ISSN 0032—874X

ПРИРОДА

10 92

Best of Nature

ПРИРОДА

ЕЖЕМЕСЯЧНЫЙ
ПОПУЛЯРНЫЙ
ЕСТЕСТВЕННО-НАУЧНЫЙ ЖУРНАЛ
РОССИЙСКОЙ АКАДЕМИИ НАУК

Издается с января 1912 года.

Адрес редакции:
117810 Москва, ГСП-1,
Мароновский пер., 26
Тел. 238-24-56

Подписано в печать 23.09.92
Формат 70x100 1/16
Бумага офсетная № 1
Офсетная печать
Усл. печ. л. 10,32
Усл. кр.-отт. 603,5 тыс.
Уч.-изд. л. 15,0
Тираж 22 541 экз.
Зак. 1059
Цена 1 р. 80 к.

Ордена Трудового Красного Знамени
Чеховский полиграфический комбинат
Министерства печати и информации
Российской Федерации
142300, г. Чехов
Московской области

№10 (926) ОКТЯБРЬ 1992
специальный выпуск

В НОМЕРЕ:

BEST OF NATURE

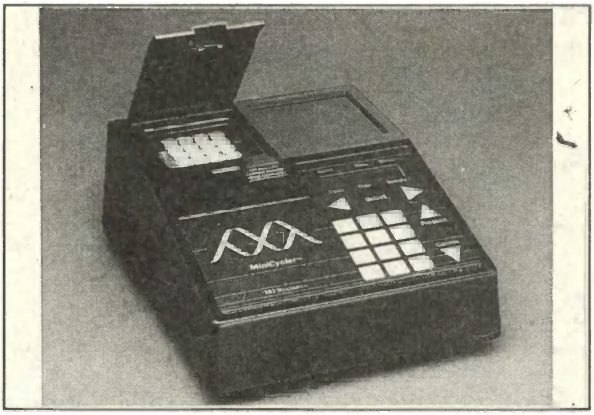
XVI. СОДЕРЖАНИЕ

РАННИЕ ГОДЫ

ФИЗИЧЕСКИЕ НАУКИ

FROM THE UNITED STATES OF AMERICA:

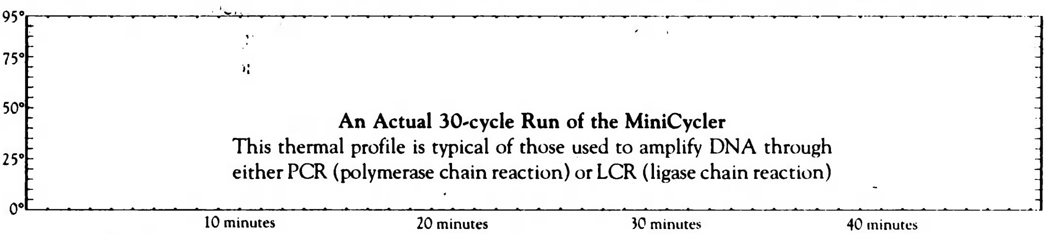
Регулятор температуры для амплификации ДНК с использованием полимеразной или лигазной цепных реакций



The MiniCycler™

A Personal Thermal Cycler that's Fast, Portable & Peltier-effect, for Automated DNA Amplification using PCR or LCR.

- **Third-generation thermoelectric (Peltier-effect) technology,** which actively *pumps* heat in and out of the sample block, for precise temperature control from -9° to 105° C.
- **Extremely fast operation,** with thermal cycling speeds that average 1.8°/sec in the 50 - 100° C range, with a maximum speed of 2.4°/sec (cycling speed is slower at below-ambient temperatures).
- **Near-isothermal block,** with a thermal homogeneity well-to-well of ±0.4° within 12 seconds, ±0.3° within 40 seconds of arrival at 92° C (homogeneity improves as incubation temperature nears ambient).
- **All solid-state,** with NO bulky refrigeration compressors and NO cumbersome coolant connections.
- **Easy-to-learn computer software,** which can hold up to 100 thermal cycling programs in memory.
- **Interchangeable sample block/heat pump assembly,** which requires no calibration. Two sample blocks are available: one holds sixteen 0.5ml microcentrifuge tubes, the other holds twenty-five 0.2ml tubes. Instrument comes complete with one sample block; the other block may be purchased separately.
- **Compact, lightweight & portable** – unit is only 22cm x 28cm x 11cm high and weighs just 3 kilograms.
- **Universal-input power circuitry,** which allows operation on any voltage between 100 - 250 Volts AC or DC. Instrument uses about 150 watts and it is relatively tolerant of power that varies in quality.



An Actual 30-cycle Run of the MiniCycler
This thermal profile is typical of those used to amplify DNA through either PCR (polymerase chain reaction) or LCR (ligase chain reaction)

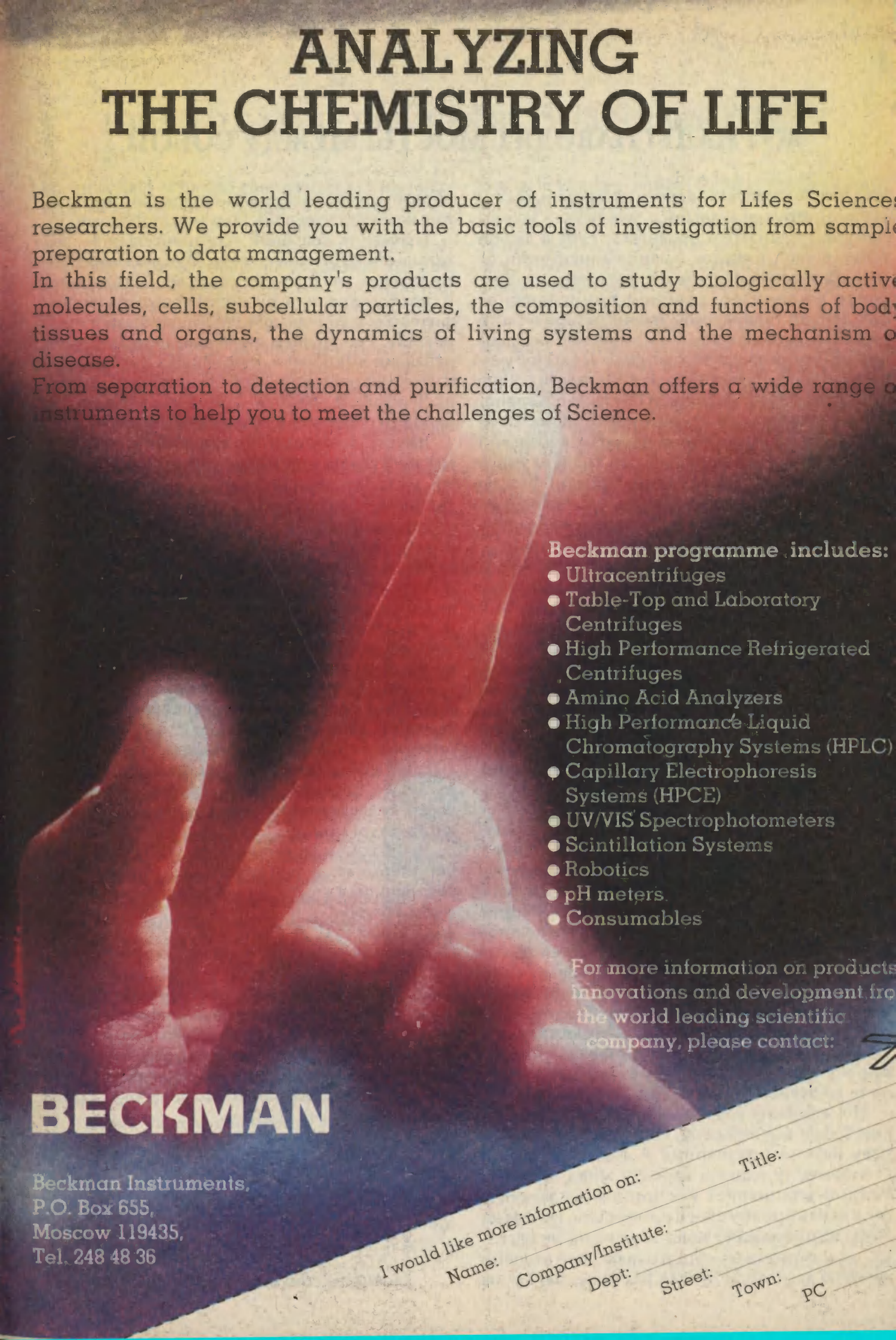
To obtain more information or a formal price quote, please contact one of the following authorized distributors:

Finland & CIS: Finnzymes Oy, Riihitontuntie 14 B, SF-02200 Espoo, Finland Tel: 358 0 420 8077 Fax 358 0 420 8653

Eastern Europe: Inovex Handelsges.m.b.H., Ameisgasse 49-51, A-1140 Wien, Austria Tel: 0222/94 85 09 Fax 0222/94 85 07 30

Instrument can also be purchased direct from manufacturer for approximately $2950 U.S. (prepaid), delivered to purchaser's customs broker at Moscow Airport. For more information, please fax or write in English:

MJ RESEARCH, INC. 24 Bridge Street • Watertown, MA 02172 U.S.A.
Tel: (617) 924-2266 Fax: (617) 924-2148


ANALYZING
THE CHEMISTRY OF LIFE
Beckman is the world leading producer of instruments for Lifes Science
researchers. We provide you with the basic tools of investigation from sampl
preparation to data management.
In this field, the company's products are used to study biologically activ
molecules, cells, subcellular particles, the composition and functions of bod
tissues and organs, the dynamics of living systems and the mechanism o
disease.
From separation to detection and purification, Beckman offers a wide range o
instruments to help you to meet the challenges of Science.
Beckman programme includes:
Ultracentrifuges
Table-Top and Laboratory Centrifuges
High Performance Refrigerated Centrifuges
Amino Acid Analyzers
High Performance Liquid Chromatography Systems (HPLC)
Capillary Electrophoresis Systems (HPCE)
UV/VIS Spectrophotometers
Scintillation Systems
Robotics
pH meters
Consumables
For more information on products innovations and development fro the world leading scientific company, please contact:
BECKMAN
Beckman Instruments,
P.O. Box 655,
Moscow 119435,
Tel. 248 48 36
I would like more information on:
Title:
Name:
Company/Institute:
Dept:
Street:
Town:
PC

ЖУРНАЛЫ НАВОДЯТ МОСТЫ МЕЖДУ СОБОЙ

Нет иного вида человеческой деятельности, столь интернационального, как наука. Законы природы одинаково проявляют себя в любой точке нашей планеты — вне зависимости от географической широты или долготы и уж тем более от государственной принадлежности данной территории, уровня исторического развития населяющего ее народа или выбранной им политической системы. Поэтому ученые — люди, посвятившие себя постижению этих законов, естественным образом объединены в главном — в своем профессиональном подходе к постижению мира. «Национальной науки нет, как нет национальной таблицы умножения», — писал А. П. Чехов, который был не только писателем, но и врачом, т. е. человеком научного мировоззрения. Оставаясь на позициях разума, с ним невозможно не согласиться.

Разумеется, в известной степени интернациональны и искусство, и экономика, и политика, и все иные сферы людских интересов, труда и взаимодействия. Но если возможно представить себе художника, творящего новое искусство на необитаемом острове, или композитора, сочиняющего сегодня одному ему понятные произведения в «башне из слоновой кости», которые завтра завоюют весь мир, то ученый в отрыве от своих коллег в других странах обречен на безнадежное повторение сделанного ранее другими.

Мне уже случалось писать о том, что для ученого лучший способ оправдать доверие общества — это предоставить результаты своей работы бесплатно в его распоряжение. Делается это с помощью журналов — именно там публикуются научные открытия, становясь общим достоянием, которым любой вправе воспользоваться, в отличие, скажем, от изобретений, патентуемых их творцами, или произведений искусства и литературы, охраняемых авторским правом. Журналы, таким образом, служат делу единения ученых, позволяя им узнавать о сделанном коллегами в своей области, и в этом смысле наука без них невозможна — во всяком случае, наука нынешняя.

Но журналы служат и иной, быть может, еще более важной цели. Они — в первую очередь научно-популярные — позволяют восстановить ту единую картину мира, которую каждый специалист воспринимает мозаично, сквозь призму своей профессии. На самом же деле природа не знает ни химии, ни физики, ни математики — это мы в высшей степени условно разделили пути ее постижения. Таким образом, журналы — это связующие нити не только между учеными, но и между науками.

Примеров подобной «соединительной» деятельности журналов немало. Однако между ними самими, как правило, связи весьма слабые — в силу конкуренции, разности подходов, географической и социальной разобщенности и иных причин. Тем ценнее опыт совместной работы редакции «Природы», старейшего российского научно-популярного журнала широкого профиля, и журнала «Nature» — видимо, вообще старейшего общенаучного журнала в мире.

Вместе с нашими британскими коллегами мы выступали и инженерами, и архитекторами, и строителями этого межжурнального моста. Вначале для него были забиты «быки» — в шести номерах «Природы» подряд публиковалось по нескольку страниц, взятых из последних номеров «Nature». И вот теперь — сразу несколько «пролетов»: подборка BEST OF NATURE. Я не собираюсь ее комментировать — читатели «Природы» сами, даже без специально подготовленных нами аннотаций, сумеют оценить и временной охват, и разнообразие тематики, и значимость публикуемых материалов. Здесь у нас явное преимущество: если британские коллеги когда-нибудь надумают опубликовать у себя подборку ЛУЧШЕЕ ИЗ «ПРИРОДЫ», им придется тратить место и время на перевод, наш же читатель в массе своей владеет современной латынью науки — английским языком.

Но об одном не могу не сказать. Приятно видеть старых авторов «Природы», вернувшихся на ее страницы в новом, но все равно легко узнаваемом обличье — Б. М. Вула, Я. Б. Зельдовича, А. Ф. Иоффе, П. Л. Капицу, Л. Д. Ландау, А. Д. Сахарова и других. Естественно, ни один серьезный журнал мира не может обойтись без этих имен. Жаль лишь, что почти все эти авторы уже никогда не напишут нам своих новых статей...

А теперь позвольте представить вам, дорогие читатели, подборку материалов дружественного нам журнала, которую открывает обращение (в переводе на русский) к вам его редактора Джона Мэддокса.

Людвиг Фаддеев,
главный редактор журнала «Природа»

JOURNALS MAKE BRIDGES BETWEEN EACH OTHER

No other field of human endeavour is as truly international as Science. The laws of Nature, blithely indifferent by and large to matters of latitude and longitude, could care even less about national borders, the extent of population's historical maturity, or the political system that such a population may have chosen, or had thrust upon it. The bond, therefore, between scientists, whose life's work it is responsibly to lay bare and illuminate these laws, is a naturally international one. «There is no national science as there is no national multiplication table» wrote Anton P. Chekhov, a physician as well as an author — and common sense makes it hard to disagree.

Of course, there is no sphere of human interest or interaction — art, economics, politics, etc. — which is not, to an extent, international. But while artists and composers can operate perfectly well from their lonely garrets and ivory conservatories — indeed can launch something truly New upon the world — a scientist isolated from his peers is doomed forever to repeat the discoveries and advances of others.

I have written before that the best way for a scientist to serve society is to submit the fruits of his labour to that society free of charge. This is what scientific journals are all about. Upon publication, a scientific discovery becomes public property; anyone who wishes may use it or build upon it as they see fit, unlike a patented invention, or a work of literature protected by copyright laws. In this way, the journals further the cause of the writing scientist, familiarizing him with developments in his field, and the work of his colleagues. Without this service, Science as we know it today simply could not exist.

The journals have an even more important goal, however. They aim to assemble necessarily specialized glimpses of scientific truth into a panoramic mosaic of the greater scientific world. Nature herself is ignorant of chemistry, physics, and mathematics: these distinctions are ours, deliberate constructions of the human mind. It is the purpose of scientific journals, then, to bridge the gaps not only between individual scientists but between the often remote fields of enquiry that scientists have created for themselves.

Despite the scientific journal's primarily «connective» function, though, the connections between journals themselves tend to be rather weak, sundered as they are by professional rivalry as well as by geographical, and often intellectual, differences. This only makes the collaboration more rewarding between PRIRODA — the oldest popular science journal in Russia — and NATURE — the oldest journal of this kind in the world.

Along with our British colleagues, we have acted as the engineers, as well as the architects, of this «inter-journal» bridge. We started by sinking a few piles — up-to-date extracts from NATURE published in six successive issues of PRIRODA — and now — with this BEST OF NATURE collection we are ready to drop several spans into place at once. The collection needs little introduction from me. Even without our specially prepared annotations, PRIRODA readers would have little trouble appreciating this collection's historical range, the variety and significance of its material. In this we have a clear advantage over our British colleagues. If they, someday, decide to publish a BEST OF PRIRODA collection, they will have to devote much time, and many pages, to the business of translation. Most of our readers, on the other hand, have a good command of English — Latin's successor as the language of modern Science.

I must just mention, though, what a joy it is to see many familiar names on the pages of PRIRODA, somewhat altered in appearance, but ultimately unmistakeable: Joffe, Kapitsa, Landau, Sakharov, Wul, Zeldovich etc.; no journal could do without them. The only pity is that they won't be writing us any new articles...

And now, dear readers, without further ado, may I introduce this collection of materials from a kindred journal, prefaced (in translation) by a few words from its editor: John Maddox.

Ludwig FADDEEV,
Editor-in-chief, «Priroda» journal

HANDS ON INNOVATION

Introducing Stratagene's 1992 Catalog

Developing Innovative Products to Make Your Job Easier

Stratagene Cloning Systems, the Molecular Biology Company that has always been identified with innovation, continues this tradition in 1992. Our newest catalog features our complete range of products, kits, instrumentation, and services, including:

- Pyrococcus Furiosus (Pfu) DNA Polymerase
- Srf I Restriction Enzyme - New 8-base Cutter
- Unique Laboratory Instrumentation
- Custom Services
- Innovative New Products for Cell Biology and More!

Call 1-800-424-5444 for more information on our complete line of products.

STRATAGENE

Contact Stratagene for the distributor nearest you.

Corporate Headquarters, USA
1-800-424-5444
Telefax: 619-535-5430

Germany, Stratagene GmbH
(06221) 40 06 34
Telefax: (06221) 40 06 39

United Kingdom, Stratagene Ltd.
(0223) 42 09 55
Telefax: (0223) 42 02 34

Stratagene France
(0590) 72 36
Telefax: (1) 44 28 19 00

Reader Service No. 539

ЖУРНАЛЫ НАВОДЯТ МОСТЫ В НАУКЕ

Без науки не было бы научных журналов, но верно ли обратное утверждение? Возможна ли наука без научных журналов? Очевидно, журналы не есть нечто обязательное. Во времена зарождения современной науки их не было, тогда существовали лишь книги. Коперник и Галилей, Ньютон и Декарт публиковали свои новые взгляды на устройство мира в виде развернутых тезисов, написанных на латыни. Эти материалы, переходя из рук в руки, медленно распространялись по Европе и изменяли сознание людей. Итак, наука могла бы существовать без научных журналов, но это была бы совсем другая наука.

Журналы не только ускорили распространение знаний о научных открытиях, но и сделали этот процесс демократичным в самом строгом смысле слова. Журналы открыли доступ к информации о всех достижениях науки для всех. Частично это объясняется почти механически: журнал выходит регулярно, ежемесячно или даже еженедельно, так что читатели знают, когда его ждать. Читатели могут также заявить, что они не получили тот или иной номер или даже выразить несогласие с выводами той или иной опубликованной статьи.

Журнал — это еще и учреждение с почтовым адресом, по которому читатели могут направить свои замечания, а авторы — послать свои статьи. Сейчас все чаще корреспонденции в журнал приходят с той или иной из 2000 компьютерных сетей, опоясавших весь мир, по электронной почте. Но технические детали не меняют принципиального положения: большинство читателей — это потенциальные авторы. Таким образом, журнал — это своего рода клуб, сообщество людей, имеющих общие интересы, которые в качестве читателей получают знания и информацию, а в качестве авторов — возможность распространять собственные идеи. Редакторам журналов приятно думать, будто они решают, что следует публиковать. На самом же деле их журналы принадлежат потребителям — авторам и читателям: редакторы могут напечатать лишь то, что им прислали авторы, а редакторская свобода ограничена тем, что желают прочесть читатели.

Журнал «Nature» начал выходить в свет в 1869 г., почти 125 лет назад и через 10 лет после публикации «Происхождения видов» Чарлза Дарвина. Англия XIX в. лишь начинала осознавать смысл научного утверждения, которое дарвинисты сделали от имени науки. По современным меркам, это утверждение было, конечно, несложным: все живые существа (точнее, виды) способны изменяться, приспосабливаясь к изменяющемуся окружению. Самым сильным аргументом Дарвина в пользу приспособляемости было проведенное им детальное изучение формы клюва многих видов зябликов, живущих на островах восточной части Тихого океана. Он убедил себя и других, что единственным объяснением наблюдаемого может быть лишь эволюция птиц на протяжении многих поколений, которая давала им возможность приспособиться к окружающей среде, в которой они оказывались.

Переход Дарвина от этого наблюдения к заключению о том, что люди тоже являют собой результат приспособительной эволюции и что они, возможно, имеют общего предка с обезьянами, был лишь догадкой, сравнимой по смелости с выводами Галилея, наблюдавшего вращение спутников вокруг Юпитера, что все планеты точно так же вращаются вокруг Солнца. Точка зрения Галилея была признана ересью; неприятие ее католической церковью заставило его почесть за благо отречься от своих взглядов. Противники Дарвина не были столь могущественными, и, к счастью, у него нашлись друзья. Один из них, Томас Генри Гексли, стал его сторонником и убедил молодого британского издателя Макмиллана начать выпуск еженедельного научного журнала.

Мы ничего не знаем о наших читателях тех ранних лет, кроме того, что в большинстве своем это были жители Великобритании. В то время лишь немногие, как, например, Джеймс Клерк Максвелл, преподавали в университетах Лондона и Кембриджа, Глазго и Эдинбурга, и их можно было назвать профессиональными учеными. Помимо того, была немногочисленная группа с профессиональными научными интересами: мелкие промышленники, приходские священники и т.п.

Лишь по случайности журнал «Nature» стал научным в строгом смысле этого слова. В последние десятилетия XIX в. его корреспонденты постоянно стремились привлечь внимание к интересным явлениям природы, которые они наблюдали: падение метеорита или необычное понижение давления, отмеченное ртутным барометром. Но на рубеже веков в журнал уже писали в основном с тем, чтобы изложить свои взгляды на свойства радиоактивности, электрона или же атомного ядра.

С самого начала у журнала «Nature» были две характерные черты, которые отличали его от других изданий того времени. Во-первых, он был последовательно интернациональным: даже самые первые еженедельные выпуски журнала содержали отчеты научных академий других стран, например, из Парижа, Филадельфии и Санкт-Петербурга. Во-вторых, в журнале всегда считали нужным давать краткий комментарий о вещах, которые, строго говоря, не являются научными, но могут повлиять на состояние науки (к примеру, высшее образование) или зависеть от ее развития (от общественного здравоохранения до радиовещания).

Именно этот журнал (с тем же самым издателем) предоставляет свои материалы вниманию читателей в октябрьском номере «Природы» — с любезного согласия ее редакции. Мы надеемся, что для читателей «Природы» последующие страницы послужат полезным путеводителем по важнейшим вехам в истории науки. Эти же страницы могут пролить свет на то, как современная профессия ученого выросла из интереса интеллигентных, широко образованных людей к миру, в котором мы живем.

Выбор статей из «Nature» для данного выпуска, надо признать, в значительной мере случаен. За 125 лет нами опубликован почти миллион страниц текста, более половины которых — в последние полстолетия. У каждого тома есть годовое оглавление, но оно не дает представления об исторической значимости материала, который содержит. Поэтому чтобы найти какую-либо статью прошлых лет, необходимо знать, о чем она, и, хотя бы ориентировочно, дату публикации. В процессе подготовки подборки материалов для публикации в этом номере «Природы» мы пришли к мысли, что нужно создать полное оглавление «Nature» с самого начала. Иначе мы не будем знать собственной истории.

Мы начинаем с декларации о задаче «Nature» которая коротко состоит в том, чтобы способствовать пониманию науки, распространяя знания о новых открытиях, что так же важно сегодня, как и в 1869 г. Отрывок из стихотворения Вильяма Вордсворта на титульном листе не нужно принимать за сентиментальное подтверждение веры в некую пантеистическую силу, которая охранит нас от зла. Напротив, это утверждение, что только рациональное постижение мира обеспечит нашу безопасность и выживание. Большей части викторианской Англии был чужд такой взгляд. Гексли с очевидностью показал это в своей вступительной статье о социальных последствиях дарвинизма (см. с.4).

Эта подборка публикаций «Nature» раннего периода слишком скудна, чтобы полно отразить богатство содержания журнала той поры. Отбирая материал для перепечатки в «Природе», мы в основном стремились проиллюстрировать вклад «Nature» в современное понимание науки. В подборке есть две отдельные линии. Читатели могут быть удивлены, обнаружив, что за все время существования журнала его вклад в физические науки даже теперь больше, чем в науки о живом.

В первые годы XIX в. мы были одним из главных проводников знаний об открытиях в области ядерной физики и квантовой теории, которые теперь стали основными в физике. Эта подборка демонстрирует то значение, которое мы придавали в 1939 г. ранним работам по делению урана — основные принципы получения ядерной энергии были изложены почти исключительно в «Nature». После второй мировой войны мы особенно активно публиковали материалы по наукам о Земле (в частности, по плитотектонике), радиоастрономии (в особенности, об открытии квазаров), а в последнее время — о свойствах высокотемпературных сверхпроводников и недавно открытых фуллеренов.

Но акцент в «Nature» заметно сместился после публикации в 1953 г. небольшой статьи Дж. Уотсона и Ф. Крика о структуре ДНК (с. 40). Интересно, что в 1930-е годы были предприняты многочисленные попытки исследовать структуру белков и других биологических молекул; первым значительным успехом на этом пути было открытие У. Астбьюри структуры кератина — белка, найденного в волосах человека и других млекопитающих (с. 37). С 1953 г. мы опубликовали результаты многих важных исследований в той области, которая сейчас получила название молекулярной биологии. Нет оснований полагать, что этот поток иссякнет в ближайшем будущем.

«Nature» продолжает интересоваться проблемами и более общего характера. Третий раздел подборки состоит в основном из статей о науке в Советском Союзе и в России. Всего несколько лет назад, до прихода «гласности», материалы такого рода обычно не доходили до читателей в вашей стране — их либо вынимали из номеров «Nature», распространявшихся в СССР, либо эти выпуски попадали в спецхраны, недоступные для простого читателя. Таким образом, этот раздел подборки может представлять особый интерес.

Мы надеемся, читатели оценят, что «Nature» подходит к вопросу управления наукой в Советском Союзе и в России точно так же, как относится к данной проблеме в Англии, США, Японии или в других местах. Мы поступаем так, будучи уверены, что сообщество исследователей по самому существу своему интернационально, что его интересы во многом одинаковы во всем мире и что «Nature» со своим богатым опытом интернациональных публикаций имеет право и даже обязанность стоять на страже интересов исследований, проводимых в любой точке мира.

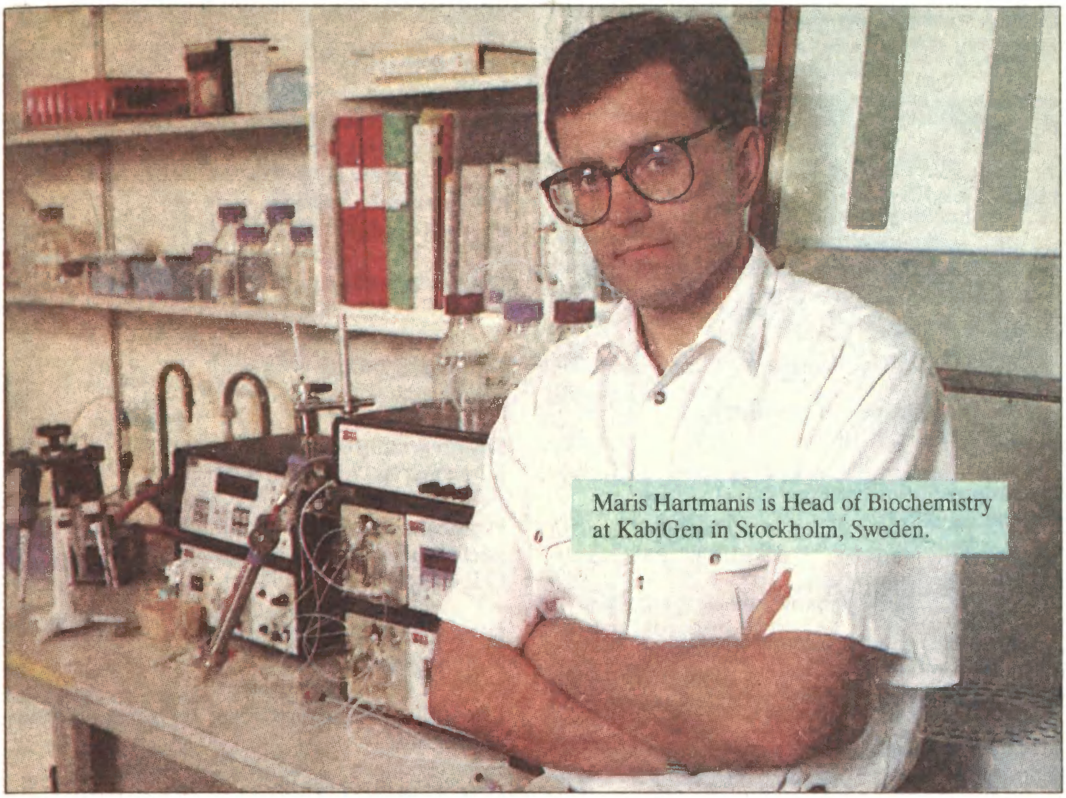
Наши международные связи были одной из наиболее привлекательных сторон нашей деятельности последних десятилетий. Сейчас только около 12% наших читателей живут в Англии, где находится главный офис редакции журнала (другие расположены в Вашингтоне, Токио и Мюнхене). В Северной Америке распространяется около 45% суммарного тиража, в Японии он быстро приближается к 10%. Чтобы упростить распространение «Nature», мы печатаем журнал в США и Японии, а также в Китае. Надеемся, что вскоре это станет возможным и в России.

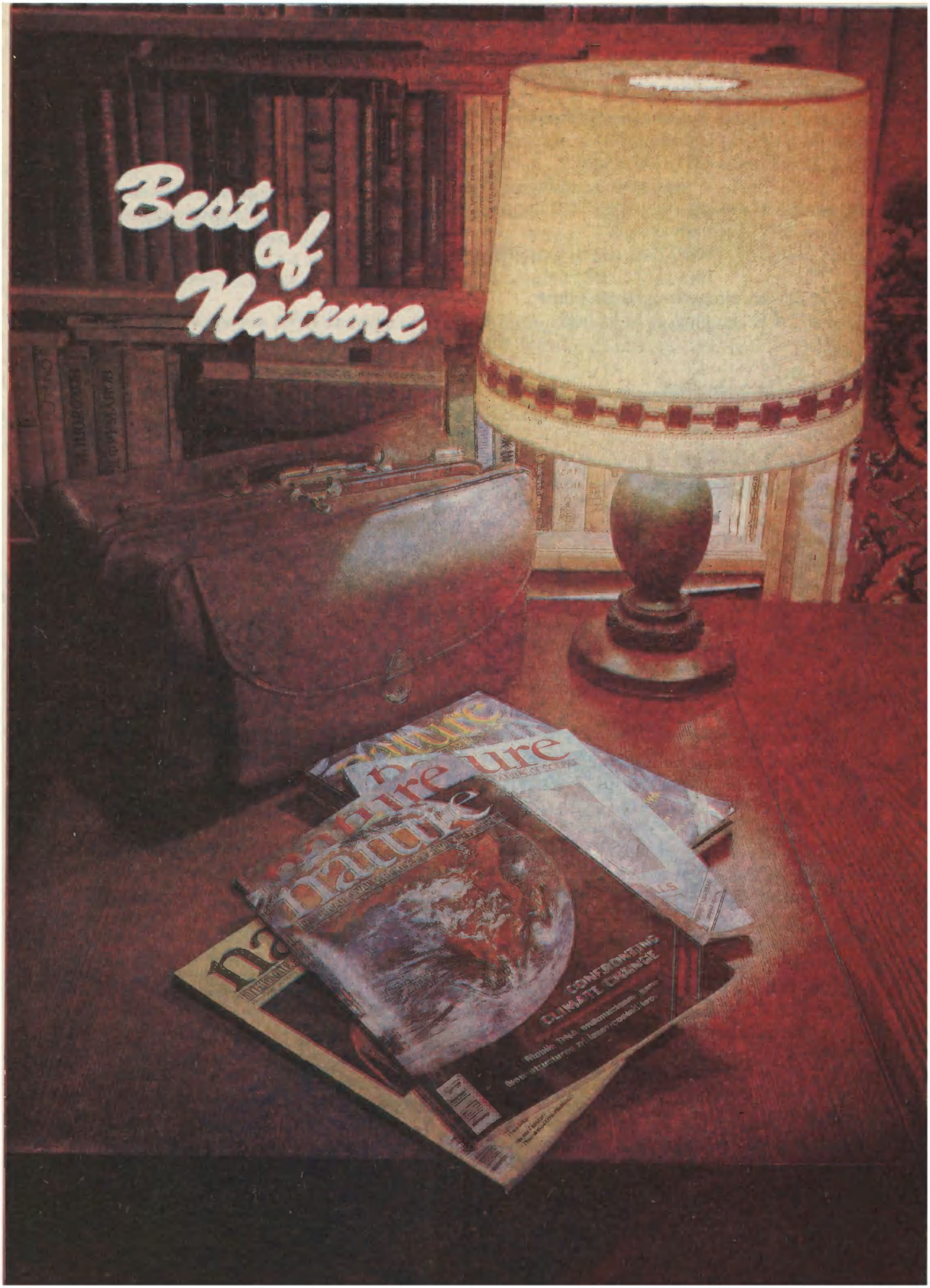
Что бы ни произошло, мы верим, что дружба, установившаяся между «Nature» и «Природой» в последние несколько лет (знак которой — этот специальный выпуск), будет углубляться и процветать. Эти два журнала значительно отличаются друг от друга по публикуемым материалам, но оба они предназначены для профессиональных ученых, которые интересуются достижениями своих коллег во всем мире. Мы рассматриваем эту дружбу как важное средство для дальнейшего упрочения нашего интернационального духа.

Почему это так важно? Потому что наука действительно интернациональна. У нас есть теперь достаточно свидетельств того, как публикация результатов важного исследования стимулирует других людей на новые впечатляющие работы. Мы также остро ощущаем, сколь часто проблемы, возникшие в одной стране, вскоре появляются и в других. И было немало случаев (слишком много — в последние годы), когда работа ученых намеренно осложнялась их собственными правительствами и когда протесты из других стран оказывались полезными.

Таковы мосты, которые наводят в науке журналы. Есть мост и другого сорта, пример которого вы найдете в статьях этой подборки: это мост между настоящим и прошлым. Мы слишком часто забываем, сколь многим наша сегодняшняя наука обязана работам людей, давно умерших; мы слишком мало ценим их. И мы не всегда осознаем, как сильно со временем изменился характер научных публикаций: они стали теперь куда более профессиональными и куда менее понятными. Одна из задач журнала «Nature», важность которой постоянно возрастает, состоит в том, чтобы перебросить мост через эту пропасть непонимания.

Джон МЭДДОКС,
главный редактор журнала «Nature»


Maris Hartmanis is Head of Biochemistry
at KabiGen in Stockholm, Sweden.


Best of Nature
CONFRONTING CLIMATE CHANGE

Every Thursday, in Royal 8vo., price 4d., No. I. published November 4, 1869.

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
Of Nature trusts the mind that builds for aye."*—WORDSWORTH.

THE object which it is proposed to attain by this periodical may be broadly stated as follows. It is intended,

FIRST, to place before the general public the grand results of Scientific Work and Scientific Discovery ; and to urge the claims of Science to a more general recognition in Education and in Daily Life ;

And, SECONDLY, to aid Scientific men themselves, by giving early information of all advances made in any branch of Natural knowledge throughout the world, and by affording them an opportunity of discussing the various Scientific questions which arise from time to time.

To accomplish this twofold object, the following plan will be followed as closely as possible :

Those portions of the Paper more especially devoted to the discussion of matters interesting to the public at large will contain :

I. Articles written by men eminent in Science on subjects connected with the various points of contact of Natural knowledge with practical affairs, the public health, and material progress; and on the advancement of Science, and its educational and civilizing functions.

II. Full accounts, illustrated when necessary, of Scientific Discoveries of general interest.

III. Records of all efforts made for the encouragement of Natural knowledge in our Colleges and Schools, and notices of aids to Science-teaching.

IV. Full Reviews of Scientific Works, especially directed to the exact Scientific ground gone over, and the contributions to knowledge, whether in the shape of new facts, maps, illustrations, tables, and the like, which they may contain.

In those portions of "NATURE" more especially interesting to Scientific men will be given :

V. Abstracts of important Papers communicated to British, American, and Continental Scientific societies and periodicals.

VI. Reports of the Meetings of Scientific bodies at home and abroad.

In addition to the above, there will be columns devoted to Correspondence.

The following eminent Scientific men are among those who have already promised to contribute Articles, or to otherwise aid in a work which it is believed may, if rightly conducted, materially assist the development of Scientific thought and work in this country :—

ABEL, F. A., F.R.S. *H.M. Chemical Department, Woolwich.*
AGASSIZ, PROF. L. *Museum of Comparative Zoology, Harvard College.*
ANDREWS, PROF. T., F.R.S. *Queen's University, Dublin.*
BASTIAN, PROF. H. C., F.R.S. *University College.*
BEALE, PROF. LIONEL S., F.R.S. *King's College.*
BENNETT, A. W., F.L.S.
BERTHELOT, PROF. *Collège de France, Paris.*
BONNEY, REV. T. G. *Cambridge.*
BRADY, H. B., F.L.S.

JOURNALS MAKE BRIDGES IN SCIENCE

Without science, there would be no scientific journals, but is the opposite true? Could there be science without the scientific journals? Evidently, the journals are not indispensible. At the birth of modern science, there were no journals, only books. Copernicus and Galileo, Newton and Decartes, published their new views of what the world is like in the form of substantial written theses, in Latin as it happens. These documents circulated slowly through Europe, changing men's minds as they found their way into one pair of hands and then another. So there could be science without the scientific journals, but it would be a very different kind of science.

What the journals have done for science is not just to speed the communication of discovery, but to make the process democratic in the strict sense. Journals have made it possible for all people to have access to information about all discoveries of importance. Part of the explanation for that is almost mechanical: a journal appears regularly, once a month or even once a week, so that its readers know when to expect it. Readers can also write to say that they have not received this month's or this week's copy, or even to say that they disagree with the conclusions of some article that has been published.

A journal is also an institution, with a postal address, to which readers can complain and contributors can send their articles. Now, and increasingly, the addresses are e-mail designations on one or other of the 2,000 computer networks that span the world, but technology does not change the principle that most readers are also potentially contributors. Thus a journal is a kind of club, a community of people with a common interest who as readers, are instructed and informed and who, as contributors, put their own ideas into circulation. Journals' editors like to think they are in charge, deciding what is published and so on. In reality, their journals are the property of their users, contributors and readers alike: they can publish only what their contributors send them, their freedom is simply that of deciding what their readers wish to read.

That is how NATURE began in 1869, almost 125 years ago and just a decade after the publication of Charles Darwin's «The Origin of Species». Nineteenth century England was still then coming to terms with the claims the Darwinists had made on behalf of science. The claim was simple-minded by modern standards, of course; living things, species to be exact, are all capable of change or of adaptation to a changing environment. Darwin's best evidence that adaptation happens was the careful study he had made of the shapes of the beaks of the many species of finches living on the islands of the eastern Pacific. He convinced himself, and others, that the only explanation must be that the characteristics of the birds had evolved, over the course of many generations, so as to fit them better to the environment in which they found themselves.

Darwin's leap from that observation to the conclusion that people are also the result of adaptive evolution, and that they probably share a common ancestor with the apes, was an act of the imagination comparable in daring to Galileo's inference, from his observation that the moons of Jupiter revolve around that planet, that all the planets also revolve about the Sun. Galileo's opinion was considered heresy; the opposition of the Catholic Church made him think it politic to recant. Darwin's opponents were not nearly as powerful, and luckily there were friends as well. One of those, Thomas Henry Huxley, became Darwin's champion. He was the chief among those who persuaded the then-young British publisher, Macmillan, to bring out a weekly journal of science.

We do not know who our readers were in those early years, except that they were mostly people who lived in Britain. There were then a few people, J. Clerk Maxwell for example, with teaching posts at universities in London and Cambridge, Glasgow and Edinburgh, who might have been called professional scientists. There was also a small army of professional people with an interest in science: small manufacturers, country parsons and the like.

NATURE became a scientific journal in the formal sense only by accident. In the decades of the nineteenth century correspondents were constantly writing to draw attention to interesting phenomena they had witnessed, i.e. the fall of a meteor, or an unusual depression of the mercury in a barometer. But, at the turn of the century, correspondents were writing to remark on the properties of radioactivity, or the discovery of the electron and the atomic nucleus.

From the outset, NATURE had two features that set it apart from other publications of the time. First, it was consistently international in character; even earliest weekly issues carried reports from scientific academies elsewhere, in Paris, Philadelphia and St Petersburg, for example. Second, NATURE has always taken it to be part of its brief to comment on matters which are not strictly scientific, but which may affect the health and wellbeing of science (higher education, for example) or which may be affected by developments in science (from public health to public broadcasting).

That is the same journal (with the same publisher) which the editors of PRIRODA have generously agreed should provide much of the text of this October issue. It is hoped that readers of PRIRODA will find the pages that follow to be a helpful guide to some important landmarks in the history of science. The same pages throw light on how the modern profession of science has grown out of the interest of intelligent broadly educated people in the world in which we live.

The selection of the articles from NATURE that follows is, it must be confessed, haphazard. In 125 years, we have published almost a million pages of text, more than half of them in the past half century. To each volume there is an index, but one necessarily compiled in ignorance of the historical importance of the material it contains. So, to find a particular article from the past, one has to know what it is and, roughly, when it appeared. The experience of making this selection of articles for PRIRODA has persuaded NATURE to make a comprehensive index of the journal from the beginning. Otherwise, we shall not know our own history.

To begin with, this is a declaration of NATURE's objective as a journal, briefly: to advance the cause of scientific understanding by spreading word of new discovery; that is as relevant now as in 1869. The snatch of poetry above the masthead, from William Wordsworth, should not be mistaken for a sentimental affirmation of faith in some pantheistic force that will keep us from harm. Rather, it is a declaration that only a rational understanding of the world can ensure our safety and survival. Much of mid-Victorian Britain was hostile to that view. Huxley makes that clear in his leading article on the social consequences of Darwinism (see page 4).

This selection of writings from NATURE's early years is too skimpy to do justice to the richness of the journal in those days. Instead, the bulk of what is reprinted in this issue of PRIRODA is chosen to illustrate NATURE's contribution to the modern understanding of science. There are two separate strands in this collection. Readers may be surprised to see that, taking all the decades together, NATURE's cumulative contribution to research in the physical sciences may, even now, be greater than that in the biological sciences.

Throughout the early years of this century, we were one of the chief vehicles for the discoveries in nuclear physics and quantum theory that now dominate the physical sciences. This collection of articles also shows the importance attached, in 1939, to the early work on the fission of uranium — the essential principles of nuclear energy had been published, almost exclusively in NATURE. Since the Second World War, we have been especially active in the Earth sciences (and plate tectonics in particular), radioastronomy (with the discovery of quasars, in particular) and, much more recently, the properties of high-temperature superconductors and the newly-discovered fullerenes.

