kronstadt. The Kapitzas had been landed gentry with Polish antecedents and the family was well represented in the professions. His mother, Olga Ieronimovna, to whom he was very close until her death in 1937, was a specialist in children's literature and folklore and an important figure in the literary world of St Petersburg. She organized a club for young writers, many of whom subsequently became famous. Her father, General Ieronim Ivanovich Stebnitski, also partly of Polish origin and a military engineer specializing in cartography, was a geographer of international repute, a corresponding member of the Imperial Academy of Sciences and an ardent traveller all over the world; it is perhaps from him that Peter Kapitza inherited his own love of travel. Much of Stebnitski's military service was in the Caucasus, where he personally mapped out the important mountain

* In England, the Russian Piotr was at first rendered as Pierre but he was usually known as Peter. The transliteration of his surname is the one he used himself, but the tz is often rendered as ts.

† After 1914 St Petersburg became Petrograd and after 1924 Leningrad.

peaks and made gravitational measurements; later he was head of the Military Topographical Department of the Imperial General Staff in St Petersburg. Unusually for his time, he arranged for his daughters to have a higher education, Olga in the humanities and Alexandra in mathematics and science.

Alexandra played an important part in Peter's upbringing for it was she who discovered, rather to the surprise of the family, that he had an unusually quick grasp of arithmetic. Though a lively boy, he had seemed a bit backward in some other respects. He never overcame a certain indistinctness and sloppiness in speech and never learned to spell properly in any language—a considerable handicap in his early career. He was admitted to the 'classical gymnasium', which specialized in the humanities, but had to leave this school in 1906 at the end of his first year because of inadequate progress and transferred to the more scientifically oriented 'realschule'. He enjoyed a kind of revenge 70 years later when, to mark his second award of the title Hero of Socialist Labour, his bust was erected in a public square of Kronstadt opposite the gymnasium from which he had been excluded! Although this exclusion evidently rankled, the 'realschule' was much more appropriate for developing his talents, and in 1912 he graduated with honours and entered the electrotechnical faculty of the St Petersburg Polytechnical Institute—for lack of Latin and Greek he was not eligible to enter the St Petersburg University, then regarded as the more prestigious institution.

His studies were interrupted by the 1914 war and for two years Kapitza was an ambulance driver at the Polish front. After demobilization he returned to the Polytechnical Institute where he graduated in 1918 and was appointed to a teaching post in the Institute, which he held until he moved to Cambridge in 1921. During these years the moving spirit in the physics world of Petrograd, and indeed of Russia, was A. F. Joffé, who was actively developing a physics school with an outlook more modern than had been traditional in Russia up to then, and with an emphasis on research and seminars to keep abreast of developments in the West. After 1918 the research effort was concentrated in the newly established Petrograd Physico-technical Institute, where Joffé succeeded in collecting a group of young and enthusiastic scientists around him who formed the nucleus of the enormous growth of Soviet physics in later years. The creation of this new institute for physics research must have seemed wildly visionary at that time since, after the chaos of the war and the upheaval of the Revolution, the economy of the country was in a disastrous state. There were great shortages of food and fuel and practically no scientific equipment, so that research could be done only on a 'do it yourself' basis. In spite of these difficulties, however, quite a lot got started.

Kapitza took up several projects of topical interest. One was the measurement of the angular momentum associated with magnetization of

matter, and his first scientific paper* was a critical review (1)† of the experiments by Einstein and de Haas, who had demonstrated the effect in 1915, and by Barnett who almost simultaneously demonstrated the inverse effect. At that time there appeared to be a contradiction between the two sets of experiments and Kapitza concluded that there must be some error in Barnett's experiment because it gave only half the classically expected ratio of angular momentum to magnetization. However, soon afterwards, Einstein and de Haas discovered an error in their experiment and confirmed Barnett's 'anomalous' result which was later explained in terms of electron spin. A second paper (2) describes an ingenious improvement in the technique of preparing Wollaston fibres; Kapitza also became adept at using Boys's bow and arrow method of producing very fine quartz fibres.

Another line of work was the study of atomic and molecular beams and together with his friend Semenov, Kapitza proposed a technique for measuring atomic magnetic moments by observing the spread of an atomic beam after passing through a strongly inhomogeneous magnetic field (6). Although their paper mentions that 'experiments in this direction have already begun' nothing further was published. In fact between the time of submission of the paper in December 1920 and its publication in 1922, Stern and Gerlach had not only proposed essentially the same idea, but carried it out in practice (Stern 1921, Gerlach & Stern 1921, 1922), and so provided the demonstration of spatial quantization for which many years later Stern was awarded a Nobel prize. A third field to which Kapitza contributed was that of X-rays and his proposal (3) to focus a broad beam of X-rays by a bent crystal seems to have anticipated what much later became an important practical technique. Johansson (1933) described the first practical realization of this idea, but does not appear to have been aware of Kapitza's paper.

Soon after Kapitza was demobilized, and following a romantic trip to Harbin in China, he married Nadezhda Kyrillovna, daughter of General Chernosvitov, and a son, Ieronim, was born in 1917. A second child was expected towards the end of 1919 but disaster struck the family. In the terrible conditions following the Revolution and the Civil War, epidemics were rife and Ieronim caught scarlet fever and died. Nadezhda was quite struck down by this blow and soon after she had given birth to a daughter (also called Nadezhda), both succumbed to the Spanish influenza that was then raging in the city. On top of this, Kapitza's father also died of 'flu at about the same time. Kapitza, who had himself had a bad bout of 'flu, was overwhelmed by these tragic losses and for a time was unable to work. His friends wondered what could be done to help

* This was in fact not his first publication. He had already published a popular article (G1) about the production of cod liver oil, based on his travels in the far North of Russia, and illustrated by his own photographs.

† Numbers given in this form refer to entries in the bibliography at the end of the text.

him forget his unhappy situation and something turned up that not only distracted Kapitza from his grief, but, as it turned out, completely changed the course of his life.

This was the setting up, at Joffé's prompting, of a 'Commission of the Russian Academy of Sciences for renewing scientific relations with other countries', which was liberally provided with foreign currency to buy equipment abroad (see Sominskii 1965, Krylov 1984). Besides Joffé, an important member of the commission was Admiral A. N. Krylov, the well known naval engineer and applied mathematician, who was later to become Kapitza's second father-in-law. Both had a high opinion of Kapitza's scientific gifts and wanted to help in his difficult personal situation; appointing him to join the commission seemed an ideal solution. Travel abroad at that time was complicated because most of the outside world had no diplomatic relations with Soviet Russia. Joffé set out in February 1921, and as soon as he got to Berlin began trying to get a visa for Kapitza, while he and other members of the commission were occupied with buying equipment. Germany, France and Holland did not want to risk admitting someone who might prove to be a young communist agitator, but England was more accommodating and eventually in May, Joffé succeeded in getting Kapitza an English visa.

At last, late in May, Kapitza arrived in England by ship and early in June he and Joffé started a round of scientific visits, which culminated in a visit to Rutherford in Cambridge on 12 July. They were received very cordially by Rutherford but when Kapitza asked if he might work in the Cavendish for a few months Rutherford was rather discouraging, saying the Laboratory was already crowded and it would be difficult to accommodate one more. Rutherford was rather taken aback when Kapitza replied by asking what accuracy he aimed at in his experiments. The answer to this seemingly irrelevant question was 2 or 3% and Kapitza then pointed out that as there were about 30 researchers in the Cavendish, one more would hardly be noticed because he would come within the experimental error! This ingenious approach persuaded Rutherford to admit Kapitza after all. As a postscript it may be mentioned that a year later Kapitza asked Rutherford why he had agreed to take him on and Rutherford laughed and said 'I can't think why, but I'm very glad that I did'.

Cambridge, 1921–34

Kapitza joined the Cavendish in July 1921 and although the original plan was for him to stay only over the winter he remained for 13 years. His introduction to Cambridge life and his impressions of Rutherford and the Cavendish are vividly described in the letters he wrote at frequent intervals to his mother in Petrograd (quoted in Danin 1966, Parry 1968

and Rubinin 1985).* The usual initiation of new research students was a month or two of practical work in the Cavendish attic on various relevant techniques under Chadwick's supervision and a few days after his arrival Kapitza wrote (24 July) 'For the time being . . . I am working only at my practical course; as to the future, I don't know. . . . Time will show. . . .' In fact his skill and assiduity were such that Chadwick was satisfied after only two weeks and by early August Kapitza had at Rutherford's suggestion chosen to study how the energy of an α-particle falls off towards the end of its range. Previously this had been measured by observing the deflection of the α-particle in a magnetic field, but the limitation of this method was that particles of low energy could not be detected. Kapitza's method was to measure the energy in a collimated beam of α-particles by the heating it produced in a plate attached to a Boys radiomicrometer. This project was brought to a successful conclusion with amazing rapidity. Within nine months of the conception of the idea he was already drafting a paper for publication and by mid-June the paper was sent off. On 19 June 1922 he wrote to his mother:

> 'Today the Crocodile summoned me twice about my manuscript. . . . It will be published in the Proceedings of the Royal Society, which is the greatest honour a piece of research can achieve here. . . . Only now have I really entered the Crocodile's school . . . which is certainly the most advanced school in the world and Rutherford is the greatest physicist and organiser. It is only now that I have felt my strength. Success gives me wings and I am carried away by my work.'

'Crocodile' was a nickname Kapitza invented for Rutherford early on and many years later he told Ritchie Calder (1951, p. 66):

> 'In Russia the crocodile is the symbol for the father of the family and is also regarded with awe and admiration because it has a stiff neck and cannot turn back. It just goes straight forward with gaping jaws—like science, like Rutherford.'

A more fanciful but probably entirely apocryphal version, popular in the Cavendish, was that it was after the crocodile in Peter Pan that swallowed an alarm clock to give advance warning of its somewhat frightening appearance. This version finds some slight support in an early letter (12 October 1921):

> 'Rutherford greets me increasingly pleasantly and asks how things are going when he sees me. But I am a little afraid of him. I work almost next door to his office. This is bad since I have to be very

* A more complete edition of these and other letters is being prepared by Mrs A. A. Kapitza. The quotations in this memoir are my own rather free translations from the Russian.

careful with my smoking: its a misfortune if he should catch you with a pipe in your mouth. But thank God he has a heavy tread and I can recognize his footsteps . . .'

The published paper (7) shows that Kapitza's enthusiasm about his experiment was indeed justified. Boys (1923) said of his radiomicrometer that 'its extreme delicacy of construction requires more than ordinary skill on the part of the user' and Kapitza not only demonstrated this skill and exploited his experience of fine quartz fibres but incorporated many ingenious features to avoid various stray effects that in his preliminary experiments completely swamped the genuine small heating effect of the α-particles. He wrote to his mother (4 December 1921) that '. . . [his device] is so sensitive that it can detect the flame of a candle 2 versts [roughly 2 km] away and respond to a temperature rise of a millionth of a degree! . . .'. His results convincingly showed that the energy fell steeply to a negligible value towards the end of the α-particle range.