But the emphasis in NATURE was significantly changed by the publication, in 1953, of the brief article by J. D. Watson and F. H. Crick on the structure of DNA (see page 40). But it is interesting that, in the 1930s, there were many attempts to investigate the structure of proteins and other biological molecules were made; W. T. Astbury's structure of keratin, the protein found in human and other mammalian hair, was the first substantial success (see page 37). Since 1953, we have published much of the important research in what is now called molecular biology. There is no prospect that this torrent of research will come to an end.

NATURE continues to deal with more general questions. The third section of reprints in this issue of PRIRODA is more general in character, and consists mostly of articles about science in the Soviet Union and, now, in Russia. Until a few years ago, and the arrival of «glasnost» , material of this kind was routinely kept from Russia readers, either by removing articles from issues of NATURE circulation in the Soviet Union or by keeping the issues concerned in rooms not accessible, to the general readership of libraries. Thus this section of reprinted material may now be of particular interest.

It is hoped that Russian readers of this issue will appreciate that NATURE deals with issues of the management of science in the Soviet Union and in Russia in exactly the same spirit as it deals with the same issues in Britain, the United States, Japan or anywhere else. We do so in the belief that the research community is genuinely international, that its interests are much the same throughout the world and that NATURE, with its long history of operating internationally, has a right and even a duty, to safeguard the interests of research everywhere.

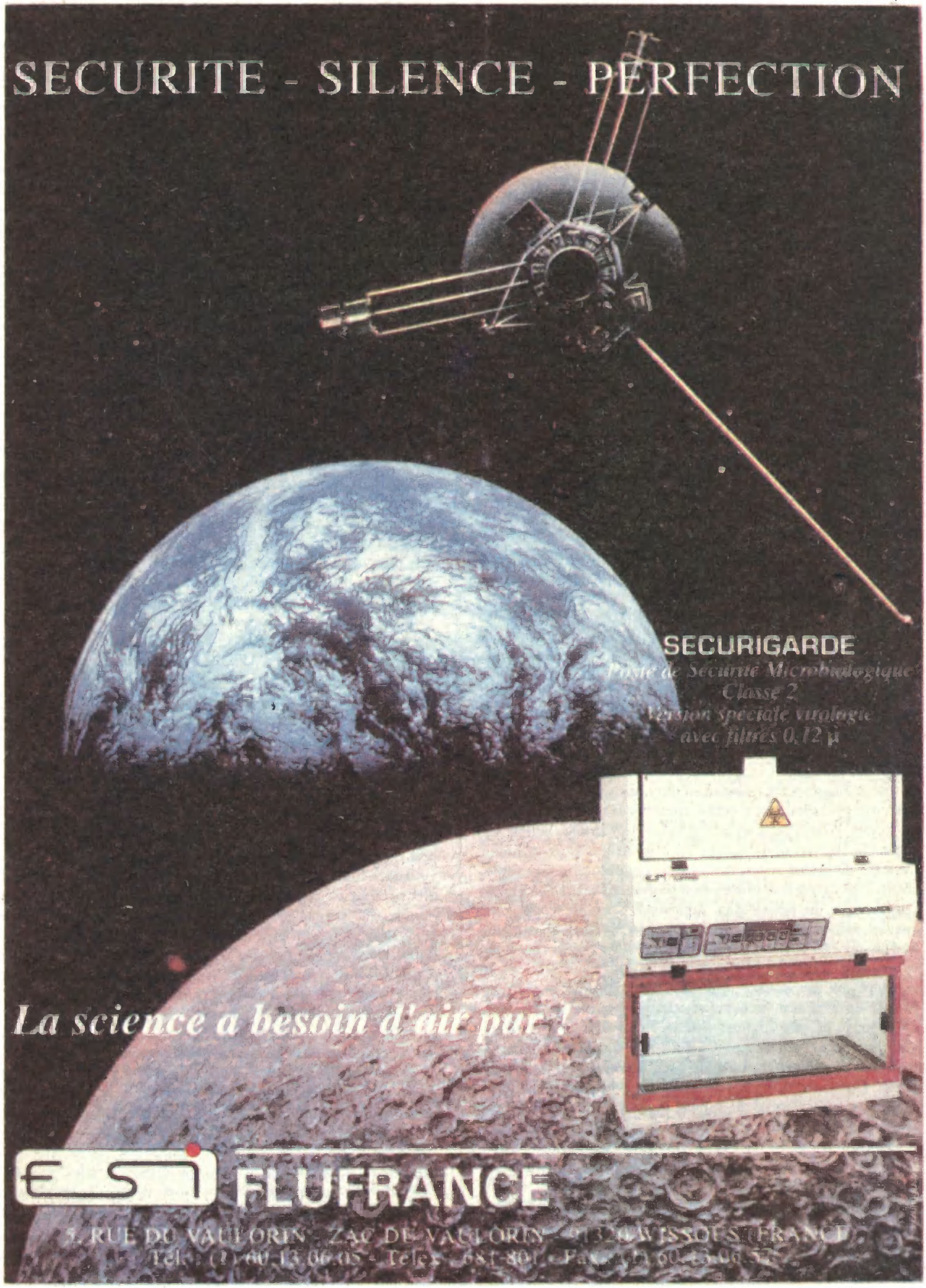
Our development internationally has been one of the most pleasing features of the past few decades. Now, only 12 per cent or so of our readers are in Britain, where our chief editorial office is located. (There are others in Washington, Tokyo and Munich.) Our circulation in North America is roughly 45 per cent of the total; that in Japan is quickly growing towards 10 per cent. To simplify the distribution of NATURE, we print the journal in the United States and Japan as well as in China. It is our hope that we shall soon be able to do the same in Russia somewhere.

Whatever happens, we hope that the friendship established between NATURE and PRIRODA in the past few years (of which this special issue is a sign) will deepen and prosper. The two journals differ considerably in the material which they publish, but each is aimed at professional scientists with an interest in what their colleagues elsewhere have discovered. We regard this friendship as an important means of further strengthening our international character.

And why does that matter? Because science is indeed international. We now have ample experience of how the publication of an important research article can stimulate people other than its authors to new and imaginative research. We are also keenly aware of how often problems arising in one country are soon followed by the same problems elsewhere. And there are many occasions, all too many of them in the past few years, when the work of professional scientists is deliberately impeded by their own governments — and when protests from elsewhere can do much to help.

Those are the bridges that journals can make in science. There is another kind of bridge well illustrated by the articles reprinted in this issue: the bridge between the present and the past. We too often forget how much our present science owes to the work of people long since dead; we honour them too little. And we forget how the character of published science has changed with the passage of time; it is now much more professional, and much less easily understood. One of NATURE's functions, increasingly important, is to help bridge that gap of understanding.

John MADDOX
Editor-in-chief, «Nature» journal


SECURITE - SILENCE - PERFECTION
SECURIGARDE
Classe 2
La science a besoin d'air pur !
FLUFRANCE
5, RUE DU VAULORIN - ZAC DE VAULORIN

DARWINISM AND NATIONAL LIFE

THE Darwinian theory has a practical side of infinite importance, which has not, I think, been sufficiently considered. The process of natural selection among wild animals is of necessity extremely slow. Starting with the assumption (now no longer a mere assumption) that the creature best adapted to its local conditions must prevail over others in the struggle for existence, the final establishment of the superior type is dependent at each step upon three accidents—first, the accident of an individual sort or variety better adapted to the surrounding conditions than the then prevailing type; secondly, the accident that this superior animal escapes destruction before it has had time to transmit its qualities; and, thirdly, the accident that it breeds with another specimen good enough not to neutralise the superior qualities of its mate. In the case of domesticated animals the progress is incomparably more rapid, because it is practicable, first, to modify the conditions of life, so as to encourage the appearance of an improved specimen; next, to cherish and protect it against disaster; and, lastly, to give it a consort not altogether unworthy of the honour of reproducing its qualities. The case of man is intermediate in rapidity of progress to the other two. The development of improved qualities cannot be insured by judicious mating, because as a rule human beings are capricious enough to marry without first laying a case for opinion before Mr. Darwin. Neither would it be easy, nor, perhaps, even allowable, to extend any special protection by law or custom to those who may be physically and intellectually the finest examples of our race. Still, two things may be done: we may vary the circumstance of life by judicious legislation, and still more easily by judicious non-legislation, so as to multiply the conditions favourable to the development of a higher type; and by the same means we may also encourage, or at least abstain from discouraging, the perpetuation of the species by the most exalted individuals for the time being to be found. Parliament, being an assembly about as devoid of any scientific insight as a body of educated men could possibly be, has not as yet consciously legislated with a view to the improvement of the English type of character. Without knowing it, however, the Legislature has sometimes stumbled on the right course, though it has more often blundered into the wrong. Our free trade policy has furnished special scope and special advantages to the energetic enterprising character, and so far has tended to perpetuate and intensify the type which has given to little England her wonderful prominence in the world. On the other hand, the steady refusal to make a career for scientific men has drained away most of our highest intellect from its proper field, and has subjected the rest to an amount of discouragement by no means favourable to increase and improvement. Our laws and customs practically check the growth of the scientific mind as much as they tend to develop the speculative and energetic commercial character.

We do not expect for a long time to hear an orator in the House of Commons commence his speech by announcing, (as a distinguished member of the Austrian Reichsrath recently did, in a debate on the relation of the different nationalities in the empire), that the whole question is whether we are prepared to accept and act upon the Darwinian theory. But even an average English M.P. may be brought to see that it may be possible, indirectly, to influence the character and prosperity of our descendants by present legislation, and none will deny that, if this is practicable, a higher duty could not be cast upon those who guide the destinies of a nation.

A glance at the operation of Darwinism in the past, will best show how potent it may be made in the future. Look at English progress and English character, and consider from this point of view to what we owe it. There were originally some natural conditions favourable to the growth of our commercial and manufacturing energy. We had an extensive coast and numerous harbours. We had also abundance of iron-stone in convenient proximity to workable coal. Other nations either wanted these advantages or were ignorant that they possessed them. These favourable conditions developed in many individuals a special adaptability to commercial pursuits. The type was rapidly reproduced and continually improved until England stood, in the field of commerce, almost alone among the nations of the world. And what is there now to sustain our pre-eminence? Nothing, or next to nothing, except the type of national character, which has been thus produced. Steam, by land and sea, has largely diminished the superiority which we derived from the nature of our coast; and coal and iron are now found and worked in a multitude of countries other than our own. Our strength in commerce, like our weakness in art, now rests almost exclusively on the national character which our history has evolved.

Take another example of the character of a people produced partly by natural conditions of existence, but far more by the artificial conditions to which evil legislation has exposed it. What has made the typical Irishman what he now is? The Darwinian theory supplies the answer. Ireland is mainly an agricultural country, with supplies of mineral wealth altogether inferior to those of England, though by no means contemptible if they were but developed. This is her one natural disadvantage, and it is trifling compared with those which we in our perversity created. For a long period we ruled Ireland on the principles of persecution and bigotry, and left only two great forces at work to form the character of the people. All that there was of meanness and selfishness and falsehood was tempted to servility and apostacy, and flourished and perpetuated itself accordingly. All that there was of nobleness and heroic determination was drawn into a separate circle, where the only qualities that throve and grew were irreconcileable hatred of the oppressor and resolute but not contented endurance. The two types rapidly reproduced themselves, and as long as the external conditions remained unaltered, they absorbed year by year more and more of the people's life; as, if Darwinism is true, they could not but do. And what is the result now? A great part of a century has elapsed since we abandoned the wretched penal laws, and yet none can fail to see in Ireland the two prevailing types of character which our ancestors artificially produced, the only change being that the two types have become, to a certain extent, amalgamated in a cross which reflects the peculiarities of each. Whether future legislation may so far modify the conditions of Irish existence as to work a gradual change in the national character, is a question of much interest, but too large to be discussed just now. In any case we can scarcely expect the results of centuries upon a national type

1, 183; 1869

to be reversed in less than a succession of generations.

Still confining myself to the past, let me point again to the very marked qualities which the conditions of their existence have produced in the people of the United States. They started with a large element of English energy already ingrained into them; they have been reinforced by millions of emigrants presumably of more than the average energy of the various races which have contributed to swell the tide. Added to this, the Americans have enjoyed the natural stimulus of a practically unlimited field for colonisation. Only the resolute, self-reliant settler could hope to prosper in the early days of their national existence; and self-reliance approaching to audacity is the special type of character which on the Darwinian hypothesis we should expect to see developed, transmitted, and increased. How far this accords with actual experience, no one can be at a loss to say. There is probably not a nation in the world whose peculiarities might not be traced with equal ease to the operation of the same universal principle. And the moral of the investigation is this: Whenever a law is sufficiently ascertained to supply a full explanation of all past phenomena falling within its scope, it may be safely used to forecast the future; and if so, then to guide our present action with a view to the interest and well-being of our immediate and remote descendants. Read by the light of Darwinism, our past history ought to solve a multitude of perplexing questions as to the probable supremacy of this or that nation in times to come in the field of commerce, as to the effects of emigration and immigration on the ultimate type likely to be developed in the country that loses and in that which gains the new element of national life, and many another problem of no less interest to ourselves and to humanity.

The subject I have thus slightly indicated seems to me to deserve a closer investigation than it has yet received: and, strange as it will sound to the ears of politicians, I cannot doubt that, in this and other ways, statesmen, if they could open their eyes, might derive abundant aid from the investigations of science, which they almost uniformly neglect and despise. H.

1, 183; 1869

Kant's View of Space

I AM quite willing to leave the readers of NATURE and the students of Kant to decide on the propriety, in English philosophical discourse, of calling Space and Time "forms of Thought," the more so as Sir W. Hamilton—a great stickler for philosophic precision—uses the term in that sense and would have been surprised to hear that he had misrepresented Kant in so doing. My opponents persist in limiting the term Thought to the restricted meaning given to it in Kant's terminology, which, in English, is restricting it to Conception or Judgment: on this ground they might deny that Imagination or Recollection could be properly spoken of as Thought. Throughout I have accepted Thought as equivalent to mental activity in general and the "forms of Thought" as the conditions of such activity. The "forms of Thought" are the forms which the thinking principle (Kant's *pure* Reason) brings with it, antecedent to all experience. The thinking principle acts through three distinct faculties: Sensibility (Intuition), Understanding (Conception), and Reason (Ratiocination): to suppose Thought absent from Intuition, is to reduce Intuition to mere sensuous impression. Therefore, whatever is a form of Intuition must be a form of Thought.

The following passage from Mr. Mahaffy's valuable translation of Kuno Fischer's work on Kant, may here be useful: "Sensibility and understanding are cognitive faculties differing not in degree but in kind, and form the *two original faculties of the human mind*" . . . . The general problem of a Critick of the Reason "is subdivided into two particular objects, as human Reason is into two particular faculties of knowledge. The first object is the investigation of the sensibility; the second, that of the understanding. The first question is, How is rational knowledge possible through sensibility? The second question, How is the same knowledge possible through the understanding?" (pp. 4, 5.)

Those who maintain that it is improper to speak of Space and Time as forms of Thought, must either maintain that Kant held Sensibility *not* to be a faculty of the Mind (thinking principle); or that the term Thought is *not*, in English discourse, a correct expression for the activity of the thinking principle. I believe that the student will agree with me in saying that, although Kant restricted the term Thought to what we call Conception or Judgment, he understood by the activity of the mental faculties (Pure Reason) what we understand by Thought.

It is not, however, to continue this discussion that I again trespass on your space; but to reply to the personal part of Mr. Sylvester's letter. He charges me with misquoting myself and with misquoting him. I said that, in my exposition, Space and Time were uniformly spoken of as forms of Intuition and I say so still. Mr. Sylvester has taken the trouble of reading that exposition without taking the trouble of understanding it; he declares that he "has marked the word intuition as occurring once and forms of sensibility several times; but forms of intuition never." His *carefulness* may be estimated by the fact that the word intuition occurs *four* times on the two pages: his *comprehension* by the fact that it is perfectly indifferent whether Sensibility or Intuition be the term employed, since sensibility is the faculty and Intuition the action of that faculty. Mr. Sylvester, not understanding this, says "If form of sensibility is as good to use as form of intuition, form of understanding ought to be as good as form of thought; but Mr. Lewes owns that the former is indefensible, whilst he avers that the latter is correct." Considering that this passage occurs in a letter which charges me with unfair misquotation, it is curious. So far from owning that the former is "indefensible," it is what I declare to be true; and, with regard to the latter, though I do think a form of Understanding is a form of Thought, my statement was altogether *away* from it, namely, that Space and Time as forms of Sensibility, would be incorrectly spoken of as forms of the Understanding.

With regard to the alleged misquotation of his own words, which he characterises as unfair and as "too much like fighting with poisoned weapons," it was a charge which both astonished and pained me. There are few things for which I have a bitterer contempt than taking such unfair advantages of an adversary. I beg to apologise to Professor Sylvester for any misrepresentation which, unintentionally, I may have been guilty of. But, in accepting his denial of the construction I placed upon his language, I must still say that, after re-reading his letter, I am at a loss to see what other construction it admits of, that has any bearing on the dispute, and that he has not expressed his meaning with sufficient clearness. Intuition and Thought are there compared with Force and Energy as terms "not convertible"; Force is detached from Energy as potential from actual and Intuition without Thought, is made to hold an analogous position. Here is the passage; let the reader judge:—

"Can Mr. Lewes point to any passage in Kant where Space and Time are designated *forms of Thought?* I shall indeed be surprised if he can do so—as much surprised as if Mr. Todhunter or Mr. Routh in their Mechanical Treatises were to treat *energy* and *force* as convertible terms. To such a misuse of the word energy it would be little to the point to urge that *force without energy is a mere potential tendency.* It is just as little to the point, in the matter at issue, for Mr. Lewes to inform the readers of NATURE that *intuition without thought is mere sensuous impression.*"

Is it to use "poisoned weapons" to interpret this as assuming that Intuition and Thought differ as potential and actual? I repeat that, since Mr. Sylvester disclaims the interpretation, my only course is to apologise for it; but, after his own misinterpretations of me, he will not, I hope, persist in attributing mine to a desire to take an unfair advantage. If I make no reply to the other points roused in the various letters it is in order not to prolong the discussion.

GEORGE HENRY LEWES

1, 386; 1870


Schleicher & Schuell
FILTRATION-SEPARATION TECHNOLOGY
Membrane Filters and Filtration Devices
Filter Papers and Filter Thimbles
PROTEIN G
PROTEIN A/G
PROTEIN A
Products for Molecular Biology and Antibody Purification
Schleicher & Schuell
We are looking for distributors of our products.
For details please contact our Export Manager.
Schleicher&Schuell GmbH · P.O.Box 4 · D-3354 Dassel · Fed. Rep. of Germany
Tel. 0 55 61-7 91-0 · Teletex sshahn 55 641 · Telefax 0 55 61-79 15 36

MENDELÉEFF'S PRINCIPLES OF CHEMISTRY.

The Principles of Chemistry. By D. Mendeléeff. Translated from the Russian (Fifth Edition) by George Kamensky, and edited by A. J. Greenaway. Two Vols. (London: Longmans, Green, and Co., 1891.)

ALL English-speaking chemists will cordially welcome the appearance of this book, if for no other reason than because its author in its preparation was led to the recognition of that fundamental principle of chemistry with which his name will always be associated—the principle which is embodied in what is now known as the periodic law. This fact alone would serve to stamp the book as one of the classics of chemical science. But, even apart from this circumstance, the work has very remarkable, and, indeed, exceptional, merits. Probably no scientific treatise ever more strikingly reflected the personality of its author. We have absolutely nothing like it in our language. In grasp of principles, in philosophic breadth, in copiousness of detail, in richness of speculation and suggestion, it is altogether unique among chemical manuals. Every true and earnest student of chemistry is certain to be profoundly influenced by it, even if he cannot always bring himself to subscribe to its doctrine. Of course, the facts are, for the most part, those which are common to all the larger treatises on systematic chemistry, but these are set out and marshalled in a manner wholly original. The intent and purpose of the book is to demonstrate the broad general principles on which chemistry as a science rests. This, it may be urged, is the intent and purpose of every chemical treatise. It may be so, but in many cases the philosophy is lost sight of—obscured, indeed, by the facts, just as the houses may obscure the view of the village.

In Mendeléeff's work experimental and practical data have their place, but only as means to an end, and that end is as evident on every page as it was in Dalton's immortal work. Fascinating as the book is, it must be admitted that it is by no means easy reading; and he who wishes to master its contents and to assimilate its teaching will need to gird up his mental loins. Part of the difficulty is doubtless due to the different genius of the languages, but much more depends upon the impossibility of entering into the spirit of an author, or of quickly realizing his drift and meaning, when his whole mode of thought is so very dissimilar to that which obtains among Western people. It may be that herein lies part of the peculiar charm of freshness of the work. The book of the Siberian chemist is to the ordinary run of text-books what the novels of Tolstoi or Turgenieff are to the common run of works of fiction. But there are difficulties of another kind. Probably no other book in our language—certainly no other chemical treatise—contains such an extraordinary number of footnotes. There is scarcely a page without a footnote, and some of the pages are practically little else than footnotes. The continuity of description or of argument is constantly being broken, often by a footnote extending over several pages, and frequently so diffuse and involved that, by the time the reader has disposed of it, the statement in the main text to which it had reference has been lost sight of, and must needs be picked up again. Moreover, the repeated interruption is aggravated by the circumstance that these notes are printed in "nonpareil small," which adds enormously to the physical fatigue of reading and studying the work. The author, indeed, recommends that they should be read only by the advanced student, or on a second perusal of the work; but we are afraid that no intelligent reader will follow this advice when once he has begun to dip into them. They are, in fact, like the postscripts of ladies' letters—often more important, more instructive, more suggestive, and more characteristic, than the main body of the text. But, in truth, the book is not fitted for a beginner: its proper readers are those for whom the footnotes are specially intended. It requires, too, to be read with discrimination. It was said by Davy that analogy is the fruitful parent of error, and it must be confessed that Mendeléeff's love of analogy frequently leads him to generalizations which are more ingenious and suggestive than intrinsically sound or well grounded.

The translator and the editor have, doubtless, had difficulties to contend with. They tell us that they have not considered themselves at liberty to make any alterations in the matter of the work, and they have striven to give a literal rendering of it. They have felt that, on the whole, it would be better to have some inelegance of language rather than risk the loss of the exact shade of the author's meaning. Unfortunately, in too many instances the translator and his editor have not gained in precision of meaning what they have lost in elegance of statement. Thus, for example, on p. 12 we read:

"The means of collecting and investigating gases were already known before Lavoisier's time, but he first showed the real part they [the means or the gases?] played in the processes," &c.

On p. 19 it is stated:

"By heating chalk (or else copper carbonate . . .) we obtain lime," &c.

Thus, too, on p. 47:

"In general terms water is called pure when it is clear and free from insoluble particles held in suspension and visible to the naked eye, from which it may be freed by filtration through charcoal, sand, or porous (natural or artificial) stones, and when it possesses a clean fresh taste. It depends [what depends?] on the absence of any tastable, decomposing organic matter, on the quantity of air and atmospheric gases in solution, and on the presence of mineral substances to the amount of about 300 grams per ton (or cubic metre, or, what is the same, 300 milligrams to a kilogram or litre of water), and of not more than 100 grams of organic matter."

Again, on p. 72 we read:

"Although in the majority of cases the solubility of solids increases with the temperature, yet, just as there are substances whose volume diminishes with a rise in temperature (for example, water from 0° to 4°), so there are not a few solid substances whose solubilities fall on heating. Glauber's salt, or sodium sulphate, historically forms a particularly instructive example of the case in question. If this salt be taken in an ignited state (!) (deprived of its water of crystallization), then its solubility," &c.

What, too, is the meaning of the statement on p. 83?

"Under ordinary circumstances the quantity of aqueous vapour [in the air] is much greater [than what?], but it varies with the moisture of the atmosphere."

Presumably, for "moisture" we are to read "temperature." On p. 164, in the description of the experiment of burning phosphorus in oxygen, it is recommended that "the cork closing the vessel should not fit tightly, otherwise it may fly off with the spoon." That the cork should fly off with the spoon is contrary to well-established precedent: if anything is to fly away with the spoon, it should, of course, be the dish on which the bell-jar is represented as resting. To say (p. 417) that common salt containing magnesium chloride "partially effloresces in a damp atmosphere" is opposed to fact, and was surely never so stated by Mendeléeff. Van der Waals's equation is written:

$$\left(p + \frac{a}{v^2}\right)(v - p) = R(1 - at),$$

instead of

$$\left(p + \frac{a}{v^2}\right)(v - b) = R(1 + at).$$

And on the same page we find $pr = c(1 + at)$, instead of $pv = c(1 + at)$.

Proper names are frequently wrong. Thus we have "Van der Waal" for van der Waals, "Becker" for Becher, "Brown" for Braun, "De Haen" for De Heen, "Frauenhofer" for Fraunhofer, "Personne" for Person, "Prout" for Proust, "Ray" for Rey, "Krütznach" for Kreutznach, "Wergtesgaden at Salzkammerhutte" for Berchtesgaden (which is not in the Salzkammergut).

We have taken the pains to compare the English version with the German translation of Jâwein and Thillot in a number of instances where the meaning is obscure, or where statements are made which appear to be erroneous, and in no single instance is the fault to be traced to the author. We think, too, that the limitation imposed on the translator and editor by themselves has operated injuriously in another way: in cases where subsequent research should modify or supplement particular statements in the original, it was surely open to them, in the interests of knowledge, to substitute truth for error. Thus we know from the work of Winkler and Hempel the conditions under which exact determinations of oxygen by means of alkaline pyrogallol can be made; we know, too, that atmospheric ammonia and nitric acid are not by any means the main sources of the supply of nitrogen to plants; ammonium chloride is not now usually prepared by sublimation. The statement of the principle of Kjeldahl's method, given on p. 246, is inaccurate: the radicle ammonium has not been obtained, nor is the old view of the nature of the so-called "ammonium amalgam" any longer tenable, nor is there any direct proof of the existence of ammonium hydrate. Flagstone, at least in this country, is not a form of carbonate of lime: it is usually a fine-grained micaceous sandstone. The apparatus employed by Cavendish in his memorable synthesis of water in no wise resembled that described and figured on p. 167; thanks to the symbol adopted by the publishing Society which bore his name, it seems now well-nigh impossible to get rid of the belief that the pear-shaped stoppered eudiometer was devised and used by him in the course of his investigation: as a matter of fact, the explosions were made in a simple Volta tube. With respect to the illustrations in general, we th k that the majority of them could well have been spared; all them have done duty in other works, and many of them are calculated to give an erroneous impression of the thing sought to be represented. Thus the coke-tower figured on p. 443 resembles nothing actually used; Fig. 60, which is stated to be a Davy lamp, is either a Mueseler or a Clanny lamp; Fig. 47 does not illustrate the method of preparing nitric acid employed in this country, nor does Fig. 93 represent a modern blast-furnace. The only figure of a zinc-furnace given is that of the practically obsolete *per descensum* method.

We have been constrained to point out these blemishes, not in any hypercritical spirit, but solely because of our wish that Mendeléeff's great work should have been given to English and American readers in as perfect a form as possible. The blemishes, after all, are only as the spots on the sun. It is a great boon to get the book even as it is, for no thoughtful reader can fail to be quickened and animated by its fruitful and suggestive pages.

T. E. T.

AN important new series of compounds, the thionylamines, in which two new hydrogen atoms of the amido group of the primary amines are replaced by the radicle thionyl SO, have been prepared by Prof. Michaelis, and are described in the curren number of *Liebig's Annalen*. It has been found that the primary amines of the fatty series when dissolved in ether react with thionyl chloride, $SOCl_2$, in a manner which is readily controlled by extraneous cooling of the vessel in which the reaction is conducted. The products are the hydrochloride of the amine employed which separates in crystals, and the new liquid thionylamine which remains dissolved in the ether, but can readily be isolated by fractional distillation. Thionyl chloride is incapable of acting upon the hydrochlorides of the amines of the fatty series, hence three molecular equivalents of the amine are required for every equivalent of thionyl chloride, according to the following equation in the case of methylamine:— $SOCl_2 + 3(CH_3.NH_2) = CH_3.N{:}SO + 2(CH_3NH_2.HCl)$. The thionylamines of this series are colourless fuming liquids which boil without decomposition and emit a most powerful odour. They are decomposed by water into the original amines and sulphur dioxide. The amines of the aromatic series likewise form thionylamines with thionyl chloride; and the hydrochlorides, unlike those of the fatty series, react with equal facility in accordance with the equation $C_6H_5NH_2.HCl + SOCl_2 = C_6H_5N{:}SO + 3HCl$. It is only necessary to cover the powdered hydrochloride of aniline with benzene, add the calculated quantity of thionyl chloride, and warm over a water bath for a short time. The lower members of the aromatic thionylamines are yellow liquids which distil without decomposition; the higher members may likewise be distilled without loss under diminished pressure. Alkalies convert them into the original amines and a sulphite, $C_6H_5N{:}SO + 2NaOH = C_6H_5NH_2 + Na_2SO_3$.

A Brief Outline of the Development of the Theory of Relativity.

By Prof. A. Einstein.

[Translated by Dr. Robert W. Lawson.]

THERE is something attractive in presenting the evolution of a sequence of ideas in as brief a form as possible, and yet with a completeness sufficient to preserve throughout the continuity of development. We shall endeavour to do this for the Theory of Relativity, and to show that the whole ascent is composed of small, almost self-evident steps of thought.

The entire development starts off from, and is dominated by, the idea of Faraday and Maxwell, according to which all physical processes involve a continuity of action (as opposed to action at a distance), or, in the language of mathematics, they are expressed by partial differential equations. Maxwell succeeded in doing this for electro-magnetic processes in bodies at rest by means of the conception of the magnetic effect of the vacuum-displacement-current, together with the postulate of the identity of the nature of electro-dynamic fields produced by induction, and the electro-static field.

The extension of electro-dynamics to the case of moving bodies fell to the lot of Maxwell's successors. H. Hertz attempted to solve the problem by ascribing to empty space (the æther) quite similar physical properties to those possessed by ponderable matter; in particular, like ponderable matter, the æther ought to have at every point a definite velocity. As in bodies at rest, electromagnetic or magneto-electric induction ought to be determined by the rate of change of the electric or magnetic flow respectively, provided that these velocities of alteration are referred to surface elements moving with the body. But the theory of Hertz was opposed to the fundamental experiment of Fizeau on the propagation of light in flowing liquids. The most obvious extension of Maxwell's theory to the case of moving bodies was incompatible with the results of experiment.

At this point, H. A. Lorentz came to the rescue. In view of his unqualified adherence to the atomic theory of matter, Lorentz felt unable to regard the latter as the seat of continuous electromagnetic fields. He thus conceived of these fields as being conditions of the æther, which was regarded as continuous. Lorentz considered the æther to be intrinsically independent of matter, both from a mechanical and a physical point of view. The æther did not take part in the motions of matter, and a reciprocity between æther and matter could be assumed only in so far as the latter was considered to be the carrier of attached electrical charges. The great value of the theory of Lorentz lay in the fact that the entire electrodynamics of bodies at rest and of bodies in motion was led back to Maxwell's equations of empty space. Not only did this theory surpass that of Hertz from the point of view of method, but with its aid H. A. Lorentz was also pre-eminently successful in explaining the experimental facts.

The theory appeared to be unsatisfactory only in *one* point of fundamental importance. It appeared to give preference to one system of coordinates of a particular state of motion (at rest relative to the æther) as against all other systems of co-ordinates in motion with respect to this one. In this point the theory seemed to stand in direct opposition to classical mechanics, in which all inertial systems which are in uniform motion with respect to each other are equally justifiable as systems of co-ordinates (Special Principle of Relativity). In this connection, all experience also in the realm of electro-dynamics (in particular Michelson's experiment) supported the idea of the equivalence of all inertial systems, *i.e.* was in favour of the special principle of relativity.

The Special Theory of Relativity owes its origin to this difficulty, which, because of its fundamental nature, was felt to be intolerable. This theory originated as the answer to the question: Is the special principle of relativity really contradictory to the field equations of Maxwell for empty space? The answer to this question appeared to be in the affirmative. For if those equations are valid with reference to a system of co-ordinates K, and we introduce a new system of co-ordinates K′ in conformity with the—to all appearances readily establishable—equations of transformation

$$\left.\begin{array}{l} x' = x - vt \\ y' = y \\ z' = z \\ t' = t \end{array}\right\}\text{(Galileo transformation),}$$

then Maxwell's field equations are no longer valid in the new co-ordinates (x', y', z', t'). But appearances are deceptive. A more searching analysis of the physical significance of space and time rendered it evident that the Galileo transformation is founded on arbitrary assumptions, and in particular on the assumption that the statement of simultaneity has a meaning which is independent of the state of motion of the system of co-ordinates used. It was shown that the field equations for *vacuo* satisfy the special principle of relativity, provided we make use of the equations of transformation stated below:

$$\left.\begin{array}{l} x' = \dfrac{x - vt}{\sqrt{1 - v^2/c^2}} \\ y' = y \\ z' = z \\ t' = \dfrac{t - vx/c^2}{\sqrt{1 - v^2/c^2}} \end{array}\right\}\text{(Lorentz transformation).}$$

In these equations x, y, z represent the co-ordinates measured with measuring-rods which are at rest with reference to the system of co-ordinates, and t represents the time measured with suitably adjusted clocks of identical construction, which are in a state of rest.

Now in order that the special principle of relativity may hold, it is necessary that all the equations of physics do not alter their form in the transition from one inertial system to another, when we make use of the Lorentz transformation for the calculation of this change. In the language of mathematics, all systems of equations that express physical laws must be co-variant with respect to the Lorentz transformation. Thus, from the point of view of method, the special principle of relativity is comparable to Carnot's principle of the impossibility of perpetual motion of the second kind, for, like the latter, it supplies us with a general condition which all natural laws must satisfy.

Later, H. Minkowski found a particularly elegant and suggestive expression for this condition of co-variance, one which reveals a formal relationship between Euclidean geometry of three dimensions and the space-time continuum of physics.

Euclidean Geometry of Three Dimensions.	*Special Theory of Relativity.*
Corresponding to two neighbouring points in space, there exists a numerical measure (distance ds) which conforms to the equation $ds^2 = dx_1^2 + dx_2^2 + dx_3^2$.	Corresponding to two neighbouring points in space-time (point events), there exists a numerical measure (distance ds) which conforms to the equation $ds^2 = dx_1^2 + dx_2^2 + dx_3^2 + dx_4^2$
It is independent of the system of co-ordinates chosen, and can be measured with the unit measuring-rod.	It is independent of the inertial system chosen, and can be measured with the unit measuring-rod and a standard clock. x_1, x_2, x_3 are here rectangular co-ordinates, whilst $x_4 = \sqrt{-1}\,ct$ is the time multiplied by the imaginary unit and by he velocity of light.
The permissible transformations are of such a character that the expression for ds^2 is invariant, *i.e.* the linear orthogonal transformations are permissible.	The permissible transformations are of such a character that the expression for ds^2 is invariant, *i.e.* those linear orthogonal substitutions are permissible which maintain the semblance of reality of x_1, x_2, x_3, x_4. These substitutions are the Lorentz transformations.
With respect to these transformations, the laws of Euclidean geometry are invariant.	With respect to these transformations, the laws of physics are invariant.

From this it follows that, in respect of its *rôle* in the equations of physics, though not with regard to its physical significance, time is equivalent to the space co-ordinates (apart from the relations of reality). From this point of view, physics is, as it were, a Euclidean geometry of four dimensions, or, more correctly, a statics in a four-dimensional Euclidean continuum.

The development of the special theory of relativity consists of two main steps, namely, the adaptation of the space-time "metrics" to Maxwell's electro-dynamics, and an adaptation of the rest of physics to that altered space-time "metrics." The first of these processes yields the relativity of simultaneity, the influence of motion on measuring-rods and clocks, a modification of kinematics, and in particular a new theorem of addition of velocities. The second process supplies us with a modification of Newton's law of motion for large velocities, together with information of fundamental importance on the nature of inertial mass.

It was found that inertia is not a fundamental property of matter, nor, indeed, an irreducible magnitude, but a property of energy. If an amount of energy E be given to a body, the inertial mass of the body increases by an amount E/c^2, where c is the velocity of light *in vacuo*. On the other hand, a body of mass m is to be regarded as a store of energy of magnitude mc^2.

Furthermore, it was soon found impossible to link up the science of gravitation with the special theory of relativity in a natural manner. In this connection I was struck by the fact that the force of gravitation possesses a fundamental property, which distinguishes it from electro-magnetic forces. All bodies fall in a gravitational field with the same acceleration, or—what is only another formulation of the same fact—the gravitational and inertial masses of a body are numerically equal to each other. This numerical equality suggests identity in character. Can gravitation and inertia be identical? This question leads directly to the General Theory of Relativity. Is it not possible for me to regard the earth as free from rotation, if I conceive of the centrifugal force, which acts on all bodies at rest relatively to the earth, as being a "real" field of gravitation, or part of such a field? If this idea can be carried out, then we shall have proved in very truth the identity of gravitation and inertia. For the same property which is regarded as *inertia* from the point of view of a system not taking part in the rotation can be interpreted as *gravitation* when considered with respect to a system that shares the rotation. According to Newton, this interpretation is impossible, because by Newton's law the centrifugal field cannot be regarded as being produced by matter, and because in Newton's theory there is no place for a "real" field of the "Koriolis-field" type. But perhaps Newton's law of field could be replaced by another that fits in with the field which holds with respect to a "rotating" system of co-ordinates? My conviction of the identity of inertial and gravitational mass aroused within me the feeling of absolute confidence in the correctness of this interpretation. In this connection I gained encouragement from the following idea. We are familiar with the "apparent" fields which are valid relatively to systems of co-ordinates possessing arbitrary motion with respect to an inertial system. With the aid of these special fields we should be able to study the law which is satisfied in general by gravitational fields. In this connection we shall have to take account of the fact that the ponderable masses will be the determining factor in producing the field, or, according to the fundamental result of the special theory of relativity, the energy density—a magnitude having the transformational character of a tensor.

On the other hand, considerations based on the metrical results of the special theory of relativity led to the result that Euclidean metrics can no longer be valid with respect to accelerated systems of co-ordinates. Although it retarded the progress of the theory several years, this enormous difficulty was mitigated by our knowledge that Euclidean metrics holds for small domains. As a consequence, the magnitude *ds*, which was physically defined in the special theory of relativity hitherto, retained its significance also in the general theory of relativity. But the co-ordinates themselves lost their direct significance, and degenerated simply into numbers with no physical meaning, the sole purpose of which was the numbering of the space-time points. Thus in the general theory of relativity the co-ordinates perform the same function as the Gaussian co-ordinates in the theory of surfaces. A necessary consequence of the preceding is that in such general co-ordinates the measurable magnitude *ds* must be capable of representation in the form

$$ds^2 = \sum_{uv} g_{uv} dx_u dx_v,$$

where the symbols g_{uv} are functions of the space-time co-ordinates. From the above it also follows that the nature of the space-time variation of the factors g_{uv} determines, on one hand the space-time metrics, and on the other the gravitational field which governs the mechanical behaviour of material points.

The law of the gravitational field is determined mainly by the following conditions: First, it shall be valid for an arbitrary choice of the system of co-ordinates; secondly, it shall be determined by the energy tensor of matter; and thirdly, it shall contain no higher differential coefficients of the factors g_{uv} than the second, and must be linear in these. In this way a law was obtained which, although fundamentally different from Newton's law, corresponded so exactly to the latter in the deductions derivable from it that only very few criteria were to be found on which the theory could be decisively tested by experiment.

The following are some of the important questions which are awaiting solution at the present time. Are electrical and gravitational fields really so different in character that there is no formal unit to which they can be reduced? Do gravitational fields play a part in the constitution of matter, and is the continuum within the atomic nucleus to be regarded as appreciably non-Euclidean? A final question has reference to the cosmological problem. Is inertia to be traced to mutual action with distant masses? And connected with the latter: Is the spatial extent of the universe finite? It is here that my opinion differs from that of Eddington. With Mach, I feel that an affirmative answer is imperative, but for the time being nothing can be proved. Not until a dynamical investigation of the large systems of fixed stars has been performed from the point of view of the limits of validity of the Newtonian law of gravitation for immense regions of space will it perhaps be possible to obtain eventually an exact basis for the solution of this fascinating question.

The Isotope Effect in the Spectrum of Silicon Nitride.

Results of a quantum theory analysis of the SiN bands and of the vibrational isotope effect in these bands were given in an earlier letter to Nature (March 22, 1924) and in a paper presented at a meeting of the American Physical Society (cf. *Phys. Rev.* 23, 554, 1924). It is now found that the equations given in the latter for these bands are incorrect. This is due to a wrong assignment of vibrational quantum numbers, corresponding to what may be described as an insidious violation of the combination principle. With the data first used, this violation was not apparent, but new data disclose systematic, although rather small, deviations. A new and, this time, correct assignment of quantum numbers has now been made. The following equation holds for the position of the null-lines of the $Si^{28}N$ bands (n' = vibrational quantum number of the initial, n'' that of the final state of the molecule):

$$\nu = 24234 \cdot 2 + 1016 \cdot 30n' - 17 \cdot 77n'^2 + 0 \cdot 41n'^3 - 0 \cdot 0049n'^4 - 1145 \cdot 00n'' + 6 \cdot 570n''^2.$$

The null-lines, it should be stated, can for many of the bands be measured directly on the plates. At the low temperature of the active nitrogen used in generating the bands, the null-line appears as a conspicuous hole in the band structure, on the low-frequency side of the head.

With the new numbering, the various apparent abnormalities previously noted disappear, and an analogy of the SiN bands to the violet CN bands is brought out. In particular, the isotope effect, previously thought abnormally large for the initial state of the molecule, is now completely normal. Agreement with the theory is exceedingly good if the emitter is assumed to be SiN. No other assumed emitter gives agreement with the experimental data; even for SiO, the agreement is poor. Thus the value of the isotope effect in the identification of the emitters of band spectra, emphasised in a previous letter (April 5, 1924), is again confirmed. As in the case of the BO bands, so in the case of SiN, the testimony of the isotope effect is backed up by the chemical evidence (Nature, Sept. 6, 1924, and *Phys. Rev.* 25, 259, (1925)).

The agreement of the results with theory is much better if the integral vibrational quantum numbers 0, 1, 2, . . . are assumed than if the half-integral numbers ½, 1½, 2½, . . . are used. In this respect SiN differs from BO, for which the data indicate (cf. refs. last cited) that half-integral values are needed.

A detailed account of the work on the SiN bands is now being prepared for publication.

Robert S. Mulliken,
National Research Fellow.
Jefferson Physical Laboratory,
Harvard University,
May 16.

Possible Existence of a Neutron

It has been shown by Bothe and others that beryllium when bombarded by α-particles of polonium emits a radiation of great penetrating power, which has an absorption coefficient in lead of about 0·3 (cm.)$^{-1}$. Recently Mme. Curie-Joliot and M. Joliot found, when measuring the ionisation produced by this beryllium radiation in a vessel with a thin window, that the ionisation increased when matter containing hydrogen was placed in front of the window. The effect appeared to be due to the ejection of protons with velocities up to a maximum of nearly 3×10^9 cm. per sec. They suggested that the transference of energy to the proton was by a process similar to the Compton effect, and estimated that the beryllium radiation had a quantum energy of 50×10^6 electron volts.

I have made some experiments using the valve counter to examine the properties of this radiation excited in beryllium. The valve counter consists of a small ionisation chamber connected to an amplifier, and the sudden production of ions by the entry of a particle, such as a proton or α-particle, is recorded by the deflexion of an oscillograph. These experiments have shown that the radiation ejects particles from hydrogen, helium, lithium, beryllium, carbon, air, and argon. The particles ejected from hydrogen behave, as regards range and ionising power, like protons with speeds up to about $3{\cdot}2 \times 10^9$ cm. per sec. The particles from the other elements have a large ionising power, and appear to be in each case recoil atoms of the elements.