This publication illustrates several characteristic features of much of Kapitza's later work: his ability to work out in practice rather complicated schemes designed to give extreme sensitivity, his thorough grasp of potential pitfalls, his speed of working and his thoroughness in interpretation. Another interesting feature is Kapitza's fondness for regarding each project as part of a larger group of investigations, but then getting excited by a new idea and forgetting about continuing the original plan. Thus the paper is described as 'Part I' and the abstract mentions that further work will be described in another paper. However no Part II ever appeared, probably because Kapitza had become completely engrossed by a new idea that came up at that time.

This idea was a possible new method of studying how the α-particle velocity varied along its track by measuring the track curvature in a magnetic field. Existing magnets were not capable of producing large enough steady fields to curve the tracks sufficiently, and Kapitza's novel idea was to use fields that were much larger but lasted for only a very short time. This marked a turning point in Kapitza's career. Once the large impulsive magnetic fields had been achieved he saw whole new vistas opening up for exploiting the technique, and this led him into pioneering work in solid state and low temperature physics. It was also the beginning of the transition of the Cavendish from the string and sealing wax tradition to the age of large machine physics.

One of the basic problems of producing an impulsive field is how to provide a large store of energy that can be converted into electrical power and rapidly discharged through a coil. An obvious candidate for such an energy store was an electrical condenser of large capacity charged to a high voltage, but such a condenser would have been expensive and Kapitza chose instead a chemical store, essentially a large accumulator of special design, which he could build himself.

He was encouraged by Rutherford's reaction to his idea; as he wrote to his mother (15 June 1922):

> 'The Crocodile is taken with my idea and thinks it will succeed. He has a devilish feeling for experiment and if he thinks that something will come out of it, that is a very good omen. His attitude towards me gets better and better . . .'

He also mentioned that a 'young physicist' (P. M. S. Blackett, 3 years his junior) was going to work with him. Blackett introduced him to the subtleties of the Wilson cloud chamber, which was used for revealing the α-tracks, but the collaboration did not continue beyond the initial stages. A few days later Kapitza wrote of another significant sign of Rutherford's rising esteem: 'The Crocodile's permission for Laurmann to come is the best demonstration of his kind attitude to me.' Emil Yanovich Laurmann (for some biographical details see Shoenberg 1954) was an Estonian who had collaborated with Kapitza in Petrograd and was remarkably skilled in all sorts of techniques such as electrical engineering, photography and, most importantly, the handling of delicate equipment. He was a little deaf and visitors to the laboratory were often startled by Kapitza's loud cries of 'Baron' when he needed Laurmann's assistance. Baron was a nickname based on a half joking tradition in Russia that the Baltic provinces were entirely populated by nobility.

Although the new project was technically much more complicated and on a larger scale than anything Kapitza had tackled before, Kapitza's drive, backed by Rutherford's support and Laurmann's assistance produced results very quickly. After some early failures he wrote on 17 August, only 2 months after starting,

> 'The preliminary experiments were completely successful. I am told that the Crocodile speaks of nothing else just now. I have been given a large room in addition to the one I already work in, and for the full scale experiment I have got permission to spend a fairly substantial sum of money.' [This was £150!]

and on 2 September:

> 'My experiments are assuming a very broad scope . . . I shall remember my last conversation with Rutherford as long as I live. After a whole lot of compliments he said 'I should be very happy if I could have the possibility of creating a special laboratory for you in which you could work with your own students.' . . . Am I really such an able person? I feel uneasy and alarmed. Will I be able to cope?'

And then at last, on 29 November, presumably after the construction of the Wilson chamber as well as the high field equipment:

> '. . . For me today is to some extent a historic day . . . In front of me is a photograph with nothing but three curved lines on it. But

these three lines show the tracks of α-particles in a magnetic field of terrific strength. These three lines have cost Professor Rutherford 150 pounds sterling and have cost me and Emil Yanovich 3½ months of extremely hard work. But here they are and everyone in the University is talking about them. Strange: only three curved lines! The Crocodile is very pleased with these three curved lines. . . . Many people have come to look at these three curved lines and admired them. Now we must go further and there is much work ahead.'

By March 1923, again only nine months after starting, a paper had been submitted that was published soon after (10). This was followed a year later, when the research had been completed, by two further papers (11, 12), the first giving details of the impulsive field method and the second describing the special design of the Wilson chamber, the elaborate switching and timing devices involved and finally a detailed account of how the curvature of the α-particle tracks varied with position along the track. Effectively the curvature measured the ratio of velocity to charge and the variation of the average charge with position could be deduced from other experiments and some appeal to theory. Thus finally a graph was obtained of velocity against range, just as in the heating experiment described earlier, and much to Kapitza's satisfaction the two agreed quite reasonably.

While all this work was progressing, Kapitza's position in Cambridge was rapidly being consolidated. In January 1923 he was officially admitted as a research student for the Ph.D. degree, with backdating to October 1921, and he obtained a year's remission in view of his Russian work. This meant he could complete his Ph.D. in the summer of 1923. He was also admitted as a member of Trinity College and soon after his Ph.D. he was awarded a Clerk Maxwell Scholarship. He continued to write ecstatically to his mother about Rutherford:

18 March 1923—'. . . The only thing that eases my work is the Crocodile's care, which can only be compared with the care of one's own father.'

15 June 1923—'. . . Yesterday . . . I met the Crocodile just as I came back from being made a Doctor of Philosophy. I asked him: "Don't you think, Professor Rutherford, that I look wiser?" "Why should you look wiser?" he replied, intrigued by my somewhat unusual question. "I have just been made a doctor" I answered. He immediately congratulated me and said, "Yes, you do look appreciably wiser and moreover you've had your hair cut" and roared with laughter. Taking such liberties with the Crocodile is generally very risky, since most often he simply sends you to the devil and I seem to be the only one in the laboratory who dares to try. . . . Such behaviour towards him from a junior is very unusual. Something like six times I

have had compliments from him such as 'fool', 'ass' etc. Most of the laboratory is astonished how such jokes are possible. . . .'

Another example of the kind of joke Kapitza had in mind was recounted many years later (G55). Kapitza had been rather shocked when at the start of his Cambridge career, Rutherford warned him that he would not tolerate his making Communist propaganda in the laboratory. When a year later Kapitza's first major paper appeared he tried a mild leg-pull:

> 'I presented Rutherford with a reprint and I made an inscription on it that this work was proof that I had come to his laboratory to do scientific work and not Communist propaganda. He got extremely angry with this inscription, swore and gave me the reprint back.* I had foreseen this and I had another reprint in reserve with an extremely appropriate inscription with which I immediately presented him. . . . Rutherford had a characteristically hot temper but cooled down just as quickly'.

The paper on α-tracks in strong magnetic fields was Kapitza's last research in Rutherford's personal area of interest. From then on he applied the impulsive field technique to other problems and extended it to produce still higher fields. The first application was a study of the Zeeman effect in fields up to 130 kG† (13, 14) in collaboration with H. W. B. Skinner. The hope was that something new might turn up at such high fields—typically 3 or 4 times higher than had ever been used before. However, in spite of the ingenuity of the technical arrangements (see P1, P2, P3), particularly as regards producing an intense light source at exactly the right moment, nothing very new did turn up. It was, of course, a great feat to make everything work properly at the same time but the results at the high fields proved to be mainly no more than reasonable extrapolations of what had been observed before at lower fields.

At about this time Kapitza started an even more ambitious project for the production of strong impulsive fields. The accumulator method was in practice limited to fields of little more than 100 kG in a reasonable experimental volume and, moreover, the accumulator required difficult repairs after comparatively few discharges. The idea of a new method was conceived by Kapitza in 1924 in the course of discussions with a Russian electrical engineer, M. P. Kostenko, who calculated that it should be practicable to design a large dynamo that on short circuit through a suitable coil could generate very high power for a very short time and so produce a field of order 10^6 G. The stored energy would be the kinetic energy of rotation of the dynamo rather than the chemical energy of the accumulator.

* The original inscribed reprint is in the Cockcroft archive at Churchill College, Cambridge.
† $1\,G = 10^{-4}\,T$.

Once again Kapitza was enthusiastically supported by Rutherford. But the expense involved was of a bigger order of magnitude than could be provided from Cavendish funds, and Rutherford had to turn to the Department of Scientific and Industrial Research (D.S.I.R.) to realize this first step into the 'machine age'. A grant of £8000 was made, spread over 4 years, and yet again in extremely short time, the centre piece of the scheme, the big dynamo, had been designed, built and tested at Metropolitan-Vickers in Manchester. On 7 July 1925 Kapitza was able to write to Kostenko:

> '. . . The machine has been quite a success. . . . It has been tested and is already installed in Cambridge. All this has taken a lot of work and energy, but as you see in the 1 year and 4 months since we first conceived the idea of this machine it has been built and installed. That's not bad . . .'.

The dynamo was patented (P4) in the names of Kapitza and Kostenko, but some difficult problems had to be tackled before it could be used to generate large fields. These took years to solve, and a detailed account of the whole scheme appeared in 1927 (16). The most challenging problems were the switch that had to close and open exactly synchronously with the dynamo cycle, during which it had to carry as much as 30 kA and the coil that had to be made strong enough to withstand the enormous magnetic stresses. The detailed design of the coil was worked out by Cockcroft (1928) and the problem is vividly illustrated in a letter Kapitza wrote to Rutherford on 12 December 1925 when Rutherford was on his way home from a trip to the Antipodes.

> 'I am writing to you in Cairo to tell you that we have already obtained fields of more than 270,000 G. . . . We couldn't go further because the coil burst with a deafening bang which I am sure would have given you much pleasure. The coil had not been externally reinforced and this we now intend to do. . . . I am very happy that on the whole all went well and now you can be assured that 98% of the money has not been wasted . . . . The accident was the most interesting part of the experiment and finally strengthens our belief in success, since we now know exactly what happens when the coil bursts. We also now know what an arc of 13,000 A looks like.'

Although the high field machine worked successfully and enabled Kapitza to open up several new and fruitful fields of research over the next few years, it is puzzling that he should have chosen the dynamo method rather than what with hindsight would seem to have been simpler and cheaper—namely storing the energy in a large capacitor. In his earlier paper on the accumulator method (11) he mentions this as a possibility but dismisses it without any very convincing argument. It is true that large capacitors at that time may not have been as reliable or as cheap as

they have become since, but a capacitor of order 10 mF that could be charged to 2.5 kV would probably have been cheaper and simpler than the dynamo. Discharge of such a capacitor through a coil comparable to that used by Kapitza could have produced fields comparable to those achieved with the dynamo. Indeed T. F. Wall (1924), an engineer at

Kapitza by B. Kustodiev (portrait owned by the Fitzwilliam Museum and at present on loan to Darwin College, Cambridge).