If we ascribe the ejection of the proton to a Compton recoil from a quantum of 52×10^6 electron volts, then the nitrogen recoil atom arising by a similar process should have an energy not greater than about 400,000 volts, should produce not more than about 10,000 ions, and have a range in air at N.T.P. of about 1·3 mm. Actually, some of the recoil atoms in nitrogen produce at least 30,000 ions. In collaboration with Dr. Feather, I have observed the recoil atoms in an expansion chamber, and their range, estimated visually, was sometimes as much as 3 mm. at N.T.P.

These results, and others I have obtained in the course of the work, are very difficult to explain on the assumption that the radiation from beryllium is a quantum radiation, if energy and momentum are to be conserved in the collisions. The difficulties disappear, however, if it be assumed that the radiation consists of particles of mass 1 and charge 0, or neutrons. The capture of the α-particle by the Be^9 nucleus may be supposed to result in the formation of a C^{12} nucleus and the emission of the neutron. From the energy relations of this process the velocity of the neutron emitted in the forward direction may well be about 3×10^9 cm. per sec. The collisions of this neutron with the atoms through which it passes give rise to the recoil atoms, and the observed energies of the recoil atoms are in fair agreement with this view. Moreover, I have observed that the protons ejected from hydrogen by the radiation emitted in the opposite direction to that of the exciting α-particle appear to have a much smaller range than those ejected by the forward radiation. This again receives a simple explanation on the neutron hypothesis.

If it be supposed that the radiation consists of quanta, then the capture of the α-particle by the Be^9 nucleus will form a C^{13} nucleus. The mass defect of C^{13} is known with sufficient accuracy to show that the energy of the quantum emitted in this process cannot be greater than about 14×10^6 volts. It is difficult to make such a quantum responsible for the effects observed.

It is to be expected that many of the effects of a neutron in passing through matter should resemble those of a quantum of high energy, and it is not easy to reach the final decision between the two hypotheses. Up to the present, all the evidence is in favour of the neutron, while the quantum hypothesis can only be upheld if the conservation of energy and momentum be relinquished at some point.

J. Chadwick.

Cavendish Laboratory,
Cambridge, Feb. 17.

New Evidence for the Neutron

Several important communications dealing with the properties of rays emitted by atomic nuclei when bombarded with α-particles have recently appeared,[1] on which we should like to make a few comments.

It has been shown by F. Joliot [2] that the rays emitted by boron under the action of α-particles from polonium are much more penetrating than had originally been indicated. Their penetrating power, while superior to that of the most powerful γ-rays obtained from radio-active sources, is inferior to that of the rays obtained from beryllium bombarded by α-particles from polonium. This result does not agree with Webster's findings, but agrees with the fact that the protons ejected from boron are slower than those ejected from beryllium. Secondly, we have shown that the ejection of protons is a general phenomenon. By means of the Wilson chamber, we have photographed the paths of the helium nuclei ejected by beryllium rays, and from absorption measurements were able to conclude that other atoms are also ejected. Further, our experiments showed for the first time the important part played by the nuclei in the absorption of the rays emitted by beryllium under the influence of α-particles, a phenomenon which clearly marked them off from all previously known radiation.

J. Chadwick was led simultaneously to the same generalisation concerning the ejection of nuclei, and he put forward the view that the penetrating rays produced by the bombardment of beryllium by α-particles from polonium are neutrons. This interpretation is necessary if energy and momentum are conserved in the collision.

Recent experiments which we have carried out with M. Savel clearly show that the rays emitted by lithium have a penetrating power, in lead, less than that of the γ-rays of polonium (they are completely absorbed by 5 mm. of lead), and that they are much more readily absorbed, at equal surface mass, by paraffin than by lead. This proves that they cannot be of an electronic or electromagnetic nature.

Our latest experiments, in collaboration with M. Savel, indicate that the protons ejected from beryllium form two groups. This suggests that there are also two groups of neutrons (each group not necessarily homogeneous); one group has a range of 28 cm. in air, and an energy of $4{\cdot}5 \times 10^6$ electron volts; the other has a range of about 70 cm. and an energy of approximately $7{\cdot}8 \times 10^6$ electron volts. We find it difficult to reconcile Chadwick's result of a *maximum range* of 40 cm. with the curves which we have obtained for the absorption of protons.

Irène Curie.
F. Joliot.

Institut du Radium,
Laboratoire Curie,
1, Rue Pierre-Curie, Paris (5e),
June 25.

[1] H. C. Webster, J. Chadwick, N. Feather, P. I. Dee, *Proc. Roy. Soc.*, A, **136**, 428, 692, 708, and 727; 1932.
[2] F. Joliot, *C.R. Ac. Sci.*, **193**, 1415; 1931.
[3] M. de Broglie and L. Leprince-Ringuet, *C.R. Ac. Sci.*, **194**, 1616; 1932.
[4] J. Chadwick, *Proc. Roy. Soc.*, A, **136**, 702; 1932.
[5] I. Curie and F. Joliot, *C.R. Ac. Sci.*, **194**, 1229; 1932.

Photography of Penetrating Corpuscular Radiation

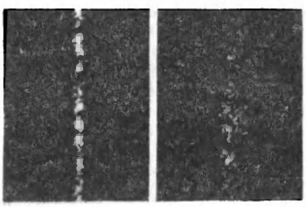
SINCE Skobelzyn [1] discovered the tracks of particles of high energy on photographs taken with a Wilson cloud chamber, this method has been used by him and others in a number of investigations [2] of the nature of penetrating radiation. Such work is laborious, since these tracks occur in only a small fraction of the total number of expansions made. We have found it possible to obtain good photographs of these high energy particles by arranging that the simultaneous discharge of two Geiger-Müller counters due to the passage of one of these particles shall operate the expansion itself. On more than 75 per cent of the photographs so obtained (the fraction depending on the ratio of the number of 'true' to 'accidental' coincidences) are found the tracks of particles of high energy.

Mott Smith and Locher [3] had previously found a correlation between the occurrence of these tracks and the discharge of a tube counter, and recently Johnson, Fleicher, and Street [4] have used the coincidence of the discharges of two counters to operate the flash which illuminates a continuously working cloud chamber.

The chamber we used has a diameter of 13 cm. and has its plane vertical, with one tube counter above and one below, so that any ray which passes straight through both counters will also pass through the illuminated part of the chamber. A magnetic field is applied at right angles to the plane of the chamber. When the cloud chamber has been made ready for use, the arrival of a coincidence is awaited. After an average wait of about two minutes, a coincidence occurs and a relay mechanism starts the expansion.

The tracks have a definite breadth due to the diffusion of the ions during the time between the passage of the ray and the attainment of supersaturation. The chamber was designed so that this time

Oxygen Hydrogen



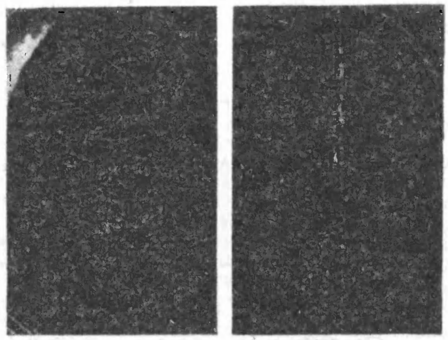
0 1 cm.

FIG. 1.

should be very small; it was in fact 0·01 sec. The observed breadth of the tracks in oxygen at 1·5 atmospheres pressure was 0·8 mm., and in hydrogen 1·8 mm. (Fig. 1). These breadths are in close agreement with the values calculated from the theoretical relation $\overline{x^2} = 2\ D\ T$, giving the mean square displacement in terms of the diffusion coefficient and the time. In spite of this breadth, the tracks in oxygen are admirably suited for accurate measurement.

The method is very economical in time in comparison with the usual method. Though the track of a fast particle may be obtained every tenth random expansion, only a few of such tracks are of use if an accurate determination of the energy of the particles is to be made. For this purpose it is desirable that a track shall lie in the plane of the chamber, for this ensures that it will be long, in perfect focus, and at right angles to the field. The fraction of random expansions which show such tracks is very small. Again, it is easier to adjust a chamber to take a few good photographs than it is to maintain the adjustment while taking many thousand.

The method has one disadvantage. The technical problem of obtaining a very large magnetic field over the whole chamber, such as has been obtained by



a *b*

FIG. 2.—Tracks of high-speed particles. (*a*): $H\rho$ about 68,000 gauss cm., corresponding electron energy 20×10^6 volts; (*b*): $H\rho$ probably about 2×10^6 gauss cm., corresponding electron energy 600×10^6 volts.

Millikan and Anderson, is difficult, since the field must be maintained for periods of some minutes, while, when making expansions at random, it is only needed for a fraction of a second.

Among one hundred stereoscopic pairs of photographs, 59 showed the track of a single high speed particle passing through both counters (Fig. 2, *a* and *b*); 17 showed either multiple tracks of varying degrees of complexity, such as have been found by other workers, or else a single track passing through one but not both counters; 24 showed no tracks. Only about ten per cent of the tracks were markedly bent in a field of about 2000 gauss. Assuming them to be electrons, their energies lay between 2 and 20 million volts. To estimate the energy of the particles producing the main group of nearly straight tracks, the angular resolving power of the apparatus was determined by measurement of tracks obtained with no magnetic field. It was found in this way that a *mean* deviation of $\frac{1}{8}°$ could be considered as significant. Since the tracks obtained with the magnetic field of 2000 gauss showed no such deviation, it was concluded that the *mean* $H\rho$ of these particles must have been greater than 2×10^6 gauss cm. If the particles were electrons, their *mean* energy must have been greater than 600 million volts, or if protons, greater than 200 million volts.

P. M. S. BLACKETT.
G. OCCHIALINI.

The Cavendish Laboratory,
Cambridge,
Aug. 21.

[1] Skobelzyn, *Z. Phys.*, **54**, 686; 1929.
[2] Skobelzyn, *Comptes rendus*, **194**, 118; 1932. Auger and Skobelzyn, *Comptes rendus*, **189**, 55; 1929. Millikan and Anderson, *Phys. Rev.*, **40**, 325; 1932.
[3] Mott Smith and Locher, *Phys. Rev.*, **38**, 1399; 1931: **39**, 883; 1932.
[4] Johnson, Fleicher, and Street, *Phys. Rev.*, **40**, 1048; 1932.

Disintegration of Light Elements by Fast Protons

SINCE the publication of our paper[1] on the disintegration of elements by fast protons, we have examined some of the light elements more carefully, using much thinner mica windows than we had previously employed on the high voltage tube. With the present arrangement, we can count particles which have passed through only 6 mm. air equivalent of absorber on their way from the target to the ionisation chamber.

In the case of lithium, we have found, in addition to the α-particle group of 8·4 cm. range, another group of particles of much shorter range. The number of these is about equal to that of the long range particles and their maximum range is about 2 cm. The ionisation produced by them indicates that they are α-particles. It will be of interest to examine whether any γ-rays are emitted corresponding to the difference of the energies of the α-particles in the two groups, but on account of the smallness of the effect to be expected, a sensitive method will be necessary.

In the case of boron, the number of particles observed increases rapidly as the total absorption between the target and the ionisation chamber is reduced. The maximum range of these particles is about 3 cm. and in our earlier experiments we determined the number of particles only after passing through the equivalent of 2·9 cm. of air, so that we were very nearly at the end of the range. Decreasing the absorber to 6 mm. of air gives an enormous increase in the number of particles. In this way about twenty-five times as many particles have been obtained from boron as from lithium under the same conditions. We estimate that there is roughly one particle emitted per two million incident protons at 500 kilovolts. The ionisation produced by the particles suggests that they are α-particles, and the energy of the main group would support the assumption that a proton enters the B_5^{11} nucleus and the resulting nucleus breaks up into three α-particles. There also seem to be present a small number of particles with ranges up to about 5 cm.

J. D. COCKCROFT.
E. T. S. WALTON.

Cavendish Laboratory,
Cambridge.
Dec. 22.

[1] *Proc. Roy. Soc.*, A, 137, 229; 1932.

131, 23; 1933

The Crystal Photoeffect

SEVERAL explanations have been proposed for the electromotive force produced in crystals of cuprous oxide by illumination, such as the light pressure hypothesis of Dember[1], the diffusion theory developed by Teichmann[2]; recently Deaglio[3] has suggested an electrolytic origin. We have shown[4] that the diffusion theory in its original form is insufficient to account for the potential distribution in an illuminated crystal. The theory shows clearly that the area struck by a light beam will have the highest positive potential. It does not seem equally clear that the spot just opposite will have the lowest negative potential, lower even than a dark section of the same crystal. It seems, however, that a good agreement with experimental facts can be reached, assuming that the equilibrium of electrons in an illuminated crystal is dynamic and consists of a continuous circulation of electrons between the bright and the dark parts of the crystal.

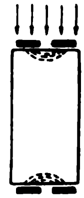


FIG. 1.

It would be erroneous to assume a proportionality between the bright energy absorbed in some part of the crystal and the electron concentration there. It is, however, correct to replace the ratio of concentrations n_1/n_2 in two small volumes by the ratio σ_1/σ_2 of conductivities. It is then possible to test experimentally the fundamental equation of the diffusion-theory of the photoelectromotive force, P:

$$P = \frac{kT}{\varepsilon} \log \frac{n_1}{n_2} = \frac{kT}{\varepsilon} \log \frac{\sigma_1}{\sigma_2} \quad . \; . \; . \; . \quad (1)$$

We used a long crystal provided with four transparent gold electrodes (Fig. 1). Measuring the ratio of conductivities at both ends of the crystal, we got for σ_1 too low, and for σ_2 too high a value because of the finite thickness of the sheet measured. Taking into account this systematic error we found a satisfactory agreement between the computed and measured values of P.

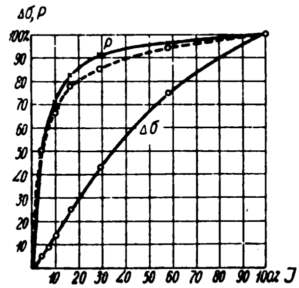


FIG. 2.

The best proof for the formula (1) is the dependence of P on the light intensity I. Using a Zeiss filter, we were able to reduce the light intensity to 4, 7, 10, 15, 30, 60, and 90 per cent of its initial value. Fig. 2 shows the corresponding values of $\Delta\sigma_1$ (photoconductivity) of observed P (full line) and of computed P (dotted line). The small discrepancy is accounted for by the systematic error in σ_1/σ_2.

It is well known, and regarded as an anomaly[5], that photoconductivity is always restricted to the red end of the absorption band and vanishes within the region of high absorption. From curves such as Fig. 2 we computed the conductivity in a layer adjacent to the electrode and found a marked photoconductivity in the whole absorption band at liquid air temperature.

ANNE JOFFÉ.
A. F. JOFFÉ.

Physical-Technical Institute,
Leningrad.
July 10.

[1] H. Dember, *Phys. Z.*, 32, 554, 856; 1931.
[2] H. Teichmann, *Proc. Roy. Soc.*, A, 139, 105; 1933.
[3] R. Deaglio, *Z. Phys.*, 83, 179; 1933.
[4] Anna Joffé and A. Joffé, *Z. Phys.*, 82, 754; 1933.
[5] A. L. Hughes and L. A. Du Bridge, "Photoelectric Phenomena", 302; 1932.

132, 168; 1933

New Evidence for the Positive Electron

THE experiments of Anderson[1] and of Blackett and Occhialini[2] on the effects produced in an expansion chamber by the penetrating radiation strongly suggest the existence of positive electrons—particles of about the same mass as an electron but carrying a positive charge.

Some observations of the effects produced by the passage of neutrons through matter, and the experiments of Curie and Joliot[3] in which they observed retrograde electron tracks in an expansion chamber, led us to consider the possibility that positive electrons might be produced in the interaction of neutrons and matter, and we have recently obtained evidence which can be interpreted in this way.

A capsule containing a polonium source and a piece of beryllium was placed close to the wall of an expansion chamber. On the inside of the wall was fixed a target of lead about 2·5 cm. square and 2 mm. thick. This lead target was thus exposed to the action of the radiation, consisting of γ-rays and neutrons, emitted from the beryllium. Expansion photographs were taken by means of a stereoscopic pair of cameras. A magnetic field was applied during the expansion, its magnitude being usually about 800 gauss.

Most of the tracks recorded in the photographs were, from the sense of their curvature, clearly due to negative electrons, but many examples were found of tracks which had one end in or near the lead target and showed a curvature in the opposite sense. Either these were due to particles carrying a positive charge or they were due to negative electrons ejected in remote parts of the chamber and bent by the magnetic field so as to end on the lead target. Statistical examination of the results supports the view that the tracks began in the target and therefore carried a positive charge.

Strong evidence for this hypothesis was acquired by placing a metal plate across the expansion chamber so as to intercept the paths of the particles. Only a few good photographs have so far been obtained in which a positively curved track passes through the plate and remains in focus throughout its path, but these leave no doubt that the particles had their origin in or near the lead target and were therefore positively charged. In one case the track had a curvature on the target side of the plate, a sheet of copper 0·25 mm. thick, corresponding to a value of $H\rho$ of 12,700 ; on the other side the curvature gave a value $H\rho = 10{,}000$. This indicates that the particle travelled from the target through the copper plate, losing a certain amount of energy in the plate. The change in the value of $H\rho$ in passing through the copper is roughly the same as for a negative electron under similar conditions. The ionising power of the particle is also about the same as that for the negative electron. These observations are consistent with the assumption that the mass and magnitude of the charge of the positive particle are the same as for the negative electron.

The manner in which these positive electrons are produced is not yet clear, nor whether they arise from the action of the neutron emitted by the beryllium or from the action of the accompanying γ-radiation. It is hoped that further experiments now in progress will decide these questions.

Our thanks are due to Mr. Gilbert for his help in the experiments.

J. CHADWICK.
P. M. S. BLACKETT.
G. OCCHIALINI

Cavendish Laboratory,
Cambridge.
March 27.

[1] Anderson, *Science*, 76, 238 ; 1932.
[2] Blackett and Occhialini, *Proc. Roy. Soc.*, A, 139, 699 ; 1933.
[3] Curie and Joliot. "L'Existence du Neutron". Hermann et Cie, Paris.

131, 473; 1933

Magnetic Moment of the Proton

THE spin of the electron has the value $\frac{1}{2} \cdot \frac{h}{2\pi}$, and its magnetic moment has the value $2\,\frac{e}{m_e c} \cdot \frac{1}{2} \cdot \frac{h}{2\pi}$, or 1 Bohr magneton. The spin of the proton has the same value, $\frac{1}{2} \cdot \frac{h}{2\pi}$, as that of the electron. Thus for the magnetic moment of the proton the value $2\,\frac{e}{m_p c} \cdot \frac{1}{2} \cdot \frac{h}{2\pi}$ = 1/1840 Bohr magneton = 1 nuclear magneton is to be expected.

So far as we know, the only method at present available for the determination of this moment is the deflection of a beam of hydrogen molecules in an inhomogeneous magnetic field (Stern-Gerlach experiment). In the hydrogen molecule, the spins of the two electrons are anti-parallel and cancel out. Thus the magnetic moment of the molecule has two sources : (1) the rotation of the molecule as a whole, which is equivalent to the rotation of charged particles, and leads therefore to a magnetic moment as arising from a circular current ; and (2) the magnetic moments of the two protons.

In the case of para-hydrogen, the spins of the two protons are anti-parallel, their magnetic moments cancel out, and only the rotational moment remains. At low temperatures (liquid air temperature), practically all the molecules are in the rotational quantum state 0 and therefore non-magnetic. This has been proved by experiment. At higher temperatures (for example, room temperature) a certain proportion of the molecules, which may be calculated from Boltzmann's law, are in higher rotational quantum states, mainly in the state 2. The deflection experiments with para-hydrogen at room temperature allow, therefore, the determination of the rotational moment, which has been found to be between 0·8 and 0·9 nuclear magnetons per unit quantum number.

In the case of ortho-hydrogen, the lowest rotational quantum state possible is the state 1. Therefore, even at the lowest temperatures, the rotational magnetic moment is superimposed on that due to the two protons with parallel spin. Since, however, the rotational moment is known from the experiments with pure para-hydrogen, the moment of the protons can be determined from deflection experiments with ortho-hydrogen, or with ordinary hydrogen consisting of 75 per cent ortho- and 25 per cent para-hydrogen. The value obtained is 5 nuclear magnetons for the two protons in the ortho-hydrogen molecule, that is, 2·5 (and not 1) nuclear magnetons for the proton.

This is a very striking result, but further experiments carried out with increased accuracy and over a wide range of experimental conditions (such as temperature, width of beam, etc.) have shown that it is correct within a limit of less than 10 per cent.

A more detailed account of these experiments will appear in the *Zeitschrift für Physik*.

I. ESTERMANN.
R. FRISCH.
O. STERN.

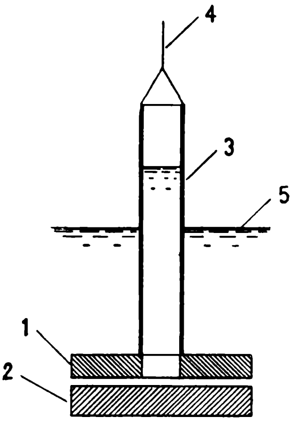
Institut für physikalische Chemie,
Hamburgischer Universität.
June 19.

132, 169; 1933

Viscosity of Liquid Helium below the λ-Point

THE abnormally high heat conductivity of helium II below the λ-point, as first observed by Keesom, suggested to me the possibility of an explanation in terms of convection currents. This explanation would require helium II to have an abnormally low viscosity; at present, the only viscosity measurements on liquid helium have been made in Toronto[1], and showed that there is a drop in viscosity below the λ-point by a factor of 3 compared with liquid helium at normal pressure, and by a factor of 8 compared with the value just above the λ-point. In these experiments, however, no check was made to ensure that the motion was laminar, and not turbulent.

The important fact that liquid helium has a specific density ρ of about 0·15, not very different from that of an ordinary fluid, while its viscosity μ is very small comparable to that of a gas, makes its kinematic viscosity $\nu=\mu/\rho$ extraordinary small. Consequently when the liquid is in motion in an ordinary viscosimeter, the Reynolds number may become very high, while in order to keep the motion laminar, especially in the method used in Toronto, namely, the damping of an oscillating cylinder, the Reynolds number must be kept very low. This requirement was not fulfilled in the Toronto experiments, and the deduced value of viscosity thus refers to turbulent motion, and consequently may be higher by any amount than the real value.



The very small kinematic viscosity of liquid helium II thus makes it difficult to measure the viscosity. In an attempt to get laminar motion the following method (shown diagramatically in the accompanying illustration) was devised. The viscosity was measured by the pressure drop when the liquid flows through the gap between the disks 1 and 2; these disks were of glass and were optically flat, the gap between them being adjustable by mica distance pieces. The upper disk, 1, was 3 cm. in diameter with a central hole of 1·5 cm. diameter, over which a glass tube (3) was fixed. Lowering and raising this plunger in the liquid helium by means of the thread (4), the level of the liquid column in the tube 3 could be set above or below the level (5) of the liquid in the surrounding Dewar flask. The amount of flow and the pressure were deduced from the difference of the two levels, which was measured by cathetometer.

The results of the measurements were rather striking. When there were no distance pieces between the disks, and the plates 1 and 2 were brought into contact (by observation of optical fringes, their separation was estimated to be about half a micron), the flow of liquid above the λ-point could be only just detected over several minutes, while below the λ-point the liquid helium flowed quite easily, and the level in the tube 3 settled down in a few seconds. From the measurements we can conclude that the viscosity of helium II is at least 1,500 times smaller than that of helium I at normal pressure.

The experiments also showed that in the case of helium II, the pressure drop across the gap was proportional to the square of the velocity of flow, which means that the flow must have been turbulent. If, however, we calculate the viscosity, assuming the flow to have been laminar, we obtain a value of the order 10^{-9} C.G.S., which is evidently still only an upper limit to the true value. Using this estimate, the Reynolds number, even with such a small gap, comes out higher than 50,000, a value for which turbulence might indeed be expected.

We are making experiments in the hope of still further reducing the upper limit to the viscosity of liquid helium II, but the present upper limit (namely, 10^{-9} C.G.S.) is already very striking, since it is more than 10^4 times smaller than that of hydrogen gas (previously thought to be the fluid of least viscosity). The present limit is perhaps sufficient to suggest, by analogy with supraconductors, that the helium below the λ-point enters a special state which might be called a 'superfluid'.

As we have already mentioned, an abnormally low viscosity such as indicated by our experiments might indeed provide an explanation for the high thermal conductivity, and for the other anomalous properties observed by Allen, Peierls, and Uddin[2]. It is evidently possible that the turbulent motion, inevitably set up in the technical manipulation required in working with the liquid helium II, might on account of the great fluidity, not die out, even in the small capillary tubes in which the thermal conductivity was measured; such turbulence would transport heat extremely efficiently by convection.

P. KAPITZA.

Institute for Physical Problems,
Academy of Sciences,
Moscow.
Dec. 3.

[1] Burton, NATURE, 135, 265 (1935); Wilhelm, Misener and Clark, Proc. Roy. Soc., A, 151, 342 (1935).
[2] NATURE, 140, 62 (1937).

141, 74; 1938

Energy obtained by Transmutation

MR. ROBERT D. POTTER, of 'Science Service', Washington, D.C., points out that the confirmation of the artificial breakdown of uranium announced in New York (see also NATURE, Feb. 11, p. 233) is in the direct succession of experiments carried out in recent years on the transmutation of the elements. For centuries, alchemists had dreamed of transmuting base metals into gold. It was imagined that enormous wealth would be at hand for the discoverer of this transmutation, and dire forecasts of the effects of this discovery were made, such as a complete revolution on the financial pattern of the world. We know that this transmutation has now been achieved for most of the known chemical elements. Transmutation's biggest result is the theoretical incentive it has provided for further physical researches.

143, 328; 1939

Origin of Stellar Energy

It is well known that matter consists of nuclei and electrons. Nevertheless it can be shown that in bodies of very large mass, this usual 'electronic' state of matter can become unstable. The reason for this lies in the fact that the 'electronic' state of matter does not lead to extremely great densities, because at such densities electrons form a Fermi gas having an immense pressure. On the other hand, it is easy to see that matter can go into another state which is much more compressible—the state where all the nuclei and electrons have combined to form neutrons.[1] Even if we assume that neutrons repel each other, this repulsion can become appreciable only at densities of the order of magnitude of nuclear densities, that is, 10^{14} gm./cm.^{3}, and the pressure of a Fermi gas consisting of neutrons is much less than that of an electronic gas of the same density, because of the greater mass of the neutrons.

Therefore, in spite of the fact that the 'neutronic' state of matter is, in usual conditions, energetically less favourable, since the reaction of neutron formation is strongly endothermic, this state must nevertheless become stable when the mass of the body is large enough. In this case, the gravitational energy gained in going over to the neutronic state with its greater density, compensates for the losses of internal energy.

It is easy to compute the critical mass of the body for which the 'neutronic' state begins to be more stable than the 'electronic' state. First of all we must calculate the energy necessary to form one neutron. For example, in the reaction $^{16}_{8}O + 8e^- = 16^1_0n$, we find from the mass defects that to form one neutron the energy required is 0·008 mass units or $1{\cdot}2 \, 10^{-5}$ ergs (7·5 Mv.). To transform one gram of matter into neutrons we thus need 7×10^{18} erg./gm.

Now we must calculate the gain in gravitational energy. The gravitational energy of the much less dense 'electronic' state can, of course, be neglected. Let us assume first of all that the neutronic state has a constant density, 10^{14} gm./cm.^{3}. The gravitational energy of a homogeneous sphere of mass M is then $3 \times 10^{-8} M^{5/3}$ ergs. For the stability of the neutronic phase we must then have $3 \times 10^{-8} M^{5/3} > 7 \times 10^{18} M$, or, $M > 10^{32}$ gm. $= 0{\cdot}05 \odot$, where $\odot$ is the mass of the sun. On the other hand, if we assume that the neutrons behave like a Fermi gas, we find for the energy $7 \times 10^{-22} M^{7/3}$ ergs and hence

$$M > 1{\cdot}5 \times 10^{30} = 10^{-3} \odot$$

which critical value is even less than on the first assumption.

When the mass of the body is greater than the critical mass, then in the formation of the 'neutronic' phase an enormous amount of energy is liberated, and we see that the conception of a 'neutronic' state of matter gives an immediate answer to the question of the sources of stellar energy. The sun during its probable time of radiation (about 2×10^9 years according to general relativity theory) must have emitted something of the order of magnitude of 3×10^{50} ergs. The liberation of this amount of energy requires the transition of only about 2 per cent of the mass of the sun (with the assumption of constant density) or even only $3 \times 10^{-3} \odot$ (with the Fermi gas model) to the 'neutronic' phase. Even for such a bright star as β Orionis, we find for the mass of the neutronic core only about $0{\cdot}1 \odot$ (with the Fermi gas model).

Thus we can regard a star as a body which has a neutronic core the steady growth of which liberates the energy which maintains the star at its high temperature; the condition at the boundary between the two phases is as usual the equality of chemical potentials. The detailed investigation of such a model should make possible the construction of a consistent theory of stars.

As regards the question of how the initial core is formed, I have already shown[2] that the formation of a core must certainly take place in a body with a mass greater than $1{\cdot}5 \odot$. In stars with smaller mass the conditions which make the formation of the initial core possible have yet to be made clear.

L. Landau.

Institute for Physical Problems,
Academy of Sciences,
Moscow.

[1] Cf. Hund, F., *Erg. d. exakten Naturw.* 15, 189 (1936).
[2] Landau, L., *Sov. Phys.*, 1, 285 (1932).

Bluffing by Eclipse Prediction

The communication by Prof. W. A. Osborne[1] has the effect of opening up the question whether the eclipse of the sun mentioned by Anna Comnena may be included in 'historical eclipses'. It is interesting to be able to state that her record seems to be quite sound. There was a total eclipse visible at Constantinople on February 16, 1086.

Most of us have obtained the small amount of knowledge we have about the Emperor and his daughter from the Waverley novel, "Count Robert of Paris". Scott for some reason chooses to describe the lady as being twenty-seven years of age, but he must have known that really her age was only fourteen years, at the time of the story, when the crusaders passed through Constantinople. He gives us a very delightful picture of the daughter reading a newly written chapter of her book to her father for his advice and approval; but here again he must have been equally well informed of the fact that she wrote the work after the emperor's death, when she had taken up her residence in a convent. Now we can all say that she must have had access to reliable records, which enabled her to state with assurance that "the whole [of the sun's] disk was darkened". Usually when a solar eclipse is mentioned by these early writers, the words used are so vague that it is impossible to say whether the eclipse was central or partial at any given locality. In the circumstances, some words of congratulation seem to be due to Anna Comnena and to the authorities who compiled the records from which she derived her information.

The total phase occurred at Constantinople about 4.12 p.m. local mean time. The track of the shadow from the Ægean to the Black Sea seems to be as follows:

Longitude	Latitude
24° 22′ E.	39° 34′ N
27° 17′ E.	41° 5′ N.
30° 17′ E.	42° 0′ N.
34° 6′ E.	43° 8′ N.

I obtained the date from the catalogue of eclipses in "L'Art de Vérifier les Dates", vol. 1. The calculations are my own.

C. J. Westland.

116 Western Road,
Christchurch, N.1,
New Zealand.
Dec. 30.

[1] Nature, 142, 837 (1938).

Disintegration of Uranium by Neutrons: a New Type of Nuclear Reaction

On bombarding uranium with neutrons, Fermi and collaborators[1] found that at least four radioactive substances were produced, to two of which atomic numbers larger than 92 were ascribed. Further investigations[2] demonstrated the existence of at least nine radioactive periods, six of which were assigned to elements beyond uranium, and nuclear isomerism had to be assumed in order to account for their chemical behaviour together with their genetic relations.

In making chemical assignments, it was always assumed that these radioactive bodies had atomic numbers near that of the element bombarded, since only particles with one or two charges were known to be emitted from nuclei. A body, for example, with similar properties to those of osmium was assumed to be eka-osmium ($Z = 94$) rather than osmium ($Z = 76$) or ruthenium ($Z = 44$).

Following up an observation of Curie and Savitch[3], Hahn and Strassmann[4] found that a group of at least three radioactive bodies, formed from uranium under neutron bombardment, were chemically similar to barium and, therefore, presumably isotopic with radium. Further investigation[5], however, showed that it was impossible to separate these bodies from barium (although mesothorium, an isotope of radium, was readily separated in the same experiment), so that Hahn and Strassmann were forced to conclude that *isotopes of barium* ($Z = 56$) *are formed as a consequence of the bombardment of uranium* ($Z = 92$) *with neutrons*.

At first sight, this result seems very hard to understand. The formation of elements much below uranium has been considered before, but was always rejected for physical reasons, so long as the chemical evidence was not entirely clear cut. The emission, within a short time, of a large number of charged particles may be regarded as excluded by the small penetrability of the 'Coulomb barrier', indicated by Gamov's theory of alpha decay.

On the basis, however, of present ideas about the behaviour of heavy nuclei[6], an entirely different and essentially classical picture of these new disintegration processes suggests itself. On account of their close packing and strong energy exchange, the particles in a heavy nucleus would be expected to move in a collective way which has some resemblance to the movement of a liquid drop. If the movement is made sufficiently violent by adding energy, such a drop may divide itself into two smaller drops.

In the discussion of the energies involved in the deformation of nuclei, the concept of surface tension of nuclear matter has been used[7] and its value has been estimated from simple considerations regarding nuclear forces. It must be remembered, however, that the surface tension of a charged droplet is diminished by its charge, and a rough estimate shows that the surface tension of nuclei, decreasing with increasing nuclear charge, may become zero for atomic numbers of the order of 100.

It seems therefore possible that the uranium nucleus has only small stability of form, and may, after neutron capture, divide itself into two nuclei of roughly equal size (the precise ratio of sizes depending on finer structural features and perhaps partly on chance). These two nuclei will repel each other and should gain a total kinetic energy of *c*. 200 Mev., as calculated from nuclear radius and charge. This amount of energy may actually be expected to be available from the difference in packing fraction between uranium and the elements in the middle of the periodic system. The whole 'fission' process can thus be described in an essentially classical way, without having to consider quantum-mechanical 'tunnel effects', which would actually be extremely small, on account of the large masses involved.

After division, the high neutron/proton ratio of uranium will tend to readjust itself by beta decay to the lower value suitable for lighter elements. Probably each part will thus give rise to a chain of disintegrations. If one of the parts is an isotope of barium[5], the other will be krypton ($Z = 92 - 56$), which might decay through rubidium, strontium and yttrium to zirconium. Perhaps one or two of the supposed barium-lanthanum-cerium chains are then actually strontium-yttrium-zirconium chains.

It is possible[8], and seems to us rather probable, that the periods which have been ascribed to elements beyond uranium are also due to light elements. From the chemical evidence, the two short periods (10 sec. and 40 sec.) so far ascribed to ^{239}U might be masurium isotopes ($Z = 43$) decaying through ruthenium, rhodium, palladium and silver into cadmium.

In all these cases it might not be necessary to assume nuclear isomerism; but the different radioactive periods belonging to the same chemical element may then be attributed to different isotopes of this element, since varying proportions of neutrons may be given to the two parts of the uranium nucleus.

By bombarding thorium with neutrons, activities are obtained which have been ascribed to radium and actinium isotopes[8]. Some of these periods are approximately equal to periods of barium and lanthanum isotopes[5] resulting from the bombardment of uranium. We should therefore like to suggest that these periods are due to a 'fission' of thorium which is like that of uranium and results partly in the same products. Of course, it would be especially interesting if one could obtain one of these products from a light element, for example, by means of neutron capture.

It might be mentioned that the body with half-life 24 min.[2] which was chemically identified with uranium is probably really ^{239}U, and goes over into an eka-rhenium which appears inactive but may decay slowly, probably with emission of alpha particles. (From inspection of the natural radioactive elements, ^{239}U cannot be expected to give more than one or two beta decays; the long chain of observed decays has always puzzled us.) The formation of this body is a typical resonance process[9]; the compound state must have a life-time a million times longer than the time it would take the nucleus to divide itself. Perhaps this state corresponds to some highly symmetrical type of motion of nuclear matter which does not favour 'fission' of the nucleus.

Lise Meitner.

Physical Institute,
Academy of Sciences,
Stockholm.

O. R. Frisch.

Institute of Theoretical Physics,
University,
Copenhagen.

Jan. 16.

[1] Fermi, E., Amaldi, F., d'Agostino, O., Rasetti, F., and Segrè, E. *Proc. Roy. Soc.*, A, 146, 483 (1934).

[2] See Meitner, L., Hahn, O., and Strassmann, F., *Z. Phys.*, 106, 249 (1937).

[3] Curie, I., and Savitch, P., *C.R.*, 208, 906, 1643 (1938).

[4] Hahn, O., and Strassmann, F., *Naturwiss.*, 26, 756 (1938).

[5] Hahn, O., and Strassmann, F., *Naturwiss.*, 27, 11 (1939).

[6] Bohr, N., Nature, 137, 344, 351 (1936).

[7] Bohr, N., and Kalckar, F., *Kgl. Danske Vid. Selskab, Math. Phys. Medd.*, 14, Nr. 10 (1937).

[8] See Meitner, L., Strassmann, F., and Hahn, O., *Z. Phys.*, 109, 538 (1938).

[9] Bethe, A. H., and Placzek, G., *Phys. Rev.*, 51, 450 (1937).

Physical Evidence for the Division of Heavy Nuclei under Neutron Bombardment

FROM chemical evidence, Hahn and Strassmann[1] conclude that radioactive barium nuclei (atomic number $Z = 56$) are produced when uranium ($Z = 92$) is bombarded by neutrons. It has been pointed out[2] that this might be explained as a result of a 'fission' of the uranium nucleus, similar to the division of a droplet into two. The energy liberated in such processes was estimated to be about 200 Mev., both from mass defect considerations and from the repulsion of the two nuclei resulting from the 'fission' process.

If this picture is correct, one would expect fast-moving nuclei, of atomic number about 40–50 and atomic weight 100–150, and up to 100 Mev. energy, to emerge from a layer of uranium bombarded with neutrons. In spite of their high energy, these nuclei should have a range, in air, of a few millimetres only, on account of their high effective charge (estimated to be about 20), which implies very dense ionization. Each such particle should produce a total of about three million ion pairs.

By means of a uranium-lined ionization chamber, connected to a linear amplifier, I have succeeded in demonstrating the occurrence of such bursts of ionization. The amplifier was connected to a thyratron which was biased so as to count only pulses corresponding to at least 5×10^5 ion pairs. About fifteen particles a minute were recorded when 300 mgm. of radium, mixed with beryllium, was placed one centimetre from the uranium lining. No pulses at all were recorded during repeated check runs of several hours total duration when either the neutron source or the uranium lining was removed. With the neutron source at a distance of four centimetres from the uranium lining, surrounding the source with paraffin wax enhanced the effect by a factor of two.

It was checked that the number of pulses depended linearly on the strength of the neutron source; this was done in order to exclude the possibility that the pulses are produced by accidental summation of smaller pulses. When the amplifier was connected to an oscillograph, the large pulses could be seen very distinctly on the background of much smaller pulses due to the alpha particles of the uranium.

By varying the bias of the thyratron, the maximum size of pulses was found to correspond to at least two million ion pairs, or an energy loss of 70 Mev. of the particle within the chamber. Since the longest path of a particle in the chamber was three centimetres and the chamber was filled with hydrogen at atmospheric pressure, the particles must ionize so heavily, in spite of their energy of at least 70 Mev., that they can make two million ion pairs on a path equivalent to 0·8 cm. of air or less. From this it can be estimated that the ionizing particles must have an atomic weight of at least about seventy, assuming a reasonable connexion between atomic weight and effective charge. This seems to be conclusive physical evidence for the breaking up of uranium nuclei into parts of comparable size, as indicated by the experiments of Hahn and Strassmann.

Experiments with thorium instead of uranium gave quite similar results, except that surrounding the neutron source with paraffin did not enhance, but slightly diminished, the effect. This gives evidence in favour of the suggestion[2] that also in the case of thorium, some, if not all, of the activities produced by neutron bombardment[3] should be ascribed to light elements. It should be remembered that no enhancement by paraffin has been found for the activities produced in thorium[3] (except for one which is isotopic with thorium and is almost certainly produced by simple capture of the neutron).

Prof. Meitner has suggested another interesting experiment. If a metal plate is placed close to a uranium layer bombarded with neutrons, one would expect an active deposit of the light atoms emitted in the 'fission' of the uranium to form on the plate. We hope to carry out such experiments, using the powerful source of neutrons which our high-tension apparatus will soon be able to provide.

O. R. FRISCH.

Institute of Theoretical Physics,
University,
Copenhagen.
Jan. 16.

[1] Hahn, O., and Strassmann, F., *Naturwiss.*, 27, 11 (1939).
[2] Meitner, L., and Frisch, O. R., NATURE [143, 239 (1939)].
[3] See Meitner, L., Strassmann, F., and Hahn, O., *Z. Phys.*, 109, 538 (1938).

Disintegration of Heavy Nuclei

THROUGH the kindness of the authors I have been informed of the content of the letters[1] recently sent to the Editor of NATURE by Prof. Meitner and Dr. Frisch. In the first letter, these authors propose an interpretation of the remarkable findings of Hahn and Strassmann as indication for a new type of disintegration of heavy nuclei, consisting in a fission of the nucleus into two parts of approximately equal masses and charges with release of enormous energy. In the second letter, Dr. Frisch describes experiments in which these parts are directly detected by the very large ionization they produce. Due to the extreme importance of this discovery, I should be glad to add a few comments on the mechanism of the fission process from the point of view of the general ideas, developed in recent years, to account for the main features of the nuclear reactions hitherto observed. *

These circumstances find their straightforward explanation in the fact, stressed by Meitner and Frisch, that the mutual repulsion between the electric charges in a nucleus will for highly charged nuclei counteract to a large extent the effect of the short-range forces between the nuclear particles in opposing a deformation of the nucleus. The nuclear problem concerned reminds us indeed in several ways of the question of the stability of a charged liquid drop, and in particular, any deformation of a nucleus, sufficiently large for its fission, may be treated approximately as a classical mechanical problem, since the corresponding amplitude must evidently be large compared with the quantum mechanical zero-point oscillations. Just this condition would in fact seem to provide an understanding of the remarkable stability of heavy nuclei in their normal state or in the states of low excitation, in spite of the large amount of energy which would be liberated by an imaginable division of such nuclei.

The continuation of the experiments on the new type of nuclear disintegrations, and above all the closer examination of the conditions for their occurrence, should certainly yield most valuable information as regards the mechanism of nuclear excitation.

N. BOHR.

At the Institute for Advanced Study,
Princeton, N.J. Jan. 20.

[1] [NATURE, 143, 239 and 275 (1939)].