Sheffield University, did propose this method and in 1926 described experiments giving fields as high as 200 kG (but for rather shorter times than Kapitza's fields), using a capacitor of about 1.4 mF charged to 2 kV. However, Wall's coils burst in more extreme conditions and he does not appear to have overcome the difficulty nor continued the experiments.

At the Solvay Congress in 1930 (28) Kapitza referred to Wall's limited success as evidence for the unsuitability of the condenser method, but this is hardly convincing and Kapitza's real reason for not trying the

capacitor method may have been his dislike of following a trodden path. In fact, with the development of more reliable capacitors and electronic methods of switching and recording, the condenser method came into its own soon after the war (for a review see Cotti 1960). The dynamo system was reproduced and successfully tested in 1939 by Y. Tanabe at Tohoku University in Sendai, but it was destroyed during an air raid on 9 July 1945 (see Tanabe 1949).

Kapitza loved showing off the special features of his installation. Even though the machinery was elaborately mounted to avoid transmitting vibrations, the considerable mechanical shock associated with a loss of 20% of the kinetic energy of the dynamo might easily have upset the delicate recording instruments. To avoid any disturbance, the coil and the recording instruments were placed about 20 metres away from the machine so that the seismic wave through the ground arrived only after the experiment was over, and the necessary space was provided by an outbuilding of the Chemistry Department. Later, when the work was transferred to the Royal Society Mond Laboratory, the long 'magnet hall' became the central feature of the building, with research rooms on either side of it. This turned out to be a very successful design even after the high field machinery had gone, because the long hall provided convenient opportunities for researchers to meet and exchange ideas.

To bring home the novelty of an experiment lasting such a short time Kapitza was fond of telling two little jokes. One was that $\frac{1}{100}$ s was a very long time if you know what to do with it, and the other was that he must certainly be the highest paid scientist in the world since he received a full academic salary for work lasting in total only a few seconds in a year. On one occasion—a rare one—Kapitza was upstaged in showing off his high field equipment. The occasion was a visit from R. W. Wood, the American physicist, himself famous for his ingenuity and his scepticism of what others could show him. Kapitza prepared a striking demonstration of the great strength of his fields by shooting a glass rod immersed in liquid oxygen (strongly paramagnetic) up to the ceiling, where it shattered, when the impulsive field was applied. Wood was accompanied by Rutherford and other Cavendish figures and they looked expectantly to see what impression had been made on him. He, however, did not seem particularly impressed. Instead, he walked up to the coil, took out the small vessel of liquid oxygen and calmly drank Kapitza's health! The spectators, not familiar with this rather difficult trick, which depends on keeping the liquid in the spheroidal state, were rather relieved to see Wood spit out the liquid again after a few seconds.

Kapitza's first major research with the new equipment was an extensive study of how the electrical resistance of metals increases with magnetic field (17–21). This was chosen partly because it was the simplest kind of measurement to make in a pulsed field and partly because it had been relatively little explored before. A general feature of the results was that

the resistance, after starting off quadratically in field, eventually changed asymptotically to a linear variation, often known as Kapitza's law of magnetoresistance. He was able to fit his results to a theory based essentially on the *ad hoc* assumption that ideally the basic law was linear but that this ideal behaviour was disturbed by the presence of randomly directed internal magnetic fields superimposed on the applied field.

Although this bold attempt to give a phenomenological theory of magnetoresistance at a time when the electronic theory of metals was still in its infancy was useful in providing a guide to further experiments, it has not stood the test of time. Later experiments, particularly at lower temperatures and with single crystals, coupled with a better understanding of the theory, showed that Kapitza's linear law was not really basic but a consequence partly of the relatively high temperature of his measurements (only down to liquid nitrogen temperature) and partly of his use of polycrystalline samples in most of his experiments (for a recent discussion see Pippard 1979). Kapitza himself realized that experiments at lower temperatures would probably be essential for a better understanding of metals and this was one of the main motivations for his work on hydrogen and helium liquefaction soon after.

Two more researches at high fields, both again of an exploratory nature, were then carried out. These were studies of the magnetization of a variety of substances (30) and of magnetostriction, mainly of single crystal bismuth (33–35). At the heart of both studies was an ingenious instrument (29) that could be used either as a balance to measure the force on a sample in an inhomogeneous magnetic field, and hence the magnetization, or as an extensometer to measure the change of length of a rod fixed at its other end. The idea was to apply the force or the change of length to a diaphragm sealing the bottom of a small vessel containing oil and itself completely immersed in a bath of the same oil. Displacement of the diaphragm squirted oil through a small hole between the inner vessel and the outer bath and so tilted a light mirror mounted on a pivot near the hole. By suitable design the deflection of a light beam reflected by the mirror could be made proportional to the force or to the extension.

The magnetization measurements did not reveal anything very novel, for it turned out once again that the laws established at lower fields could safely be extrapolated into the new range of higher fields. The observation of magnetostriction in bismuth was, however, the first such observation on a diamagnetic and Kapitza also developed the theory for specifying how the anisotropy of the effect was determined by crystal symmetry. Kapitza's interest in the somewhat anomalous properties of bismuth started a tradition that continued long after he had left Cambridge (see Shoenberg 1978), and his emphasis on the importance of chemical purity and single crystals was also a valuable legacy to those who followed in his footsteps.

In the course of all this work, there were several important

developments in Kapitza's life and career. In January 1925 he was appointed to an official University position, that of Assistant Director of Magnetic Research. In October 1925 he was elected to a Research Fellowship at Trinity College and he became a popular member of the High Table, mixing easily with young and old of all specialties. He greatly valued his association with the College where he lived until he was married and where he continued to dine frequently during the rest of his time in Cambridge. He was particularly pleased when, many years later (in 1966), he was elected to an Honorary Fellowship.

In the spring of 1927 he went to Paris and on 27 April wrote to Rutherford (his spelling has not been corrected):

> 'I am going to be married. . . . What do you think about it??!! I feer you are rather angrey. This is why I propose to have no honeymoon and bring my wife in a few day's time after my wedding to Cambridge . . . I hope you understand that I am a victime of my own 300,000 gausse and I have to confece that the dose which I received is rather a strong one. . . . You see that even in more important questions I have a quick dessision and gread speed of action.'

The lady in question was Anna Alekseyevna, daughter of the Admiral Krylov mentioned earlier. Although Anna's father stayed in Russia after the Revolution her mother emigrated to Paris and it was there that Anna completed her education as an archaeologist; she was also an accomplished artist.

Rutherford's reply (29th April) to Kapitza's announcement also deserves quotation:

> 'I received your letter at breakfast this morning and I read it with much interest and amusement. . . . My wife and I unite in sending our warmest congratulations. . . . They say it is a bad wife that does not help a little, so I shall expect your work to make even faster progress. . . . I am not surprised at the news as I had heard rumours of your magnetic susceptibility under intense attractive fields.'

The marriage was a very happy one and Anna was not only a charming hostess to their many friends, but a great support to Peter in the difficult times he had to go through on several occasions later. Two sons were born in Cambridge: Sergei in 1928, a distinguished physicist and a very successful presenter of popular science on Soviet television, and Andrei in 1931, a well known Antarctic explorer and geographer.

In 1929 Kapitza was elected to Fellowship of the Royal Society at the first election after he had been proposed. This in itself was a rare distinction but even rarer was the election of a foreigner—for Kapitza had always retained his Soviet citizenship. In fact the Statutes at that time did not exclude the election of a foreigner provided that he was an 'inhabitant of His Majesty's dominions', but such elections were indeed rare, the

previous one having been in 1914! But to cap everything, his scientific distinction was almost simultaneously recognized in his own country by election to Corresponding Membership of the Soviet Academy of Sciences.

Early in 1930 Kapitza discussed with Rutherford the possibility of setting up a new laboratory that could house not only the high field equipment, but also provide cryogenic facilities to extend his researches to much lower temperatures. Rutherford backed this idea enthusiastically and persuaded the Royal Society to give £15 000 for building such a laboratory. The money was found from a bequest to the Royal Society by Ludwig Mond and the new laboratory was called the Royal Society Mond Laboratory. At the same time Kapitza was appointed to a Royal Society Messel Professorship. All the complicated negotiations between the Royal Society, the University and D.S.I.R. were completed remarkably quickly and smoothly—partly because of the dominant position of Rutherford in all three bodies and partly through the drive of Kapitza and Cockcroft—and by 1931 a handsome modern building designed by the architect H. C. Hughes was going up in the courtyard of the Cavendish.

The first step in the advance towards lower temperatures was a hydrogen liquefier that was completed by Kapitza in collaboration with Cockcroft shortly before the Mond Laboratory was ready (36). A difficulty in the liquefaction of hydrogen is that extremely pure hydrogen must be used to avoid condensed impurities blocking up the narrow-bore regenerator tubes. This difficulty was ingeniously circumvented by using only a small volume of extremely pure hydrogen in a closed circuit, which cooled ordinary commercial hydrogen and allowed the impurities to settle at the bottom of the heat exchanger without any blockage. A special feature of the liquefier room was a very light roof designed to blow off and relieve the pressure in the event of an explosion. Kapitza was fond of warning nervous visitors of this possibility, though in the event it never occurred.

In constructing a helium liquefier, Kapitza as usual chose a completely original method (39, 40, P10). Instead of relying on cooling with liquid hydrogen to get the helium below its Joule–Thomson inversion temperature, the main cooling was achieved by adiabatic expansion in a piston and cylinder engine. This of course was a well known principle but up to then the problem of how to lubricate the piston had never been solved. Kapitza achieved this by the ingenious expedient of using the helium gas itself as the lubricant. The machine was completed in 1934 and proved important not only in providing liquid helium for over 10 years of Cambridge research, but in supplying the basic idea for a factory built helium liquefier designed by S. C. Collins (1947) at the Massachusetts Institute of Technology (M.I.T.). The commercial availability of these Collins machines revolutionized low temperature physics by making

liquid helium easily accessible all over the world, rather than at only a few specialized centres. Kapitza had hoped that Cambridge would have the first liquid helium in England, but he was upstaged by F. A. Lindemann at Oxford, who brought Mendelssohn over from Germany with one of Simon's miniature liquefiers and triumphantly announced (Lindemann & Keeley 1933) the first liquid helium in England a year ahead of Kapitza. However, Kapitza could claim that his was the first liquid helium that could be looked at (the Oxford liquid was hidden in a metal vessel).

The first liquid helium in the Royal Society Mond Laboratory, 21 April 1934.

The opening of the Mond Laboratory by Stanley Baldwin, then Chancellor of the University, was a great occasion with a luncheon at Corpus Christi College hosted by the Vice-Chancellor, followed by speeches in the Arts School, and finally the opening itself with a gilded crocodile-shaped key and a visitation of the laboratory. Baldwin's speech accepting the Royal Society's gift to the University caused some amused puzzlement, for much of it was almost identical with what Rutherford had said earlier in the proceedings. It seems that Rutherford had forgotten he had already used his remarks in briefing Baldwin! During the laboratory visit Kapitza provided a nice example of his style of irreverent teasing. He was overheard replying to a query from Baldwin: 'Yes indeed. You can believe me, I'm not a politician'. In the course of the opening, a lifesize carving of a crocodile on the wall just outside the main entrance and a bas-relief of Rutherford himself, inside the foyer, both by Eric Gill, were revealed for the first time; the bas-relief gave rise to some controversy about the shape of Rutherford's nose (see Oliphant 1972).

Something should now be said about other aspects of his work and leisure in Cambridge. The Kapitza Club (see p. 346) represents his major contribution, albeit informal, to teaching activities. His other teaching

took the form of supervising research students and some lecturing. The latter consisted of eight weekly lectures on 'Recent researches in magnetism' and I have lively memories of attending this course in 1932. I found it fascinating not only for its content, but also for the intriguing presentation. His strong Russian accent and rather high-pitched voice, his peculiar English constructions and his habit of writing on the blackboard something quite different from what he had said, were entertaining, but sometimes made it difficult to follow. Once I came up after the lecture and asked him to clarify a contradiction in my notes. His reply was 'If I make everything so clear that there are no contradictions, there is nothing left for you to think about.' He also gave many semi-popular lectures and once described his technique: 'I try to pitch it so that 95% understand 5% and that 5% fully understand 95%. I always tell a joke in the first 5 minutes and if they laugh I know they are understanding my English well enough.'

Kapitza supervised only a few research students. The earliest were J. D. Cockcroft, subsequently famous for his nuclear physics work, and W. L. Webster, who did some pioneer work on ferromagnetism. The only others before the move to the Mond were D. S. Kothari, who later made distinguished contributions to astrophysics in India, and A. G. Hill who joined Kothari briefly. At that time the usual practice in the Cavendish was for the Cavendish Professor to be the official supervisor of all the research students, though the detailed supervision was often done by Rutherford's lieutenants. However, after the move to the Mond, Kapitza was named as the official supervisor of two more research students: myself, in 1932, and C. J. Milner, later Professor of Applied Physics at the University of New South Wales, in 1933.

Both Milner and I recall Kapitza's stimulating supervision and his original way of helping to overcome our experimental difficulties. I was put on to the measurement of the transverse magnetostriction of bismuth crystals in the field of an ordinary electromagnet. The expected change of length was very small, only of order 10^{-7} cm, and when I was feeling a bit desperate because the apparatus was still 100 times too insensitive, Kapitza would point out half a dozen simple improvements, each of which should give a factor 2 improvement and then throw in perhaps two rather unorthodox suggestions, each of which should give a factor 3. So the problem seemed to be solved, with even a bit in hand, since $2^6 \times 3^2$ is nearly 600. But when he had gone and I started trying his ideas, the factors of 2 proved to be only perhaps 1.2 and the factors of 3 perhaps 1.5 so that the gain was only perhaps 6 rather than 600. However, I was that much nearer my goal and ready for the next batch of suggestions. Milner was also impressed by the fertility of Kapitza's inventiveness, though he gradually learnt that many of Kapitza's ingenious suggestions carried some fatal flaw. But even if only perhaps one in five proved sound, this one might well hit the jackpot and overcome the difficulty.

Kapitza had many interests and skills outside his scientific work. One of his early enthusiasms was the internal combustion engine. Soon after his arrival in Cambridge he bought a motor-cycle and on 16 August 1921 he wrote to his mother:

> 'Everything went very well although we weren't going slowly, but then we had a spill. There are six bruises and scratches on my body but the worst is my face. If you could only see it! . . . When I came into the lab. I created quite a sensation. Now I shall be much more careful.'

Before long he graduated to a car (first a Lagonda, then a Triumph and later a Vauxhall, chosen because full working drawings were available) and acquired a reputation for rather reckless driving. When he was going particularly fast he would reassure a nervous passenger by saying that his high speedometer reading was in k.p.h. rather than m.p.h. On one occasion he solved the problem of overtaking a slow car observing the speed limit in Richmond Park by going off the road on to the grass and passing rapidly on the wrong side. Perhaps the best story is of his Trinity friend, the Reverend F. A. Simpson, sitting behind him on a country drive. As they approached a dangerous corner Kapitza pressed the accelerator and turned round to say 'Pray God Simpson, pray God'. It seems the prayers were effective. He had, indeed, rather a weakness for teasing clergymen; another example was when a clergyman, a guest dining in Hall, asked Kapitza who Eddington was and was told 'He knows far more about the Heavens than you do'.

As already mentioned, he was skilled at constructing and handling delicate equipment and he loved any activity involving fine mechanisms. One of his hobbies, dating from his schooldays, was repairing watches and clocks and making the replacement parts himself. Another hobby involving manual skill was conjuring. Sergei Kapitza recalls how fascinated he was as a small boy seeing his father appear to swallow table knives and bite bits off china plates. E. T. S. Walton remembers a train journey when Kapitza showed off the three-card trick ('find the lady') as convincingly as any professional trickster. In his memoirs (1968) the famous, but rather staid, applied mathematician Timoshenko recalls Kapitza's glee in making various eminent scientists look a little silly by involving them as stooges in his demonstrations. A rather different type of skill at which Kapitza excelled was chess and Smyslov, the Russian one-time world champion, said that he could not take a game with Kapitza lightly.

Finally, some comments on Kapitza's personality and style as remembered by those who knew him in his Cambridge days. At times he could be almost a text-book example of the absent-minded professor. If a question was addressed to him at such a time his typical reaction would be 'What you say?' and only after several repetitions of the question and

his 'What you say?' would he deal with the question—though sometimes he might simply walk off leaving the question unanswered. Occasionally this may have been a ploy to give himself time to think, but usually it was genuine absent-mindedness, or rather an indication that his mind was fully occupied by something else. Indeed when he was trying out a new idea in the laboratory he would become so completely absorbed as to lose all sense of time and sometimes Anna would have to come in to remind him that they were expecting guests to dinner. He paid little attention to his personal appearance and a Cambridge legend is that he was once refused admission to a formal gathering at the Senate House because he was wearing a blazer and carpet slippers rather than the obligatory dark suit and black shoes. But more often he was cheerful and outgoing, a great charmer and very good company. He loved an argument, he found something interesting to say on almost any subject and he was an excellent raconteur with an enormous repertoire of anecdotes, often of the shaggy dog variety, but usually with a Russian twist. Occasionally the point of the story would be obscure to someone not familiar with Russian traditions or because of Kapitza's peculiar English, but his laughter over his own joke was so infectious that those around him found themselves joining in, even if they hadn't altogether understood the joke.

It was perhaps the combination of his outgoing personality, his genuine interest in people and his curiosity about what made them tick that enabled him to overcome shyness and reserve and make friends of people like the rather reserved Chadwick, the taciturn Cockcroft, the usually silent Dirac and even the austere A. E. Housman of Trinity. These qualities also enabled him to gain the friendship and support of Rutherford, though here, as discussed by Wilson (1983), other factors were also relevant. Rutherford seems to have been captivated by Kapitza's boldness, both his boldness of scientific vision and his boldness in treating Rutherford with much less reverence and respect than the great man was accustomed to from his juniors, whilst at the same time openly showing his admiration of Rutherford's genius. A subject of joking in the Cavendish was Rutherford's fondness for using blunt stub ends of pencil, and on Rutherford's 60th birthday Kapitza presented him with a silver propelling pencil accompanied by a facetious congratulatory letter. Rutherford seems to have enjoyed this kind of lighthearted teasing, though Kapitza's gift did not wean him from his stub ends. When Kapitza had settled in Moscow some years later, Rutherford wrote how much he missed their evening walks after Sunday dinner in Trinity.

Two other aspects of Kapitza's personality will be evident from much of what has been said: his fondness of boasting and exaggeration and his enormous self-confidence. However, usually these were displayed in a somewhat tongue-in-cheek manner which endeared rather than repelled. For instance N. Kurti, recalls that when Kapitza visited Berlin in 1930 and F. E. Simon (who liked precision) asked for his exact English

address, he said with a chuckle 'Kapitza, England' will do. He loved above all to achieve something that was unique or at least exceptional. Thus he was proud of his unique distinction of being both an F.R.S. and a Soviet Academician and of his extremely rare status of remaining a Soviet citizen yet holding an important post in England. His self-confidence was based on realistic assessment of the problem in hand, and usually this self-confidence proved justified by the successful outcome of whatever he was trying to achieve. Rutherford was certainly impressed to see that Kapitza's confidence in his ingenious approach to his α-ray problems was justified by the successful results he obtained so quickly. This made Rutherford all the more ready to accept Kapitza's persuasive arguments about the potential importance of high magnetic fields and low temperatures and to give his full backing in developing a new branch of physics outside his own field.

The Kapitza Club, 1922–66

When Kapitza came to Cambridge in 1921 he missed the lively discussions of the Joffé seminars in Petrograd and he soon started an informal discussion group of his own, providing an injection of Russian temperament into his phlegmatic English colleagues. The first meeting was on 17 October 1922 with Kapitza giving a talk on magnetism. Although the emphasis was always on informality, Kapitza did insist on certain rules. No one could remain a member after missing more than a small number of consecutive meetings, and members were regarded as permanent only after they had themselves given a talk.

This weekly seminar soon came to be called the 'Kapitza Club', but Kapitza himself used to refer to it modestly as 'the club', though he was well aware of the name others used, and of the significance of the club in stimulating Cambridge physics. In a letter to Anna from Moscow in 1935 he wrote: '. . . Even in Cambridge I left a mark. Take the club which it is the custom to connect with my name, of which like old Pickwick, I was a permanent president. I think it will stay a long time . . .' Soon after its inception, a minute book was started and the tradition was established that the speaker should add his signature to the title of his talk. The original minute book is in Moscow, but a photocopy together with the second (final) volume, is in the Cockcroft archive at Churchill College. The record provided by these minutes is impressive. Among the speakers were not only the members, but also many leading physicists outside Cambridge: Bohr, Ehrenfest, Franck, Heisenberg and Langevin, to mention only a few. Most of the key advances of the time were eagerly discussed at the club and sometimes ahead of publication. Thus Landau spoke of the magnetism of metallic electrons in 1930, and in 1932 Chadwick, Lea and Feather reported the discovery of the neutron

(though the title was cautiously entered as 'Neutrons?') and Cockcroft reported the artificial disintegration of lithium.

Kapitza greatly encouraged interruptions and to break the ice he would himself often ask seemingly naïve questions. Sometimes such questions would provide a useful hint to the speaker that he was talking over the heads of his audience and nearly always they would start an argument and so clarify an obscure issue. R. W. Ditchburn recalls how on one occasion an American visitor presented a theory based on the theorem that the currents in a network adjust themselves to make the energy production a maximum. Kapitza interrupted to ask 'Don't you mean a minimum?' and others supported him. To decide the issue Kapitza took down from his bookshelves Maxwell's famous treatise, which he regarded almost as a Bible, and said 'Ah yes it says a minimum in Book II, Chapter 7' and someone chipped in 'Verse 12' causing a big laugh, (but for once Kapitza couldn't see the joke).