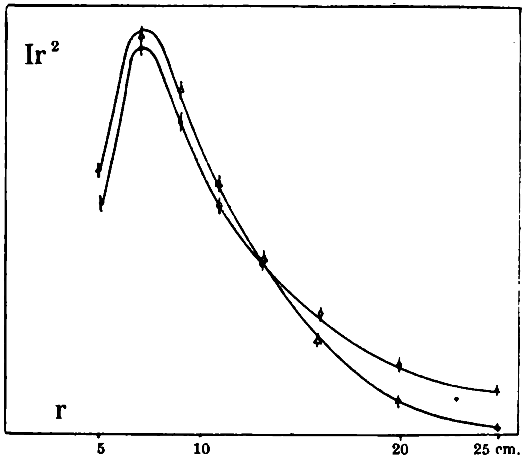
*Abridged

Liberation of Neutrons in the Nuclear Explosion of Uranium

RECENT experiments[1,2] have revealed the existence of a new kind of nuclear reaction: neutron bombardment of uranium and thorium leads to an explosion of the nucleus, which splits up into particles of inferior charge and weight, a considerable amount of energy being liberated in this process. Assuming a partition into two particles only, so that the nuclear mass and charge of uranium have to be distributed between two lighter nuclei, the latter contain considerably more neutrons than the heaviest stable isotopes with the same nuclear charges. (A splitting into, for example, ^{98}Rb and ^{141}Cs means an excess of 11 neutrons in the first, and of 8 neutrons in the second of these two nuclei.) There seem to be two possibilities of getting rid of this neutron excess. By the emission of a β-ray, a neutron is transformed into a proton, thus reducing the neutron excess by two units; in the example given above, five and four successive β-activities respectively would be needed to restore the neutron-proton stability ratio. In fact, the explosion products have been observed to be β-active and several periods have been recorded, so that a part, at least, of the neutron excess is certainly disposed of in this way. Another possible process is the direct liberation of neutrons, taking place either as a part of the explosion itself, or as an 'evaporation' from the resulting nuclei which would be formed in an excited state.

In order to find some evidence of this second phenomenon, we studied the density distribution of the thermal neutrons produced by the slowing down of photo-neutrons from a Ra γ - Be source in a 1·6 molar solution of uranyl nitrate and in a 1·6 molar solution of ammonium nitrate (the hydrogen contents of these two solutions differ by only 2 per cent). Plotting Ir^2 as a function of r (where r is the distance between the source and a given point, and I is the local density of thermal neutrons at the same point, measured by the activity induced in a dysprosium detector), a curve is obtained the area of which is proportional to $Q.\tau$, Q being the number of neutrons per second emitted by the source or formed in the solution and τ the mean time a neutron spends in the solution before being captured[3,4]. Any additional nuclei, which do not produce neutrons, brought into the solution, will increase the chances of capture and therefore decrease τ and the area. If, however, these dissolved nuclei are neutron-producing, Q will be greater and the area of the curve will tend to increase. Evidence of neutron production, as indicated by an actual increase of the area, will only be obtained if the gain through Q (neutron production) is greater than the loss through τ (neutron capture). This loss can anyway be studied separately, since it has been shown[5] that the introduction of nuclei which act merely by capture or by increasing the hydrogen content of the solution can affect the shape of the density curve only in a characteristic way: the modified curve can always be brought to coincide with the primitive curve by multiplying all abscissæ by a suitable factor and all ordinates by another factor.

The accompanying graph shows the two curves obtained. At small distances from the source the neutron density is greater in the ammonium solution than in the uranyl solution; at distances greater than 13 cm., the reverse is true. In other words, the decrease of the neutron density with the distance is appreciably slower in the uranyl solution.



△, Ammonium nitrate O, Uranyl nitrate

The observed difference must be ascribed to the presence of uranium. Since the two curves cannot be brought to coincide by the transformation mentioned above, the uranium nuclei do not act by capture only; an *elastic* diffusion by uranium nuclei would have an opposite effect: it would 'contract' the abscissæ, instead of stretching them. The density excess, shown by the uranyl curve beyond 13 cm., must therefore be considered as a proof of neutron production due to an interaction between the primary neutrons and the uranium nuclei. A reaction of the well-known $(n,2n)$ type is excluded because our primary neutrons are too slow for such a reaction (90 per cent of Ra + Be photo-neutrons have energies smaller than 0·5 Mev. and the remaining 10 per cent are slower than 1 Mev.).

The degree of precision of the experiment does not permit us to attribute any significance to the small increase of the area in the uranyl curve (as compared to the ammonium curve), which we obtain by extrapolating the curves towards greater distances. In any event, an inferior limit for the cross-section for the production of a neutron can be obtained by assuming that the density excess due to this production is equal throughout the whole curve to the excess observed at $r = 25$ cm.; this limit, certainly inferior to the actual value, is 6×10^{-25} cm.^{2}.

Our measurements yield no information on the energy of the neutrons produced. If, among these neutrons, some possess an energy superior to 2 Mev., one might hope to detect them by a (n,p) process, for example, by the $^{32}S(n,p)^{32}P$ reaction. An experiment of this kind, Ra γ - Be still being used as the primary neutron source, is under way.

The interest of the phenomenon observed as a step towards the production of exo-energetic transmutation chains is evident. However, in order to establish such a chain, more than one neutron must be produced for each neutron absorbed. This seems to be the case, since the cross-section for the liberation of a neutron seems to be greater than the cross-section for the production of an explosion. Experiments with solutions of varying concentration will give information on this question.

H. VON HALBAN, JUN.
F. JOLIOT.
L. KOWARSKI.

(See overleaf for address.)

Laboratoire de Chimie Nucléaire,
Collège de France,
Paris.
March 8.

[1] Joliot, F., *C.R.*, **208**, 341 (1939).
[2] Frisch, O. R., NATURE, **143**, 276 (1939).
[3] Amaldi, E., and Fermi, E., *Phys. Rev.* **50**, 899 (1936).
[4] Amaldi, E., Hafstad, L., and Tuve, M., *Phys. Rev.*, **51**, 896 (1937).
[5] Frisch, O. R., von Halban, jun., H., and Koch, J., *Danske Vidensk. Kab.*, **15**, 10 (1938).

Products of the Fission of the Uranium Nucleus

O. Hahn and F. Strassmann[1] have discovered a new type of nuclear reaction, the splitting into two smaller nuclei of the nuclei of uranium and thorium under neutron bombardment. Thus they demonstrated the production of nuclei of barium, lanthanum, strontium, yttrium, and, more recently, of xenon and cæsium.

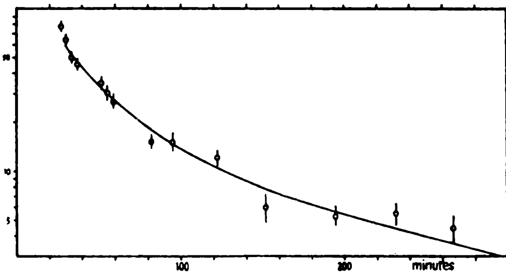
It can be shown by simple considerations that this type of nuclear reaction may be described in an essentially classical way like the fission of a liquid drop, and that the fission products must fly apart with kinetic energies of the order of hundred million electron-volts each[2]. Evidence for these high energies was first given by O. R. Frisch[3] and almost simultaneously by a number of other investigators[4].

The possibility of making use of these high energies in order to collect the fission products in the same way as one collects the active deposit from alpha-recoil has been pointed out by L. Meitner (see ref. 3). In the meantime, F. Joliot has independently made experiments of this type[5]. We have now carried out some experiments, using the recently completed high-tension equipment of the Institute of Theoretical Physics, Copenhagen.

A thin layer of uranium hydroxide, placed at a distance of 1 mm. from a collecting surface, was exposed to neutron bombardment. The neutrons were produced by bombarding lithium or beryllium targets with deuterons of energies up to 800 kilovolts. In the first experiments, a piece of paper was used as a collecting surface (after making sure that the paper did not get active by itself under neutron bombardment). About two minutes after interrupting the irradiation, the paper was placed near a Geiger-Müller counter with aluminium walls of 0·1 mm. thickness. We found a well-measurable activity which decayed first quickly (about two minutes half-value period) and then more slowly. No attempt was made to analyse the slow decay in view of the large number of periods to be expected.

The considerable intensity, however, of the collected activity encouraged us to try to get further information by chemical separations. The simplest experiment was to apply the chemical methods which have been developed in order to separate the 'transuranium' elements from uranium and elements immediately below it[6]. The methods had to be slightly modified on account of the absence of uranium in our samples and in view of the light element activities discovered by Hahn and Strassmann[1].

In these experiments, the collecting surface was water, contained in a shallow trough of paraffin wax. After irradiation (of about one hour) a small sample of the water was evaporated on a piece of aluminium foil; its activity was found to decay to zero. It was checked in other ways, too, that the water was not contaminated by uranium. To the rest of



the water we added 150 mgm. barium chloride, 15 mgm. lanthanum nitrate, 15 mgm. platinum chloride and enough hydrochloric acid to get an acid concentration of 7 per cent. Then the platinum was precipitated with hydrogen sulphide, in the usual way; the precipitate was carefully rinsed and dried and then placed near our counter.

The results of three such experiments were found to be in mutual agreement. The decay of the activity was in one case followed for 28 hours. For comparison, a sample of uranium irradiated for one hour was treated chemically in the same way. The two decay curves were in perfect agreement with one another and with an old curve obtained by Hahn, Meitner and Strassmann under the same conditions. In the accompanying diagram the circles represent our recoil experiment while the full line represents the uranium precipitate. A comparison of the activity (within the first hour after irradiation) of the precipitate and of the evaporated sample showed that the precipitate contained about two thirds of the total activity collected in the water. After about two hours, however, the evaporated sample was found to decay considerably more slowly than the precipitate, presumably on account of the more long-lived fission products found by Hahn and Strassmann[1].

From these results, it can be concluded that the 'transuranium' nuclei originate by fission of the uranium nucleus. Mere capture of a neutron would give so little kinetic energy to the nucleus that only a vanishing fraction of these nuclei could reach the water surface. So it appears that the 'transuranium' periods, too, will have to be ascribed to elements considerably lighter than uranium.

In conclusion, we wish to thank Dr. T. Bjerge, Dr. J. Koch and K. J. Brostrøm for putting the high-tension plant at our disposal and for kind help with the irradiations. We are also grateful to Prof. N. Bohr for the hospitality extended to us at the Institute of Theoretical Physics, Copenhagen.

LISE MEITNER.

Physical Institute,
Academy of Sciences,
Stockholm.

O. R. FRISCH.

Institute of Theoretical Physics,
University,
Copenhagen.
March 6.

[1] Hahn, O., and Strassmann, F., *Naturwiss.*, **27**, 11, 89, and 163 (1939).
[2] Meitner, L., and Frisch, O. R., NATURE, **143**, 239 (1939). Bohr, N., NATURE, **143**, 330 (1939).
[3] Frisch, O. R., NATURE, **143**, 276 (1939).
[4] Fowler, R. D., and Dodson, R. W., NATURE, **143**, 233 (1939). Jentschke, W., and Prankl, F., *Naturwiss.*, **27**, 134 (1939).
[5] Joliot, F., *C. R.*, **208**, 341 (1939).
[6] Hahn, O., Meitner, L., and Strassmann, F., *Chem. Ber.*, **69**, 905 (1936); and **70**, 1374 (1937).

Dielectric Constants of Some Titanates

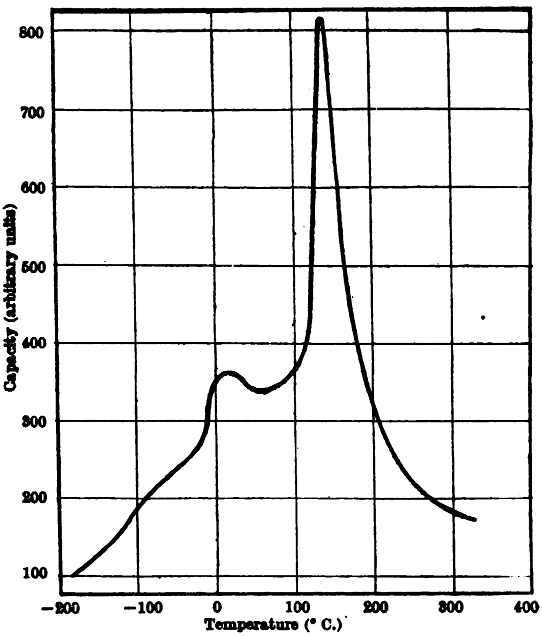
THE oxides and carbonates of beryllium, magnesium, cerium, zinc, strontium, cadmium and barium were heated with titanium oxide to a temperature of about 1,500° C. The dielectric constants of the titanates thus obtained were, for beryllium titanate 70, for magnesium 17, for calcium 115, for zinc 30, for strontium 155, for cadmium 62, exceeding 1,000 for barium. The measurements were carried out at room temperature at a frequency of 1 M Hz.

The titanates may be divided into two groups according to their place in the periodic system and the values of their dielectric constants. The titanates of beryllium, calcium, strontium and barium belong to the first group, and those of magnesium, zinc and cadmium to the second one. Such a division in general coincides with the division of the titanates according to the type of their crystal lattice. The crystal lattices of the titanates of calcium, strontium and barium are of the perovskite type, whereas those of magnesium and cadmium are of the ilmenite type. The atomic or ionic polarizability depends upon the structure of the crystal, and for this reason the magnitude of the dielectric constant depends upon the type of the lattice.

The dielectric constants of the perovskites investigated were found to grow with the increase of the size of the alkaline-earth ion. The electron polarizability of the ion increases with the radius. A more important factor, however, is that with the increase of the radius of the alkaline-earth ion located in the centre of the elementary cell, the distance between the titanium and oxygen ions increases as well.

Barium titanate having a very large dielectric constant, the distance between the titanium and the oxygen ions exceeds the sum of their radii. Such a 'loose' structure of the crystal lattice leads to considerable atomic polarizability.

The dielectric constant of barium titanate varies considerably with the temperature. The variation of the capacity of a barium titanate condenser with the temperature is shown in the accompanying graph.



The present investigation was carried out with J. M. Golgman. A more detailed article will be published in the *C.R. Acad. Sci. URSS*.

B. WUL.

Lebedev Physical Institute,
Academy of Sciences of the U.S.S.R.,
Moscow. June 18.

156, 480; 1945

Barium Titanate: a New Ferro-Electric

I HAVE earlier discussed the high permittivity of barium titanate and its temperature behaviour[1]. Additional experimental results such as the dependence of permittivity on the electrical field strength, the dielectric hysteresis loops and the maximum in the thermal capacity at the permittivity peak temperature show that this substance is a new type of ferro-electric. It should be noted that, as distinct from other known ferro-electrics, barium titanate does not contain hydrogen. For practical purposes, it is important to note that it may be used as an electrical insulator in which the ferro-electric properties are manifest over a wide temperature range. A detailed article will be published in the *Journal of Physics*.

Prof. W. Jackson and W. Reddish[2] have recently announced that the peak permittivity-temperature behaviour observed in barium titanate is also characteristic of the solid solution of $BaTiO_3-SrTiO_3$. It seems very probable that solid solutions of $BaTiO_3-SrTiO_3$ are ferro-electrics, as it is known[3] that mixed isomorphous crystals of Rochelle salt $NaKC_4H_4O_6.4H_2O$ and $NaNH_4C_4H_4O_6.4H_2O$ are ferro-electrics.

In my former communication[1], I mentioned the experimental result that the dielectric constant of titanates of perovskite structure were found to grow with an increase in the size of the alkaline-earth ion. It was clear that this referred to pure materials, and should not be extended to the permittivity-temperature behaviour of a mixed ferro-electric substance near the Curie point.

B. WUL

Lebedev Physical Institute,
Academy of Sciences of the U.S.S.R.,
Moscow.
April 17.

[1] Wul, B., *Nature*, **156**, 480 (1945).
[2] Jackson, W., and Reddish, W., *Nature*, **156**, 717 (1945).
[3] Eremeyev, M., and Kurtschatow, B., *Phys. Z. Sowjetunion*, **3**, 304 (1933).

157, 808; 1946

Australopithecinæ or Dartians

WHEN Prof. Raymond Dart, of the University of the Witwatersrand, Johannesburg, announced in *Nature*[1] the discovery of a juvenile *Australopithecus* and claimed for it a human kinship, I was one of those who took the point of view that when the adult form was discovered it would prove to be near akin to the living African anthropoids—the gorilla and chimpanzee[2]. Like Prof. Le Gros Clark[3], I am now convinced, on the evidence submitted by Dr. Robert Broom[4], that Prof. Dart was right and that I was wrong; the Australopithecinæ are in or near the line which culminated in the human form. My only complaint now is the length of the name which the extinct anthropoid of South Africa must for ever bear. Seeing that Prof. Dart not only discovered them but also rightly perceived their true nature, I have ventured, when writing of the Australopithecinæ, to give them the colloquial name of 'Dartians', thereby saving much expenditure of ink and of print. The Dartians are ground-living anthropoids, human in posture, gait and dentition, but still anthropoid in facial physiognomy and in size of brain. It is much easier to say there was a 'Dartian' phase in man's evolution than to speak of one which was 'australopithecine'.

ARTHUR KEITH
Downe, Kent.
Feb. 15.

[1] *Nature*, 115, 195 (1925).
[2] *Nature*, 115, 234 (1925).
[3] *Nature*, 159, 216 (1947).
[4] "The South African Fossil Ape-Men: The Australopithecinæ" (1946).

159, 377; 1947

OBSERVATIONS ON THE TRACKS OF SLOW MESONS IN PHOTOGRAPHIC EMULSIONS*

By C. M. G. LATTES, DR. G. P. S. OCCHIALINI and DR. C. F. POWELL

H. H. Wills Physical Laboratory, University of Bristol

INTRODUCTION. In recent experiments, it has been shown that charged mesons, brought to rest in photographic emulsions, sometimes lead to the production of secondary mesons. We have now extended these observations by examining plates exposed in the Bolivian Andes at a height of 5,500 m., and have found, in all, forty examples of the process leading to the production of secondary mesons. In eleven of these, the secondary particle is brought to rest in the emulsion so that its range can be determined. In Part 1 of this article, the measurements made on these tracks are described, and it is shown that they provide evidence for the existence of mesons of different mass. In Part 2, we present further evidence on the production of mesons, which allows us to show that many of the observed mesons are locally generated in the 'explosive' disintegration of nuclei, and to discuss the relationship of the different types of mesons observed in photographic plates to the penetrating component of the cosmic radiation investigated in experiments with Wilson chambers and counters.

* This article contains a summary of the main features of a number of lectures given, one at Manchester on June 18 and four at the Conference on Cosmic Rays and Nuclear Physics, organised by Prof. W. Heitler, at the Dublin Institute of Advanced Studies, July 5-12. A complete account of the observations, and of the conclusions which follow from them, will be published elsewhere.

160, 453; 1947

Part I. Existence of Mesons of Different Mass

As in the previous communications[1], we refer to any particle with a mass intermediate between that of a proton and an electron as a meson. It may be emphasized that, in using this term, we do not imply that the corresponding particle necessarily has a strong interaction with nucleons, or that it is closely associated with the forces responsible for the cohesion of nuclei.

We have now observed a total of 644 meson tracks which end in the emulsion of our plates. 451 of these were found, in plates of various types, exposed at an altitude of 2,800 m. at the Observatory of the Pic du Midi, in the Pyrenees; and 193 in similar plates exposed at 5,500 m. at Chacaltaya in the Bolivian Andes. The 451 tracks in the plates exposed at an altitude of 2,800 m. were observed in the examination of 5 c.c. emulsion. This corresponds to the arrival of about 1·5 mesons per c.c. per day, a figure which represents a lower limit, for the tracks of some mesons may be lost through fading, and through failure to observe tracks of very short range. The true number will thus be somewhat higher. In any event, the value is of the same order of magnitude as that we should expect to observe in delayed coincidence experiments at a height of 2,800 m., basing our estimates on the observations obtained in similar experiments at sea-level, and making reasonable assumptions about the increase in the number of slow mesons with altitude. It is therefore certain that the mesons we observe are a common constituent of the cosmic radiation.

Photomicrographs of two of the new examples of secondary mesons, Nos. III and IV, are shown in Figs. 1 and 2. Table 1 gives details of the characteristics of all events of this type observed up to the time of writing, in which the secondary particle comes to the end of its range in the emulsion.

TABLE 1

Event No.	Range in emulsion in microns of Primary meson	Secondary meson
I	133	613
II	84	565
III	1040	621
IV	133	591
V	117	638
VI	49	595
VII	460	616
VIII	900	610
IX	239	666
X	256	637
XI	81	590

Mean range 614 ± 8 μ. Straggling coefficient $\sqrt{\Sigma\Delta_i^2/n}$ = 4·3 per cent, where $\Delta_i = R_i - \bar{R}$, R_i being the range of a secondary meson, and $\bar{R}$ the mean value for n particles of this type.

The distribution in range of the secondary particles is shown in Fig. 3. The values refer to the lengths of the projections of the actual trajectories of the particles on a plane parallel to the surface of the emulsion. The true ranges cannot, however, be very different from the values given, for each track is inclined at only a small angle to the plane of the emulsion over the greater part of its length. In addition to the results for the secondary mesons which stop in the emulsion, and which are represented in Fig. 3 by black squares, the length of a number of tracks from the same process, which pass out of the emulsion when near the end of their range, are represented by open squares.

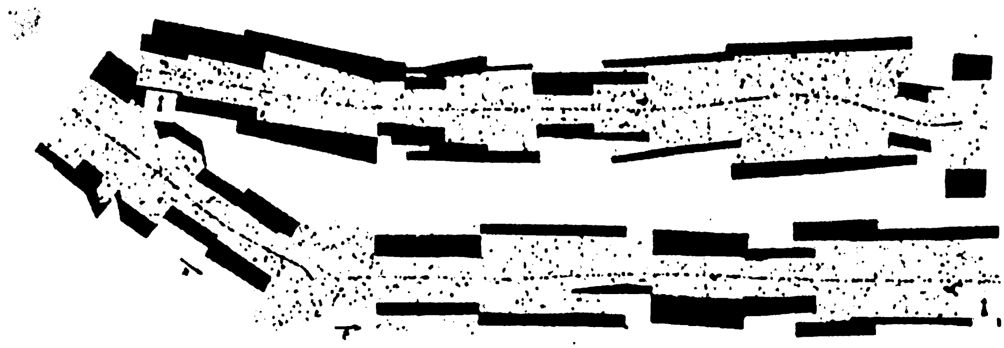


Fig. 1. OBSERVATION BY MRS. I. POWELL. COOKE × 95 ACHROMATIC OBJECTIVE; C2 ILFORD NUCLEAR RESEARCH EMULSION LOADED WITH BORON. THE TRACK OF THE μ-MESON IS GIVEN IN TWO PARTS, THE POINT OF JUNCTION BEING INDICATED BY *a* AND AN ARROW

The μ-Decay of Mesons

Two important conclusions follow from these measurements. Our observations show that the directions of ejection of the secondary mesons are orientated at random. We can therefore calculate the probability that the trajectory of a secondary meson, produced in a process of the type which we observe, will remain within the emulsion, of thickness 50 μ, for a distance greater than 500 μ. If we assume, as a first approximation, that the trajectories are rectilinear, we obtain a value for the probability of 1 in 20. The marked Coulomb scattering of mesons in the Nuclear Research emulsions will, in fact, increase the probability of 'escape'. The six events which we observe in plates exposed at 2,800 m., in which the secondary particle remains in the emulsion for a distance greater than 500 μ, therefore correspond to the occurrence in the emulsion of 120 $\pm$ 50 events of this particular type. Our observations, therefore, prove that the production of a secondary meson is a common mode of decay of a considerable fraction of those mesons which come to the end of their range in the emulsion.

Second, there is remarkable consistency between the values of the range of the secondary mesons, the variation among the individual values being similar to that to be expected from 'straggling', if the particles are always ejected with the same velocity. We can therefore conclude that the secondary mesons are all of the same mass and that they are emitted with constant kinetic energy.

If mesons of lower range are sometimes emitted in an alternative type of process, they must occur much less frequently than those which we have observed; for the geometrical conditions, and the greater average grain-density in the tracks, would provide much more favourable conditions for their detection. In fact, we have found no such mesons of shorter range. We cannot, however, be certain that mesons of greater range are not sometimes produced. Both the lower ionization in the beginning of the trajectory, and the even more unfavourable conditions of detection associated with the greater lengths of the tracks, would make such a group, or groups, difficult to observe. Because of the large fraction of the mesons which, as we have seen, can be attributed to the observed process, it is reasonable to assume that alternative modes of decay, if they exist, are much less frequent than that which we have observed. There is, therefore, good evidence for the production of a single homogeneous group of secondary mesons, constant in mass and kinetic energy. This strongly suggests a fundamental process, and not one involving an interaction of a primary meson with a particular type of nucleus in the emulsion. It is convenient to refer to this process

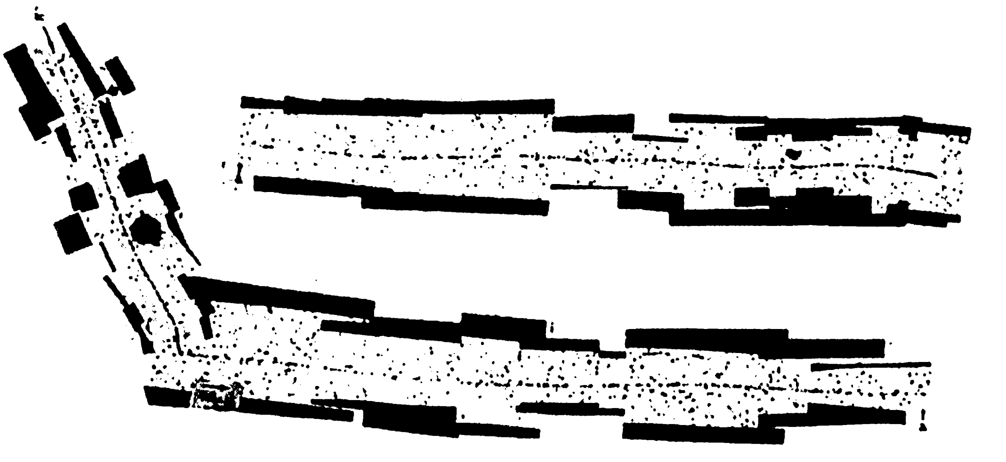


Fig. 2. COOKE × 95 ACHROMATIC OBJECTIVE. C2 ILFORD NUCLEAR RESEARCH EMULSION LOADED WITH BORON

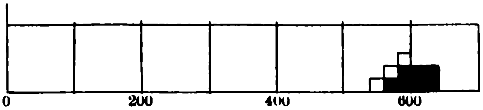


FIG. 3. DISTRIBUTION IN RANGE OF TEN SECONDARY MESONS. THOSE MARKED ■ STOP IN THE EMULSION; THE THREE MARKED □ LEAVE THE EMULSION WHEN NEAR THE END OF THEIR RANGE. MEAN RANGE OF SECONDARY MESONS, 606 MICRONS. THE RESULTS FOR EVENTS NOS. VIII TO XI ARE NOT INCLUDED IN THE FIGURE

in what follows as the μ-decay. We represent the primary mesons by the symbol π, and the secondary by μ. Up to the present, we have no evidence from which to deduce the sign of the electric charge of these particles. In every case in which they have been observed to come to the end of their range in the emulsion, the particles appear to stop without entering nuclei to produce disintegrations with the emission of heavy particles.

Knowing the range-energy relation for protons in the emulsion, the energy of ejection of the secondary mesons can be deduced from their observed range, if a value of the mass of the particles is assumed. The values thus calculated for various masses are shown in Table 2.

TABLE 2

Mass in m_e	100	150	200	250	300
Energy in MeV.	3·0	3·6	4·1	4·5	4·85

No established range-energy relation is available for protons of energies above 13 MeV., and it has therefore been necessary to rely on an extrapolation of the relation established for low energies. We estimate that the energies given in Table 2 are correct to within 10 per cent.

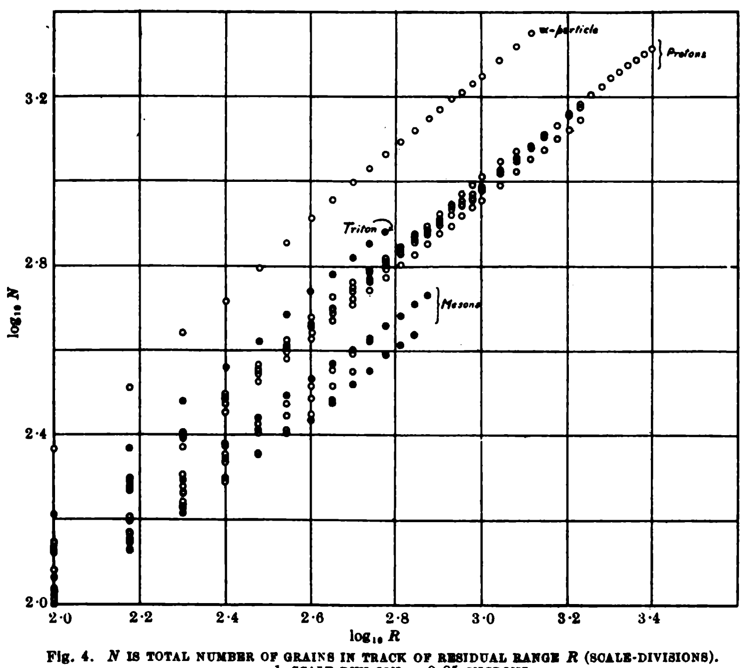


Fig. 4. N IS TOTAL NUMBER OF GRAINS IN TRACK OF RESIDUAL RANGE R (SCALE-DIVISIONS). 1 SCALE-DIVI ION = 0·85 MICRONS

Evidence of a Difference in Mass of π- and μ-Mesons

It has been pointed out[1] that it is difficult to account for the μ-decay in terms of an interaction of the primary meson with the nucleus of an atom in the emulsion leading to the production of an energetic meson of the same mass as the first. It was therefore suggested that the observations indicate the existence of mesons of different mass. Since the argument in support of this view relied entirely on the principle of the conservation of energy, a search was made for processes which were capable of yielding the necessary release of energy, irrespective of their plausibility on other grounds. Dr. F. C. Frank has re-examined such possibilities in much more detail, and his conclusions are given in an article to follow. His analysis shows that it is very difficult to account for our observations, either in terms of a nuclear disintegration, or of a 'building-up' process in which, through an assumed combination of a negative meson with a hydrogen nucleus, protons are enabled to enter stable nuclei of the light elements with the release of binding energy. We have now found it possible to reinforce this general argument for the existence of mesons of different mass with evidence based on grain-counts.

We have emphasized repeatedly[1] that it is necessary to observe great caution in drawing conclusions about the mass of particles from grain-counts. The main source of error in such determinations arises from the fugitive nature of the latent image produced in the silver halide granules by the passage of fast particles. In the case of the μ-decay process, however, an important simplification occurs. It is reasonable to assume that the two meson tracks are formed in quick succession, and are subject to the same degree of fading. Secondly, the complete double track in such an event is contained in a very small volume of the emulsion, and the processing conditions are therefore identical for both tracks, apart from the variation of the degree of development with depth. These features ensure that we are provided with very favourable conditions in which to determine the ratio of the masses of the π- and μ-mesons, in some of these events.

In determining the grain density in a track, we count the number of individual grains in successive intervals of length 50 μ along the trajectory, the observation being made with optical equipment giving large magnification ($\times$ 2,000), and the highest available resolving power. Typical results for protons and mesons are shown in Fig. 4. These results were obtained from observations on the tracks in a single plate, and it will be seen that there is satisfactory resolution between the curves for particles of different types. The 'spread' in the results for different particles of the same type can be attributed to the different degrees of fading associated with the different times of passage of the particles through the emulsion during an exposure of six weeks.

Applying these methods to the examples of the μ-decay process, in which the secondary mesons come to the end of their range in the emulsion, it is found that in every case the line representing the observations on the primary meson lies above that for the secondary particle. We can therefore conclude that there is a significant difference in the grain-density in the tracks of the primary and secondary mesons, and therefore a difference in the mass of the particles. This conclusion depends, of course, on the assumption that the π- and μ-particles carry equal charges. The grain-density at the ends of the tracks, of particles of both types, are consistent with the view that the charges are of magnitude $|e|$.

A more precise comparison of the masses of the π- and μ-mesons can only be made in those cases in which the length of the track of the primary meson in the emulsion is of the order of 600 μ. The probability of such a favourable event is rather small, and the only examples we have hitherto observed are those listed as Nos. III and VIII in Table 1. A mosaic of micrographs of a part only of the first of these events is reproduced in Fig. 1, for the length of the track of the μ-meson in the emulsion exceeds 1,000 μ. The logarithms of the numbers of grains in the tracks of the primary and secondary mesons in this event are plotted against the logarithm of the residual range in Fig. 5. By comparing the residual ranges at which the grain-densities in the two tracks have the same value, we can deduce the ratio of the masses. We thus obtain the result $m_\pi/m_\mu = 2\cdot 0$. Similar measurements on event No. VIII give the value 1·8. In considering the significance which can be attached to this result, it must be noticed that in addition to the standard deviations in the number of grains counted, there are other possible sources of error. Difficulties arise, for example, from the fact that the emulsions do not consist of a completely uniform distribution of silver halide grains. 'Islands' exist, in which the concentration of grains is significantly higher, or significantly lower, than the average values, the variations being much greater than those associated with random fluctuations. The measurements on the other examples of μ-decay are much less reliable on account of the restricted range of the π-mesons in the emulsion; but they give results lower than the above values. We think it unlikely, however, that the true ratio is as low as 1·5.

The above result has an important bearing on the interpretation of the μ-decay process. Let us assume that it corresponds to the spontaneous decay of the heavier π-meson, in which the momentum of the μ-meson is equal and opposite to that of an emitted photon. For any assumed value of the mass of the μ-meson, we can calculate the energy of ejection of the particle from its observed range, and thus determine its momentum. The momentum, and hence the energy of the emitted photon, is thus defined; the mass of the π-meson follows from the relation

$$c^2 m_\pi = c^2 m_\mu + E_\mu + h\nu.$$

It can thus be shown that the ratio m_π/m_μ is less than 1·45 for any assumed value of m_μ in the range from 100 to 300 m_e, m_e being the mass of the electron (see Table 3). A similar result is obtained if it is assumed that a particle of low mass, such as an electron or a neutrino, is ejected in the opposite direction to the μ-meson.

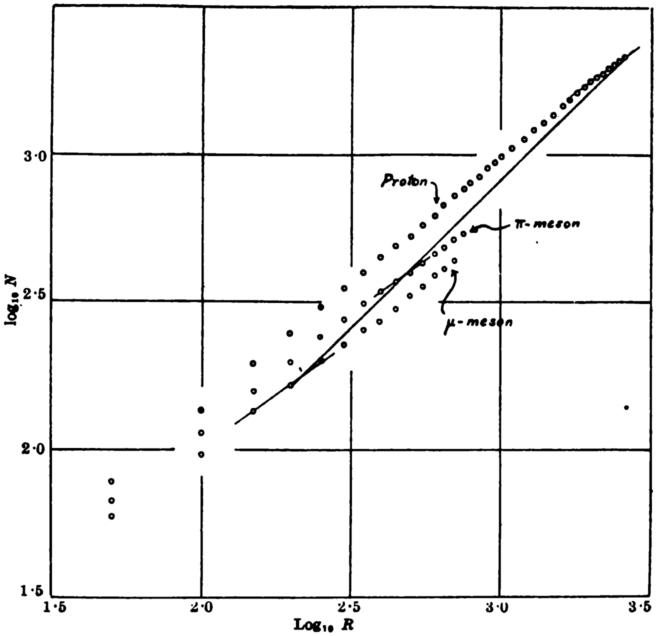


Fig. 5. N IS TOTAL NUMBER OF GRAINS IN TRACK OF RESIDUAL RANGE R (SCALE-DIVISIONS). 1 SCALE-DIVISION = 0·85 MICRONS
THE 45°-LINE CUTS THE CURVES OF THE MESONS AND PROTON IN THE REGION OF THE SAME GRAIN DENSITY

TABLE 3

Assumed mass m_μ	E (MeV.)	$h\nu$ (MeV.)	m_π	m_π/m_μ ± 3 per cent
100 m	3·0	17	140 m_e	1·40
150	3·6	23	208	1·35
200	4·1	29	264	1·32
250	4·5	34	325	1·30
300	4·85	39	387	1·29

On the other hand, if it is assumed that the momentum balance in the μ-decay is obtained by the emission of a neutral particle of mass equal to the μ-meson mass, the calculated ratio is about 2·1 : 1.

Our preliminary measurements appear to indicate, therefore, that the emission of the secondary meson cannot be regarded as due to a spontaneous decay of the primary particle, in which the momentum balance is provided by a photon, or by a particle of small rest-mass. On the other hand, the results are consistent with the view that a neutral particle of approximately the same rest-mass as the μ-meson is emitted. A final conclusion may become possible when further examples of the μ-decay, giving favourable conditions for grain-counts, have been discovered.

[1] *Nature*, **159**, 93, 186, 694 (1947).

MAGNETIC ANOMALIES OVER OCEANIC RIDGES

By F. J. VINE and Dr. D. H. MATTHEWS
Department of Geodesy and Geophysics, University of Cambridge

TYPICAL profiles showing bathymetry and the associated total magnetic field anomaly observed on crossing the North Atlantic and North-West Indian Oceans are shown in Fig. 1. They illustrate the essential features of magnetic anomalies over the oceanic ridges: (1) long-period anomalies over the exposed or buried foothills of the ridge; (2) shorter-period anomalies over the rugged flanks of the ridge; (3) a pronounced central anomaly associated with the median valley. This pattern has now been observed in the North Atlantic[1,2], the Antarctic[3], and the Indian Oceans[4,5]. In this article we describe an attempt to account for it.

The general increase in wave-length of the anomalies away from the crest of the ridge is almost certainly associated with the increase in depth to the magnetic crustal material[1]. Local anomalies of short-period may often be correlated with bathymetry, and explained in terms of reasonable susceptibility contrasts and crustal configurations; but the long-period anomalies of category (1) are not so readily explained. The central anomaly can be reproduced if it is assumed that a block of material very strongly magnetized in the present direction of the Earth's field underlies the median valley and produces a positive susceptibility contrast with the adjacent crust. It is not clear, however, why this considerable susceptibility contrast should exist beneath the median valley but not elsewhere under the ridge. Recent work in this Department has suggested a new mechanism.

In November 1962, H.M.S. *Owen* made a detailed magnetic survey over a central part of the Carlsberg Ridge as part of the International Indian Ocean Expedition. The area (50 × 40 nautical miles; centred on 5° 25′ N., 61° 45′ E.) is predominantly mountainous, depths ranging from 900 to 2,200 fathoms, and the topographic features are generally elongated parallel to the trend of the Ridge. This elongation is more marked on the total magnetic field anomaly map where a trough of negative anomalies, flanked by steep gradients, separates two areas of positive anomalies. The trough of negative anomalies corresponds to a general depression in the bottom topography which represents the median valley of the Ridge. The positive anomalies correspond to mountains on either side of the valley.

In this low magnetic latitude (inclination −6°) the effect of a body magnetized in the present direction of the Earth's field is to reduce the strength of the field above it, producing a negative anomaly over the body and a slight positive anomaly to the north. Here, over the centre of the Ridge, the bottom topography indicates the relief of basic extrusives such as volcanoes and fissure eruptives, and there is little sediment fill. The bathymetry, therefore, defines the upper surface of magnetic material having a considerable intensity of magnetization, potentially as high as any known igneous rock type[6], and probably higher, because it is extrusive, than the main crustal layer beneath. That the topographic features *are* capable of producing anomalies is immediately apparent on comparing the bathymetric and the anomaly charts; several have well-defined anomalies associated with them.

Two comparatively isolated volcano-like features were singled out and considered in detail. One has an associated negative anomaly as one would expect for normal magnetization, the other, completely the reverse anomaly pattern, that is, a pronounced positive anomaly suggesting reversed magnetization. Data on the topography of each feature and its associated anomaly were fed into a computer and an intensity and direction of magnetization for each obtained. Fig. 2 shows the directions of the resulting vectors plotted on a stereographic projection. Having computed the magnetic vector by a 'best fit' process, the computer recalculated the anomaly over the body, assuming this vector, thus giving an indication of the accuracy of fit. The fit was good for the case of reversed magnetization but poor for that of approximately normal magnetization. The discrepancy is scarcely surprising since we have ignored the effects of adjacent topography, and the interference of other anomalies in the vicinity. In addition, the example of normal magnetization is near a corner of the area where the control of contouring is less precise. The other example is central where the control is good. In both cases the intensity of magnetization deduced was about 0·005 e.m.u.; this is equivalent to an

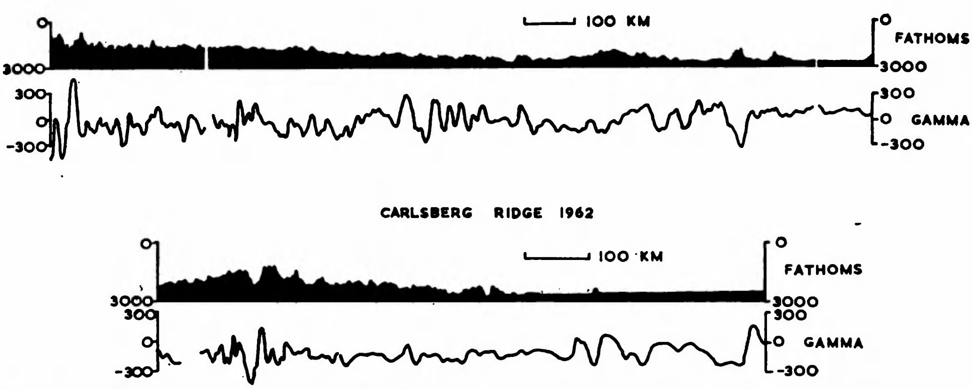


Fig. 1. Profiles showing bathymetry and the associated total magnetic field anomaly observed on crossing the North Atlantic and the north-west Indian Oceans. Upper profile from 45° 17′ N. 28° 27′ W. to 45° 19′ N. 11° 29′ W. Lower profile from 30° 5′ N. 61° 57′ E. to 10° 10′ N. 66° 27′ E.

199, 947; 1963

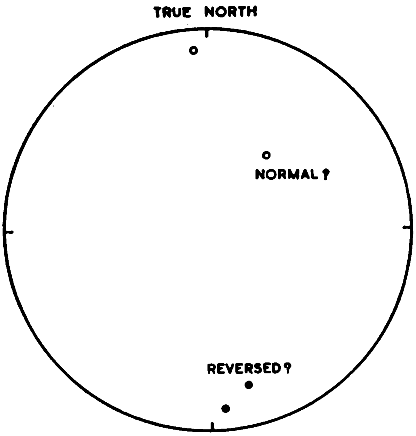


Fig. 2. Directions of the magnetic vectors obtained by the computer programme plotted on a stereographic projection, together with the present field vector and its reverse. Bearings and inclinations: present field vector 356°; −6° (up); computed vectors 038°; −40° (up); 166° 30′; +13° (down)

effective susceptibility of ± 0·0133: (effective susceptibility = total intensity of magnetization (remanent + induced)/present total magnetic field intensity : mean value for basalts of the order of 0·01).

In addition, three profiles, perpendicular to the trend of the Ridge, have been considered. Computed profiles along these, assuming infinite lateral extent of the bathymetric profile, and uniform normal magnetization, bear little resemblance to the observed profiles (Fig. 3). These results suggested that whole blocks of the survey area might be reversely magnetized. The dotted curve in Fig. 3 *B* was computed for a model in which the main crustal layer and overlying volcanic terrain were divided into blocks about 20 km wide, alternately normally and reversely magnetized. The blocks were given the effective susceptibility values shown in the caption to Fig. 4 (3).

Work on this survey led us to suggest that some 50 per cent of the oceanic crust might be reversely magnetized and this in turn has suggested a new model to account for the pattern of magnetic anomalies over the ridges.

The theory is consistent with, in fact virtually a corollary of, current ideas on ocean floor spreading[7] and periodic reversals in the Earth's magnetic field[8]. If the main crustal layer (seismic layer 3) of the oceanic crust is formed over a convective up-current in the mantle at the centre of an oceanic ridge, it will be magnetized in the current direction of the Earth's field. Assuming impermanence of the ocean floor, the whole of the oceanic crust is comparatively young, probably not older than 150 million years, and the thermo-remanent component of its magnetization is therefore either essentially normal, or reversed with respect to the present field of the Earth. Thus, if spreading of the ocean floor occurs, blocks of alternately normal and reversely magnetized material would drift away from the centre of the ridge and parallel to the crest of it.

This configuration of magnetic material could explain the lineation or 'grain' of magnetic anomalies observed over the Eastern Pacific to the west of North America[6]

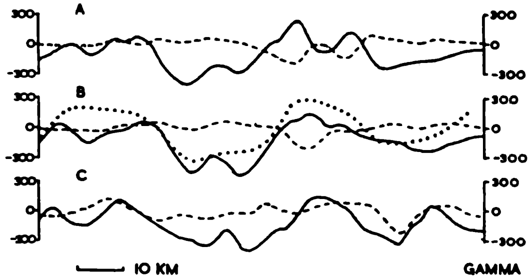


Fig. 3. Observed and computed profiles across the crest of the Carlsberg Ridge. Solid lines, observed anomaly; broken lines, computed profile assuming uniform normal magnetization and an effective susceptibility of 0·0133; dotted line, assuming reversals—see text. The computed profiles were obtained assuming infinite lateral extent of the bathymetric profiles

(probably equivalent to the long-period anomalies of category (1)). Here north–south highs and lows of varying width, usually of the order of 20 km, are bounded by steep gradients. The amplitude and form of these anomalies have been reproduced by Mason[9,10], but the most plausible of the models used involved very severe restrictions on the distribution of lava flows in crustal layer 2. They are readily explained in terms of reversals assuming the model shown in Fig. 4 (1). It can be shown that this type of anomaly pattern will be produced for virtually all orientations and magnetic latitudes, the amplitude decreasing as the trend of the ridge approaches north–south or the profile approaches the magnetic equator. The pronounced central anomaly over the ridges is also readily

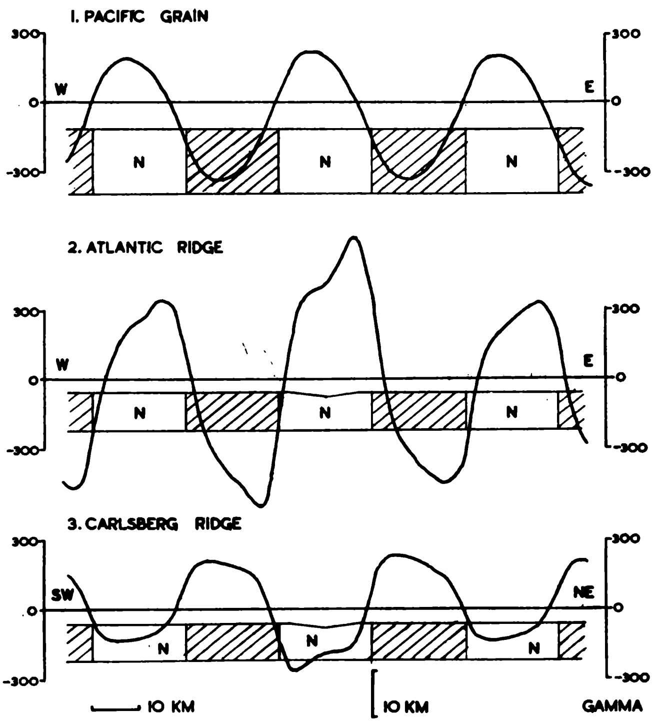


Fig. 4. Magnetic profiles computed for various crustal models. Crustal blocks marked *N*, normally magnetized; diagonally shaded blocks, reversely magnetized. Effective susceptibility of blocks, 0·0027, except for the block under the median valley in profiles 2 and 3, 0·0053

(1) Pacific Grain. Total field strength, T = 0·5 œrsted; inclination, I = 60°; magnetic bearing of profile, θ = 073°. (2) Mid-Atlantic Ridge, T = 0·48 œrsted; I = 65°; θ = 120°. (3) Carlsberg Ridge, T = 0·376 œrsted; I = −6°; θ = 044°

explained in terms of reversals. The central block, being most recent, is the only one which has a uniformly directed magnetic vector. This is comparable to the area of normally magnetized late Quaternary basics in Central Iceland[11,12] on the line of the Mid-Atlantic Ridge. Adjacent and all other blocks have doubtless been subjected to subsequent vulcanism in the form of volcanoes, fissure eruptions, and lava flows, often oppositely magnetized and hence reducing the effective susceptibility of the block, whether initially normal or reversed. The effect of assuming a reduced effective susceptibility for the adjacent blocks is illustrated for the North Atlantic and Carlsberg Ridges in Fig. 4 (2, 3).

In Fig. 4, no attempt has been made to reproduce observed profiles in detail, the computations simply show that the essential form of the anomalies is readily achieved. The whole of the magnetic material of the oceanic crust is probably of basic igneous composition; however, variations in its intensity of magnetization and in the topography and direction of magnetization of surface extrusives could account for the complexity of the observed profiles. The results from the preliminary Mohole drilling[13,14] are considered to substantiate this conception. The drill penetrated 40 ft. into a basalt lava flow at the bottom of the hole, and this proved to be reversely magnetized[13]. Since the only reasonable explanation of the magnetic anomalies mapped near the site of the drilling is that the area is underlain by a block of normally magnetized crustal material[14], it appears that the drill penetrated a layer of reversely magnetized lava overlying a normally magnetized block.

In Fig. 4 it will also be noticed that the effective susceptibilities assumed are two to five times less than that derived for the isolated features in the survey area described. Although no great significance can be attached to this derived intensity it is suggested that the fine-grained extrusives (basalts) of surface features are more highly magnetized than the intrusive material of the main crustal layer which, in the absence of evidence to the contrary, we assume to be of analogous chemical composition (that is, gabbros). This would appear to be consistent with recent investigations of the magnetic properties of basic rocks[6].

The vertical extent of the magnetic crust is defined by the depth to the curie-point isotherm. In the models this has been assumed to be at 20 km below sea-level over the deep ocean but at a depth of 11 km beneath the centre of the ridges where the heat flow and presumably the thermal gradient are higher. These assumptions are questionable but not critical because the amplitude of the simulated anomaly depends on both the thickness of the block and its effective susceptibility, and, although the thickness is in doubt by a factor of two, the susceptibility is in doubt by a factor of ten. Present magnetic declination has been assumed throughout the calculations: it would probably have been better to have ignored this, as in palæomagnetism, assuming that true north approximates to the mean of secular variations; but this is unimportant and in no way affects the essential features of the computations.

In order to explain the steep gradients and large amplitudes of magnetic anomalies observed over oceanic ridges all authors have been compelled to assume vertical boundaries and high-susceptibility contrasts between adjacent crustal blocks. It is appreciated that magnetic contrasts within the oceanic crust can be explained without postulating reversals of the Earth's magnetic field; for example, the crust might contain blocks of very strongly magnetized material adjacent to blocks of material weakly magnetized in the same direction. However, the model suggested in this article seems to be more plausible because high susceptibility contrasts between adjacent blocks can be explained without recourse to major inhomogeneities of rock type within the main crustal layer or to unusually strongly magnetized rocks.

We thank Dr. R. G. Mason and K. Kunaratnam of the Imperial College of Science and Technology, London, for details of the three-dimensional programme used in this work. The programme was originally devised by K. Kunaratnam for a Ferranti *Mercury* Computer. It has been rewritten for use on *Edsac* 2. We also thank the Director of the Cambridge University Mathematical Laboratory for permission to use *Edsac* 2, and Sir Edward Bullard for his advice and encouragement throughout.

This work was partly supported by a grant from the U.S. Office of Naval Research (Contract No. *N*62558–3542).

1 Heezen, B. C., Ewing, M., and Miller, E. T., *Deep Sea Res.*, 1, 25 (1953).
2 Keen, M. J., *Nature*, 197, 888 (1963).
3 Adams, R. D., and Christoffel, D. A., *J. Geophys. Res.*, 67, 805 (1962).
4 Heirtzler, J. R., *Tech. Rep.* No. 2, *Lamont Geol. Obs., New York* (1961).
5 Matthews, D. H., *et al.*, *Admiralty Marine Sci. Pub. No.* 4 (in the press).
6 Bullard, E. C., and Mason, R. G., *The Sea*, 3, edit. by Hill, M. N. (in the press).
7 Dietz, R. S., *Nature*, 190, 854 (1961).
8 Cox, A., Doell, R. R., and Dalrymple, G. B., *Nature*, 198, 1049 (1963).
9 Mason, R. G., *Geophys. J.*, 1, 320 (1958).
10 Mason, R. G., and Raff, A. D., *Bull. Geol. Soc. Amer.*, 72, 1259 (1961).
11 Hospers, J., *Geol. Mag.*, 91, 352 (1954).
12 Thorarinsson, S., Einarsson, T., and Kjartansson, G., *Intern. Geog. Cong.* (*Norden*), Excursion E.I.1 (1960).
13 Cox, A., and Doell, R. R., *J. Geophys. Res.*, 67, 3997 (1962).
14 Raff, A. D., *J. Geophys. Res.*, 68, 955 (1963).

CRYSTALLOGRAPHY

Whisker Growth from Quartz

THE growth of whiskers from fused quartz has recently been reported by Jaccodine and Kline[1]. Whiskers have also been observed on single crystals of quartz prepared by the hydrothermal process[2]. X-ray examination has now revealed the nature of these growths.

Two whiskers, about 17μ and 35μ respectively in diameter, obtained from the furnace process, were each found to be single crystals of high-cristobalite, with the whisker growth axis along [111]. The cubic lattice constant $a = 6{\cdot}99 \pm 0{\cdot}02$ Å., as compared with the literature value[3] of 7·0459 Å. Precession photographs, with the whisker growth axis along the spindle axis, contained only sharp Bragg reflexions; more than a single reciprocal layer appeared for any given orientation. Reflexions obtained on the rotation photographs taken about [111] were drawn out along the row lines through an angle of about 4°, indicating a lack of long-range order normal to this axis Both whiskers were isotropic, viewed in polarized light.

A single whisker of about 50μ diameter, produced in the hydrothermal process, was identified as α-quartz, with the whisker growth axis along *c*. The hexagonal lattice constants were measured as $a = 4{\cdot}93 \pm 0{\cdot}02$, $c = 5{\cdot}41 \pm 0{\cdot}02$ Å., in agreement with the literature values[4] of 4·90333 and 5·40485 Å. respectively.

Mr. F. A. Barbieri took the X-ray photographs used in this work.

S. C. ABRAHAMS
C. D. STOCKBRIDGE

Bell Telephone Laboratories,
Murray Hill, New Jersey,
and
Whippany,
New Jersey.

1 Jaccodine, R. J., and Kline, R. K., *Nature*, 189, 298 (1961).
2 Buehler, E., and Walker, A. C., *Sci. Monthly*, 69, 148 (1949); *Indust. Eng. Chem.*, 42, 1369 (1950).
3 Lukesh, J., *Amer. Min.*, 27, 226 (1942).
4 Cooper, A. S., *Acta Cryst.*, 15 (in the press).

Observation of a Rapidly Pulsating Radio Source

by

A. HEWISH
S. J. BELL
J. D. H. PILKINGTON
P. F. SCOTT
R. A. COLLINS

Mullard Radio Astronomy Observatory,
Cavendish Laboratory,
University of Cambridge

Unusual signals from pulsating radio sources have been recorded at the Mullard Radio Astronomy Observatory. The radiation seems to come from local objects within the galaxy, and may be associated with oscillations of white dwarf or neutron stars.

In July 1967, a large radio telescope operating at a frequency of 81·5 MHz was brought into use at the Mullard Radio Astronomy Observatory. This instrument was designed to investigate the angular structure of compact radio sources by observing the scintillation caused by the irregular structure of the interplanetary medium[1]. The initial survey includes the whole sky in the declination range $-08° < \delta < 44°$ and this area is scanned once a week. A large fraction of the sky is thus under regular surveillance. Soon after the instrument was brought into operation it was noticed that signals which appeared at first to be weak sporadic interference were repeatedly observed at a fixed declination and right ascension; this result showed that the source could not be terrestrial in origin.

Systematic investigations were started in November and high speed records showed that the signals, when present, consisted of a series of pulses each lasting $\sim 0·3$ s and with a repetition period of about 1·337 s which was soon found to be maintained with extreme accuracy. Further observations have shown that the true period is constant to better than 1 part in 10^7 although there is a systematic variation which can be ascribed to the orbital motion of the Earth. The impulsive nature of the recorded signals is caused by the periodic passage of a signal of descending frequency through the 1 MHz pass band of the receiver.

The remarkable nature of these signals at first suggested an origin in terms of man-made transmissions which might arise from deep space probes, planetary radar or the reflexion of terrestrial signals from the Moon. None of these interpretations can, however, be accepted because the absence of any parallax shows that the source lies far outside the solar system. A preliminary search for further pulsating sources has already revealed the presence of three others having remarkably similar properties which suggests that this type of source may be relatively common at a low flux density. A tentative explanation of these unusual sources in terms of the stable oscillations of white dwarf or neutron stars is proposed.

Position and Flux Density

The aerial consists of a rectangular array containing 2,048 full-wave dipoles arranged in sixteen rows of 128 elements. Each row is 470 m long in an E.–W. direction and the N.–S. extent of the array is 45 m. Phase-scanning is employed to direct the reception pattern in declination and four receivers are used so that four different declinations may be observed simultaneously. Phase-switching receivers are employed and the two halves of the aerial are combined as an E.–W. interferometer. Each row of dipole elements is backed by a tilted reflecting screen so that maximum sensitivity is obtained at a declination of approximately $+30°$, the overall sensitivity being reduced by more than one-half when the beam is scanned to declinations above $+90°$ and below $-5°$. The beamwidth of the array to half intensity is about $\pm\frac{1}{2}°$ in right ascension and $\pm 3°$ in declination; the phasing arrangement is designed to produce beams at roughly 3° intervals in declination. The receivers have a bandwidth of 1 MHz centred at a frequency of 81·5 MHz and routine recordings are made with a time constant of 0·1 s; the r.m.s. noise fluctuations correspond to a flux density of $0·5 \times 10^{-26}$ W m^{-2} Hz^{-1}. For detailed studies of the pulsating source a time constant of 0·05 s was usually employed and the signals were displayed on a multi-channel 'Rapidgraph' pen recorder with a time constant of 0·03 s. Accurate timing of the pulses was achieved by recording second pips derived from the *MSF* Rugby time transmissions.

A record obtained when the pulsating source was unusually strong is shown in Fig. 1*a*. This clearly displays the regular periodicity and also the characteristic irregular variation of pulse amplitude. On this occasion the largest pulses approached a peak flux density (averaged over the 1 MHz pass band) of 20×10^{-26} W m^{-2} Hz^{-1}, although the mean flux density integrated over one minute only amounted to approximately $1·0 \times 10^{-26}$ W m^{-2} Hz^{-1}. On a more typical occasion the integrated flux density would be several times smaller than this value. It is

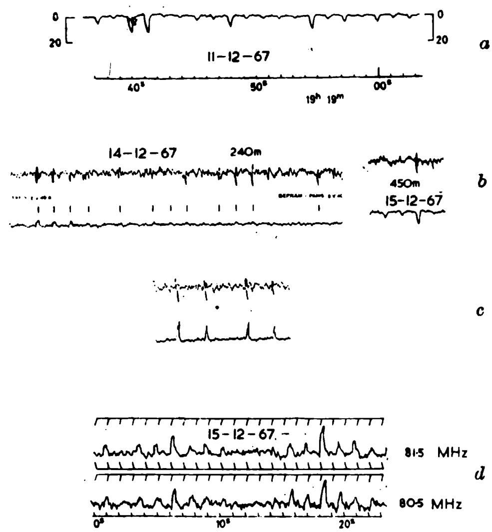


Fig. 1. *a*, A record of the pulsating radio source in strong signal conditions (receiver time constant 0·1 s). Full scale deflexion corresponds to 20×10^{-26} W m^{-2} Hz^{-1}. *b*, Upper trace: records obtained with additional paths (240 m and 450 m) in one side of the interferometer. Lower trace: normal interferometer records. (The pulses are small for $l = 240$ m because they occurred near a null in the interference pattern; this modifies the phase but not the amplitude of the oscillatory response on the upper trace.) *c*, Simulated pulses obtained using a signal generator. *d*, Simultaneous reception of pulses using identical receivers tuned to different frequencies. Pulses at the lower frequency are delayed by about 0·2 s.

therefore not surprising that the source has not been detected in the past, for the integrated flux density falls well below the limit of previous surveys at metre wavelengths.

The position of the source in right ascension is readily obtained from an accurate measurement of the "cross-over" points of the interference pattern on those occasions when the pulses were strong throughout an interval embracing such a point. The collimation error of the instrument was determined from a similar measurement on the neighbouring source 3C 409 which transits about 52 min later. On the routine recordings which first revealed the source the reading accuracy was only ±10 s and the earliest record suitable for position measurement was obtained on August 13, 1967. This and all subsequent measurements agree within the error limits. The position in declination is not so well determined and relies on the relative amplitudes of the signals obtained when the reception pattern is centred on declinations of 20°, 23° and 26°. Combining the measurements yields a position

$$\alpha_{1950} = 19\text{h}\ 19\text{m}\ 38\text{s} \pm 3\text{s}$$

$$\delta_{1950} = 22^\circ\ 00' \pm 30'$$

As discussed here, the measurement of the Doppler shift in the observed frequency of the pulses due to the Earth's orbital motion provides an alternative estimate of the declination. Observations throughout one year should yield an accuracy of ±1′. The value currently attained from observations during December–January is $\delta = 21^\circ\ 58' \pm 30'$, a figure consistent with the previous measurement.

Time Variations

It was mentioned earlier that the signals vary considerably in strength from day to day and, typically, they are only present for about 1 min, which may occur quite randomly within the 4 min interval permitted by the reception pattern. In addition, as shown in Fig. 1a, the pulse amplitude may vary considerably on a time-scale of seconds. The pulse to pulse variations may possibly be explained in terms of interplanetary scintillation[1], but this cannot account for the minute to minute variation of mean pulse amplitude. Continuous observations over periods of 30 min have been made by tracking the source with an E.–W. phased array in a 470 m × 20 m reflector normally used for a lunar occultation programme. The peak pulse amplitude averaged over ten successive pulses for a period of 30 min is shown in Fig. 2a. This plot suggests the possibility of periodicities of a few minutes duration, but a correlation analysis yields no significant result. If the signals were linearly polarized, Faraday rotation in the ionosphere might cause the random variations, but the form of the curve does not seem compatible with this mechanism. The day to day variations since the source was first detected are shown in Fig. 2b. In this analysis the daily value plotted is the peak flux density of the greatest pulse. Again the variation from day to day is irregular and no systematic changes are clearly evident, although there is a suggestion that the source was significantly weaker during October to November. It therefore appears that, despite the regular occurrence of the pulses, the magnitude of the power emitted exhibits variations over long and short periods.

Instantaneous Bandwidth and Frequency Drift

Two different experiments have shown that the pulses are caused by a narrow-band signal of descending frequency sweeping through the 1 MHz band of the receiver. In the first, two identical receivers were used, tuned to frequencies of 80·5 MHz and 81·5 MHz. Fig. 1d, which illustrates a record made with this system, shows that

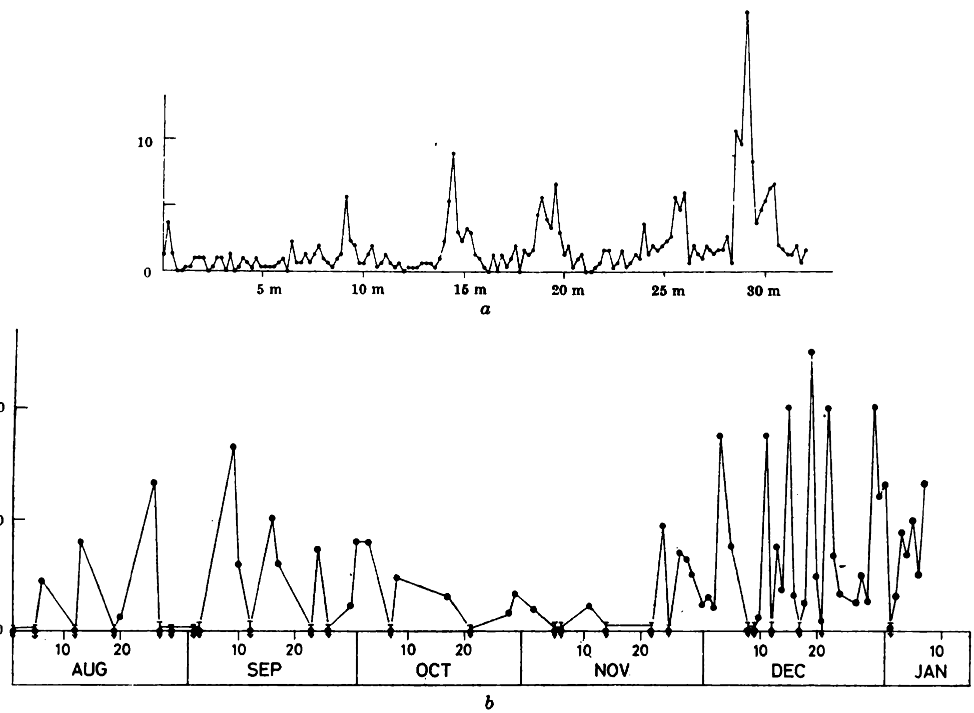


Fig. 2. a, The time variation of the smoothed (over ten pulses) pulse amplitude. b, Daily variation of peak pulse amplitude. (Ordinates are in units of W m^{-2} Hz^{-1} × 10^{-26}.)

the lower frequency pulses are delayed by about 0·2 s. This corresponds to a frequency drift of ~ -5 MHz s^{-1}. In the second method a time delay was introduced into the signals reaching the receiver from one-half of the aerial by incorporating an extra cable of known length l. This cable introduces a phase shift proportional to frequency so that, for a signal the coherence length of which exceeds l, the output of the receiver will oscillate with period

$$t_0 = \frac{c}{l}\left(\frac{d\nu}{dt}\right)^{-1}$$

where $d\nu/dt$ is the rate of change of signal frequency. Records obtained with $l = 240$ m and 450 m are shown in Fig. 1*b* together with a simultaneous record of the pulses derived from a separate phase-switching receiver operating with equal cables in the usual fashion. Also shown, in Fig. 1*c*, is a simulated record obtained with exactly the same arrangement but using a signal generator, instead of the source, to provide the swept frequency. For observation with $l > 450$ m the periodic oscillations were slowed down to a low frequency by an additional phase shifting device in order to prevent severe attenuation of the output signal by the time constant of the receiver. The rate of change of signal frequency has been deduced from the additional phase shift required and is $d\nu/dt = -4{\cdot}9 \pm 0{\cdot}5$ MHz s^{-1}. The direction of the frequency drift can be obtained from the phase of the oscillation on the record and is found to be from high to low frequency in agreement with the first result.

The instantaneous bandwidth of the signal may also be obtained from records of the type shown in Fig. 1*b* because the oscillatory response as a function of delay is a measure of the autocorrelation function, and hence of the Fourier transform, of the power spectrum of the radiation. The results of the measurements are displayed in Fig. 3 from which the instantaneous bandwidth of the signal to exp (-1), assuming a Gaussian energy spectrum, is estimated to be 80 ± 20 kHz.

Pulse Recurrence Frequency and Doppler Shift

By displaying the pulses and time pips from *MSF* Rugby on the same record the leading edge of a pulse of reasonable size may be timed to an accuracy of about

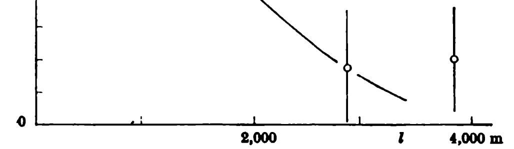


Fig. 3. The response as a function of added path in one side of the interferometer.

0·1 s. Observations over a period of 6 h taken with the tracking system mentioned earlier gave the period between pulses as $P_{obs} = 1{\cdot}33733 \pm 0{\cdot}00001$ s. This represents a mean value centred on December 18, 1967, at 14 h 18 m UT. A study of the systematic shift in the frequency of the pulses was obtained from daily measurements of the

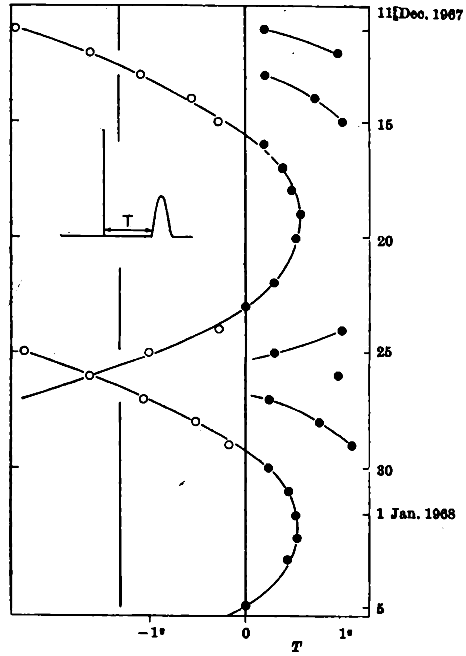


Fig. 4. The day to day variation of pulse arrival time.

time interval T between a standard time and the pulse immediately following it as shown in Fig. 4. The standard time was chosen to be 14 h 01 m 00 s UT on December 11 (corresponding to the centre of the reception pattern) and subsequent standard times were at intervals of 23 h 56 m 04 s (approximately one sidereal day). A plot of the variation of T from day to day is shown in Fig. 4. A constant pulse recurrence frequency would show a linear increase or decrease in T if care was taken to add or subtract one period where necessary. The observations, however, show a marked curvature in the sense of a steadily increasing frequency. If we assume a Doppler shift due to the Earth alone, then the number of pulses received per day is given by

$$N = N_0\left(1 + \frac{v}{c}\cos\varphi\sin\frac{2\pi n}{366{\cdot}25}\right)$$

where N_0 is the number of pulses emitted per day at the source, v the orbital velocity of the Earth, φ the ecliptic latitude of the source and n an arbitrary day number obtained by putting $n = 0$ on January 17, 1968, when the Earth has zero velocity along the line of sight to the source. This relation is approximate since it assumes a circular orbit for the Earth and the origin $n = 0$ is not exact, but it serves to show that the increase of N observed can be explained by the Earth's motion alone within the accuracy currently attainable. For this purpose it is convenient to estimate the values of n for which $\delta T/\delta n = 0$, corresponding to an exactly integral value of N. These occur at $n_1 = 15{\cdot}8 \pm 0{\cdot}1$ and $n_2 = 28{\cdot}7 \pm 0{\cdot}1$, and since N is increased by exactly one pulse between these dates we have

$$1 = \frac{N_0 v}{c}\cos\varphi\left[\sin\frac{2\pi n_2}{366{\cdot}25} - \sin\frac{2\pi n_1}{366{\cdot}25}\right]$$

This yields $\varphi = 43° \; 36' \pm 30'$ which corresponds to a declination of $21° \; 58' \pm 30'$, a value consistent with the declination obtained directly. The true periodicity of the source, making allowance for the Doppler shift and using the integral condition to refine the calculation, is then

$$P_0 = 1{\cdot}3372795 \pm 0{\cdot}0000020 \text{ s}$$

By continuing observations of the time of occurrence of the pulses for a year it should be possible to establish the constancy of N_0 to about 1 part in 3×10^8. If N_0 is indeed constant, then the declination of the source may be estimated to an accuracy of $\pm 1'$; this result will not be affected by ionospheric refraction.

It is also interesting to note the possibility of detecting a variable Doppler shift caused by the motion of the source itself. Such an effect might arise if the source formed one component of a binary system, or if the signals were associated with a planet in orbit about some parent star. For the present, the systematic increase of N is regular to about 1 part in 2×10^7 so that there is no evidence for an additional orbital motion comparable with that of the Earth.

The Nature of the Radio Source

The lack of any parallax greater than about 2′ places the source at a distance exceeding 10^3 A.U. The energy emitted by the source during a single pulse, integrated over 1 MHz at 81·5 MHz, therefore reaches a value which must exceed 10^{17} erg if the source radiates isotropically. It is also possible to derive an upper limit to the physical dimension of the source. The small instantaneous bandwidth of the signal (80 kHz) and the rate of sweep ($-4{\cdot}9$ MHz s^{-1}) show that the duration of the emission at any given frequency does not exceed 0·016 s. The source size therefore cannot exceed $4{\cdot}8 \times 10^3$ km.

An upper limit to the distance of the source may be derived from the observed rate of frequency sweep since impulsive radiation, whatever its origin, will be dispersed during its passage through the ionized hydrogen in interstellar space. For a uniform plasma the frequency drift caused by dispersion is given by

$$\frac{\mathrm{d}\nu}{\mathrm{d}t} = -\frac{c}{L}\frac{\nu^3}{\nu_p^{\,2}}$$

where L is the path and ν_p the plasma frequency. Assuming a mean density of 0·2 electron cm^{-3} the observed frequency drift ($-4{\cdot}9$ MHz s^{-1}) corresponds to $L \sim 65$ parsec. Some frequency dispersion may, of course, arise in the source itself; in this case the dispersion in the interstellar medium must be smaller so that the value of L is an upper limit. While the interstellar electron density in the vicinity of the Sun is not well known, this result is important in showing that the pulsating radio sources so far detected must be local objects on a galactic distance scale.

The positional accuracy so far obtained does not permit any serious attempt at optical identification. The search area, which lies close to the galactic plane, includes two twelfth magnitude stars and a large number of weaker objects. In the absence of further data, only the most tentative suggestion to account for these remarkable sources can be made.

The most significant feature to be accounted for is the extreme regularity of the pulses. This suggests an origin in terms of the pulsation of an entire star, rather than some more localized disturbance in a stellar atmosphere. In this connexion it is interesting to note that it has already been suggested[2,3] that the radial pulsation of neutron stars may play an important part in the history of supernovae and supernova remnants.

A discussion of the normal modes of radial pulsation of compact stars has recently been given by Meltzer and Thorne[4], who calculated the periods for stars with central densities in the range 10^5 to 10^{19} g cm^{-3}. Fig. 4 of their paper indicates two possibilities which might account for the observed periods of the order 1 s. At a density of 10^7 g cm^{-3}, corresponding to a white dwarf star, the fundamental mode reaches a minimum period of about 8 s; at a slightly higher density the period increases again as the system tends towards gravitational collapse to a neutron star. While the fundamental period is not small enough to account for the observations the higher order modes have periods of the correct order of magnitude. If this model is adopted it is difficult to understand why the fundamental period is not dominant; such a period would have readily been detected in the present observations and its absence cannot be ascribed to observational effects. The alternative possibility occurs at a density of 10^{13} g cm^{-3}, corresponding to a neutron star; at this density the fundamental has a period of about 1 s, while for densities in excess of 10^{13} g cm^{-3} the period rapidly decreases to about 10^{-3} s.

If the radiation is to be associated with the radial pulsation of a white dwarf or neutron star there seem to be several mechanisms which could account for the radio emission. It has been suggested that radial pulsation would generate hydromagnetic shock fronts at the stellar surface which might be accompanied by bursts of X-rays and energetic electrons[2,3]. The radiation might then be likened to radio bursts from a solar flare occurring over the entire star during each cycle of the oscillation. Such a model would be in fair agreement with the upper limit of $\sim 5 \times 10^3$ km for the dimension of the source, which compares with the mean value of 9×10^3 km quoted for white dwarf stars by Greenstein[5]. The energy requirement for this model may be roughly estimated by noting that the total energy emitted in a 1 MHz band by a type III solar burst would produce a radio flux of the right order if the source were at a distance of $\sim 10^3$ A.U. If it is assumed that the radio energy may be related to the total flare energy ($\sim 10^{32}$ erg)[6] in the same manner as for a solar flare and supposing that each pulse corresponds to one flare, the required energy would be $\sim 10^{39}$ erg yr^{-1}; at a distance of 65 pc the corresponding value would be $\sim 10^{47}$ erg yr^{-1}. It has been estimated that a neutron star may contain $\sim 10^{51}$ erg in vibrational modes so the energy requirement does not appear unreasonable, although other damping mechanisms are likely to be important when considering the lifetime of the source[4].

The swept frequency characteristic of the radiation is reminiscent of type II and type III solar bursts, but it seems unlikely that it is caused in the same way. For a white dwarf or neutron star the scale height of any atmosphere is small and a travelling disturbance would be expected to produce a much faster frequency drift than is actually observed. As has been mentioned, a more likely possibility is that the impulsive radiation suffers dispersion during its passage through the interstellar medium.

More observational evidence is clearly needed in order to gain a better understanding of this strange new class of radio source. If the suggested origin of the radiation is confirmed further study may be expected to throw valuable light on the behaviour of compact stars and also on the properties of matter at high density.

We thank Professor Sir Martin Ryle, Dr J. E. Baldwin, Dr P. A. G. Scheuer and Dr J. R. Shakeshaft for helpful discussions and the Science Research Council who financed this work. One of us (S. J. B.) thanks the Ministry of Education of Northern Ireland and another (R. A. C.) the SRC for a maintenance award; J. D. H. P. thanks ICI for a research fellowship.

Received February 9, 1968.

1 Hewish, A., Scott, P. F., and Wills, D., *Nature*, **203**, 1214 (1964).
2 Cameron, A. G. W., *Nature*, **205**, 787 (1965).
3 Finzi, A., *Phys. Rev. Lett.*, **15**, 599 (1965).
4 Meltzer, D. W., and Thorne, K. S., *Ap. J.*, **145**, 514 (1966).
5 Greenstein, J. L., in *Handbuch der Physik*, *L.*, 161 (1958).
6 Fichtel, C. E., and McDonald, F. B., in *Annual Review of Astronomy and Astrophysics*, **5**, 351 (1967).

Magnetic Models of Pulsars

SEVERAL months have passed since the publication of the discovery of pulsars[1] and some tens of papers on this subject have appeared[2]. So far, however, not enough attention has been paid to the mechanism of radiation. The radio emission from pulsars must come from a low-density plasma atmosphere around the object—a medium which has well known physical properties. This means that the requirements following from an analysis of the radiation mechanism are important in choosing models of pulsars. From this point of view, we have always believed that the most probable pulsar model is a pulsating white dwarf. There is far more radiating plasma in such a model than in a neutron star, and a mechanism for the radiation is clearly seen—shock waves propagating through the atmosphere are responsible for the short pulses of radio emission. It is also evident that in the absence of a stellar magnetic field, radial pulsations of the star lead to non-polarized radiation. The observations of strong polarization[3] therefore impelled us to consider models of pulsating magnetic white dwarfs. The assumption that some white dwarfs have a rather strong magnetic field is natural enough and, as far as we know, meets no difficulties. On the other hand, even comparatively small fields ($H \sim 10$–100 oersted) have radical effects on the movements of particles and the propagation of waves in a stellar atmosphere. In particular, the "stellar wind" from a magnetic star can come from the polar regions. What is more, in the atmosphere of pulsating magnetic stars acceleration of particles must occur. When the field is regular, some acceleration will certainly take place as a result of magnetic pumping[4]. If there are also irregular fields—rather probable in this case—cumulative acceleration mechanism may also be effective[5]. The accelerated particles will fill the radiation belts of the star and come directly outwards from the polar regions to form two streams (for a dipole field). The particles will also be "poured" out of the radiation belts in subpolar regions.

A number of features of magnetic models of pulsars will be treated in more detail in a paper we are preparing for publication. Here we present only the basic considerations and the results (which were presented in a report at the panel on pulsars at the Trieste Symposium on Contemporary Physics, June 24, 1968). Here we discuss three models, without consideration of the intermediate cases.

Model 1: Streams of rapid particles—with power $P_s \sim n_s v_s \varepsilon_s S$ where n_s, v_s and ε_s are respectively the density, velocity and kinetic energy of particles in a stream and S is the cross-section of the stream—come from the polar regions. At each pulse a shock wave ejects from the atmosphere of the star into the stream a "cold" plasma having density n and a total volume $V \sim N/n$ where N is the number of particles in the cold plasma. In such a "stream-plasma" system, plasma waves are generated, producing radio waves which are emitted by pulsars from the polar regions. The radiation power of the pulsar pulse is $\lesssim P_0 \sim 10^{30}$ ergs s^{-1}. We have shown in some unpublished work that when the induced scattering is taken into account, the coefficient of transformation of longitudinal waves into transverse waves in pulsars may be of the order of unity. The stream can generate plasma waves the energy density of which is equal to the energy density of the stream $n_s \varepsilon_s$. Thus it follows that $P_0 \sim P_s$. If we assume that $V_s \sim C$, $\varepsilon_s \sim Mc^2 \sim 10^{-3}$ ergs for the proton stream, $\varepsilon_s \sim mc^2 \sim 10^{-6}$ ergs for the electron stream and $S \sim \dfrac{4\pi r^2}{30} \sim 10^{17}$ cm^2 (the radius of the star $r \sim 5 \times 10^8$ cm), then the necessary number of particles per unit volume will amount to $n_s \sim 3 \times 10^5$ cm^{-3} for the proton stream and $n_s \sim 3 \times 10^8$ cm^{-3} for the electron stream. In the ejected plasma $n \sim m\omega^2/4\pi e^2 \sim 10^8$ cm^{-3} for a radiating frequency $\omega \sim 6 \times 10^8$ s^{-1}. The thickness of the cold plasma layer L must be not less than 5×10^6 cm—this is necessary for excitation of plasma waves and their complete conversion into electromagnetic radiation. Hence we find the volume occupied by the plasma $V \gtrsim 5 \times 10^{23}$ cm^3 and the total number of particles in it $N \sim nV \gtrsim 5 \times 10^{31}$ electrons. It is worth considering here the relevance of pulsars to the problem of the origin of cosmic rays. The density of pulsars, assuming quasi-isotropic radio emission, is of the order of 10^{-5} pc^{-3} (ref. 6), so it is unlikely that the galaxy contains more than 10^7 pulsars. If each of them generates cosmic rays with a power 10^{30} ergs s^{-1} the total contribution from all pulsars will be 10^{37} ergs s^{-1}. At the same time, the cosmic rays in the galaxy must be produced with a power not less than 10^{40} ergs s^{-1} (ref. 7). Besides, there is no mechanism in pulsars for accelerating particles with $\varepsilon \gg Mc^2 \sim 10^9$ eV. At present there are therefore no good grounds for considering pulsars to be an effective source of galactic cosmic rays.

Model 2: Under the action of shock waves, particles from the radiation belt are poured out in subpolar regions which resemble auroral zones. There occurs in these regions a sharp non-equilibrium velocity distribution of high energy electrons with the resulting generation in the atmosphere of the star of plasma waves (because of instability in the stream-plasma system and loss-cone instability[8]) and radio waves (for example, at the expense of synchronous instability[9]). In the case of stream instability, model 2 is, in a way, close to model 1. For the coherent synchrotron mechanism, optimal estimations of conditions in the region of generation of radio emission with frequency $\omega = 2\pi\nu \sim 6 \times 10^8$ s^{-1} are as follows. The number of poured out particles per unit volume $n_s \sim 5 \times 10^5$ electrons cm^{-3}, and their energy ε_s is $\sim 10\ mc^2$; the cold plasma density $n \sim 3 \times 10^7$ electrons cm^{-3}, its temperature $T \gtrsim 2 \times 10^8$ °K and the field strength $H \sim 10$ oersteds. The pulse shape of waves radiated from the auroral zones may be close to what is observed, according to F. G. Smith at the Trieste Symposium on Contemporary Physics, 1968.

Model 3: The radio emission is generated at the front of the shock wave as a result of the excitation of plasma waves and their further transformation into radio waves. In the presence of the magnetic field, collisionless shock waves (even though they are formed by pulsations*) will excite intensive plasma waves only for quasi-transverse propagation across the magnetic field, that is, only in the equatorial region. Because of this it is possible to explain, in principle, the polarization of the radiation. The radio emission of model 3 is similar to type II radio bursts from the Sun. The essential difference, however, is that for pulsars the transformation coefficient of the plasma waves into radio waves is several orders of magnitude greater than that for the Sun. This is because of the high energy density of the radio emission leading to the induced scattering effect. The energy density of radio emission in model 3 is comparable with that of the field $H^2/8\pi$ (see ref. 6). Hence, in the emitting region $H \sim 10$ oersteds and $\omega_H \sim 2 \times 10^8$ s^{-1}.

The separation of pulses into two subpulses[2] is accounted for by the presence of two emitting regions (the two poles). With an asymmetrical orientation of the dipole, the intensity of the subpulse will be different from the main pulse. Another feature is pulse splitting, produced by the structure of a collisionless shock wave. In models 2 and 3 there may be breaks in the radio emission of pulsars because after the ejection by the shock wave of cold plasma responsible for radio emission, some time is needed for its restoration. In model 2 we deal with the "exhaustion" of the radiation belts and the time to fill them again with particles.

Analysis of the polarization of radiation allows us to find some criteria which characterize the magnetic field and the plasma density in the region of pulsations and in the vicinity of the star. So, in order to explain the linear polarization, it is necessary that the generation take place in the condition of quasi-transverse propa-

* Excitation of shock waves depends on their velocity[10] and therefore, for a magnetic star, will be different in equatorial and auroral regions. The fact that this circumstance is taken into account may be useful in estimating the degree of reality in different models of the radio emission from pulsars.

gation of radio waves and transition to quasi-longitudinal propagation is realized in the region with $n \lesssim 10^{3}$ cm^{-3}. (We have prepared for publication a detailed consideration of the problem of polarization.) The absence of correlation of radio emission at close frequencies (discussed by J. G. Bolton at the Trieste Symposium on Contemporary Physics, June 1968) may be explained, in principle, by radio wave scattering in the interstellar medium[11].

We have assumed that the dimension of the emitting region is less than or of the order of white dwarf dimensions, $r \sim 5\times10^{8}$ cm. We must take into account, however, that for coherent mechanisms of radio emission, when we are dealing with the negative reabsorption, the estimation of the emitting region dimensions $L \lesssim c\tau$ (τ is the pulse duration) may appear groundless. In fact, $c\tau$ is only the upper limit for the dimension of the region of which the optical depth (for wave amplification) is of the order of unity. Hence the dimensions of the emitting region of pulsars may be much greater than 5×10^{8} cm. The grounds for identifying pulsars with objects of small sizes therefore arise only from consideration connected with estimations of the pulsation period ($\sim$1 s). At the same time the analysis of radiation mechanisms testifies that the magnetic models of white dwarf seem more reasonable than a number of the other models discussed in the literature.

V. L. Ginzburg
V. V. Zheleznyakov
V. V. Ziatsev

P. N. Lebedev Physical Institute,
Academy of Sciences,
Moscow.

Received August 5, 1968.

[1] Hewish, A., Bell, S. J., Pilkington, J. D. H., Scott, P. F., and Collins, R. A., *Nature*, **217**, 709 (1968).
[2] Maran, S. P., and Cameron, A. G. W., *Pulsars* (June 1968).
[3] Lyne, A., and Smith, F. G., *Nature*, **218**, 124 (1968).
[4] Alfven, H., and Fälthammar, C. G., *Cosmical Electrodynamics* (Oxford, 1963).
[5] Syrovatskii, S. I., *JETP*, **50**, 1133 (1966); *Astron. J. USSR*, **43**, 340 (1966).
[6] Pilkington, J. D. H., Hewish, A., Bell, S. J., and Cole, T. W., *Nature*, **218**, 126 (1968).
[7] Ginzburg, V. L., and Syrovatskii, S. I., *Origin of Cosmic Rays* (Pergamon Press, 1964).
[8] Rosenbluth, M. N., and Post, R. F., *Phys. Fluids*, **8**, 547 (1965).
[9] Zheleznyakov, V. V., *JETP*, **51**, 570 (1966); *Astron. J. USSR*, **44**, 42 (1967).
[10] Zeldovich, Ya. B., *Astron. J. USSR* (in the press).
[11] Eroukhimov, L. M., and Pisareva, V. V., *Astron. Circ. USSR* (in the press).