Some of the spirit of the discussion and argument comes over in brief remarks in the minute book. For instance in 1923 G. N. Lewis talked on 'What is the origin of radioactive substances?' and Kapitza recorded a characteristic remark: 'It is not advisable to look on the phenomena on the stars if you do not understand the phenomena on the earth'. In the same year Skinner spoke about the newly discovered Compton effect and the comment 'Compton is wrong' appears over the initials of Kapitza, Skinner, Hartree and Lennard-Jones. But a few months later 'Compton right we hope' is initialled by Stoner and Blackett, while Kapitza and others still backed the wrong horse and wrote 'We hope wrong'. Not long after a sensational report in the Sheffield Daily Telegraph (26 May 1924) of a death-ray machine invented by T. F. Wall (perhaps an offshoot from his condenser discharge experiments?), the minute book records 'Will Dr. Wall disintigrate the Univerce? P.K.'

The 377th meeting of the club on 21 August 1934 was the last with Kapitza in charge; when he was unable to return from the Soviet Union, Cockcroft took over and after the war I kept the club going for another 12 years. However as physics became more compartmentalized it was only rarely that physicists from one speciality could be persuaded to come and learn what was going on in a different field and eventually I felt that the club had had its day. The last regular meeting, the 675th, was on 4 March 1958 with a paper by Sir Ronald Fisher on 'Probability and scientific inference'. This, however, was not quite the last meeting. In 1966 Kapitza was at last able to visit Cambridge again and a special meeting was arranged on 10 May with quite a few present who had been members when Kapitza was in charge more than 30 years earlier. The scientific highlight of the meeting was a review by Kapitza and Dirac of a project (38) they had mooted in 1933 of scattering electrons by standing light waves. In 1933 light sources were too weak and electronic detectors too insensitive to produce any detectable Bragg reflection, but with the

Cockcroft, Dirac and Kapitza at the final meeting in 1966 of the Kapitza Club in Gonville and Caius College.

technical advances since then the project no longer seemed as 'way out' as it had seemed then.

It is convenient to conclude this section with a brief account of the seminars that Kapitza organized in his Moscow institute after 1936. These, like his Cambridge club, acquired a considerable reputation. As in Cambridge, they were at first intimate affairs with a dozen or so regulars from the Institute itself and occasionally a few guests. The audience sat in easy chairs in Kapitza's huge office and instead of the Cambridge coffee and biscuits, Russian tea and caviare sandwiches were provided. Kapitza presided in his idiosyncratic way and there was the same tradition of irreverent interruptions from him and the audience.* After the war and after the eight year interval of Kapitza's demotion, the seminars resumed, but gradually became much larger, with audiences of a hundred or so in a large lecture room, rather than the small group in Kapitza's office. Although little of the informality and intimacy survived the greatly increased numbers, the seminars still served a useful purpose in bringing

* For an amusing skit based on the early days of the seminar see Shalnikov (1975).

together physicists from all over Moscow, and it was regarded as a great privilege to be invited to give a seminar. Eventually, although regular dates were set aside, only a few seminars were actually given and during the last ten years or so they practically ceased.

Return to the Soviet Union, 1934

As already mentioned, Kapitza was rather proud of his unusual status of directing a prestigious laboratory in Cambridge while remaining a Soviet citizen and being able to go in and out of the Soviet Union at will. After 1926 he visited the Soviet Union nearly every summer, and always had his permission to return underwritten by high-up figures in the Soviet political and scientific establishment. During these trips he visited his mother, gave lectures, consulted and usually managed to have a good holiday in the Caucasus or Crimea. As early as 1929 he had been invited by L. B. Kamenev (then an important political figure with special responsibility for science) to head the big Ukrainian Physico-Technical Institute being set up in Kharkov. Kapitza was able to convince Kamenev that he could more usefully develop his work in Cambridge for the time being, while acting as a consultant to the new Institute during short visits in the summer. Although his friends wondered whether his exclusive status could continue indefinitely, Kapitza laughingly shrugged off any warnings and in August 1934 made his usual summer trip, accompanied by his wife and travelling in their Vauxhall across Scandinavia. After attending a congress in Leningrad and lecturing in Kharkov, he and Anna were preparing to return to England from Leningrad early in October when the blow fell. Kapitza was told that his permission to return was no longer valid and that he would have to stay in the Soviet Union. After a few days Anna was allowed to return to Cambridge to look after the children and on 10 October she was able to tell Rutherford what had happened.

The story of the subsequent developments is one of almost Byzantine complexity and will only be outlined here; for a more detailed account see Wilson (1983, chapter 16). The reason for Kapitza's retention has been the subject of much speculation, but the basic reason was probably that suggested by Rutherford in a letter to Sir Frank Smith (then Secretary of the Department of Scientific and Industrial Research):

> 'I think I told you that Kapitza in one of his expansive moods in Russia told the Soviet engineers that he himself would be able to alter the whole face of electrical engineering in his lifetime. . . . This seems to me a very probable explanation of their action and is due to our friend's love of the limelight.'

Two other circumstances were also probably relevant. One was that George Gamow, a top young Soviet theoretician, had recently failed to

return from a visit to the West and Kapitza's retention may have been to some extent a tit-for-tat. The other was the great expansion of the Soviet economy in the early 5-year plans, of which a concomitant was the need for a rapid growth of science. In particular, Krzhizhanovski, an electrical engineer who held an influential position in the political establishment, wanted Kapitza to build an Institute in Moscow on the lines of the Mond, which would, as Krzhizhanovski thought, revolutionize the production of electricity.

Kapitza was utterly devastated by his inability to return to Cambridge and tried to disabuse the authorities of the exaggerated ideas they had formed about the immediate technical relevance of his work. For a time he sulked and thought seriously of taking up biophysics with Pavlov in Leningrad if he could not continue his work at Cambridge. But it was dangerous to sulk too long in those days and gradually Kapitza began to go along with the idea of working in Moscow, although he still hoped that a return to England might be possible.

Indeed, considerable efforts were being made by Rutherford on his behalf. He appealed to I. M. Maiski, the Soviet Ambassador, in very diplomatic terms, he got eminent scientists abroad, such as Langevin and Bohr, to make discreet representations to Soviet establishment figures, and representations were made to Stanley Baldwin, then Prime Minister. Nothing came of all these attempts, even though the whole affair had been kept private, so that no loss of face would have been involved if the Soviet authorities had made a concession. Eventually, however, and inevitably, the affair leaked into the Press with a big splash in the *News Chronicle* on 24 April 1935. On 1 May *The Times* published letters from both Rutherford and Gowland Hopkins (then President of the Royal Society), setting out the facts in a restrained fashion and appealing for Kapitza to be allowed to return to Cambridge to complete the work he had started. The effect of these letters was somewhat undermined by a jingoistic letter to *The Times* on 7 May from the chemist H. E. Armstrong (then the senior Fellow of the Royal Society), who claimed that England had no need of foreign scientists in general and of Kapitza in particular: 'Instead of leading a lotus life at Cambridge, he, too, may well be doing national work (in Russia) of a far higher importance than even that involved in magnetizing atoms to destruction'. Finally Maiski publicly defended the Soviet action in retaining Kapitza and concluded: 'Cambridge would no doubt like to have all the world's greatest scientists in its laboratories, in much the same way as the Soviet Union would like to have Lord Rutherford and others of your great physicists in her laboratories'.

By the end of 1934, though still unhappy with his situation and still often thinking of giving up physics, Kapitza began to cooperate in planning the new Institute of which, as he learnt from the newspapers, he had been appointed Director (the Institute was established on 23 December). Together with A. I. Shalnikov, a clever young experimenter

from Leningrad, he toured Moscow looking for suitable sites. They settled on an attractive place on the Lenin Hills and the new Institute for Physical Problems (I.F.P.) began to go up in May 1935. According to Khrushchev's memoirs (1974, p. 63)—not the most reliable of sources—this choice site had originally been reserved for the American Embassy, but Stalin had taken against Bullitt, the then Ambassador, and decreed that Kapitza should have it.

Kapitza continued to feel very frustrated and miserable all through the early months of 1935. He greatly missed his family and his work and he was treated with some reserve by scientific colleagues who were either too afraid or else too important to see him, so that he felt particularly lonely living alone in the Metropole Hotel. In his negotations with V. I. Mezhlauk (then an important political figure and Chairman of the State Planning Commission) he had great difficulty in getting across the idea that his work lay primarily in pure rather than applied physics and that he couldn't do anything useful unless he had facilities similar to those in Cambridge. At times he had fits of depression and he wrote to Anna in April: '. . . Here I sit all alone. What for? I do not understand. I want to scream and break furniture. I sometimes think I begin to go mad.'

However, gradually Kapitza's views began to prevail and even before the newspaper publicity, the Soviet authorities had started negotiations with Rutherford for transferring Kapitza's special equipment from the Mond to the new Institute. The newspaper publicity seems to have acted as a spur to settle things more quickly, though Mezhlauk was unpleasant to Kapitza about it. Kapitza wrote to Anna on 5 May:

> '. . . I said [to Mezhlauk] I obey in all I was ordered. But some of the orders sound as if Beethoven was told to write the 4th Symphony to order; of course Beethoven could conduct an orchestra to order, but would scarcely be willing to write a symphony to order, in any case a good one . . .'

Things began to get easier in other respects. The 'guardian angels went back to heaven' (he was no longer under surveillance), he was allowed to travel freely within the Soviet Union, he was assigned a good flat, frequent theatre tickets and a good car. The car evidently meant a lot to him:

> 'Yesterday the Institute received a very good car, big and comfortable and which I can use while I am Director. It is rather fun to have such a car. When I came to the door of the Metropole the porter helped me out . . .'

The negotiations went on with many ups and downs all through the summer of 1935; several times Kapitza threatened to resign and take up biophysics. At one stage there was an intervention by Molotov, who got Kapitza to write to Rutherford saying that he was a loyal Soviet citizen and that he was prepared to work in the laboratory being built for him in

Moscow. This letter finishes rather pathetically:

> 'I miss you, my laboratory and specially my work and it is not to be expected that I soon will be able to resume it, and all this makes me very unhappy. The stupidity of the created position is that it is based on complete misunderstanding as everyone concerned really acts with the best intentions.'

The most decisive stage of the negotiations came in August with visits to Moscow by Adrian and Dirac, when both had the opportunity of frank discussions with Kapitza that they were able to report to Rutherford. Eventually, by November a definite agreement had been reached between Cambridge University and the Soviet authorities, giving Kapitza most of what he wanted and on 5 November he formally resigned his Directorship of the Mond Laboratory. The agreement provided that in return for payment of £30000 the University would transfer to Moscow the high field equipment and duplicates of such items (e.g. the liquefiers) as were needed in Cambridge to continue the Mond's activities. In addition the University was to give a year's leave of absence to Kapitza's key assistants, Laurmann and Pearson, so that they could help Kapitza set up all the equipment in the new Institute as quickly as possible.

In the mean time Anna had had the difficult task of winding up everything in Cambridge and by the end of 1935 she and the children had rejoined Kapitza in Moscow, where before long they moved into a comfortable and central apartment. Even though things now began to hum, with Cockcroft in Cambridge devotedly organizing the immense task of sending the equipment to Moscow—the big dynamo had already left by the end of December 1935—and Kapitza struggling to get everything ready to install the equipment as soon as it arrived, he still felt frustrated that he could not return to scientific work immediately.

His mood may be judged from one of his letters to Rutherford (March 1936) in the course of which he says:

> 'I feel myself very miserable here, better than last year, but not so happy as I was in Cambridge. Anna's return brought me much comfort and happiness and this is very important as I was here very lonely, quite alone. . . . Your letter reminded me of my happy years in Cambridge and then I felt you as you are, rough in words and manners and good in your heart such as I like you, and this makes me feel rather happier. The lost Paradise!'

This letter was in reply to one in which Rutherford, while sympathizing with Kapitza's difficulties, had expressed himself rather bluntly about a previous letter from Kapitza in which he had impatiently complained about various delays in the transfer arrangements and had made further requests that Rutherford thought excessive.

By the summer of 1936 the building of the Institute was far advanced,

most of the vital items of equipment had arrived and so had Laurmann and Pearson. They immediately got busy installing the machinery and familiarizing the Russian technicians with its operation and by the end of 1936 the I.F.P. was a going concern. Kapitza carried on his scientific work there to the end of his life apart from two interruptions: from 1941 to 1943, when the Institute was evacuated to Kazan during the war, and from 1946 to 1954 when he was temporarily dismissed from his Directorship.

Moscow and Kazan, 1934–46

The Institute for Physical Problems (see references G7 and G28) was in part almost a Chinese copy of the Mond with a long magnet hall and adjacent research rooms, a liquefier room with a light roof and of course much of the original Mond equipment or duplicates, such as the distinctive-looking central switchboard and the distribution boards in each room. However, in other respects the accommodation was on a rather grander scale than the Mond. The Director's office was immense, there was a large meeting hall and considerable office space. Though the administration was inevitably larger than in the Mond, Kapitza succeeded in keeping it to a minimum by eliminating much of the elaborate paper work involved in planning and finance that was usual in most Soviet institutes. He was fond of saying that over-elaborate planning in research was rather like a doctor having to prescribe medicine for an illness his patient would have in a year's time. In a report to the Praesidium of the Academy of Sciences (G28) he said:

> 'The Finance people wanted to introduce the so-called thematic accounting whereby expenditure was reported in detail for each line of research. In my correspondence with the Commissariat I asked: "When looking at a Rembrandt picture are you really interested in the great artist's expenditure for brushes and canvas? Then why are you so interested in the cost of equipment and materials when you consider research?" A fruitful research is worth incomparably more than the expenditure involved in it. The cost of research in money terms is incommensurable with its cultural value. I asked: "How much funds in the Commissariat's opinion should Isaac Newton have been given for his work which culminated in the discovery of universal gravitation?" Yet the Commissariat remained adamant . . . and I think they would have gained the upper hand were it not for the intervention of the U.S.S.R. Council of People's Commissars. Finally our Institute was given a simplified financial system which saved the director from everyday troubles and endless "combinations" of work. Under the new system, for example, the institute employs a single bookkeeper, and when we are short-handed he finds time to help us in testing equipment, taking records and making measurements.'

The Institute was attractively sited with gardens and parks around it, and the living quarters looked out on a tennis court, which became a skating rink in winter. There was a magnificent house close by for the Kapitzas, which the Russians called the 'Cottage' (under the impression that this was the English word for any detached house) even though the house looked more like a small palace. Not only was the Institute well equipped, but it also provided a high standard of technical assistance. Indeed Kapitza had an extraordinary flair for picking good people to assist him. The glassblower, A. V. Petushkov, was a virtuoso who loved the challenge of a difficult problem. Similarly, the senior mechanics Minakov and Yakovlev, and Kapitza's personal assistant S. I. Filimonov, who later got a doctorate and became an important member of the scientific staff, were all masters of their craft. Another key figure was Kapitza's secretary, Oleg Pisarzhevski, who was able to shield Kapitza from many administrative chores. Later he left to become a scientific journalist and was replaced by Pavel Rubinin, a man of many parts, who again was well able to deal with many matters on his own initiative.

The main themes of the I.F.P. for its first 20 years or so were essentially the same as those of the Mond: magnetism and low temperature physics, though later the scope was extended to include plasma physics, which became Kapitza's chief personal interest. His first project (44) in Moscow was to extend his earlier Zeeman effect experiments to the threefold higher magnetic fields of the dynamo method. Once again the experiments confirmed the validity of the theory up to the highest fields and nothing very exciting turned up, but this work was a morale booster in demonstrating that the Cambridge equipment was able to work just as effectively in its new environment. It was also somewhat of a swan song because this was the last time the high field equipment was used. It remained, however, enshrined in its magnet hall and became essentially a museum piece to show to visitors.

In parallel with this high field work, rapid progress was being made in completing the cryogenic facilities, and Kapitza took up two new lines of research that became his main preoccupation for the next ten years. One of these, the study of the transport properties of helium II (liquid helium below its λ-point) led to a series of remarkable discoveries and the award of two Nobel prizes: one to Landau for his theory of quantum liquids, based largely on Kapitza's experiments and the other, very belatedly, to Kapitza himself. The second line of research was more technically oriented: the development of a new and more efficient method of liquefying air and hence, by fractional distillation, of manufacturing oxygen on an industrial scale.

Kapitza's interest in liquid helium was stimulated by the Keesoms' discovery (1936) of the extraordinarily high thermal conductivity of helium II and by the experiments of Allen, Peierls & Uddin (1937), which showed a nonlinear dependence of heat flow on temperature

gradient. This led him to the idea that the heat transport might be associated with convection, brought about by an extremely low viscosity, rather than conduction. To observe such a low viscosity, however, the liquid flow must be through very fine channels, otherwise it would be governed entirely by turbulence, and to achieve this Kapitza studied the radial flow under gravity of the liquid between two optical flats separated by a gap of order 1 μm or less. The result was that the viscosity was indeed many orders of magnitude less than had been inferred from previous experiments in which the flow must have been turbulent rather than laminar. Indeed Kapitza was able to set only an upper limit to any possible viscosity and suggested, by analogy with superconductivity, that below the λ-point liquid helium became a 'superfluid'.

By coincidence, similar results were being obtained in Cambridge by Allen & Misener at the same time, though entirely independently. Their flow was through fine capillaries rather than through a narrow gap and they too found only an upper limit (comparable to Kapitza's result) to any possible viscosity. The Cambridge group first learnt of the Moscow experiments from W. L. Webster, who had been visiting Kapitza, and showed Cockcroft the note Kapitza had sent to *Nature*. Cockcroft then wrote to Kapitza about the Cambridge work and sent a note of Allen and Misener's results to *Nature*, in which they commented that Kapitza's explanation of the high heat conductivity by convection was probably too simple. The two notes appeared together ((43) and Allen & Misener 1938), though the appended dates of submission (3 and 22 December 1937) imply a slight priority for Kapitza.

During the short remaining time before the interruption of the war, an intense effort both in Moscow and in the West went into trying to understand the behaviour of helium II more fundamentally. Important contributions were made by Allen & Jones, who discovered the thermo-mechanical ('fountain') effect; by H. London, who predicted the inverse mechanocaloric effect; by Daunt & Mendelssohn, who demonstrated this inverse effect; and by Tisza, who produced a phenomenological two-fluid theory, though without any sound fundamental basis. The decisive results were, however, obtained in a series of ingenious experiments by Kapitza (47, 48). It was this work that enabled Landau (1941) to develop a fundamental quantum theory of liquid helium that provided a rigorous basis for the two-fluid idea and also led to important new predictions.

The major result of the experiments was to show that heat flow was accompanied by mechanical flow. Thus when heat flowed from a closed bulb containing helium II through a capillary into a bath of helium II, a light vane suspended opposite the mouth of the capillary could be seen to deflect. A rather spectacular demonstration of the effect, involving a masterpiece of Petushkov's glassblowing art, was provided by a miniature reaction turbine consisting of six capillaries radiating out of a blackened closed bulb and bent near their ends to point round the circumference of

the circle. This whole 'spider', as Kapitza called it (in spite of its entomological inadequacy), was mounted on a needle pivot and when light shone on the bulb the spider began to spin rapidly, up to 120 rev./min, in reaction to the liquid flowing out of its legs.

This outflow of liquid accompanying heat flow immediately raised the question of how the amount of liquid in the closed bulb was conserved. At first Kapitza proposed that there was a compensating flow inwards restricted to a thin layer close to the capillary wall and having a different heat content from the bulk liquid because of proximity to the wall. However, a more convincing explanation was provided by Landau's theory, according to which the inflowing liquid was the superfluid component of the two-fluid ensemble. This component exerted no force on the capillary or the suspended vane and carried no entropy, while all the entropy of the liquid was concentrated in the outflowing normal component that deflected the vane or made the spider rotate. In his second study (48) Kapitza used a somewhat different experimental arrangement in which liquid could be made to flow into a bulb containing a heater and thermometer through a very narrow channel (a gap between flats, as in his superfluidity experiments). He found that for sufficiently low power in the heater there was no temperature difference between inside and outside and in this régime the power in the heater was directly proportional to the rate of mass flow into the bulb. The detailed analysis of these experiments provided a quantitative confirmation of the two-fluid theory according to which only the superfluid component, carrying no entropy, could flow into the bulb.

In the course of these studies Kapitza also discovered that if heat flows from a solid surface into helium II there is a temperature discontinuity at the surface. This has come to be known as the 'Kapitza boundary effect' and has been the subject of much study in postwar years. All this work was broken off in 1941 when the Soviet Union was invaded, and Kapitza himself did not continue it after the war. However, he did actively encourage further work and two of the most striking new predictions of Landau's theory were convincingly confirmed in his Institute. One was the existence of 'second sound', essentially a temperature rather than a pressure wave, as E. M. Lifshitz (1944) pointed out, which was demonstrated by Peshkov (1944). The other was an elegant and direct demonstration of the two-fluid theory by Andronikashvili (1946), who showed that the moment of inertia of a pile of closely packed disks was enhanced by only the normal component of the liquid between the discs. An entertaining account of all this work is given in Andronikashvili's (1980) memoirs.

Kapitza's other major research effort at this time was the development of a new method of liquefying air, with a view to simplifying and cheapening the large-scale production of oxygen for industrial use. The methods then in use achieved the final cooling by the Joule–Thomson

effect, which involves high pressures, so that the machinery was inevitably bulky and expensive. Kapitza (45, P13) proposed instead the use of an expansion turbine, requiring only about 5 atm, to cool the air all the way to its liquefaction temperature, and showed that with a novel radial–axial design a higher efficiency could be achieved than was possible with the conventional impulse type of turbine.