220, 355; 1968

Clustering of Pulsars along the Galactic Plane

We recently described[1] the discovery of seven pulsars found in a search at the Molonglo Radio Observatory using the 408 MHz radio telescope. A conclusion reached on the basis of this search was that pulsars are galactic objects associated with the disk or the spiral arms, but with a somewhat anomalous distribution. A zone of avoidance along the inner regions of the galactic plane was suggested, based on the argument that dispersion in the thin layer of an ionized medium closely confined to the galactic plane results in the loss of the distant pulsars. This communication describes the discovery of further pulsars which follow the same general pattern of galactic distribution.

The search technique of "early" and "late" beams[1] was used in the observations. In addition, the centre beam signal was split into two frequency bands centred at 407 MHz and 409 MHz. The recording of the pulse arrival times in the two bands, using a magnetic tape recorder, allows measurement of the rate of drift of pulse frequency[2]. The three new pulsars are listed in Table 1 with the provisional *MP* designation. The right ascensions (1950 epoch) of each pulsar are listed for the nominal declination and for declinations 2° above and below. The energy densities of strong pulses are estimated in accordance with the system used for the previous seven pulsars[1].

Table 1. CHARACTERISTICS OF THE THREE NEW PULSARS

	MP 0940	*MP* 0959	*MP* 1747
$\alpha_{1950\cdot0}$ at $\delta+2°$	42^{s}	52^{s}	59^{s}
$\alpha_{1950\cdot0}$ at δ	$09^{h}\,40^{m}\,40^{s}\pm2^{s}$	$09^{h}\,59^{m}\,51^{s}\pm2^{s}$	$17^{h}\,47^{m}\,56^{s}\pm1^{s}$
$\alpha_{1950\cdot0}$ at $\delta-2$	39^{s}	49^{s}	51^{s}
$\delta_{1950\cdot0}$	$-56°\pm2°$	$-56°\pm2°$	$-48°\pm2°$
l	279°	281°	344°
b	$-3°$	$-1°$	$-11°$
P	$0{\cdot}662\pm0{\cdot}003$	$1{\cdot}438\pm0{\cdot}003$	$0{\cdot}742\pm0{\cdot}003$
$\left(-\frac{d\nu}{dt}\right)_{408}$ MHz s^{-1}	57	90	200
$\int n\,dl$ cm^{-3} pc	145 ± 15	90 ± 10	40 ± 10
W_{p} ms	30 ± 10	50 ± 10	20 ± 5
U_{408} 10^{-26} J m^{-2} Hz^{-1}	$\sim0{\cdot}1$	$\sim0{\cdot}1$	$\sim0{\cdot}1$

Two of the new pulsars lie in the direction $l=280°$, near the galactic plane. The dispersion measurements indicate that they are distant objects. The pulsar *MP* 0940 shows the greatest dispersion yet measured. The distribution of pulsars in the direction of the inner and the local spiral arms with galactic latitude $b=\pm10°$ is shown in Fig. 1.

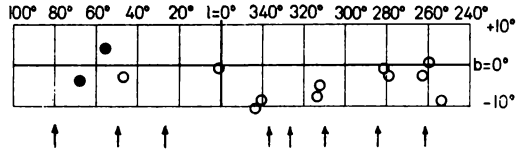


Fig. 1. Distribution of pulsars in the direction of the inner and local spiral arms with $|b|\leq10°$. Arrows indicate "tangential points" of the spiral arms. Pulsars discovered at Molonglo are represented by open circles.

Two pulsars are clustered in the direction $l=310°$, which * is associated with the "tangential point" of the Sagittarius spiral arm. A zone of avoidance was proposed to explain the absence of pulsars along the plane itself. The tendency for pulsars to be concentrated south of the galactic plane, reported earlier[1], seems to be a real effect. In the recent search programme more observing time was devoted to positive galactic latitudes between $l=0°$ and $l=280°$. Still no pulsars have been found. The asymmetry of the distribution may be due to the real absence of pulsars, to an asymmetry in the dispersing medium or to chance effects of pulsar amplitude fluctuations[4] coupled with the use of a transit instrument.

An alternative explanation of the clustering of radio sources may be sought in the association of pulsars with supernovae[5,6]. If pulsars are formed during a supernova explosion a number of such objects might be formed. The present cluster members, although not perfectly matched in dispersion measure or repetition rate, may in fact be associated objects. At present it seems that the difficulties of pulsar searches may be compounded by confusion effects due to clusters.

We wish to thank Professor B. Y. Mills for helpful discussions in all stages of the pulsar work. This work was done with financial grants from the Australian Research Grants Committee and the US National Science Foundation.

R. Wielebinski
A. E. Vaughan
M. I. Large

Cornell-Sydney University Astronomy Centre,
School of Physics,
University of Sydney.

Received November 28, 1968.

[1] Large, M. I., Vaughan, A. E., and Wielebinski, R., *Nature*, **220**, 753 (1968).
[2] Mills, B. Y., *IAU-URSI Symposium No. 20*, 102 (Australian Acad. Sci., 1964).
[3] Large, M. I., and Vaughan, A. E., *Nature*, **220**, 43 (1968).
[4] Wielebinski, R., *Nature*, **219**, 1135 (1968).
[5] Large, M. I., Vaughan, A. E., and Mills, B. Y., *Nature*, **220**, 340 (1968).
[6] *Intern. Astro. Union Circular* (November 1968).

221, 47; 1969

*Abridged

Quantum effects in cosmology

A.A. Starobinskii and Ya.B. Zel'dovich

Two recent symposia on quantum cosmology and quantum gravity* reflected the ardent hope of deriving the ultimate explanation of all of nature from the fundamental laws of microphysics. This goal is more ambitious than that of the classical astronomers, who, given the initial conditions, wished to calculate precisely all the subsequent motions of the planets, stars and so on. The question now is what are the initial conditions for the Universe as a whole that are prescribed by fundamental theory?

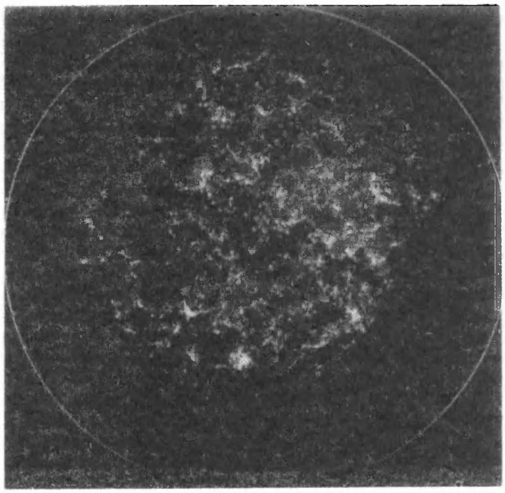
These initial conditions refer to the moment of the very beginning of the expansion of the Universe, about 10–15 billion (10^9) years ago. At that moment, nature was characterized by ultracurved space-time, ultrastrong fields and very massive particles not yet discovered in laboratories. Quantum cosmology is the application of quantum theory to this exotic situation. Its renaissance during the past few years was caused mainly by the success of the inflationary model of the Universe (stressed by A.H. Guth, Harvard–Smithsonian Center for Astrophysics).

Inflation, a stage of rapid accelerated expansion of the Universe with a scale factor that depends exponentially on time, makes it possible to relate the initial conditions of the Universe to such directly measurable quantities as the anisotropy of the cosmic background radiation temperature, the large-scale distribution of galaxies and the spectrum of stochastic gravitational-wave background. As a result, quantum cosmology seems to be strongly connected with both microphysics and observational astronomy. The most interesting reports discussed the structure of the Universe at very large scales (far beyond the present-day cosmological horizon), the predictability and interpretation of the wavefunction in quantum gravity and quantum cosmology, and the evolution of multi-dimensional ('Kaluza–Klein') models with lagrangians (equations of motion) modified by quantum corrections following from the more fundamental superstring theory.

It is certain that only statistical predictions for the initial conditions of the Universe can be obtained. But a look at the night sky or at a map of distant galaxies shows that this is sufficient: galaxies are distributed without any accurate design (see figure). They do like to be together, however, forming clumps, filaments (with galaxies attached like pearls on a necklace) or surfaces surrounding voids.

The evolution of the Universe since it was about 0.1 s old is characterized by particle energies available in existing accelerators. The predictions concerning nuclear reactions in the famous 'first three minutes' are confirmed by observations. This is a triumph of the hot Big Bang picture. But the observed abundances of D, ^{3}He, ^{4}He and other elements imply that the density of matter consisting of protons and neutrons (such as stars and dust) is less than 10 per cent of the total matter density of the Universe as deduced from the dynamics of clusters of galaxies. There are several arguments that suggest that more than 90 per cent of the total density is in the form of 'dark' matter. This must be some kind of matter that does not interact with the electromagnetic field (so consisting of neutral particles and/or fields) and affected only weakly by forces other than gravity.

'Cosmic strings', also hypothesized as a part of the zoo inhabiting the Universe, would contribute to the total density, but, more importantly, they would generate a significant intensity of gravitational radiation, creating specific perturbations that lead to galaxy formation. If the strings are superconducting as predicted by some variants of fundamental theory (E. Witten, Princeton University), they are also capable of generating very-low-frequency electromagnetic waves that cannot be observed directly but could heat surrounding plasma. The overall picture of the Universe and of its immediate past will remain incomplete as long as the nature of dark matter and the question of strings are not solved.



*A map of the 1.27 million galaxies brighter than magnitude 19 in the northern sky. It was prepared by M. Seldner, B. Siebers, E.J. Groth and P.J.E. Peebles (*Astr. J. **82**, 249–256; 1977*) from the* Lick Catalog *of galaxies drawn up by C.D. Shane and C.A. Wirtanen. The map is broken down into 10 min × 10 min cells each of which is assigned a brightness proportional to the number of galaxies it contains.*

Astronomical Journal

Descriptions of the very early stage of the evolution of the Universe also combine the well established and still unknown. The idea of inflation now seems inescapable. One needs a period when the matter filling the Universe had very unusual properties — positive energy density ε but negative pressure P with $P \simeq -\varepsilon$. The rigorous description of this matter requires the introduction of an effective (not necessarily fundamental) classical scalar field ψ with a proper potential $V(\psi)$. One of the most important achievements of this idea is the explanation (Ya.B.Z.) of the very expansion of the Universe. In Einstein's general relativity, gravitational acceleration depends on $\varrho+3P/c^2$ (c is the speed of light) not simply on the mass density ϱ. Given that $\varrho c^2=\varepsilon$ (from Einstein's mass–energy equivalence), acceleration for the unusual matter, $\varrho+3P/c^2 \simeq -2\varrho$, has a negative value. This means that there was gravitational repulsion instead of attraction, explaining the expansion.

Other properties of the Universe can be explained if the inflationary era ends by the transformation of the peculiar cold, negative-pressure matter into very hot plasma. In particular, small inhomogeneities leading to the formation of galaxies are generated by quantum fluctuations of the scalar field ψ amplified by the expansion of the Universe. The amplitude of the inhomogeneities evaluated from observations of the 3-K background radiation and from the distribution of galaxies gives information about the properties of both the scalar field ψ and

*Quantum Cosmology Workshop, 1–3 May 1987, Fermilab; IV Seminar *Quantum Gravity*, 25–29 May 1987, Moscow.

the present-day dark matter. This is yet another example of the mutual influence between astronomy and fundamental physics. A.D. Sakharov was the first to suggest, in 1965, although in a completely different, hydrodynamical context, the role of quantum fluctuations in the formation of the structure of the Universe.

In most previous studies of the inflationary model, two restrictive assumptions were made for the sake of simplicity: both the effective scalar field ψ driving the inflation and space-time were taken to be deterministic (classical) and homogeneous (with small inhomogeneous perturbations). Recently both these assumptions were omitted (A.A.S. and A.D. Linde, Lebedev Physical Institute). It seems that the evolution of the quasi-homogeneous large-scale part of the scalar field is essentially determined by its small-scale quantum fluctuations induced by the expansion of the Universe.

As a result, the large-scale field and space-time metric become stochastic (stochastic inflation). The evolution of the large-scale field is described by the Fokker–Planck type equation for the probability distribution $p(\psi,a)$ (a is the cosmological scale factor) with different realizations of the stochastic process in different points of space which become causally disconnected in the course of the exponential expansion during the inflationary stage. The space-time metric follows the evolution of the scalar field: it remains classical but becomes stochastic and inhomogeneous at large scales, too.

Eternal inflation

Two striking consequences follow: first, the inflationary stage as a whole appears to be eternal, having no definite end (though in some models each 'observer' spends only a finite time in it); second, the Universe after inflation is not at all homogeneous at super-large-scales which we cannot observe now (a typical value is $10^{100,000}$ cm; the present cosmological horizon is 10^{28} cm). In particular, the three-dimensional hypersurfaces of approximate homogeneity (constant-energy-density hypersurfaces) need not be the 'Cauchy' hypersurfaces of the whole space-time, and may have complicated topological structures. In other words, a picture (hypothesized earlier) of an infinite number of Friedmann universes created from one maternal de Sitter universe arises naturally. This is the price the modern inflationary theory has to pay for the explanation of approximate homogeneity on scales of the order of cosmological horizon.

This sheds new light on the problem of the origin of the Universe. It does not preclude a quantum transition creating the closed Universe with the homogeneous field ψ from nothing. But a new possibility arises: the eternal existence of the expanding, locally homogeneous quasi de Sitter space-time supported by quantum fluctuations that has no definite three-dimensional topology and constantly creates separate Friedmann worlds.

Another fundamental problem put forward by S.W. Hawking (Cambridge University) about 10 years ago is so-called quantum unpredictability or loss of quantum coherence. It can be thought of as connected with the formation and subsequent evaporation of very small black holes during real processes, like the scattering of two energetic particles. Imagine the black hole after evaporation as a minuscule closed world geometrically detached from our space. If it carries some information inaccessible to us, the phase of the particles after scattering is not known, and the interference with simple scattering (not accompanied by black-hole creation) is incomplete. Thus, a new type of indeterminacy is superimposed on that associated with quantum gravity.

Now Hawking has developed an elegant mathematical description of this process that does not include the consideration of black holes at all. He uses the euclidean (imaginary-time) formalism in quantum gravity where the process of spontaneous detachment of a closed three-world may be described by a virtual 'wormhole' connecting two asymptotically euclidean regions, its radius being of the order of the Planck length (about 10^{-33} cm). Particles can go back and forth through this wormhole.

Is it possible to verify the suggested loss of coherence in laboratory experiments? The question is difficult because gravitational forces are very small compared with other interactions. The Planck mass 10^{-5} g, which is probably the smallest possible mass of a black hole, is much greater than the mass of a proton at rest (10^{-24} g), or even of a proton accelerated in the future supercollider (20 TeV, or 3×10^{-20} g). The proposed effect is of the order of unity for fundamental (not composite) scalar (spin-zero) particles, but we do not know any such particles. For particles of higher spin, the effect is suppressed by powers of the ratio of the particles' rest mass to the Planck mass. One prediction is that baryons (such as protons) should decay to leptons (such as electrons), the characteristic lifetime being of the order of 10^{50} yr. The problem is not only that this time is too long for the process to be detected, but that gravitational decay will be masked by much more rapid baryon decay (preserving quantum coherence) predicted by grand unified theories (with a characteristic lifetime of $10^{32}-10^{35}$ yr).

The application of quantum unpredictability to cosmology shows that the Universe cannot be in a pure quantum state even at its beginning, so that it has to be described not by the 'wavefunction of the Universe', but by a density matrix (Hawking; D.N. Page, Pennsylvania State University).

Fifty years ago, Dirac argued that the large difference in strength between gravitation and electromagnetism means that these two forces are of different origin. The prevalent idea now, anticipated by Einstein, is the unity of all forces of nature. But we understand better how difficult the problem is. At present, the most promising candidate for the 'theory of everything' (TOE) is superstring theory. Einstein's theory of gravity arises in its low-energy limit (with energies much less than the Planck energy 10^{19} Gev), so that general relativity is not an exact theory.

Additional curvature

The TOE should predict corrections that add more curvature (thus, they are negligible at low curvatures). The exact form of these corrections has yet to be determined. Furthermore, space-time in superstring theory is 10-dimensional, therefore six spatial dimensions must somehow compactify. Account of these properties gives rise to new multi-dimensional cosmological models (some were reported by S. Deser, Brandeis University). Another tantalizing goal is to deduce inflation from superstring theory, in particular, to derive the exact form of the potential $V(\psi)$. Up to now only a few steps have been made in this direction. One approach (D.V. Nanopoulos, CERN) is based on the so-called no-scale supersymmetry suggested by J. Ellis and collaborators a few years ago.

Last, but not least, is the problem of the vacuum energy density in flat space-time (equivalent to the problem of the cosmological constant). Vacuum is defined as the state of minimum energy density, which need not be equal to zero. The quantum approach to the problem tells us that every elementary bosonic field (such as the electromagnetic one) gives an infinite positive contribution to the energy density, and every fermionic field, an infinite negative contribution. Laboratory measurements and, with much higher precision, astronomical observations tell us that these positive and negative infinities compensate each other with fantastic precision, the energy density of the vacuum being less than 10^{-8} erg cm^{-3}. There is no theoretical explanation of this miraculous compensation. The best result is achieved with the above-mentioned 'no-scale' supersymmetry, which can explain why the vacuum energy density should be less than 10^{46} erg cm^{-3}, which corresponds to an energy scale of supersymmetry breaking of about 100 Gev. □

A.A. Starobinskii is at the Landau Institute for Theoretical Physics, Academy of Sciences of the USSR, Moscow, USSR.

331, 673; 1988

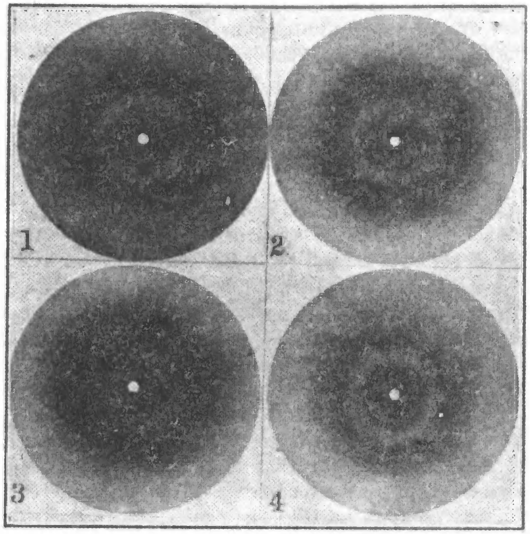
X-Ray Studies of Protein Structure*

By W. T. Astbury, Textile Physics Laboratory, University of Leeds

ACCORDING to the classical researches of Fischer and others, proteins would appear to be essentially polypeptides, giant molecules formed by the repeated condensation of α-amino-acids. This concept leads naturally to the idea of long chain-molecules like that of cellulose, the structure of which was worked out some years ago by a particularly happy combination of chemical and X-ray methods[1]. Similar methods applied to one of the simplest proteins, *fibroin*, the fibre substance of natural silk, show that, for silk at least, the hypothesis is substantially correct[1]; that, in fact, this fibre is a kind of molecular yarn or sliver built up by chain-like molecules, fully-extended polypeptides, lying roughly parallel to the fibre axis. The approximate dimensions of these chain-molecules may be predicted from atomic data already available, and they are found to fit in well with the results of X-ray analysis.

The X-ray photographs of all natural protein fibres, hair, muscle, collagen, feather, etc., show certain features in common with that of natural silk, and it seems clear that they are all built up in very much the same manner from chain-molecules lying along, or simply related to, the fibre axis. But in other respects there are well-marked differences which indicate that the straight-chain configuration found in silk cannot be true in general. Specially does this objection hold in the case of mammalian hair. On stretching hair, however, a new X-ray photograph is obtained which shows again the characteristics of fully-extended polypeptides. The chain-molecules of *keratin*, the protein of hair, must therefore be normally in some regularly folded state from which they can be pulled out straight, and to which they return when the tension is released[2].

Mammalian hairs, spines, horn, etc., are elastic over a range recalling even that of rubber, and X-rays thus indicate that the property resides in the keratin molecule itself, which can be transformed from a folded configuration (α-keratin) to the straight-chain configuration (β-keratin) and back again an indefinite number of times. The structure of β-keratin[3] (Fig. 2) appears to be that of a polypeptide 'grid' built up by interactions and combinations between the various 'side-chains' of neighbouring 'main-chains'. To accommodate these interactions the grid buckles, so to speak, in such a way that the main-chains fold in planes transverse to the side-chains. In the presence of water the folds can be pulled out by mechanical force, but they resume their normal configuration when the stretching force is removed. The basis of the 'setting' of hair when steamed in the stretched state is the hydrolytic breakdown of certain of the cross-linkages of the grid which are put under stress when the molecule is stretched. The broken cross-linkages then ultimately re-form in new (unstressed) positions, thereby eliminating the driving force of contraction. If, however, the cross-linkages are broken but not allowed to re-form, a more labile state is induced in which the (modified) keratin molecule can be made to contract to a length even shorter than that of normal α-keratin ('supercontraction').



FIGS. 1–4. X-ray photographs of (1) a protein in the α-configuration, (2) a protein in the β-configuration, (3) disoriented denatured protein, and (4) stretched denatured albumin. (Fibre axis or axis of extension vertical.)

The X-ray photograph of washed and dried muscle (Fig. 1) is remarkably like that of α-keratin[4,5]. The photograph in the main arises from the chief muscle protein, *myosin*[6], which must be presumed to exist normally in a folded, or α-, configuration. If, therefore, the elastic elements in muscle are by analogy with keratin the myosin chain-molecules, the contraction of muscle corresponds to the 'supercontraction' of keratin, and it should be possible to transform both myosin and muscle into a straight-chain, or β-, configuration[4]. These transformations have now been accomplished[5], by stretching myosin film and washed muscle

* Substance of three lectures delivered at the Royal Institution on February 20 and 27 and March 5.

respectively, and the former has also been made to show 'supercontraction' after the manner of the contraction of muscle itself[6,7]. Figs. (1) and (2) are typical α- and β-photographs, respectively, (1) being that of the foot retractor muscle (washed and dried) of *Mytilus edulis*, and (2) that of stretched horn.

Considering now these two further points : (*a*), that in spite of the close resemblance between their X-ray photographs the sulphur content of myosin is quite small compared with that of keratin, which among proteins is outstandingly rich in sulphur ; and (*b*), that the side-chain breakdown shown by X-ray analysis to precede the supercontracting state of keratin has been found by Speakman[8] to be largely concerned with the cystine –S–S– linkage between neighbouring main-chains, we gain the strong impression that hair protein is no other than a kind of muscle protein 'vulcanised' in order to reduce its elastic sensitivity and at the same time impart resistance to chemical attack.

The molecule of feather keratin appears to be in a slightly contracted β-configuration. X-rays show that it can be stretched continuously and reversibly over a range of some seven per cent[9].

Svedberg's investigations with the ultra-centrifuge[10] indicate that the molecules of many soluble proteins are large globular units or combinations of such units, and crystallographic examination and certain X-ray results support this view. Most X-ray photographs, however, taken without any special precautions, suggest almost the antithesis of this and indicate that the molecules either consist of, or generate spontaneously, polypeptide chains configurationally analogous to β-keratin. The idea thus takes shape that most proteins as usually examined by X-rays are in a degenerate state, that their original specific configuration has broken down partially or completely to form disoriented polypeptide chain-bundles[11]. The hypothesis is further strengthened by the observation[11] that when proteins are deliberately 'denatured', for example by heat, the resemblance to disoriented β-keratin becomes even more pronounced. This will be clear by comparing Figs. (2) and (3), the latter being a powder photograph of boiled egg-white—or for that matter, to all intents and purposes, any denatured protein. A crucial test, therefore, is to see whether it is possible to obtain an X-ray photograph like that of *oriented* β-keratin simply by stretching a denatured protein.

This test has now been carried out, and artificial fibres and films of denatured albumins and globulins have been shown to give, on stretching, X-ray photographs typical of oriented bundles of fully-extended polypeptides. The crystallographic orientation, though, of these photographs is not always the same : whereas stretched denatured edestin, for example, gives a photograph like Fig. (2), corresponding to chains lying *along* the axis of extension, a stretched film of 'poached' egg-white gives a photograph like Fig. (4), corresponding to chains lying *across* the axis of extension. The observed types of photograph can be explained only on the assumption that the polypeptide chains in denatured albumins are in general much shorter than those in denatured globulins[12].

It should be noted that films and fibres of denatured proteins are elastic, and often over a great range, presumably because the denaturation process usually results in a random, and maybe sometimes incomplete, liberation of chains which in part coalesce into bundles and in part remain in an irregularly coiled-up state from which they may be pulled out by tension, rather in the manner of the polyprene chains when rubber is stretched.

The immediate question of the future concerns the precise relation or relations between the 'globular' proteins and the chain-molecules to which they give rise so readily. Which is the more fundamental, the globular form or the chain form, and is one obtained from the other by a process of simple coiling or uncoiling, or is the chain form a consequence of the linear polymerisation or condensation of the globular form ? At the moment there is evidence for both possibilities[12], though it seems more likely that in general denaturation involves little more than (*a*) the dissolution of intramolecular co-valent linkages, in particular the –S–S– linkage, (*b*) the uncoiling of a chain system, and finally (*c*) the coagulation or 'crystallisation' of the liberated chains into bundles configurationally analogous to those of β-keratin[11]. It must be emphasised, however, that though denaturation appears always to lead to the formation of polypeptide chain-bundles, it does not follow that all chain-bundles are necessarily denatured. The muscle protein, myosin, for example, may be said to be 'configurationally disposed' towards denaturation—and indeed it denatures with extreme ease—but so long as it is not allowed to dry it tends to remain soluble. It is clear that if irreversibility is to be avoided, then at least the chains must be kept from too intimate relations with one another.

What then constitutes reversibility of denaturation, if there is such a thing, and Anson and Mirsky[13] maintain that there is ? At the moment, the answer given by X-rays would appear to be this, that it is determined by (*a*) the extent to which a globular protein may be uncoiled reversibly, and (*b*) the possibility of keeping the liberated chains from agglomerating into parallel bundles ; or in other words, of avoiding more intimate or widespread interaction than obtained in the original globular configuration. Much seems to depend on the way the globular proteins are built up in the first place. If they are built up piecemeal and not by the coiling of one or more polypeptide chains, then reversible uncoiling is perhaps unlikely.

A purely formal approach to the constitution of the globular proteins may be made by combining the results of X-ray analysis with the study of protein monolayers. From the X-ray examination of the fibrous proteins we must conclude that in fully-extended polypeptide chains, in β-keratin or denatured edestin, for example, the average length of an amino-acid residue is about 3½ A., its thickness is about 4½ A., and its average width in the side-chain direction is about 10 A. The density of a fibrous or denatured protein, therefore, should be about $0{\cdot}0105R$, where R is the average residue-weight of the amino-acids in question. If we take R to be of the order of 120, this gives a density of 1·26 gm. per c.c.—and it is a fact that proteins do have very much this sort of density. Similarly, protein monolayers formed of parallel arrays of more or less fully-extended polypeptides with their side-chains dipping into the substrate should have a common area (not allowing for hydration) of about $95{\cdot}5/R$ sq. metres per mgm.; that is, for $R=120$, about 0·795 sq. m. per mgm. Now Gorter and his collaborators[14], for example, find always in the region of *p*H 1 an area (extrapolated to zero pressure) of the order of 1 sq. m. per mgm., and a similar area often at the isoelectric point also, whether the molecular weight be 35,000 (Svedberg's 'unit') or a multiple of that; for example, insulin (*p*H 5), 0·875: pepsin (*p*H 2·7), 1·0: zein (*p*H 5·5), 1·07: ovalbumin (*p*H 4·7), 0·88: casein (*p*H 4·6), 1·04 sq. m. per mgm. The natural conclusion is that protein monolayers, under certain conditions at least, are formed by the liberation of polypeptide chains from an originally globular configuration in something the same way as in the process of denaturation.

A further helpful step forward was made by Gorter[15] when he showed that the area of a protein monolayer, under the conditions defined above, corresponds to that of a set of spheres of radius about 22 A., which is the radius found by Svedberg for his spherical units of weight 35,000. (More recently Bernal and Crowfoot, in the only reasonably successful X-ray analyses of unaltered single protein crystals so far accomplished, have arrived at a similar result for the molecules of pepsin and insulin[16].) Gorter's result may also be derived from first principles by means of the X-ray data given above; but it is difficult to proceed to the obvious inference that globular proteins are by way of being simply curved monolayers with the side-chains directed radially, because the calculated density (about 1·14) of such systems is too low.

It is a significant fact that the density of the globular proteins is roughly the same as that of the fibrous proteins. What other possibilities are there then that conform to Gorter's finding? One is the cylinder whose height is equal to its diameter, and whose area, therefore, is again $4\pi r^2$, but the calculated density of this (about 1) is still less satisfying. Actually there is no solution along these lines *except by building a system out of pieces of monolayer separated by the characteristic 'side-chain spacing' found by X-ray analysis. We have thus to place four disks of monolayer of diameter about* 40 *A. on top of one another at a distance of about* 10 *A. apart.* Both the weight and the dimensions then correspond to those of Svedberg's units, the density is correct, and the area of the liberated monolayer would be equal to the surface area of spheres of the same diameter. Furthermore, though X-ray data on the globular proteins are still so meagre, such an arrangement fits in well with present indications[11,12] that their structure must in many cases be somehow closely related to that of the fibrous proteins.

Just recently, Wrinch has arrived at similar conclusions along quite different lines of reasoning[17]. In effect, she has succeeded in generalising the two features of the α–β keratin transformation that it was found necessary to postulate in order to explain quantitatively the experimental facts: (*a*) that the chain should fold hexagonally at regular intervals, and (*b*) that the side-chains should stand out transverse to the plane of folding. Perhaps there is some justification then after all for the suggestion[18] that in a way keratin is the grandfather of all proteins.

[1] K. H. Meyer and H. Mark, "Der Aufbau der hochpolymeren organischen Naturstoffe", 1930.

[2] W. T. Astbury, *J. Soc. Chem. Ind.*, **49**, 441 (1930). *J. Text. Sci.*, **4**, 1 (1931). "Fundamentals of Fibre Structure", 1933. W. T. Astbury and A. Street, *Phil. Trans. Roy. Soc.*, A, **230**, 75 (1931).

[3] W. T. Astbury and H. J. Woods, NATURE, **126**, 913 (1930); *Phil. Trans. Roy. Soc.*, A, **232**, 333 (1933); W. T. Astbury and W. A. Sisson, *Proc. Roy. Soc.*, A, **150**, 533 (1935); H. J. Woods, NATURE, **132**, 709 (1933).

[4] W. T. Astbury, *Trans. Faraday Soc.*, **29**, 193 (1933); Cold Spring Harbor Symposia on Quantitative Biology, **2**, 15 (1934); *Kolloid-Z.*, **69**, 340 (1934).

[5] W. T. Astbury and S. Dickinson, NATURE, **135**, 95, 765 (1935).

[6] G. Boehm and H. H. Weber, *Kolloid-Z.*, **61**, 269 (1932).

[7] H. H. Weber, *Pflüg. Arch.*, **235**, 205 (1934).

[8] J. B. Speakman, NATURE, **132**, 930 (1933); Jubilee Issue of *J. Soc. Dyers and Colourists*, **34** (1934).

[9] W. T. Astbury and T. C. Marwick, NATURE, **130**, 309 (1932); W. T. Astbury, *Trans. Faraday Soc.*, **29**, 206 (1933); *Kolloid-Z.*, **69**, 340 (1934).

[10] See, for example, *Chem. Reviews*, **14**, 1 (1934), and numerous papers in NATURE, *J. Amer. Chem. Soc.*, *Kolloid-Z.*, etc.

[11] W. T. Astbury and R. Lomax, NATURE, **133**, 795 (1934); *J. Chem Soc.*, 846 (1935).

[12] W. T. Astbury, S. Dickinson, and K. Bailey, *Biochem. J.*, **29**, 2351 (1935).

[13] See papers in *J. Gen. Physiol.* over the last ten years.

[14] E. Gorter and F. Grendel, *Trans. Faraday Soc.*, **22**, 477 (1926): *Proc. Kon. Akad. Wetensch.*, **29**, 1262 (1926); *Biochem. Z.*, **201**, 391 (1928); E. Gorter, J. van Ormondt, and F. J. P. Dom, *Proc. Kon Akad. Wetensch.*, **35**, 838 (1932); E. Gorter and J. van Ormondt, ibid., **36**, 922 (1933); *Biochem. J.*, **29**, 48 (1935); E. Gorter and G. T. Philippi, *Proc. Kon. Akad. Wetensch.*, **37**, 788 (1934); E. Gorter, ibid., **37**, 20 (1934); *Amer. J. Diseases of Children*, **47**, 945 (1934); *J. Gen. Physiol.*, **18**, 421 (1935); E. Gorter and W. A. Seeder, ibid., **18**, 427, (1935); etc.

[15] E. Gorter and F. Grendel, *Proc. Kon. Akad. Wetensch.*, **32**, 770 (1929).

[16] J. D. Bernal and D. Crowfoot, NATURE, **133**, 794 (1934); D Crowfoot, ibid., **135**, 591 (1935).

[17] D. M. Wrinch, NATURE, **137**, 411 (1936).

[18] W. T. Astbury, *Kolloid-Z.*, **69**, 340 (1934); W. T. Astbury and R. Lomax, NATURE, **133**, 795 (1934).

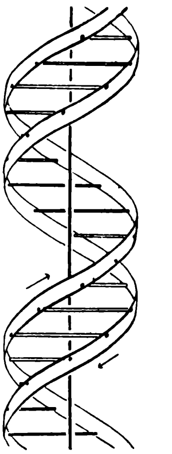
MOLECULAR STRUCTURE OF NUCLEIC ACIDS

A Structure for Deoxyribose Nucleic Acid

WE wish to suggest a structure for the salt of deoxyribose nucleic acid (D.N.A.). This structure has novel features which are of considerable biological interest.

A structure for nucleic acid has already been proposed by Pauling and Corey[1]. They kindly made their manuscript available to us in advance of publication. Their model consists of three intertwined chains, with the phosphates near the fibre axis, and the bases on the outside. In our opinion, this structure is unsatisfactory for two reasons: (1) We believe that the material which gives the X-ray diagrams is the salt, not the free acid. Without the acidic hydrogen atoms it is not clear what forces would hold the structure together, especially as the negatively charged phosphates near the axis will repel each other. (2) Some of the van der Waals distances appear to be too small.

Another three-chain structure has also been suggested by Fraser (in the press). In his model the phosphates are on the outside and the bases on the inside, linked together by hydrogen bonds. This structure as described is rather ill-defined, and for this reason we shall not comment on it.



This figure is purely diagrammatic. The two ribbons symbolize the two phosphate—sugar chains, and the horizontal rods the pairs of bases holding the chains together. The vertical line marks the fibre axis

We wish to put forward a radically different structure for the salt of deoxyribose nucleic acid. This structure has two helical chains each coiled round the same axis (see diagram). We have made the usual chemical assumptions, namely, that each chain consists of phosphate diester groups joining β-D-deoxyribofuranose residues with 3′,5′ linkages. The two chains (but not their bases) are related by a dyad perpendicular to the fibre axis. Both chains follow right-handed helices, but owing to the dyad the sequences of the atoms in the two chains run in opposite directions. Each chain loosely resembles Furberg's[2] model No. 1; that is, the bases are on the inside of the helix and the phosphates on the outside. The configuration of the sugar and the atoms near it is close to Furberg's 'standard configuration', the sugar being roughly perpendicular to the attached base. There is a residue on each chain every 3·4 A. in the *z*-direction. We have assumed an angle of 36° between adjacent residues in the same chain, so that the structure repeats after 10 residues on each chain, that is, after 34 A. The distance of a phosphorus atom from the fibre axis is 10 A. As the phosphates are on the outside, cations have easy access to them.

The structure is an open one, and its water content is rather high. At lower water contents we would expect the bases to tilt so that the structure could become more compact.

The novel feature of the structure is the manner in which the two chains are held together by the purine and pyrimidine bases. The planes of the bases are perpendicular to the fibre axis. They are joined together in pairs, a single base from one chain being hydrogen-bonded to a single base from the other chain, so that the two lie side by side with identical *z*-co-ordinates. One of the pair must be a purine and the other a pyrimidine for bonding to occur. The hydrogen bonds are made as follows: purine position 1 to pyrimidine position 1; purine position 6 to pyrimidine position 6.

If it is assumed that the bases only occur in the structure in the most plausible tautomeric forms (that is, with the keto rather than the enol configurations) it is found that only specific pairs of bases can bond together. These pairs are: adenine (purine) with thymine (pyrimidine), and guanine (purine) with cytosine (pyrimidine).

In other words, if an adenine forms one member of a pair, on either chain, then on these assumptions the other member must be thymine; similarly for guanine and cytosine. The sequence of bases on a single chain does not appear to be restricted in any way. However, if only specific pairs of bases can be formed, it follows that if the sequence of bases on one chain is given, then the sequence on the other chain is automatically determined.

It has been found experimentally[3,4] that the ratio of the amounts of adenine to thymine, and the ratio of guanine to cytosine, are always very close to unity for deoxyribose nucleic acid.

It is probably impossible to build this structure with a ribose sugar in place of the deoxyribose, as the extra oxygen atom would make too close a van der Waals contact.

The previously published X-ray data[5,6] on deoxyribose nucleic acid are insufficient for a rigorous test of our structure. So far as we can tell, it is roughly compatible with the experimental data, but it must be regarded as unproved until it has been checked against more exact results. Some of these are given in the following communications. We were not aware of the details of the results presented there when we devised our structure, which rests mainly though not entirely on published experimental data and stereochemical arguments.

It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.

Full details of the structure, including the conditions assumed in building it, together with a set of co-ordinates for the atoms, will be published elsewhere.

We are much indebted to Dr. Jerry Donohue for constant advice and criticism, especially on interatomic distances. We have also been stimulated by a knowledge of the general nature of the unpublished experimental results and ideas of Dr. M. H. F. Wilkins, Dr. R. E. Franklin and their co-workers at King's College, London. One of us (J. D. W.) has been aided by a fellowship from the National Foundation for Infantile Paralysis.

J. D. WATSON
F. H. C. CRICK

Medical Research Council Unit for the Study of the Molecular Structure of Biological Systems,
Cavendish Laboratory, Cambridge.
April 2.

[1] Pauling, L., and Corey, R. B., *Nature*, **171**, 346 (1953); *Proc. U.S. Nat. Acad. Sci.*, **39**, 84 (1953).
[2] Furberg, S., *Acta Chem. Scand.*, **6**, 634 (1952).
[3] Chargaff, E., for references see Zamenhof, S., Brawerman, G., and Chargaff, E., *Biochim. et Biophys. Acta*, **9**, 402 (1952).
[4] Wyatt, G. R., *J. Gen. Physiol.*, **36**, 201 (1952).
[5] Astbury, W. T., Symp. Soc. Exp. Biol. 1, Nucleic Acid, 66 (Camb. Univ. Press, 1947).
[6] Wilkins, M. H. F., and Randall, J. T., *Biochim. et Biophys. Acta*, **10**, 192 (1953).

LYSENKO IN PERSPECTIVE

Agrobiology

Essays on Problems of Genetics, Plant Breeding and Seed Growing. By T. D. Lysenko. (Translated from the Fourth Russian edition.) Pp. 636+12 plates. (Moscow: Foreign Languages Publishing House; London: Collet's Holdings, Ltd., 1954.) 15*s*.

BRITISH research workers regard polemical papers with suspicion, not to say distaste. This circumstance has engendered among us a delicate art of scientific (as distinct from literary) criticism. A critic is permitted to demonstrate that his colleague's facts are incorrect. He can go further and demonstrate that his colleague's hypotheses are inconsistent with the facts. But he cannot with propriety go further still and impute motives or reasons for his colleague's inaccuracies: as that his technique was sloppy, or that his ambition drove him to publish prematurely, or that his political views coloured his writing, or simply that he is a liar. Therefore we would not expect to read in the *Annals of Botany* a passage like the following: "Why has . . . one of our outstanding biologists fallen into error in his work on mutation of species? . . . Because he has departed from the principles of Darwinism . . . [and because his] statements have not been subjected to the appropriate comradely criticism without which, as our teacher Stalin points out, science cannot advance".

This quotation is from the *Botanicheski Zhurnal* (Botanical Journal) for December 1952. The writer is a *bona fide* botanist and the outstanding biologist he refers to is T. D. Lysenko. British scientific journals would not be likely to accept such a passage as this, even if it were about Lysenko, and it is important to remember this difference in intellectual climate when we discuss Soviet science. Of course, the Russians publish scores of papers which are as objective and impersonal as the papers in British journals. But when they do descend to scandal they do so unambiguously and in public.

This much understanding of the Russian temperament is a prerequisite for reading the recently translated volume of Lysenko's collected essays on agrobiology. If the British reader approaches it as he would (for example) a volume of essays from Rothamsted he will be nauseated. If he approaches it to gain an understanding not of agrobiology but of Lysenko's career he will be deeply interested. For this book contains some of the data necessary for an understanding of the recent history of biology in the U.S.S.R.

It begins with two essays on phasic development in plants, published nineteen years ago. Lysenko had stumbled upon the discovery made by Gassner in 1918 that certain plants ripen earlier if subjected to a cold period, and he spent some years unsuccessfully trying to apply Gassner's technique on a commercial scale. These early essays, though tedious and unconvincing, do make some pretence of recording and interpreting observations. Their obvious intention is not to advance science but to apply science to agriculture. It has to be remembered that while Lysenko was working on phasic development Russia was passing through a terrible crisis. Compulsory collectivization of farms had brought about a collapse of food production, and millions were dying of starvation. It is therefore not surprising that Lysenko's work was tendentious.

There follow in "Agrobiology" half a dozen essays which are little more than harangues to collective farmers. For Lysenko had discovered his flair. He had an uncanny power of persuading peasants to adopt better methods of husbandry and to take a pride in increasing yields; and that was what Russia needed twenty years ago: not so much more science in farming, but simply more common sense in farming. In fulfilling this need Lysenko was notably successful.

His success brought him into conflict with the genuine biologists of Russia for two reasons. First, he sincerely imagined that his successes as a propagandist for better farming were due to his ideas about biology, for he was not content with a pragmatic approach to crop yields: he had to rationalize his methods with the pseudo-philosophical theories of men like Michurin. This he tried to do in a series of violently polemical papers (1937–48), many of which are reproduced in this book. His train of thought was transparently tendentious. "If," he says (p. 230), "I have made a trenchant assault on Mendel's law . . . it is primarily because this law greatly hinders me in my work." Secondly, he sincerely imagined that the failure of other Russian biologists to improve the efficiency of agriculture was due to their preoccupation with fruitless scientific problems and fundamentally unsound theories. The natural reaction of Russian biologists was to ignore Lysenko and his colleagues as troublesome cranks; and this they did successfully until 1948. In that year the State made the decision to give Lysenko official support, and his powerful adversaries were driven into obscurity. It is a mistake to suppose that any reputable Russian scientist was under the impression that Lysenko had made any major discovery. It was a temporary triumph of expediency over truth. As Zhukovsky said at the time, it is important "at this juncture to cherish the prestige" of Lysenko.