The working out of the whole scheme involved a profound understanding of both physical and engineering principles, and the successful construction of a laboratory-scale liquefier in less than 2 years was a considerable achievement. The machine delivered about 30 kg of liquid air per hour after a start-up time of only about 20 min and with an expenditure of $1.2\,\mathrm{kW\,h\,kg^{-1}}$, a good performance even by today's standards. An important part of the design was how to overcome the instabilities associated with the very fast rotation of the turbine rotor. The analysis involved was presented in a separate paper (46) and the methods were patented (P12, P16). P. B. Moon (1978) in his Rutherford Lecture, mainly about other applications of high-speed rotors, comments enthusiastically on the elegance of Kapitza's analysis, which he found directly applicable to his own work.

Shortly after Russia entered the war, the I.F.P., together with many other Academy institutions, was evacuated to Kazan, where the equipment was set up in the University, and one of the main efforts was the rapid development of an industrial-scale plant for oxygen production based on the expansion turbine method. It was a remarkable achievement that in spite of the very difficult conditions (lack of fuel, extreme shortage of food, etc.) a large-scale pilot plant was successfully completed during the two years that the evacuation lasted. Kapitza was appointed head of a new government department dealing with oxygen production and after the return from evacuation in 1943 he was also in charge of a factory being built at Balashikha near Moscow. All this work resulted in a number of patents (P14–P18). His design of expansion turbine has proved to be the basis of much of the world's industrial production of oxygen and his pioneer work is still greatly respected by industrial engineers.

Kapitza received many official recognitions of his achievements: election as a full Academician in 1939, Stalin prizes in 1941 and 1943, Orders of Lenin in 1943 and 1944, and for his oxygen work the title of Hero of Socialist Labour together with a third Order of Lenin and the Hammer and Sickle gold medal in May 1945. But soon afterwards there were ominous signs that all was not well. To quote Andronikashvili (1980):

> 'All sorts of odd things began to happen in the Institute in the spring of 1946. Commissions kept arriving to investigate the activities of the Institute and then the status of the commissions got higher with the arrival of Ministers. They began to investigate

> Kapitza's oxygen project. They spoke badly of the factory at Balashikha—not productive enough, uneconomical . . . late in meeting its targets etc . . .'

All this culminated in the summer of 1946 in a curt announcement on the bulletin board that:

> 'P. L. Kapitza, having shown a cavalier attitude to both Soviet and foreign achievement in the technology of oxygen production and having failed to meet the scheduled dates for introducing new installations into the metallurgical industry, is relieved of his duties as Director of the Institute for Physical Problems.'

It is likely, however, that this sudden turnabout in Kapitza's fortunes occured for quite different reasons than those officially stated, though no doubt the fact that Kapitza had trodden on many toes in putting through his oxygen project also played its part (see G86). The real reason was probably Kapitza's refusal to work in the organization set up under Beria (then head of the Ministry of Internal Affairs, which controlled the K.G.B.*) to develop the Soviet atom bomb immediately after the American bombs had been exploded (see, for instance, York 1976). Kapitza is said to have written to Stalin that Beria was 'like the conductor of an orchestra with the baton in his hand but without a score'. Beria wanted to arrest Kapitza for such insubordination, but Stalin in his unpredictable way, perhaps because he admired Kapitza's courage, vetoed this proposal and said dismissal would be sufficient.

This was not Kapitza's first brush with the K.G.B., and perhaps the memory of that earlier occasion may have rankled and also played a part in his dismissal. During the great prewar purges Landau was arrested in 1938 and Kapitza showed great courage by intervening on his behalf. He wrote to Stalin saying that his work could hardly go on without Landau and later sent a reminder to Molotov. Eventually he was summoned by a high-ranking K.G.B. official who kept trying to offer Kapitza a fat dossier. Kapitza however succeeded in outfacing him, saying he understood nothing about legal technicalities, and insisting that though Landau may have had a sharp tongue he was certainly no spy. Landau was indeed soon released and many years later in a tribute for Kapitza's 70th birthday wrote (1964):

> 'These years [the late 1930s] are memorable to me for another but very sad reason. Because of a stupid denunciation I was arrested and accused of being a German spy. Now I can sometimes even find this funny, but then it was no joke. I spent a year in prison and it was clear that I couldn't last another 6 months—I was simply dying. Kapitza went to the Kremlin and announced that he would have to leave his

* The familiar name K.G.B. will be used throughout, though in fact it was called the N.K.V.D. in the 1930s and M.G.B. in the early 1950s.

Institute if I wasn't released. I was released. It is hardly necessary to say that such an action in those years required no little courage, great humanity and crystal-clear honesty.'

Nikolina Gora, 1946–54

Although Kapitza was dismissed from his Institute he retained his position (and salary) as an Academician and chose to live at his dacha (country house) at Nikolina Gora, where he managed to carry on scientific work until he was reinstated eight years later, when it was acknowledged that the unfavourable report on his oxygen work was mistaken. Most of his effort went into building up a laboratory in various outhouses at the dacha where, aided by his sons (particularly Sergei) and Filimonov, he could continue experimental work. This laboratory was jokingly called the Izba for Physical Problems (izba means a peasant's hut) with the same initials as the I.F.P.

He also wrote theoretical papers on various topics, such as heat transfer in two-dimensional turbulent flow (50), wave flow in a thin layer of viscous fluid (51, 52), formation of sea waves by wind (54), and the hydrodynamic theory of lubrication in rolling (59). All this work was characterized by considerable ingenuity in simplifying the mathematical analysis without losing the essence of the problem; the lubrication work is described as 'classic' in a recent review (Dowson 1979). The theory of wave flow along liquid flowing over the outside surface of a cylindrical tube was verified experimentally in collaboration with Sergei (53), with carefully designed apparatus entirely built with the relatively primitive resources of the izba, and rather in the style of Kapitza's early work in the Cavendish.

But a good deal of other work was going on at Nikolina Gora that because of its potential applications was initially classified and only published in 1962 (66, 67). An early hint of this work was a paper on ball lightning (60) in which it was suggested that the ball of hot gas is essentially a resonator fed by high-frequency radio waves typically of wavelength 50 cm, which are somehow generated by the storm. Later Kapitza tried to verify this idea experimentally (73), but no conclusive evidence was obtained and his hypothesis has not found general acceptance. Another and less direct hint (55) was a novel theoretical demonstration of the stability of an inverted pendulum with the point of support vibrating much faster than the period of the pendulum; an elegant experimental demonstration of the effect was also described.

As was later made clear, both these studies were spin-offs from Kapitza's main thrust, the development of powerful high-frequency radio generators of the magnetron type. In a series of researches, essentially completed by 1952, he had worked out the detailed theory in an explicit form of a 'planatron' (essentially a linear magnetron, some-

times called a 'dicotron') and constructed and tested a working model. The merit of his approach was that it provided an objective method of design of powerful oscillators, in contrast to the trial and error methods that had mostly been used up to then. It was the formal analogy of the differential equations involved with those of the vibrating pendulum support problem that led him to tackle the latter by the same technique. The planatron gave several kilowatts output of 10 cm waves and a striking observation was that if the waves fell on a quartz sphere containing low-pressure helium, a vivid discharge took place, which after a few seconds raised the temperature enough to melt the quartz. It was this observation that led to the ball lightning hypothesis, but more significantly, it led to the idea that with a still more powerful microwave source a plasma might be heated sufficiently to produce a thermonuclear reaction. The next step was the development of a more powerful type of magnetron, called a 'nigotron' (after Nikolina Gora), but by the time this was done Kapitza was back in his old Institute. What had been achieved at Nikolina Gora was impressive considering the limited resources available and again illustrates Kapitza's drive and his intellectual powers in formulating and solving difficult problems. However, during the 10 years before publication much of this achievement was overtaken by developments in microwave generators elsewhere (for reviews see Okress 1961 and Harvey 1963) and its impact has not been great outside the Soviet Union.

Moscow, 1954–84

Once back in Moscow Kapitza worked mainly on the nigotrons, though he still maintained his interest in liquefaction techniques (61, 64, 65, 85). The scale of the high power electronics work was greatly increased and a special wing was set aside for the 'Physical Laboratory' where the nigotrons could be developed and applied. The secrecy of the work was partly lifted in 1962 and more completely in 1969, when striking observations, made as early as 1958, of very hot plasma were described (77). The glowing region had a sharply defined shape that could be varied from spherical to stringlike and it could be kept well away from the walls of the vessel into which the microwaves were fed. Spectral studies suggested that electron temperatures of order 10^6 K were reached, though the ion temperatures were much lower.

Kapitza was convinced that it should be possible to exploit such hot plasma strings to reach the conditions necessary for a sustained thermonuclear reaction and that his microwave heating approach offered a better chance of success than the Tokamak and laser heating methods so actively being pursued in the Soviet Union and in the West. He proposed a schematic design for a realistic reactor (78, P19) and continued hot plasma experiments (79–84) on an ever larger scale to the end of his life. Although he claimed to have achieved electron temperatures as high as

5×10^7K (83, 84) his claim may have been based on faulty diagnostic methods, and some of the basic assumptions in his analysis of the problem have been questioned. The whole research must be regarded as a brave effort in Kapitza's typical style of going it alone and taking little notice of what others were doing around him, but he could hardly compete with the big battalions. Although he produced much interesting plasma physics, his efforts were largely eclipsed by the work of the powerful teams at the Kurchatov Institute and in the West using the Tokamak system. It should be noted, however, that there is an echo of Kapitza's ideas in the latest Tokamaks that make use of microwave heating of plasma confined in quasi-d.c. magnetic fields (see, for example, Alikaev *et al.* 1985).

In his style of running his Institute, Kapitza modelled himself closely on Rutherford and often explicitly said so. When giving advice about possible research projects he was fond of telling how, many years ago, Moseley had suggested several projects to Rutherford and Rutherford with unerring intuition had picked the winner. Like Rutherford he insisted that laboratory work should stop at 6 pm (later 7 pm) except by special permission, so that research workers would have time to reflect. Just as Rutherford had once insisted that Kapitza should take a holiday after a period of hectic work, so Kapitza too insisted on occasion that his subordinates must take a break whether it suited them or not. Oliphant (1972) recalls an occasion when Rutherford was so impatient to see a photographic oscillograph record that he ruined it and stained his clothes by awkward handling before the film had been properly processed; so too was Kapitza famous for spoiling films and wetting his clothes by undue impatience. Appalling handwriting was another common feature, though this was perhaps coincidental. Again like Rutherford, he made a point of keeping in touch with what everyone was doing, and made shrewd comments even if the topic was remote from his personal research. Often he would achieve this contact through a rule he made that any new apparatus had to be approved by him before it was made by the workshop and this gave him the opportunity of reviewing progress from time to time. Also it was usual for any completed work to be presented at his seminar before it could be sent for publication.

Kapitza's fondness for nicknames such as 'Crocodile' and 'Baron' has already been mentioned—another was 'Rat' for Anna because of her burrowing in archaeological archives (his calling out 'Rat' to her at the theatre caused some alarm to neighbouring ladies on one occasion!). It was perhaps poetic justice that he and his Institute should also be widely known by nicknames. The Institute and the seminar were usually called the Kapichnik, which sounds a bit comic in Russian, though otherwise harmless, but his own nickname was less flattering. It came about when a visitor asked someone in the Institute 'What sort of a chap is your boss? Is he a man or a beast?' The answer was a bit hesitant: 'It's difficult to say—a

bit of each perhaps' and the visitor immediately said 'Oh, I see, a centaur' and 'centaur' stuck for many years, though later it was gradually replaced by the more affectionate 'grandfather'.

There was indeed some truth behind the 'centaur' characterization. He could on occasion fly into a rage and be very abusive (once again like Rutherford), he could be autocratic in dealing with subordinates, and if, unreasonably, he took against someone, it was difficult to make him change his mind. On the other side of the coin, however, he could be extremely generous in helping where help was needed—indeed not only generous, but courageous. His intervention to save Landau is only one example. Other examples were his public denunciation of anti-semitism (G18), his speaking out for the dissident geneticist, Zh. Medvedev, who had been put in a psychiatric institution, his sponsoring talks on genetics at his seminar by opponents of Lysenko, at a time when Lysenko was still in favour, and opposing a move to expel Sakharov from the Academy by the reminder that even the Nazis had never expelled Einstein from the Prussian Academy. Moreover, everyone in the Institute greatly appreciated the remarkable working conditions he had created, probably unique in the Soviet Union, and the protection they enjoyed there in various ways. And of course, above all, when he was in the mood, it was a delight to experience his sense of fun and warm cordiality, which he would extend to everyone irrespective of rank.

During the last half of his life, although Kapitza was very actively pursuing his laboratory work he managed to find time for many other activities. He thought deeply about the organization of science and its relation to technology and in various speeches and articles he was often sharply critical of the establishment. An example is his report (G28) on the organization of his own Institute, where he emphasizes that pure scientists should not be made to develop the technical applications of their work:

> '. . . if Semenov [his close friend] were to attempt to build an internal combustion engine . . . the result would only be a waste of precious time and energy best spent in pure science, where he is nothing less than a virtuoso. . . . If the singer cannot possibly accompany his own songs, why force him to do it? . . .'

However, Kapitza points out that he himself is an exception to the rule, being both scientist and engineer!

Closely related was Kapitza's active interest in education (G59, G64, G66, G80). He took a leading part in organizing a new and somewhat elitist kind of university institution, the Moscow Physico-Technical Institute (M.F.T.I.) where students came into active contact with research during their last undergraduate years. This enabled the research institute to get to know the students well and so to be able to pick the best of them for further research in a rational way. The M.F.T.I. was set up in

1946 just before Kapitza's demotion and he lectured there for a year or two afterwards; after his reinstatement he continued to be involved in the organization of M.F.T.I. but did not give many lectures himself. He took an active part in examining, and devised (partly during his time at Nikolina Gora) many problems of a kind requiring intuition and understanding rather than mere book learning (B5). He was concerned with maintaining a high standard of scientific publication and from 1956 was Editor-in-chief of the *Journal of Experimental and Theoretical Physics*. He also took an active interest in global problems such as ecology and energy (G69, G76–G79) and participated in the Pugwash movement.

He was much in demand for keynote addresses at conferences and could on occasion make somewhat sharp remarks, such as in commenting on what he thought was rather an inadequate level of presentation at a low-temperature meeting: 'It is important not only that the dish should be nutritious, but that it should be well served'. He could always rise to the occasion and say something original when he had to respond after a ceremonial award or give an address commemorating some great figure in science. In such addresses and articles (B8) he showed a talent for picking out what it was that characterized the life and work of his subjects and made them great. Particularly interesting are his recollections of Rutherford (G10, G55). In 1971 he organized a Rutherford Centenary Colloquium in Moscow (B7) to which he invited the few British physicists still alive with personal memories of the great man. One of these, T. E. Allibone, in a letter to *The Times* (3 November 1971) complained that he had been unable to persuade the British Post Office to issue a Rutherford commemorative stamp, and said that he had asked Kapitza how he managed to persuade the Soviet postal authorities to issue one. Kapitza pointed to a second telephone on his desk and said it was connected

The Rutherford stamp.

directly to the Kremlin. He had picked it up and said 'Mr Brezhnev, I want a stamp to commemorate the centenary of the birth of Lord Rutherford' and Brezhnev had said 'O.K.'. Allibone regretted that he had no second telephone on his own desk!

For many years Kapitza was denied the possibility of travelling beyond the Eastern Bloc countries, even though he was frequently invited. However, in 1965 the ice was broken when he was allowed to travel to Copenhagen to receive the Niels Bohr Gold Medal of the Danish Engineering Society. A year later he was awarded the Rutherford Medal of the Institute of Physics and returned to England after a lapse of 32 years. He made Cambridge his headquarters and was delighted to find so many of his old friends still there, most of them now establishment figures. He particularly appreciated the hospitality of Churchill College, of which his old friend Cockcroft was then Master, and while in Cambridge he arranged for his former house at 173 Huntingdon Road to be used by the College as a hostel (now known as Kapitza House), particularly for Soviet academic visitors. In subsequent years he was able to indulge his love of travel to the full and visited Canada, U.S.A., India, Switzerland and many other countries to receive honorary degrees, honorary membership of academies and medals (for a full list see Kedrov 1984). He came back to England on two more occasions: in 1973 to receive the Simon Memorial Prize of the Institute of Physics and in 1976 to give the Bernal Lecture (G78) at the Royal Society. On the second occasion, although saddened to find so many of his old friends no longer alive, he enjoyed the opportunity of making new friends during his stay at Churchill College, which had made him an Honorary Fellow in 1974.

The culminating event was the award of the Nobel Prize in 1978, for his work in low-temperature physics. In his Nobel lecture (83), Kapitza rather wittily made it clear that he thought the award was a bit belated:

> '. . . I left this field some 30 years ago, although at the Institute under my direction low temperature research is still being done. Personally I am now studying plasma phenomena at those very high temperatures that are necessary for a thermonuclear reactor. . . . I think that as a subject for the lecture it is of more interest than my past low temperature work. For it is said "les éxtrèmes se touchent" '.

He then went on to review his plasma work. The visit to Stockholm has been amusingly described by Rubinin (1984).

The Kapitzas also led an active social life in Moscow, enjoying the company not only of their family (five grandchildren and seven great-grandchildren as well as the two sons and numerous nephews, nieces and cousins) but of a wide circle of friends among the 'intelligentsia': artists, sculptors, writers, musicians, actors, film directors and so on. On his 80th

birthday almost everyone in the Soviet cultural world was at the huge party given at Nikolina Gora. Kapitza supported many non-conformist artists by arranging exhibitions of their work at the Institute, particularly during the 1960s. These exhibitions were the talk of Moscow and helped the artists sell some of their work. Kapitza himself bought a few, though his own collection was mostly of Kustodiev, Fonvisin and Falk, whose paintings had been frowned on in the Stalin era but had, by the 1960s, partly returned to favour. Kustodiev's fine portrait (see p. 337) of Kapitza as a young man is at present on loan to Darwin College, Cambridge from the Fitzwilliam Museum. The Kapitzas' social life extended also to visitors from abroad, particularly those with Cambridge connections. Many such visitors recall the warm hospitality they received in Moscow or Nikolina Gora and Kapitza's lively conversation.

It is astonishing that he managed to combine such a busy social and intellectual life outside the laboratory with his intensive scientific work. This intense activity continued almost to the end of his life, though latterly he was becoming rather frail. At the end of March 1984 he had a severe stroke and died on 8 April after a few days in hospital. The announcement of his death in *Pravda* was signed by all the Politbureau as well as by the top scientific establishment, and many tributes have appeared in Soviet journals (particularly in *Priroda* of June 1984) as well as abroad. Great preparations had been in hand for his 90th birthday, but sadly it was instead a memorial meeting at the Institute, a moving occasion with many personal recollections by close colleagues and friends.

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One of the photographs in the frontispiece was taken in 1937, the other was taken by the author in 1966 at Cambridge.

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(B2) 1964 *Collected papers of P. L. Kapitza* (ed. D. ter Haar), vol. 1, 1964; vol. 2, 1965; vol. 3, 1967; vol. 4, 1985. Oxford: Pergamon Press.

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Patents‡

(P1)	1925	226857	(With H. F. Heath) Improvements in electric storage apparatus.
(P2)		226858	(With H. F. Heath) Improvements relating to the production of high tension discharges.
(P3)		227149	Improvements relating to flash illumination.
(P4)	1926	254349	(With M. Kostenko) Electric impulse generator.
(P5)		259272	Improvements in electric current breakers.
(P6)	1929	308329	(With G. V. Lomonossoff) Improvements in brakes.
(P7)		310093	(With G. V. Lomonossoff) Improvements relating to brakes for vehicles.
(P8)		312259	(With G. V. Lomonossoff) Improvements in electromagnets.
(P9)		318474	(With G. V. Lomonossoff) Improvements relating to clutches and variable-speed gearing for power transmission apparatus.
(P10)	1935	433860	Improvements relating to the production of low temperatures.
(P11)	1939	511337	Coupling means including a stabilizing device for securing high speed rotors to shafts.
(P12)	1940	520762	Improved device for stabilizing high speed turbine and like rotors.
(P13)	1941	540555	Improvements in expansion turbines for low temperature plants.
(P14)	1948	610572	Twin expansion engine for production of low temperatures.
(P15)		614006	A method and means of producing liquid oxygen or liquid air rich in oxygen.
(P16)	1949	615464	Means for damping transverse oscillations at high shaft speeds.
(P17)		622469	Means for rectification.
(P18)		625107	Method and means for rectification and distillation of liquids with low temperature boiling points.
(P19)	1971	1256686	Improvements in and relating to thermo-nuclear plasma producing arrangements.

* The books B3, B4, B6, B8 and B9 are mostly different selections of the popular, semi-popular and miscellaneous general articles listed above; the most extensive of these is B8.

† Subsequent volumes of *HPE* (**2**, 1963; **3**, 1964; **4**, 1965; **5**, 1968, **6**, 1969) were edited by P. L. Kapitza and L. A. Vainshtein. Only volumes 1 and 2 were translated into English (*HPME*).

‡ Only the British patents are listed (the year is that of final acceptance, often several years later than the date of application). Most of the inventions are also patented in the U.S.A. and some in France, Germany and the U.S.S.R. No foreign patents have been found for inventions other than those in the British list.

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* Those marked † are full length biographies, though Biew (1956) is almost pure science fiction. The other books and articles listed contain material, not always reliable, and sometimes no more than a mention, about Kapitza, but the list is only a selection.

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