"Agrobiology" reproduces in full Lysenko's speech to the Academy of Agricultural Sciences on this melancholy occasion in 1948. There follow a series of essays in which Lysenko's ideas, now free from public criticism, undergo an unrestrained proliferation. He claims to have produced, by manipulating the environment, soft wheats from hard wheats and rye from wheat. He writes less agricultural propaganda and more woolly theory. Apart from advocating the sowing of forest belts on hills, he has little to say which is likely to catch the imagination of the peasant. His articles, notably one (p. 582) published in 1952 for the "Soviet Encyclopedia", become more speculative and more fantastic. Meanwhile, Russian agriculture has again entered upon a crisis, and N. S. Khrushchev, first secretary of the Central Committee of the Party, initiated in 1953 a new set of reforms on collective farms. He heavily criticized the previous policy of the Ministry of Agriculture, and it seems as though he is now willing to charge Lysenko with the very sort of inefficacy with which Lysenko charged his critics in 1948. For on March 29, 1954, the official journal of the Communist Party published the following statement: "The monopolisation of science leads . . . to the cutting off of people who do not think on orthodox lines, and living scientific thought is stifled. This has been shown, for instance, in the All Union Academy of Agricultural Sciences".

Lysenko is still director of this Academy. For a time, and probably due to the critical condition of Soviet agriculture, the Russian Government backed a man who would otherwise have sunk into the scientific obscurity he deserves. His book of essays is a warning against confusing science with practice; and it now seems that it may mark the end of a deplorable incident in the history of scientific thought.

E. ASHBY

GENERAL NATURE OF THE GENETIC CODE FOR PROTEINS

By Dr. F. H. C. CRICK, F.R.S., LESLIE BARNETT, Dr. S. BRENNER and Dr. R. J. WATTS-TOBIN

Medical Research Council Unit for Molecular Biology, Cavendish Laboratory, Cambridge

THERE is now a mass of indirect evidence which suggests that the amino-acid sequence along the polypeptide chain of a protein is determined by the sequence of the bases along some particular part of the nucleic acid of the genetic material. Since there are twenty common amino-acids found throughout Nature, but only four common bases, it has often been surmised that the sequence of the four bases is in some way a code for the sequence of the amino-acids. In this article we report genetic experiments which, together with the work of others, suggest that the genetic code is of the following general type:

(*a*) A group of three bases (or, less likely, a multiple of three bases) codes one amino-acid.

(*b*) The code is not of the overlapping type (see Fig. 1).

(*c*) The sequence of the bases is read from a fixed starting point. This determines how the long sequences of bases are to be correctly read off as triplets. There are no special 'commas' to show how to select the right triplets. If the starting point is displaced by one base, then the reading into triplets is displaced, and thus becomes incorrect.

(*d*) The code is probably 'degenerate'; that is, in general, one particular amino-acid can be coded by one of several triplets of bases.

The Reading of the Code

The evidence that the genetic code is not overlapping (see Fig. 1) does not come from our work, but from that of Wittmann[1] and of Tsugita and Fraenkel-Conrat[2] on the mutants of tobacco mosaic virus produced by nitrous acid. In an overlapping triplet code, an alteration to one base will in general change three adjacent amino-acids in the polypeptide chain. Their work on the alterations produced in the protein of the virus show that usually only one amino-acid at a time is changed as a result of treating the ribonucleic acid (RNA) of the virus with nitrous acid. In the rarer cases where two amino-acids are altered (owing presumably to two separate deaminations by the nitrous acid on one piece of RNA), the altered amino-acids are not in adjacent positions in the polypeptide chain.

Brenner[3] had previously shown that, if the code were universal (that is, the same throughout Nature), then all overlapping triplet codes were impossible. Moreover, all the abnormal human hæmoglobins studied in detail[4] show only single amino-acid changes. The newer experimental results essentially rule out all simple codes of the overlapping type.

If the code is not overlapping, then there must be some arrangement to show how to select the correct triplets (or quadruplets, or whatever it may be) along the continuous sequence of bases. One obvious suggestion is that, say, every fourth base is a 'comma'. Another idea is that certain triplets make 'sense', whereas others make 'nonsense', as in the comma-free codes of Crick, Griffith and Orgel[5]. Alternatively, the correct choice may be made by starting at a fixed point and working along the sequence of bases three (or four, or whatever) at a time. It is this possibility which we now favour.

Experimental Results

Our genetic experiments have been carried out on the *B* cistron of the r_{II} region of the bacteriophage *T*4, which attacks strains of *Escherichia coli*. This is the system so brilliantly exploited by Benzer[6,7]. The r_{II} region consists of two adjacent genes, or 'cistrons', called cistron *A* and cistron *B*. The wild-type phage will grow on both *E. coli B* (here called *B*) and on *E. coli K*12 (λ) (here called *K*), but a phage which has lost the function of either gene will not grow on *K*. Such a phage produces an *r* plaque on *B*. Many point mutations of the genes are known which behave in this way. Deletions of part of the region are also found. Other mutations, known as 'leaky', show partial function; that is, they will grow on *K* but their plaque-type on *B* is not truly wild. We report here our work on the mutant *P* 13 (now re-named *FC* 0) in the *B*1 segment of the *B* cistron. This mutant was originally produced by the action of proflavin[8].

We[9] have previously argued that acridines such as proflavin act as mutagens because they add or delete a base or bases. The most striking evidence in favour of this is that mutants produced by acridines are seldom 'leaky'; they are almost always completely lacking in the function of the gene. Since our note was published, experimental data from two sources have been added to our previous evidence: (1) we have examined a set of 126 r_{II} mutants made with acridine yellow; of these only 6 are leaky (typically about half the mutants made with base analogues are leaky); (2) Streisinger[10] has found that whereas mutants of the lysozyme of phage *T*4 produced by base-analogues are usually leaky, all lysozyme mutants produced by proflavin are negative, that is, the function is completely lacking.

If an acridine mutant is produced by, say, adding a base, it should revert to 'wild-type' by deleting a base. Our work on revertants of *FC* 0 shows that it usually

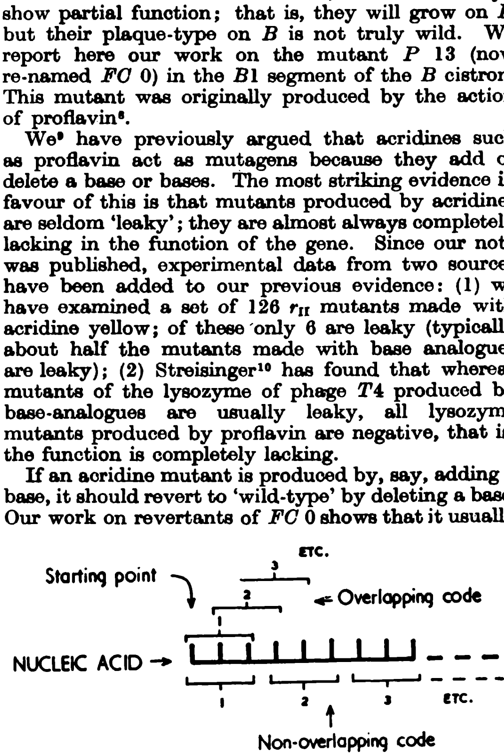


Fig. 1. To show the difference between an overlapping code and a non-overlapping code. The short vertical lines represent the bases of the nucleic acid. The case illustrated is for a triplet code

reverts not by reversing the original mutation but by producing a second mutation at a nearby point on the genetic map. That is, by a 'suppressor' in the same gene. In one case (or possibly two cases) it may have reverted back to true wild, but in at least 18 other cases the 'wild type' produced was really a double mutant with a 'wild' phenotype. Other workers[11] have found a similar phenomenon with r_{II} mutants, and Jinks[12] has made a detailed analysis of suppressors in the h_{III} gene.

The genetic map of these 18 suppressors of *FC* 0 is shown in Fig. 2, line *a*. It will be seen that they all fall in the *B*1 segment of the gene, though not all of them are very close to *FC* 0. They scatter over a region about, say, one-tenth the size of the *B* cistron. Not all are at different sites. We have found eight sites in all, but most of them fall into or near two close clusters of sites.

In all cases the suppressor was a non-leaky *r*. That is, it gave an *r* plaque on *B* and would not grow on *K*. This is the phenotype shown by a complete deletion of the gene, and shows that the function is lacking. The only possible exception was one case where the suppressor appeared to back-mutate so fast that we could not study it.

Each suppressor, as we have said, fails to grow on *K*. Reversion of each can therefore be studied by the same procedure used for *FC* 0. In a few cases these mutants apparently revert to the original wild-type, but usually they revert by forming a double mutant. Fig. 2, lines *b*–*g*, shows the mutants produced as suppressors of these suppressors. Again all these new suppressors are non-leaky *r* mutants, and all map within the *B*1 segment for one site in the *B*2 segment.

Once again we have repeated the process on two of the new suppressors, with the same general results, as shown in Fig. 2, lines *i* and *j*.

All these mutants, except the original *FC* 0, occurred spontaneously. We have, however, produced one set (as suppressors of *FC* 7) using acridine yellow as a mutagen. The spectrum of suppressors we get (see Fig. 2, line *h*) is crudely similar to the spontaneous spectrum, and all the mutants are non-leaky *r*'s. We have also tested a (small) selection of all our mutants and shown that their reversion-rates are increased by acridine yellow.

Thus in all we have about eighty independent *r* mutants, all suppressors of *FC* 0, or suppressors of suppressors, or suppressors of suppressors of suppressors. They all fall within a limited region of the gene and they are all non-leaky *r* mutants.

The double mutants (which contain a mutation plus its suppressor) which plate on *K* have a variety of plaque types on *B*. Some are indistinguishable from wild, some can be distinguished from wild with difficulty, while others are easily distinguishable and produce plaques rather like *r*.

We have checked in a few cases that the phenomenon is quite distinct from 'complementation', since the two mutants which separately are phenotypically *r*, and together are wild or pseudo-wild,

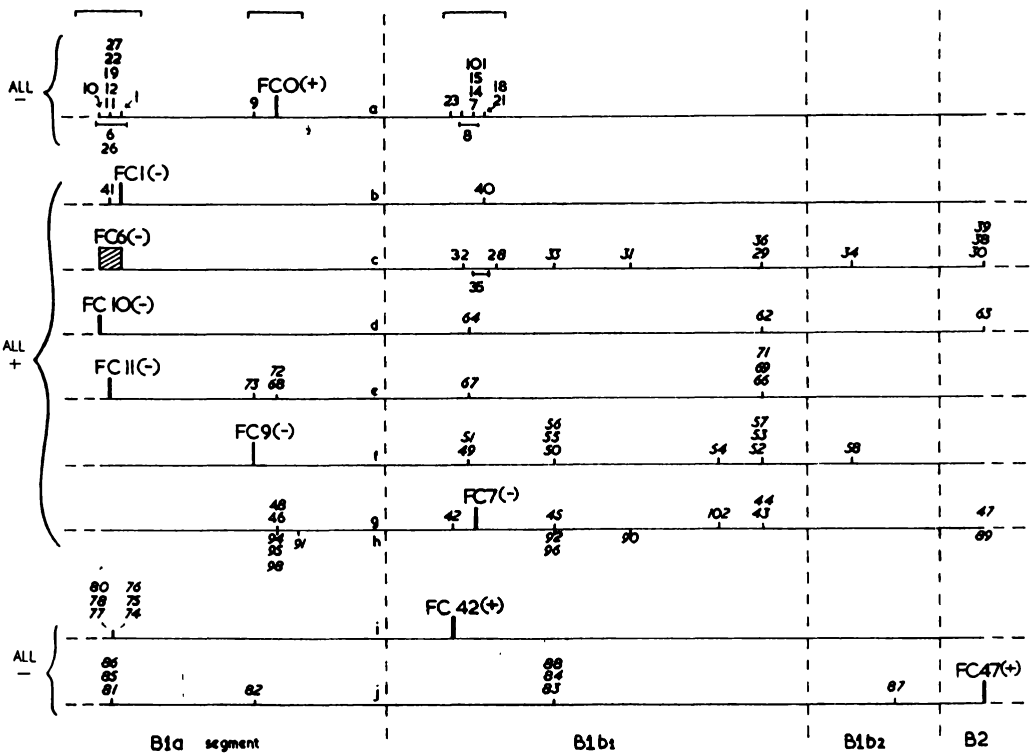


Fig. 2. A tentative map—only very roughly to scale—of the left-hand end of the *B* cistron, showing the position of the *FC* family of mutants. The order of sites within the regions covered by brackets (at the top of the figure) is not known. Mutants in italics have only been located approximately. Each line represents the suppressors picked up from one mutant, namely, that marked on the line in bold figures

must be put together in the same piece of genetic material. A simultaneous infection of *K* by the two mutants in separate viruses will not do.

The Explanation in Outline

Our explanation of all these facts is based on the theory set out at the beginning of this article. Although we have no direct evidence that the *B* cistron produces a polypeptide chain (probably through an RNA intermediate), in what follows we shall assume this to be so. To fix ideas, we imagine that the string of nucleotide bases is read, triplet by triplet, from a starting point on the left of the *B* cistron. We now suppose that, for example, the mutant *FC* 0 was produced by the insertion of an additional base in the wild-type sequence. Then this addition of a base at the *FC* 0 site will mean that the reading of all the triplets to the right of *FC* 0 will be shifted along one base, and will therefore be incorrect. Thus the amino-acid sequence of the protein which the *B* cistron is presumed to produce will be completely altered from that point onwards. This explains why the function of the gene is lacking. To simplify the explanation, we now postulate that a suppressor of *FC* 0 (for example, *FC* 1) is formed by deleting a base. Thus when the *FC* 1 mutation is present by itself, all triplets to the right of *FC* 1 will be read incorrectly and thus the function will be absent. However, when both mutations are present in the same piece of DNA, as in the pseudo-wild double mutant *FC* (0 + 1), then although the reading of triplets between *FC* 0 and *FC* 1 will be altered, the original reading will be restored to the rest of the gene. This could explain why such double mutants do not always have a true wild phenotype but are often pseudo-wild, since on our theory a small length of their amino-acid sequence is different from that of the wild-type.

For convenience we have designated our original mutant *FC* 0 by the symbol + (this choice is a pure convention at this stage) which we have so far considered as the addition of a single base. The suppressors of *FC* 0 have therefore been designated −. The suppressors of these suppressors have in the same way been labelled as +, and the suppressors of these last sets have again been labelled − (see Fig. 2).

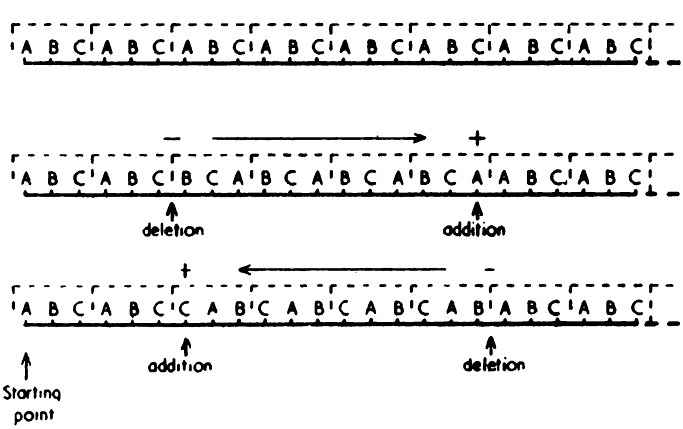


Fig. 3. To show that our convention for arrows is consistent. The letters *A*, *B* and *C* each represent a different base of the nucleic acid. For simplicity a repeating sequence of bases, *ABC*, is shown. (This would code for a polypeptide for which every amino-acid was the same.) A triplet code is assumed. The dotted lines represent the imaginary 'reading frame' implying that the sequence is read in sets of three starting on the left

Double Mutants

We can now ask: What is the character of any double mutant we like to form by putting together in the same gene any pair of mutants from our set of about eighty? Obviously, in some cases we already know the answer, since some combinations of a + with a − were formed in order to isolate the mutants. But, by definition, no pair consisting of one + with another + has been obtained in this way, and there are many combinations of + with − not so far tested.

Now our theory clearly predicts that all combinations of the type + with + (or − with −) should give an *r* phenotype and not plate on *K*. We have put together 14 such pairs of mutants in the cases listed in Table 1 and found this prediction confirmed.

Table 1. Double Mutants having the *r* Phenotype

− With −	+ With +	
FC (1 + 21)	*FC* (0 + 58)	*FC* (40 + 57)
FC (23 + 21)	*FC* (0 + 38)	*FC* (40 + 58)
FC (1 + 23)	*FC* (0 + 40)	*FC* (40 + 55)
FC (1 + 9)	*FC* (0 + 55)	*FC* (40 + 54)
	FC (0 + 54)	*FC* (40 + 38)

At first sight one would expect that all combinations of the type (+ with −) would be wild or pseudo-wild, but the situation is a little more intricate than that, and must be considered more closely. This springs from the obvious fact that if the code is made of triplets, any long sequence of bases can be read correctly in one way, but incorrectly (by starting at the wrong point) in two different ways, depending whether the 'reading frame' is shifted one place to the right or one place to the left.

If we symbolize a shift, by one place, of the reading frame in one direction by → and in the opposite direction by ←, then we can establish the convention that our + is always at the head of the arrow, and our − at the tail. This is illustrated in Fig. 3.

We must now ask: Why do our suppressors not extend over the whole of the gene? The simplest postulate to make is that the shift of the reading frame produces some triplets the reading of which is 'unacceptable'; for example, they may be 'nonsense', or stand for 'end the chain', or be unacceptable in some other way due to the complications of protein structure. This means that a suppressor of, say, *FC* 0 must be within a region such that no 'unacceptable' triplet is produced by the shift in the reading frame between *FC* 0 and its suppressor. But, clearly, since for any sequence there are *two* possible misreadings, we might expect that the 'unacceptable' triplets produced by a → shift would occur in different places on the map from those produced by a ← shift.

Examination of the spectra of suppressors (in each case putting in the arrows → or ←) suggests that while the → shift is acceptable anywhere within our region (though not outside it) the shift ←, starting from points near *FC* 0, is acceptable over only a more limited stretch. This is shown in Fig. 4. Somewhere in the left part of our region, between *FC* 0 or *FC* 9 and the *FC* 1 group, there must be one or more unacceptable triplets when a ← shift is made; similarly for the region to the right of the *FC* 21 cluster. Thus we predict that a combination of a + with a − will be wild or pseudo-wild if it involves a → shift, but that such pairs involving a ← shift will be phenotypically *r* if the arrow crosses one or more of the forbidden places, since then an unacceptable triplet will be produced.

Table 2. DOUBLE MUTANTS OF THE TYPE (+ WITH −)

− \ +	*FC* 41	*FC* 0	*FC* 40	*FC* 42	*FC* 58*	*FC* 63	*FC* 38
FC 1	*W*	*W*	*W*		W		W
FC 86		W	W	W	W	W	
FC 9	*r*	*W*	W	W	*W*		W
FC 82	*r*		W	W	W	W	
FC 21	*r*	*W*			W		W
FC 88	*r*	*r*			W	W	
FC 87	*r*	*r*	*r*	*r*			W

W, wild or pseudo-wild phenotype; *W*, wild or pseudo-wild combination used to isolate the suppressor; *r*, *r* phenotype.
* Double mutants formed with *FC* 58 (or with *FC* 34) give sharp plaques on *K*.

We have tested this prediction in the 28 cases shown in Table 2. We expected 19 of these to be wild, or pseudo-wild, and 9 of them to have the *r* phenotype. In all cases our prediction was correct. We regard this as a striking confirmation of our theory. It may be of interest that the theory was constructed before these particular experimental results were obtained.

Rigorous Statement of the Theory

So far we have spoken as if the evidence supported a triplet code, but this was simply for illustration. Exactly the same results would be obtained if the code operated with groups of, say, 5 bases. Moreover, our symbols + and − must not be taken to mean literally the addition or subtraction of a single base.

It is easy to see that our symbolism is more exactly as follows:

+ represents $+m$, modulo n
− represents $-m$, modulo n

where n (a positive integer) is the coding ratio (that is, the number of bases which code one amino-acid) and m is any integral number of bases, positive or negative.

It can also be seen that our choice of reading direction is arbitrary, and that the same results (to a first approximation) would be obtained in whichever direction the genetic material was read, that is, whether the starting point is on the right or the left of the gene, as conventionally drawn.

Triple Mutants and the Coding Ratio

The somewhat abstract description given above is necessary for generality, but fortunately we have convincing evidence that the coding ratio is in fact 3 or a multiple of 3.

This we have obtained by constructing triple mutants of the form (+ with + with +) or (− with − with −). One must be careful not to make shifts across the 'unacceptable' regions for the ← shifts, but these we can avoid by a proper choice of mutants.

Table 3. TRIPLE MUTANTS HAVING A WILD OR PSEUDO-WILD PHENOTYPE

FC (0 + 40 + 38)
FC (0 + 40 + 58)
FC (0 + 40 + 57)
FC (0 + 40 + 54)
FC (0 + 40 + 55)
FC (1 + 21 + 23)

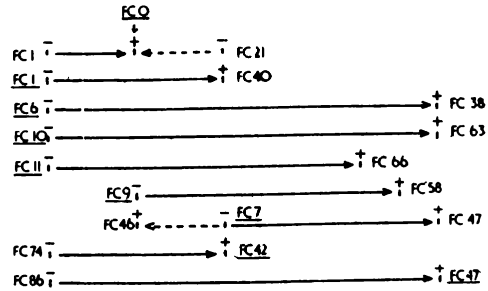


Fig. 4. A simplified version of the genetic map of Fig. 2. Each line corresponds to the suppressor from one mutant, here underlined. The arrows show the range over which suppressors have so far been found, the extreme mutants being named on the map. Arrows to the right are shown solid, arrows to the left dotted

We have so far examined the six cases listed in Table 3 and in all cases the triples are wild or pseudo-wild.

The rather striking nature of this result can be seen by considering one of them, for example, the triple (*FC* 0 with *FC* 40 with *FC* 38). These three mutants are, by themselves, all of like type (+). We can say this not merely from the way in which they were obtained, but because each of them, when combined with our mutant *FC* 9 (—), gives the wild, or pseudo-wild phenotype. However, either singly or together in pairs they have an *r* phenotype, and will not grow on *K*. That is, the function of the gene is absent. Nevertheless, the combination of all three in the same gene partly restores the function and produces a pseudo-wild phage which grows on *K*.

This is exactly what one would expect, in favourable cases, if the coding ratio were 3 or a multiple of 3.

Our ability to find the coding ratio thus depends on the fact that, in at least one of our composite mutants which are 'wild', at least one amino-acid must have been added to or deleted from the polypeptide chain without disturbing the function of the gene-product too greatly.

This is a very fortunate situation. The fact that we can make these changes and can study so large a region probably comes about because this part of the protein is not essential for its function. That this is so has already been suggested by Champe and Benzer[18] in their work on complementation in the r_{II} region. By a special test (combined infection on *K*, followed by plating on *B*) it is possible to examine the function of the *A* cistron and the *B* cistron separately. A particular deletion, 1589 (see Fig. 5) covers the right-hand end of the *A* cistron and part of the left-hand end of the *B* cistron. Although 1589 abolishes the *A* function, they showed that it allows the *B* function to be expressed to a considerable extent. The region of the *B* cistron deleted by 1589 is that into which all our *FC* mutants fall.

Joining two Genes Together

We have used this deletion to re-inforce our idea that the sequence is read in groups from a fixed starting point. Normally, an alteration confined to the *A* cistron (be it a deletion, an acridine mutant, or any other mutant) does not prevent the expression of the *B* cistron. Conversely, no alteration within the *B* cistron prevents the function of the *A* cistron. This implies that there may be a region between the two cistrons which separates them and allows their functions to be expressed individually.

We argued that the deletion 1589 will have lost this separating region and that therefore the two (partly damaged) cistrons should have been joined together. Experiments show this to be the case, for now an alteration to the left-hand end of the *A* cistron, if combined with deletion 1589, can prevent the *B* function from appearing. This is shown in Fig. 5. Either the mutant *P*43 or *X*142 (both of which revert strongly with acridines) will prevent the *B* function when the two cistrons are joined, although both of these mutants are in the *A* cistron. This is also true of *X*142 *S*1, a suppressor of *X*142 (Fig. 5, case *b*). However, the double mutant (*X*142 with *X*142 *S*1), of the type (+ with −), which by itself is pseudo-wild, still has the *B* function when combined with 1589 (Fig. 5, case *c*). We have also tested in this way the 10 deletions listed by Benzer[7], which fall wholely to the left of 1589. Of these, three (386, 168 and 221) prevent the *B* function (Fig. 5, case *f*), whereas the other seven show it (Fig. 5, case *e*). We surmise that each of these seven has lost a number of bases which is a multiple of 3. There are theoretical reasons for expecting that deletions may not be random in length, but will more often have lost a number of bases equal to an integral multiple of the coding ratio.

It would not surprise us if it were eventually shown that deletion 1589 produces a protein which consists of part of the protein from the *A* cistron and part of that from the *B* cistron, joined together in the same polypeptide chain, and having to some extent the function of the undamaged *B* protein.

Is the Coding Ratio 3 or 6 ?

It remains to show that the coding ratio is probably 3, rather than a multiple of 3. Previous rather rough extimates[10,14] of the coding ratio (which are admittedly very unreliable) might suggest that the coding ratio is not far from 6. This would imply, on our theory, that the alteration in *FC* 0 was not to one base, but to two bases (or, more correctly, to an even number of bases).

We have some additional evidence which suggests that this is unlikely. First, in our set of 126 mutants produced by acridine yellow (referred to earlier) we have four independent mutants which fall at or

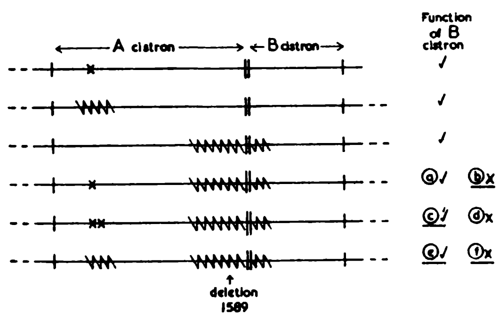


Fig. 5. Summary of the results with deletion 1589. The first two lines show that without 1589 a mutation or a deletion in the *A* cistron does not prevent the *B* cistron from functioning. Deletion 1589 (line 3) also allows the *B* cistron to function. The other cases, in some of which an alteration in the *A* cistron prevents the function of the *B* cistron (when 1589 is also present), are discussed in the text. They have been labelled (*a*), (*b*), etc., for convenience of reference, although cases (*a*) and (*d*) are not discussed in this paper. ✓ implies function; × implies no function

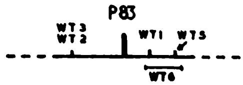


Fig. 6. Genetic map of *P* 83 and its suppressors, *WT* 1, etc. The region falls within segment *B*9*a* near the right-hand end of the *B* cistron. It is not yet known which way round the map is in relation to the other figures

close to the *FC* 9 site. By a suitable choice of partners, we have been able to show that two are + and two are −. Secondly, we have two mutants (*X*146 and *X*225), produced by hydrazine[15], which fall on or near the site *FC* 30. These we have been able to show are both of type −.

Thus unless both acridines and hydrazine usually delete (or add) an even number of bases, this evidence supports a coding ratio of 3. However, as the action of these mutagens is not understood in detail, we cannot be certain that the coding ratio is not 6, although 3 seems more likely.

We have preliminary results which show that other acridine mutants often revert by means of close suppressors, but it is too sketchy to report here. A tentative map of some suppressors of *P* 83, a mutant at the other end of the *B* cistron, in segment *B* 9*a*, is shown in Fig. 6. They occur within a shorter region than the suppressors of *FC* 0, covering a distance of about one-twentieth of the *B* cistron. The double mutant *WT* (2 + 5) has the *r* phenotype, as expected.

Is the Code Degenerate?

If the code is a triplet code, there are 64 (4 × 4 × 4) possible triplets. Our results suggest that it is unlikely that only 20 of these represent the 20 amino-acids and that the remaining 44 are nonsense. If this were the case, the region over which suppressors of the *FC* 0 family occur (perhaps a quarter of the *B* cistron) should be very much smaller than we observe, since a shift of frame should then, by chance, produce a nonsense reading at a much closer distance. This argument depends on the size of the protein which we have assumed the *B* cistron to produce. We do not know this, but the length of the cistron suggests that the protein may contain about 200 amino-acids. Thus the code is probably 'degenerate', that is, in general more than one triplet codes for each amino-acid. It is well known that if this were so, one could also account for the major dilemma of the coding problem, namely, that while the base composition of the DNA can be very different in different micro-organisms, the amino-acid composition of their proteins only changes by a moderate amount[16]. However, exactly how many triplets code amino-acids and how many have other functions we are unable to say.

Future Developments

Our theory leads to one very clear prediction. Suppose one could examine the amino-acid sequence of the 'pseudo-wild' protein produced by one of our double mutants of the (+ with −) type. Conventional theory suggests that since the gene is only altered in two places, only two amino-acids would be changed. Our theory, on the other hand, predicts that a string of amino-acids would be altered, covering the region of the polypeptide chain corresponding to the region on the gene between the two mutants. A good protein on which to test this hypothesis is the lysozyme of the phage, at present being studied chemically by Dreyer[17] and genetically by Streisinger[10].

At the recent Biochemical Congress at Moscow, the audience of Symposium I was startled by the announcement of Nirenberg that he and Matthaei[18] had produced polyphenylalanine (that is, a polypeptide all the residues of which are phenylalanine) by adding polyuridylic acid (that is, an RNA the bases of which are all uracil) to a cell-free system which can synthesize protein. This implies that a sequence of uracils codes for phenylalanine, and our work suggests that it is probably a triplet of uracils.

It is possible by various devices, either chemical or enzymatic, to synthesize polyribonucleotides with defined or partly defined sequences. If these, too, will produce specific polypeptides, the coding problem is wide open for experimental attack, and in fact many laboratories, including our own, are already working on the problem. If the coding ratio is indeed 3, as our results suggest, and if the code is the same throughout Nature, then the genetic code may well be solved within a year.

We thank Dr. Alice Orgel for certain mutants and for the use of data from her thesis, Dr. Leslie Orgel for many useful discussions, and Dr. Seymour Benzer for supplying us with certain deletions. We are particularly grateful to Prof. C. F. A. Pantin for allowing us to use a room in the Zoological Museum, Cambridge, in which the bulk of this work was done.

1 Wittman, H. G., Symp. 1, Fifth Intern. Cong. Biochem., 1961, for refs. (in the press).
2 Tsugita, A., and Fraenkel-Conrat, H., *Proc. U.S. Nat. Acad. Sci.*, **46**, 636 (1960); *J. Mol. Biol.* (in the press).
3 Brenner, S., *Proc. U.S. Nat. Acad. Sci.*, **43**, 687 (1957).
4 For refs. see Watson, H. C., and Kendrew, J. C., *Nature*, **190**, 670 (1961).
5 Crick, F. H. C., Griffith, J. S., and Orgel, L. E., *Proc. U.S. Nat. Acad. Sci.*, **43**, 416 (1957).
6 Benzer, S., *Proc. U.S. Nat. Acad. Sci.*, **45**, 1607 (1959), for refs. to earlier papers.
7 Benzer, S., *Proc. U.S. Nat. Acad. Sci.*, **47**, 403 (1961); see his Fig. 3.
8 Brenner, S., Benzer, S., and Barnett, L., *Nature*, **182**, 983 (1958).
9 Brenner, S., Barnett, L., Crick, F. H. C., and Orgel, A., *J. Mol. Biol.*, **3**, 121 (1961).
10 Streisinger, G. (personal communication and in the press).
11 Feynman, R. P.; Benzer, S.; Freese, E. (all personal communications).
12 Jinks, J. L., *Heredity*, **16**, 153, 241 (1961).
13 Champe, S., and Benzer, S. (personal communication and in preparation).
14 Jacob, F., and Wollman, E. L., *Sexuality and the Genetics of Bacteria* (Academic Press, New York, 1961). Levinthal, C. (personal communication).
15 Orgel, A., and Brenner, S. (in preparation).
16 Sueoka, N. *Cold Spring Harb. Symp. Quant. Biol.* (in the press).
17 Dreyer, W. J., Symp. 1, Fifth Intern. Cong. Biochem., 1961 (in the press).
18 Nirenberg, M. W., and Matthaei, J. H., *Proc. U.S. Nat. Acad. Sci.*, **47**, 1588 (1961).

192, 1227; 1961

Transformation in Yeast: Evidence of a Real Genetic Change by the Action of DNA

In a previous paper[1] a successful transformation of yeast akin to the transformation of bacteria was reported and it was shown by the normal Mendelian segregation that the newly acquired abilities were localized exclusively in the nucleus.

Deoxyribonucleic acid (DNA) was extracted from *Saccharomyces chevalieri* (W_{332})[1] and dissolved in brewery wort, which was used as culture medium for the yeasts R_7 and $K_{83}S_{58}$; the donor yeast can ferment sucrose and raffinose 1/3, but the acceptor can ferment only mono-saccharides. After the DNA treatment. cells were isolated which had acquired the ability to ferment sucrose or both sucrose and raffinose. Back crosses with the recessive yeast showed a regular Mendelian segregation.

Harris and Thompson[2], repeating these experiments, were unable to confirm the results, although they used the same yeasts and the same extraction method. Laskowski and Lochmann[3] also failed to obtain a transformation in yeast, but these latter workers did not report sufficient technical details to allow judgment of their results in contrast to the former.

Instead of going into the minor differences between the technique used by Harris and Thompson and by me, a general comment may be made which might account for the negative results published. According to my experience, it is not yet possible to predict the biological activity of a DNA preparation. Sometimes the extract is active, and hereditary characters from one yeast are transferred into the other yeast, whereas at other times no results can be obtained. This might be ascribed either to (*a*) inactivation of the extract, or to (*b*) incompetence of the yeast. These varying results are obtained without any indication of the cause and without any demonstrable change of the extraction method. Attempts to trace the origin of this peculiar phenomenon have been unsuccessful and work on the subject is proceeding. Nevertheless, some of the experiments have shown a transforming activity[4,5].

The purification of the extracted DNA without loss of activity has been attempted by fractionation by means of the ion-exchange 'Ecteola'-cellulose column. As estimated by the absorption spectrum curves, very pure fractions of DNA were obtained; but the hope of being able to fractionate in this way the DNA into fractions containing only one gene or a restricted number of genes was not realized. In these experiments two haploid yeasts (*Sacch. cerevisiae*) deficient in adenine and tryptophan respectively were treated with these purified fractions of DNA prepared from the diploid non-deficient yeast *Sacch. chevalieri*. The former can ferment maltose, the latter cannot ferment this sugar. The treated cultures were sporulated on Fowel's agar and in most instances many asci were found, indicating the diploid state. Single cell cultures which could not ferment maltose were often obtained and were able to synthesize adenine or tryptophan.

These experiments permit the conclusion that both dominant and recessive characters can be transferred. Experiments with a negative result, that is. without a transformation effect, point to the absence of mutagenic factors in the DNA preparations. Thus I have been able to hybridize non-sporulating yeasts, for example, the bottom yeasts used in the brewing industry, by the transformation procedure.

W. F. F. Oppenoorth
Nationaal Instituut voor Brouwgerst,
Mout en Bier-T.N.O.,
Polderstraat 10, Rotterdam-25.

1 Oppenoorth, W. F. F., *Antonie van Leeuwenhoek J. Microbiol. Serol.*, **26**, 129 (1960); *European Brewery Conv. Proc. Seventh Cong., Rome*, 180 (1959).
2 Harris, G., and Thompson, C. C., *Nature*, **188**, 1212 (1960).
3 Laskowski, W., and Lochmann, E. R., *Naturwiss.*, **48**, 225 (1961).
4 Oppenoorth, W. F. F., *Brewers' Digest*, **35**, 12, 61 (1960).
5 Oppenoorth, W. F. F., *European Brewery Conv. Proc. Eighth Cong., Vienna* (in the press).

193, 706; 1962

RECENT DISCOVERIES OF FOSSIL HOMINIDS IN TANGANYIKA: AT OLDUVAI AND NEAR LAKE NATRON

By Dr. L. S. B. LEAKEY and Mrs. M. D. LEAKEY
Coryndon Museum Centre for Prehistory and Palæontology, Nairobi

EXCAVATIONS were continued at Olduvai Gorge throughout 1963 with funds generously provided by the Research Committee of the National Geographic Society, Washington, D.C. Some work was also undertaken at a new site on the western side of Lake Natron. The main work at Olduvai was concerned with the cultural sequence and stratigraphy of Bed II, Olduvai, but a little work was carried out in Bed I and also in Bed IV. We thank the U.S. National Geographic Society for financial support.

During the period under review a number of hominid remains have been found, some of which are of outstanding importance.

(*A*) In the early part of the year, work was continued at site *V.E.K.* IV where fragments of a thick hominid skull were found by Miss M. Cropper in 1962. A number of additional fragments of this skull, including a damaged palate, were recovered. This specimen has not yet been examined. Its possible relationship to the fossil human remains found at Kanjera in 1931–32 is likely to be of interest. This specimen is catalogued as Olduvai, Hominid No. 12.

(*B*) In October 1963, part of the vault of a small skull associated with the greater part of a mandible and parts of both maxillæ, was found at site *M.N.K.* II. The lower third molars are fully erupted but as yet unworn, the upper third molars are just emerging from the alveoli. A late adolescent age is therefore indicated. The parts of the skull preserved include most of the occipital bone, which articulates very nicely with the two parietals, of which the right is the more complete; there are also a part of the frontal and parts of both temporals (Figs. 1 and 2). The mandible contains all the cheek teeth and both the canines in good condition, and all four incisors are present but the crowns are somewhat damaged (Fig. 3).

This specimen exhibits all the special morphological characters which could be seen in the juvenile mandible from Olduvai from *F.L.K.N.N.* I and the associated

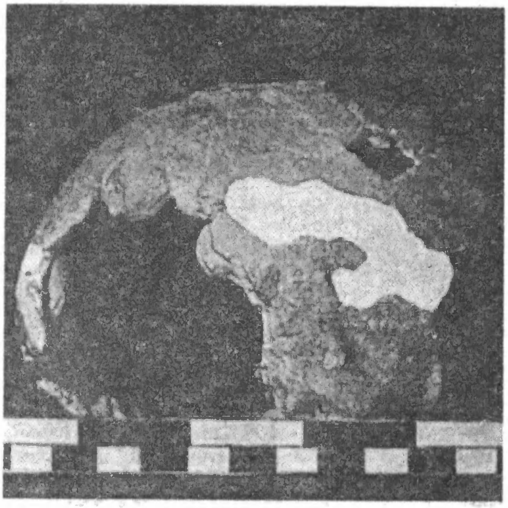


Fig. 1. The new hominid skull from site *M.N.K.* II Olduvai. Profile view

Fig. 2. The new skull. Occipital view

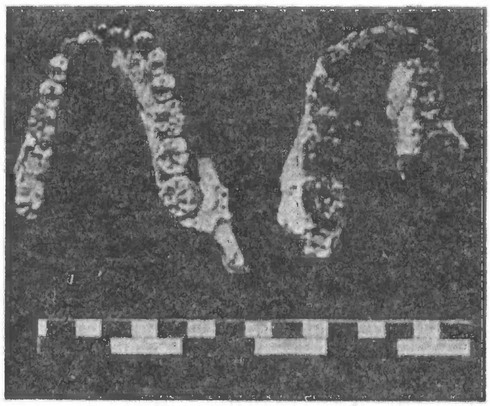


Fig. 3. The mandible associated with the skull from *M.N.K.* II Olduvai (left) next to the juvenile mandible found in 1960 from *F.L.K.N.* I

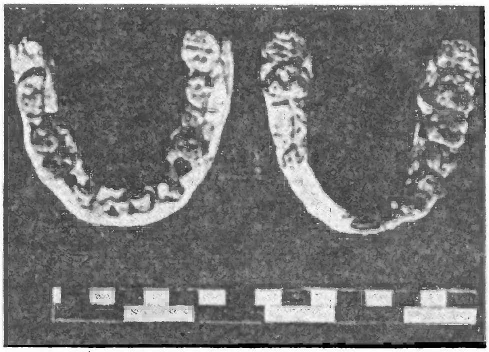


Fig. 4. The maxillary teeth (right) and mandibular teeth (left) of the broken skull from site *F.L.K.* II, Maiko Gully

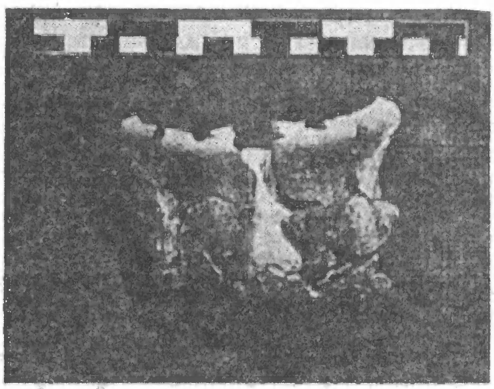


Fig. 5. Part of the frontal of the broken-up skull from *F.L.K.* II, Maiko Gully

parietals, which were briefly described in *Nature* (189, 649; 1961, and 191, 417; 1961). The new specimen is recorded as Olduvai Hominid No. 13.

(*C*) A few fragments of a cranial vault of similar form to (*A*) here, but probably a younger individual also from site *M.N.K.* II. This specimen is listed as Olduvai Hominid No. 14.

(*D*) Three teeth of an adult—probably a male—were also found at site *M.N.K.* II, but in a deposit stratigraphically overlying that which yielded (*B*) and (*C*) here.

All these three specimens come from deposits which are approximately midway in Bed II but which appear to antedate the appearance of hand axes.

(*E*) Considerable parts of the cranial vault, as well as most of the teeth of a young adult, in which the third upper and lower molars are just coming into occlusion. This specimen had been washed out by heavy rainfall at site *F.L.K.* II, Maiko Gully; it had, moreover, been afterwards trampled on and very badly broken up by herds of Masai cattle before it was discovered by one of our senior African staff. It is derived from deposits 3–4 ft. above the marker bed at the top of Bed I.

Parts of this specimen had been washed a long way from the original site and others had been carried away attached by mud and clay to the feet of cattle. The work of recovery and piecing together the hundreds of fragments will continue for some considerable time. The maxillary teeth of the right side, some of the mandibular teeth and parts of the frontal of this specimen are seen in Figs. 4 and 5.

The importance of this particular find is that it provides the greater part of the frontal bone, with the supra-orbital region, of an individual in which the teeth clearly show that it belongs to the same morphological group as the *F.L.K.N.N.* I juvenile of 1960 and the specimens listed here, rather than to the Australopithecine sub-family as represented by *Zinjanthropus*, or the South African representatives.

In addition to the five individuals represented in the material listed here, a sixth important discovery was made in January 1964, in deposits with a fauna of Middle Pleistocene age lying on the west side of Lake Natron, some fifty miles north-east of Olduvai Gorge. Initial exploration of this area was carried out under the leadership of my son, Richard Leakey, who was later joined by Mr. Glynn Isaac, who took charge of the scientific side of the work. Mrs. Isaac, Mr. Richard Rowe and Philip Leakey also took part, as well as a number of our African staff.

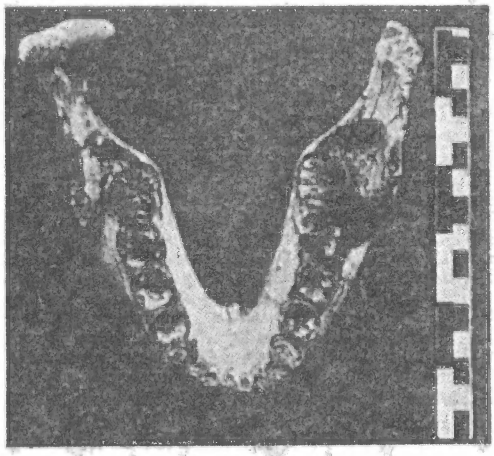


Fig. 6. A new mandible of *Australopithecus* (*Zinjanthropus*) type from the new site west of Lake Natron, north-east of Olduvai

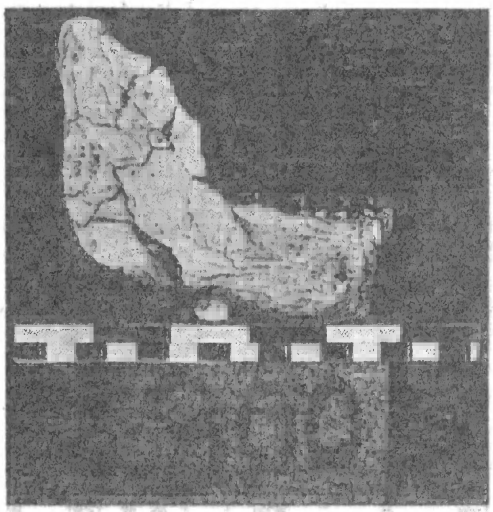


Fig. 7. Side view of the mandible of *Australopithecus* (*Zinjanthropus*) type from west of Lake Natron

On January 11 one of our African staff, Mr. Kamoya Kimeu, located a magnificent fossil hominid jaw *in situ* (see Figs. 6 and 7). This jaw, unlike specimens (*A*), (*B*), (*C*) and (*D*) here, represents an unmistakable australopithecine and provides, for the first time, a mandible representing this sub-family from East Africa.

It will be recalled that in earlier notes in *Nature* and elsewhere, we have stressed the fact that the juvenile and the other fossil remains from site *F.L.K.N.N.* I, found in 1960, did not represent an australopithecine such as *Australopithecus* (*Zinjanthropus*) *boisei*, but were wholly distinct and different. It was stated that these must be thought of as representing a contemporary and primitive hominine branch of the Hominidae.

We refrained from giving a scientific name to the material from site *F.L.K.N.N.* I—the juvenile and the female—together with other specimens representing the same type (the molar tooth from site *M.K.* I) until there were better data on which to decide just where to place this type of hominid in the taxonomic sequence. The new material found in 1963 makes it possible to draw conclusions and to give a diagnosis for a new species of the genus *Homo*. This diagnosis and a preliminary description by Leakey, Tobias and Napier follow this article.

202, 5; 1964

A NEW SPECIES OF THE GENUS *HOMO* FROM OLDUVAI GORGE

By Dr. L. S. B. LEAKEY
Coryndon Museum, Centre for Prehistory and Palæontology
Prof. P. V. TOBIAS
University of Witwatersrand, Johannesburg
AND
Dr. J. R. NAPIER
Unit of Primatology and Human Evolution, Royal Free Hospital Medical School, University of London

THE recent discoveries of fossil hominid remains at Olduvai Gorge have strengthened the conclusions—which each of us had reached independently through our respective investigations—that the fossil hominid remains found in 1960 at site *F.L.K.N.N.* I, Olduvai, did not represent a creature belonging to the sub-family Australopithecinae *.

We were preparing to publish the evidence for this conclusion and to give a scientific name to this new species of the genus *Homo*, when the new discoveries, which are described by L. S. B. and M. D. Leakey in the preceding article, were made.

An examination of these finds has enabled us to broaden the basis of our diagnosis of the proposed new species and has fully confirmed the presence of the genus *Homo* in the lower part of the Olduvai geological sequence, earlier than, contemporary with, as well as later than, the *Zinjanthropus* skull, which is certainly an australopithecine.

For the purpose of our description here, we have accepted the diagnosis of the family Hominidae, as it was proposed by Sir Wilfrid Le Gros Clark in his book *The Fossil Evidence for Human Evolution* (110; 1955). Within this family we accept the genus *Australopithecus* with, for the moment, three sub-genera (*Australopithecus*, *Paranthropus* and *Zinjanthropus*) and the genus *Homo*. We regard *Pithecanthropus* and possibly also *Atlanthropus* (if it is indeed distinct) as species of the genus *Homo*, although one of us (L. S. B. L.) would be prepared to accept sub-generic rank.

It has long been recognized that as more and more discoveries were made, it would become necessary to revise the diagnosis of the genus *Homo*. In particular, it has become clear that it is impossible to rely on only one or two characters, such as the cranial capacity or an erect posture, as the necessary criteria for membership of the genus. Instead, the total picture presented by the material available for investigation must be taken into account.

We have come to the conclusion that, apart from *Australopithecus* (*Zinjanthropus*), the specimens we are dealing with from Bed I and the lower part of Bed II at Olduvai represent a single species of the genus *Homo* and not an australopithecine. The species is, moreover, clearly distinct from the previously recognized species of the genus. But if we are to include the new material in the genus *Homo* (rather than set up a distinct genus for it, which we believe to be unwise), it becomes necessary to revise the diagnosis of this genus. Until now, the definition of *Homo* has usually centred about a 'cerebral Rubicon' variably set at 700 c.c. (Weidenreich), 750 c.c. (Keith) and 800 c.c. (Vallois). The proposed new definition follows:

Family HOMINIDAE (as defined by Le Gros Clark, 1955)

Genus *Homo* Linnæus.

Revised diagnosis of the genus Homo. A genus of the Hominidae with the following characters: the structure of the pelvic girdle and of the hind-limb skeleton is adapted to habitual erect posture and bipedal gait; the fore-limb is shorter than the hind-limb; the pollex is well developed and fully opposable and the hand is capable not only of a power grip but of, at the least, a simple and usually well developed precision grip †; the cranial capacity is very variable but is, on the average, larger than the range of capacities of members of the genus *Australopithecus*, although the lower part of the range of capacities in the genus *Homo* overlaps with the upper part of the range in *Australopithecus*; the capacity is (on the average) large relative to body-size and ranges from about 600 c.c. in earlier forms to more than 1,600 c.c.; the muscular ridges on the cranium range from very strongly marked to virtually imperceptible, but the temporal crests or lines never reach the midline; the frontal region of the cranium is without undue post-orbital constriction (such as is common in members of the genus *Australopithecus*); the supra-orbital region of the frontal bone is very variable, ranging from a massive and very salient supra-orbital torus to a complete lack of any supra-orbital projection and a smooth brow region; the facial skeleton varies from moderately prognathous to orthognathous, but it is not concave (or dished) as is common in members of the Australopithecinae; the anterior symphyseal contour varies from a marked retreat to a forward slope, while the bony chin may be entirely lacking, or may vary from a slight to a very strongly developed mental trigone; the dental arcade is evenly rounded with no diastema in most members of the genus; the first lower premolar is clearly bicuspid with a variably developed lingual cusp; the

* See also *Nature* of March 7, pp. 967, 969, and preceding articles in this issue.

† For the definition of 'power grip' and 'precision grip', see Napier, J. R., *J. Bone and Joint Surg.*, 38, B, 902 (1956).

202, 7; 1964

molar teeth are variable in size, but in general are small relative to the size of these teeth in the genus *Australopithecus*; the size of the last upper molar is highly variable, but it is generally smaller than the second upper molar and commonly also smaller than the first upper molar; the lower third molar is sometimes appreciably larger than the second; in relation to the position seen in the Hominoidea as a whole, the canines are small, with little or no overlapping after the initial stages of wear, but when compared with those of members of the genus *Australopithecus*, the incisors and canines are not very small relative to the molars and premolars; the teeth in general, and particularly the molars and premolars, are not enlarged bucco-lingually as they are in the genus *Australopithecus*; the first deciduous lower molar shows a variable degree of molarization.

Genus *Homo* Linnæus
Species *habilis* sp. nov.

(*Note:* The specific name is taken from the Latin, meaning 'able, handy, mentally skilful, vigorous'. We are indebted to Prof. Raymond Dart for the suggestion that *habilis* would be a suitable name for the new species.)

A species of the genus *Homo* characterized by the following features:

A mean cranial capacity greater than that of members of the genus *Australopithecus*, but smaller than that of *Homo erectus*; muscular ridges on the cranium ranging from slight to strongly marked; chin region retreating, with slight or no development of the mental trigone; maxillæ and mandibles smaller than those of *Australopithecus* and within the range for *Homo erectus* and *Homo sapiens*; dentition characterized by incisors which are relatively large in comparison with those of both *Australopithecus* and *Homo erectus*; canines which are proportionately large relative to the premolars; premolars which are narrower (in bucco-lingual breadth) than those of *Australopithecus*, but which fall within the range for *Homo erectus*; molars in which the absolute dimensions range between the lower part of the range in *Australopithecus* and the upper part of the range in *Homo erectus*; a marked tendency towards bucco-lingual narrowing and mesiodistal elongation of all the teeth, which is especially evident in the lower premolars (where it expresses itself as a marked elongation of the talonid) and in the lower molars (where it is accompanied by a rearrangement of the distal cusps); the sagittal curvature of the parietal bone varies from slight (within the hominine range) to moderate (within the australopithecine range); the external sagittal curvature of the occipital bone is slighter than in *Australopithecus* or in *Homo erectus*, and lies within the range of *Homo sapiens*; in curvature as well as in some other morphological traits, the clavicle resembles, but is not identical to, that of *Homo sapiens sapiens*; the hand bones differ from those of *Homo sapiens sapiens* in robustness, in the dorsal curvature of the shafts of the phalanges, in the distal attachment of *flexor digitorum superficialis*, in the strength of fibro-tendinous markings, in the orientation of trapezium in the carpus, in the form of the scaphoid and in the marked depth of the carpal tunnel; however, the hand bones resemble those of *Homo sapiens sapiens* in the presence of broad, stout, terminal phalanges on fingers and thumb, in the form of the distal articular surface of the capitate and the ellipsoidal form of the metacarpo-phalangeal joint surfaces; in many of their characters the foot bones lie within the range of variation of *Homo sapiens sapiens*; the hallux is stout, adducted and plantigrade; there are well-marked longitudinal and transverse arches; on the other hand, the 3rd metatarsal is relatively more robust than it is in modern man, and there is no marked difference in the radii of curvature of the medial and lateral profiles of the trochlea of the talus.

Geological horizon. Upper Villafranchian and Lower Middle Pleistocene.

Type. The mandible with dentition and the associated upper molar, parietals and hand bones, of a single juvenile individual from site *F.L.K.N.N.* I, Olduvai, Bed I.

This is catalogued as Olduvai Hominid 7.

Paratypes. (*a*) An incomplete cranium, comprising fragments of the frontal, parts of both parietals, the greater part of the occipital, and parts of both temporals, together with an associated mandible with canines, premolars and molars complete on either side but with the crowns of the incisors damaged, parts of both maxillæ, having all the cheek teeth except the upper left fourth premolar. The condition of the teeth suggests an adolescent. This specimen, from site *M.N.K.* II, Olduvai, Bed II, is catalogued as Olduvai Hominid 13.

(*b*) The associated hand bones, foot bones and probably the clavicle, of an adult individual from site *F.L.K.N.N.* I, Olduvai, Bed I. This is catalogued as Olduvai Hominid 8.

(*c*) A lower premolar, an upper molar and cranial fragments from site *F.L.K.* I, Olduvai, Bed I (the site that yielded also the *Australopithecus* (*Zinjanthropus*) skull). This is catalogued as Olduvai Hominid 6. (It is possible that the tibia and fibula found at this site belong with *Homo habilis* rather than with *Australopithecus* (*Zinjanthropus*). These limb bones have been reported on by Dr. P. R. Davis (*Nature*, March 7, 1964, p. 967).

(*d*) A mandibular fragment with a molar in position and associated with a few fragments of other teeth from site *M.K.* I, Olduvai, Bed I. This specimen is catalogued as Olduvai Hominid 4.

Description of the type. Preliminary descriptions of the specimens which have now been designated the type of *Homo habilis*, for example, the parts of the juvenile found at site *F.L.K.N.N.* I in 1960, have already been published in *Nature* by one of us (189, 649; 191, 417; 1961). A further detailed description and report on the parietals, the mandible and the teeth are in active preparation by one of us (P. V. T.), while his report on the cranial capacity (preceding article) as well as a preliminary note on the hand by another of us (*Nature*, 196, 409; 1962) have been published. We do not propose, therefore, of give a more detailed description of the type here.

Description of the paratypes. A preliminary note on the clavicle and on the foot of the adult, which represents paratype (*b*), was published in *Nature* (188, 1050; 1961), and a further report on the foot by Dr. M. H. Day and Dr. J. R. Napier was published in *Nature* of March 7, 1964, p. 969.

The following additional preliminary notes on the other paratypes have been prepared by one of us (P. V. T.).

Description of Paratypes

(*a*) *Olduvai Hominid* 13 *from* M.N.K. *II*. An adolescent represented by a nearly complete mandible with complete, fully-erupted lower dentition, a right maxillary fragment including palate and all teeth from P^3 to M^3, the latter in process of erupting; the corresponding left maxillary fragment with M^1 to M^3, the latter likewise erupting, the isolated left P^3; parts of the vault of a small, adult cranium, comprising much of the occipital, including part of the posterior margin of *foramen magnum*, parts of both parietals, right and left temporosphenoid fragments, each including the mandibular fossa and foramen ovale. The distal half of a humeral shaft (excluding the distal extremity) may also belong to Olduvai Hominid 13. The *corpus mandibulae* is very small, both the height and thickness at M_1 falling below the australopithecine range and within the hominine range. All the teeth are small compared with those of Australopithecinae, most of the dimensions falling at or below the lower extreme of the australopithecine ranges. On the other hand, practically all the dental dimensions can be accommodated within the range of fossil Homininae. The Olduvai Hominid 13 teeth show the characteristic mesiodistal elongation and labiolingual narrowing, in some teeth the *L/B* index exceeding even those of the type Olduvai Hominid 7, and paratype Olduvai Hominid 6. The occipital bone has a relatively slight sagittal curvature, the Occipital Sagittal Index being outside the range for australopithecines and for *Homo erectus pekinensis* and within the range for *Homo sapiens*. On the other hand, the parietal sagittal curvature is more marked than in all but one australopithecine and in all the Pekin fossils, the index falling at the top of the range of population means for modern man. Both parietal and occipital bones are very small in size, being exceeded in some dimensions by one or two australopithecine crania and falling short in all dimensions of the range for *Homo erectus pekinensis*. The form of the parietal—antero-posteriorly elongated and bilaterally narrow, with a fairly abrupt lateral descent in the plane of the parietal boss—reproduces closely these features in the somewhat larger parietal of the type specimen (Olduvai Hominid 7 from *F.L.K.N.N.* I).

(*b*) *Olduvai Hominid* 6 *from* F.L.K. *I*. An unworn lower left premolar, identified as P_3, an unworn, practically complete crown and partly developed roots of an upper molar, either M^1 or M^2, as well as a number of fragments of cranial vault. These remains were found at the *Zinjanthropus* site and level, some *in situ* and some on the surface. Both teeth are small for an australopithecine, especially in buccolingual breadth, but large for *Homo erectus*. The marked tendency to elongation and narrowing imparts to both teeth an *L/B* index outside the range for all known australopithecine homologues and even beyond the range for *Homo erectus pekinensis*. The elongating-narrowing tendency is more marked in this molar than in the upper molar belonging to the type specimen (Olduvai Hominid 7) from *F.L.K.N.N.* I.

(*c*) *Olduvai Hominid* 8 *from* F.L.K.N.N. *I*. Remains of an adult individual found on the same horizon as the type specimen, and represented by two complete proximal phalanges, a fragment of a rather heavily worn tooth (premolar or molar), and a set of foot-bones possessing most of the specializations associated with the plantigrade propulsive feet of modern man. Probably the clavicle found at this site belongs to this adult rather than to the juvenile type-specimen; it is characterized by clear overall similarities to the clavicle of *Homo sapiens sapiens*.

(*d*) *Olduvai Hominid* 4 *from* M.K. *I*. A fragment of the posterior part of the left *corpus mandibulae*, containing a well-preserved, fully erupted molar, either M_2 or M_3. The width of the mandible is 19·2 m level with the mesial half of the molar, but the maximum width must have been somewhat greater. The molar is 15·1 mm in mesiodistal length and 13·0 mm in buccolingual breadth; it is thus a small and narrow tooth by australopithecine standards, but large in comparison with *Homo erectus* molars. There are several other isolated dental fragments, including a moderately worn molar fragment. These are stratigraphically the oldest hominid remains yet discovered at Olduvai.

Referred Material

Olduvai Hominid 14 *from* M.W.K. *II*. (1) A juvenile represented by a fragment of the right parietal with clear, unfused sutural margins; two smaller vault fragments with sutural margins; a left and a right temporal fragment, each including the mandibular fossa.

(2) A fragmentary skull with parts of the upper and lower dentition of a young adult from site *F.L.K.* II, Maiko Gully, Olduvai, Bed II, is also provisionally referred to *Homo habilis*. This specimen is catalogued as Olduvai Hominid 16. It is represented by the complete upper right dentition, as well as some of the left maxillary teeth, together with some of the mandibular teeth. The skull fragments include parts of the frontal, with both the external orbital angles preserved, as well as the supraorbital region, except for the glabella; parts of both parietals and the occipital are also represented.

Implications for Hominid Phylogeny

In preparing our diagnosis of *Homo habilis*, we have not overlooked the fact that there are several other African (and perhaps Asian) fossil hominids whose status may now require re-examination in the light of the new discoveries and of the setting up of this new species. The specimens originally described by Broom and Robinson as *Telanthropus capensis* and which were later transferred by Robinson to *Homo erectus* may well prove, on closer comparative investigation, to belong to *Homo habilis*. The Kanam mandibular fragment, discovered by the expedition in 1932 by one of us (L. S. B. L.), and which has been shown to possess archaic features (Tobias, *Nature*, 185, 946; 1960), may well justify further investigation along these lines. The Lake Chad craniofacial fragment, provisionally described by M. Yves Coppens in 1962, as an australopithecine, is not, we are convinced, a member of this sub-family. We understand that the discoverer himself, following his investigation of the australopithecine originals from South Africa and Tanganyika, now shares our view in this respect. We believe that it is very probably a northern representative of *Homo habilis*.

Outside Africa, the possibility will have to be considered that the teeth and cranial fragments found at Ubeidiyah on the Jordan River in Israel may also belong to *Homo habilis* rather than to *Australopithecus*.

Cultural Association

When the skull of *Australopithecus* (*Zinjanthropus*) *boisei* was found on a living floor at *F.L.K.* I, no remains of any other type of hominid were known from the early part of the Olduvai sequence. It seemed reasonable, therefore, to assume that this skull represented the makers of the Oldowan culture. The subsequent discovery of remains of *Homo habilis* in association with the Oldowan culture at three other sites has considerably altered the position. While it is possible that *Zinjanthropus* and *Homo habilis* both made stone tools, it is probable that the latter was the more advanced tool maker and that the *Zinjanthropus* skull represents an intruder (or a victim) on a *Homo habilis* living site.

The recent discovery of a rough circle of loosely piled stones on the living floor at site *D.K.* I, in the lower part of Bed I, is noteworthy. This site is geologically contemporary with *M.K.* I, less than one mile distant, where remains of *Homo habilis* have been found. It seems that the early hominids of this period were capable of making rough shelters or windbreaks and it is likely that *Homo habilis* may have been responsible.

Relationship to *Australopithecus* (*Zinjanthropus*)

The fossil human remains representing the new species *Homo habilis* have been found in Bed I and in the lower and middle part of Bed II. Two of the sites, *M.K.* I and *F.L.K.N.N.* I, are geologically older than that which yielded the skull of the australopithecine *Zinjanthropus*. One site, *F.L.K.* I, has yielded both *Australopithecus* (*Zinjanthropus*) and remains of *Homo habilis*, while two sites are later, namely *M.N.K.* II and *F.L.K.* II Maiko gully. The new mandible of *Australopithecus* (*Zinjanthropus*) type from Lake Natron, reported in the preceding article by Dr. and Mrs. Leakey, was associated with a fauna of Bed II affinities.

It thus seems clear that two different branches of the Hominidae were evolving side by side in the Olduvai region during the Upper Villafranchian and the lower part of the Middle Pleistocene.

Viral RNA-dependent DNA Polymerase

Two independent groups of investigators have found evidence of an enzyme in virions of RNA tumour viruses which synthesizes DNA from an RNA template. This discovery, if upheld, will have important implications not only for carcinogenesis by RNA viruses but also for the general understanding of genetic transcription: apparently the classical process of information transfer from DNA to RNA can be inverted.

RNA-dependent DNA Polymerase in Virions of RNA Tumour Viruses

DNA seems to have a critical role in the multiplication and transforming ability of RNA tumour viruses[1]. Infection and transformation by these viruses can be prevented by inhibitors of DNA synthesis added during the first 8–12 h after exposure of cells to the virus[1-4]. The necessary DNA synthesis seems to involve the production of DNA which is genetically specific for the infecting virus[5,6], although hybridization studies intended to demonstrate virus-specific DNA have been inconclusive[1]. Also, the formation of virions by the RNA tumour viruses is sensitive to actinomycin D and therefore seems to involve DNA-dependent RNA synthesis[1-4,7]. One model which explains these data postulates the transfer of the information of the infecting RNA to a DNA copy which then serves as template for the synthesis of viral RNA[1,2,7]. This model requires a unique enzyme, an RNA-dependent DNA polymerase.

No enzyme which synthesizes DNA from an RNA template has been found in any type of cell. Unless such an enzyme exists in uninfected cells, the RNA tumour viruses must either induce its synthesis soon after infection or carry the enzyme into the cell as part of the virion. Precedents exist for the occurrence of nucleotide polymerases in the virions of animal viruses. Vaccinia[8,9]—a DNA virus, Reo[10,11]—a double-stranded RNA virus, and vesicular stomatitis virus (VSV)[12]—a single-stranded RNA virus, have all been shown to contain RNA polymerases. This study demonstrates that an RNA-dependent DNA polymerase is present in the virions of two RNA tumour viruses: Rauscher mouse leukaemia virus (R-MLV) and Rous sarcoma virus. Temin[13] has also identified this activity in Rous sarcoma virus.

Incorporation of Radioactivity from ^{3}H-TTP by R-MLV

A preparation of purified R-MLV was incubated in conditions of DNA polymerase assay. The preparation incorporated radioactivity from ^{3}H-TTP into an acid-insoluble product (Table 1). The reaction required Mg^{2+}, although Mn^{2+} could partially substitute and each of the four deoxyribonucleoside triphosphates was necessary for activity. The reaction was stimulated strongly by dithiothreitol and weakly by NaCl (Table 1). The kinetics of incorporation of radioactivity from ^{3}H-TTP by R-MLV are shown in Fig. 1, curve 1. The reaction rate accelerates for about 1 h and then declines. This time-course may indicate the occurrence of a slow activation of the polymerase in the reaction mixture. The activity is approximately proportional to the amount of added virus.

For other viruses which have nucleotide polymerases in their virions, there is little or no activity demonstrable unless the virions are activated by heat, proteolytic enzymes or detergents[9-12]. None of these treatments increased the activity of the R-MLV DNA polymerase. In fact, incubation at 50° C for 10 min totally inactivated the R-MLV enzyme as did inclusion of trypsin (50 μg/ml.) in the reaction mixture. Addition of as little as 0·01 per cent 'Triton N-101' (a non-ionic detergent) also markedly depressed activity.

Table 1. PROPERTIES OF THE RAUSCHER MOUSE LEUKAEMIA VIRUS DNA POLYMERASE

Reaction system	pmoles ^{3}H-TMP incorporated in 45 min
Complete	3·31
Without magnesium acetate	0·04
Without magnesium acetate + 6 mM $MnCl_2$	1·50
Without dithiothreitol	0·38
Without NaCl	2·18
Without dATP	< 0·10
Without dCTP	0·12
Without dGTP	< 0·10

A preparation of R-MLV was provided by the Viral Resources Program of the National Cancer Institute. The virus had been purified from the plasma of infected Swiss mice by differential centrifugation. The preparation had a titre of $10^{4\cdot88}$ spleen enlarging doses (50 per cent end point) per ml. Before use the preparation was centrifuged at 105,000*g* for 30 min and the pellet was suspended in 0·137 M NaCl–0·003 M KCl–0·01 M phosphate buffer (*p*H 7·4)–0·6 mM EDTA (PBS–EDTA) at 1/20 of the initial volume. The concentrated virus suspension contained 3·1 mg/ml. of protein. The assay mixture contained, in 0·1 ml., 5 μmoles Tris-HCl (*p*H 8·3) at 37° C, 0·6 μmole magnesium acetate, 6 μmoles NaCl, 2 μmoles dithiothreitol, 0·08 μmole each of dATP, dCTP and dGTP, 0·001 μmole [^{3}H-*methyl*]–TTP (708 c.p.m. per pmole) (New England Nuclear) and 15 μg viral protein. The reaction mixture was incubated for 45 min at 37° C. The acid-insoluble radioactivity in the sample was then determined by addition of sodium pyrophosphate, carrier yeast RNA and trichloroacetic acid followed by filtration through a membrane filter and counting in a scintillation spectrometer, all as previously described[12]. The radioactivity of an unincubated sample was subtracted from each value (less than 7 per cent of the incorporation in the complete reaction mixture).

Characterization of the Product

The nature of the reaction product was investigated by determining its sensitivity to various treatments. The product could be rendered acid-soluble by either pancreatic deoxyribonuclease or micrococcal nuclease but was unaffected by pancreatic ribonuclease or by alkaline hydrolysis (Table 2). The product therefore has the properties of DNA. If 50 μg/ml. of deoxyribonuclease was added to a reaction mixture there was no loss of acid-insoluble product. The product is therefore protected from the enzyme, probably by the envelope of the virion, although merely diluting the reaction mixture into 10 mM $MgCl_2$ enables the product to be digested by deoxyribonuclease (Table 2).

Table 2. CHARACTERIZATION OF THE POLYMERASE PRODUCT

Expt.	Treatment	Acid-insoluble radioactivity	Percentage undigested product
1	Untreated	1,425	(100)
	20 μg deoxyribonuclease	125	9
	20 μg micrococcal nuclease	69	5
	20 μg ribonuclease	1,361	96
2	Untreated	1,644	(100)
	NaOH hydrolysed	1,684	100

For experiment 1, 93 μg of viral protein was incubated for 2 h in a reaction mixture twice the size of that described in Table 1, with ^{3}H-TTP having a specific activity of 1,133 c.p.m. per pmole. A 50 μl. portion of the reaction mixture was diluted to 5 ml. with 10 mM $MgCl_2$ and 0·5 ml. aliquots were incubated for 1·5 h at 37° C with the indicated enzymes. (The sample with micrococcal nuclease also contained 5 mM $CaCl_2$.) The samples were then chilled, precipitated with trichloroacetic acid and radioactivity was counted. For experiment 2, two standard reaction mixtures were incubated for 45 min at 37° C, then to one sample was added 0·1 ml. of 1 M NaOH and it was boiled for 5 min. It was then chilled and both samples were precipitated with trichloroacetic acid and counted. In a separate experiment (unpublished) it was shown that the alkaline hydrolysis conditions would completely degrade the RNA product of the VSV virion polymerase.

Localization of the Enzyme and its Template

To investigate whether the DNA polymerase and its template were associated with the virions, a R-MLV suspension was centrifuged to equilibrium in a 15–50 per cent sucrose gradient and fractions of the gradient were assayed for DNA polymerase activity. Most of the activity was found at the position of the visible band of virions (Fig. 2). The density at this band was 1·16 g/cm^3, in agreement with the known density of the virions[14]. The polymerase and its template therefore seem to be constituents of the virion.

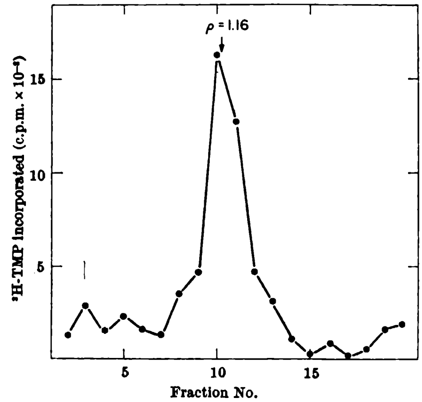


Fig. 2. Localization of DNA polymerase activity in R-MLV by isopycnic centrifugation. A preparation of R-MLV containing 150 μg of protein in 50 μl. was layered over a linear 5·2 ml. gradient of 15-50 per cent sucrose in PBS-EDTA. After centrifugation for 2 h at 60,000 r.p.m. in the Spinco 'SW65' rotor, 0·27 ml. fractions of the gradient were collected and 0·1 ml. portions of each fraction were incubated for 60 min in a standard reaction mixture. The acid-precipitable radioactivity was then collected and counted. The density of each fraction was determined from its refractive index. The arrow indicates the position of a sharp, visible band of light-scattering material which occurred at a density of 1·16.

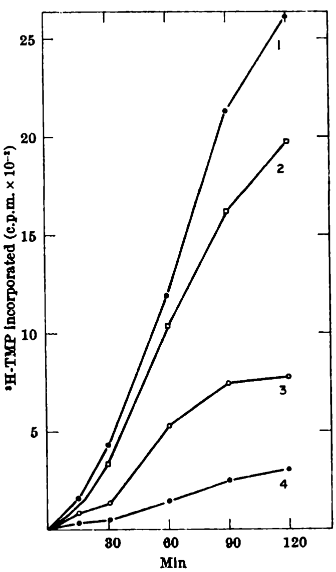


Fig. 1. Incorporation of radioactivity from ^{3}H-TTP by the R-MLV DNA polymerase in the presence and absence of ribonuclease. A 1·5-fold standard reaction mixture was prepared with 30 μg of viral protein and ^{3}H-TTP (specific activity 950 c.p.m. per pmole). At various times, 20 μl. aliquots were added to 0·5 ml. of non-radioactive 0·1 M sodium pyrophosphate and acid insoluble radioactivity was determined[11]. For the preincubated samples, 0·06 ml. of H_2O and 0·01 ml. of R-MLV (30 μg of protein) were incubated with or without 10 μg of pancreatic ribonuclease at 22° C for 20 min, chilled and brought to 0·15 ml. with a concentrated mixture of the components of the assay system. Curve 1, no treatment; curve 2, preincubated; curve 3, 10 μg ribonuclease added to the reaction mixture; curve 4, preincubated with 10 μg ribonuclease.

The Template is RNA

Virions of the RNA tumour viruses contain RNA but no DNA[15,16]. The template for the virion DNA polymerase is therefore probably the viral RNA. To substantiate further that RNA is the template, the effect of ribonuclease on the reaction was investigated. When 50 μg/ml. of pancreatic ribonuclease was included in the reaction mixture, there was a 50 per cent inhibition of activity during the first hour and more than 80 per cent inhibition during the second hour of incubation (Fig. 1, curve 3). If the virions were preincubated with the enzyme in water at 22° C and the components of the reaction mixture were then added, an earlier and more extensive inhibition was evident (Fig. 1, curve 4). Preincubation in water without ribonuclease caused only a slight inactivation of the virion polymerase activity (Fig. 1, curve 2). Increasing the concentration of ribonuclease during preincubation could inhibit more than 95 per cent of the DNA polymerase activity (Table 3). To ensure that the inhibition by ribonuclease was attributable to the enzymic activity of the added protein, two other basic proteins were preincubated with the virions. Only ribonuclease was able to inhibit the reaction (Table 3). These experiments substantiate the idea that RNA is the template for the reaction. Hybridization experiments are in progress to determine if the DNA is complementary in base sequence to the viral RNA.

Ability of the Enzyme to Incorporate Ribonucleotides

The deoxyribonucleotide incorporation measured in these experiments could be the result of an RNA polymerase activity in the virion which can polymerize deoxyribonucleotides when they are provided in the reaction mixture. The VSV RNA polymerase and the R-MLV DNA polymerase were therefore compared. The VSV RNA polymerase incorporated only ribonucleotides. At its pH optimum of 7·3 (my unpublished observation), in the presence of the four common ribonucleoside triphosphates, the enzyme incorporated ^{3}H-GMP extensively[12]. At this pH, however, in the presence of the four deoxyribonucleoside triphosphates, no ^{3}H-TMP incorporation was demonstrable (Table 4). Furthermore, replacement of even a single ribonucleotide by its homologous deoxyribonucleotide led to no detectable synthesis (my unpublished observation). At pH 8·3, the optimum for the R-MLV DNA polymerase, the VSV polymerase catalysed much less ribonucleotide incorporation and no significant deoxyribonucleotide incorporation could be detected.

Table 3. EFFECT OF RIBONUCLEASE ON THE DNA POLYMERASE ACTIVITY OF RAUSCHER MOUSE LEUKAEMIA VIRUS

Conditions	pmoles ^{3}H-TMP incorporation
No preincubation	2·50
Preincubated with no addition	2·20
Preincubated with 20 μg/ml. ribonuclease	0·69
Preincubated with 50 μg/ml. ribonuclease	0·31
Preincubated with 200 μg/ml. ribonuclease	0·08
Preincubated with no addition	3·69
Preincubated with 50 μg/ml. ribonuclease	0·52
Preincubated with 50 μg/ml. lysozyme	3·67
Preincubated with 50 μg/ml. cytochrome c	3·97

In experiment 1, for the preincubation, 15 μg of viral protein in 5 μl. of solution was added to 45 μl. of water at 4° C containing the indicated amounts of enzyme. After incubation for 30 min at 22° C, the samples were chilled and 50 μl. of a 2-fold concentrated standard reaction mixture was added. The samples were then incubated at 37° C for 45 min and acid-insoluble radioactivity was measured. In experiment 2, the same procedure was followed, except that the preincubation was for 20 min at 22° C and the 37° C incubation was for 60 min.

Table 4. COMPARISON OF NUCLEOTIDE INCORPORATION BY VESICULAR STOMATITIS VIRUS AND RAUSCHER MOUSE LEUKAEMIA VIRUS

Precursor	pH	Incorporation in 45 min (pmoles) Vesicular stomatitis virus	Mouse leukaemia virus
^{3}H-TTP	8·3	< 0·01	2·3
^{3}H-TTP (omit dATP)	8·3	N.D.	0·06
^{3}H-TTP (omit dATP; plus ATP)	8·3	N.D.	0·08
^{3}H-GTP	8·3	0·43	< 0·03
^{3}H-GTP	7·3	3·7	< 0·03

When ^{3}H-TTP was the precursor, standard reaction conditions were used (see Table 1). When ^{3}H-GTP was the precursor, the reaction mixture contained, in 0·1 ml., 5 μmoles Tris-HCl (pH as indicated), 0·6 μmoles magnesium acetate, 0·3 μmoles mercaptoethanol, 9 μmoles NaCl, 0·08 μmole each of ATP, CTP, UTP; and 0·001 μmole ^{3}H-GTP (1,040 c.p.m. per pmole). All VSV assays included 0·1 per cent 'Triton N-101' (ref. 12) and 2–5 μg of viral protein. The R-MLV assays contained 15 μg of viral protein.

The R-MLV polymerase incorporated only deoxyribonucleotides. At pH 8·3, ^{3}H-TMP incorporation was readily demonstrable but replacement of dATP by ATP completely prevented synthesis (Table 4). Furthermore, no significant incorporation of ^{3}H-GMP could be found in the presence of the four ribonucleotides. At pH 7·3, the R-MLV polymerase was also inactive with ribonucleotides. The polymerase in the R-MLV virions is therefore highly specific for deoxyribonucleotides.

DNA Polymerase in Rous Sarcoma Virus

A preparation of the Prague strain of Rous sarcoma virus was assayed for DNA polymerase activity (Table 5). Incorporation of radioactivity from ^{3}H-TTP was demonstrable and the activity was severely reduced by omission of either Mg^{2+} or dATP from the reaction mixture. RNA-dependent DNA polymerase is therefore probably a constituent of all RNA tumour viruses.

These experiments indicate that the virions of Rauscher mouse leukaemia virus and Rous sarcoma virus contain a DNA polymerase. The inhibition of its activity by ribonuclease suggests that the enzyme is an RNA-dependent DNA polymerase. It seems probable that all RNA tumour viruses have such an activity. The existence of this enzyme strongly supports the earlier suggestions[1-7] that genetically specific DNA synthesis is an early event in the replication cycle of the RNA tumour viruses and that DNA is the template for viral RNA synthesis. Whether the viral DNA ("provirus")[2] is integrated into the host genome or remains as a free template for RNA synthesis will require further study. It will also be necessary to determine whether the host DNA-dependent RNA polymerase or a virus-specific enzyme catalyses the synthesis of viral RNA from the DNA.

Table 5. PROPERTIES OF THE ROUS SARCOMA VIRUS DNA POLYMERASE

Reaction system	pmoles ^{3}H-TMP incorporated in 120 min
Complete	2·06
Without magnesium acetate	0·12
Without dATP	0·19

A preparation of the Prague strain (sub-group C) of Rous sarcoma virus[16] having a titre of 5 × 10^7 focus forming units per ml. was provided by Dr Peter Vogt. The virus was purified from tissue culture fluid by differential centrifugation. Before use the preparation was centrifuged and the pellet dissolved in 1/10 of the initial volume as described for the R-MLV preparation. For each assay 15 μl. of the concentrated Rous sarcoma virus preparation was assayed in a standard reaction mixture by incubation for 2 h. An unincubated control sample had radioactivity corresponding to 0·14 pmole which was subtracted from the experimental values.

I thank Drs G. Todaro, F. Rauscher and R. Holdenreid for their assistance in providing the mouse leukaemia virus. This work was supported by grants from the US Public Health Service and the American Cancer Society and was carried out during the tenure of an American Society Faculty Research Award.

DAVID BALTIMORE

Department of Biology,
Massachusetts Institute of Technology,
Cambridge,
Massachusetts 02139.

Received June 2, 1970.

1 Green, M., *Ann. Rev. Biochem.*, **39** (1970, in the press).
2 Temin, H. M., *Virology*, **23**, 486 (1964).
3 Bader, J. P., *Virology*, **22**, 462 (1964).
4 Vigier, P., and Golde, A., *Virology*, **23**, 511 (1964).
5 Duesberg, P. H., and Vogt, P. K., *Proc. US Nat. Acad. Sci.*, **64**, 939 (1969).
6 Temin, H. M., in *Biology of Large RNA Viruses* (edit. by Barry, R., and Mahy, B.) (Academic Press, London, 1970).
7 Temin, H. M., *Virology*, **20**, 577 (1963).
8 Kates, J. R., and McAuslan, B. R., *Proc. US Nat. Acad. Sci.*, **58**, 134 (1967).
9 Munyon, W., Paoletti, E., and Grace, J. T. J., *Proc. US Nat. Acad. Sci.*, **58**, 2280 (1967).
10 Shatkin, A. J., and Sipe, J. D., *Proc. US Nat. Acad. Sci.*, **61**, 1462 (1968).
11 Borsa, J., and Graham, A. F., *Biochem. Biophys. Res. Commun.*, **33**, 895 (1968).
12 Baltimore, D., Huang, A. S., and Stampfer, M., *Proc. US Nat. Acad. Sci.* **66** (1970, in the press).
13 Temin, H. M., and Mizutani, S., *Nature*, **226**, 1211 (1970) (following article).
14 O'Conner, T. E., Rauscher, F. J., and Zeigel, R. F., *Science*, **144**, 1144 (1964).
15 Crawford, L. V., and Crawford, E. M., *Virology*, **13**, 227 (1961).
16 Duesberg, P., and Robinson, W. S., *Proc. US Nat. Acad. Sci.*, **55**, 219 (1966).
17 Duff, R. G., and Vogt, P. K., *Virology*, **39**, 18 (1969).

226, 1209; 1970

RNA-dependent DNA Polymerase in Virions of Rous Sarcoma Virus

INFECTION of sensitive cells by RNA sarcoma viruses requires the synthesis of new DNA different from that synthesized in the S-phase of the cell cycle (refs. 1, 2 and unpublished results of D. Boettiger and H. M. T.); production of RNA tumour viruses is sensitive to actinomycin D[3,4]; and cells transformed by RNA tumour viruses have new DNA which hybridizes with viral RNA[5,6]. These are the basic observations essential to the DNA provirus hypothesis—replication of RNA tumour viruses takes place through a DNA intermediate, not

226, 1211; 1970

through an RNA intermediate as does the replication of other RNA viruses[7].

Formation of the provirus is normal in stationary chicken cells exposed to Rous sarcoma virus (RSV), even in the presence of 0·5 μg/ml. cycloheximide (our unpublished results). This finding, together with the discovery of polymerases in virions of vaccinia virus and of reovirus[8–11], suggested that an enzyme that would synthesize DNA from an RNA template might be present in virions of RSV. We now report data supporting the existence of such an enzyme, and we learn that David Baltimore has independently discovered a similar enzyme in virions of Rauscher leukaemia virus[12].

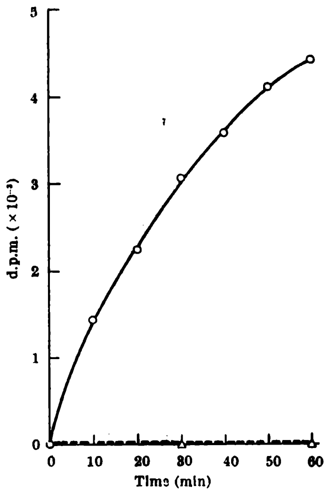


Fig. 1. Kinetics of incorporation. Virus treated with 'Nonidet' and dithiothreitol at 0° C and incubated at 37° C (○—○) or 80° C (△ - - - △) for 10 min was assayed in a standard polymerase assay. ○, Unheated; △, heated.

The sources of virus and methods of concentration have been described[13]. All preparations were carried out in sterile conditions. Concentrated virus was placed on a layer of 15 per cent sucrose and centrifuged at 25,000 r.p.m. for 1 h in the 'SW 25.1' rotor of the Spinco ultracentrifuge on to a cushion of 60 per cent sucrose. The virus band was collected from the interphase and further purified by equilibrium sucrose density gradient centrifugation[14]. Virus further purified by sucrose velocity density gradient centrifugation gave the same results.

Table 1. ACTIVATION OF ENZYME

System	^{3}H-TTP incorporated (d.p.m.)
No virions	0
Non-disrupted virions	255
Virions disrupted with 'Nonidet'	
At 0° + DTT	6,780
At 0° − DTT	4,420
At 40° + DTT	5,000
At 40° − DTT	425

Purified virions untreated or incubated for 5 min at 0° C or 40° C with 0·25 per cent 'Nonidet P-40' (Shell Chemical Co.) with 0 or 1 per cent dithiothreitol (DTT) (Sigma) were assayed in the standard polymerase assay.

The polymerase assay consisted of 0·125 μmoles each of dATP, dCTP, and dGTP (Calbiochem) (in 0·02 M Tris-HCl buffer at pH 8·0, containing 0·33 M EDTA and 1·7 mM 2-mercaptoethanol); 1·25 μmoles of $MgCl_2$ and 2·5 μmoles of KCl; 2·5 μg phosphoenolpyruvate (Calbiochem); 10 μg pyruvate kinase (Calbiochem); 2·5 μCi of ^{3}H-TTP (Schwarz) (12 Ci/mmole); and 0·025 ml. of enzyme (10^8 focus forming units of disrupted Schmidt-Ruppin virus, $A_{280\,nm} = 0{\cdot}30$) in a total volume of 0·125 ml. Incubation was at 40° C for 1 h. 0·025 ml. of the reaction mixture was withdrawn and assayed for acid-insoluble counts by the method of Furlong[15].

To observe full activity of the enzyme, it was necessary to treat the virions with a non-ionic detergent (Tables 1 and 4). If the treatment was at 40° C the presence of dithiothreitol (DTT) was necessary to recover activity. In most preparations of virions, however, there was some activity: 5–20 per cent of the disrupted virions, in the absence of detergent treatment, which probably represents disrupted virions in the preparation. It is known that virions of RNA tumour viruses are easily disrupted[16,17], so that the activity is probably present in the nucleoid of the virion.

Table 2. REQUIREMENTS FOR ENZYME ACTIVITY

System	^{3}H-TTP incorporated (d.p.m.)
Complete	5,675
Without $MgCl_2$	186
Without $MgCl_2$, with $MnCl_2$	5,570
Without $MgCl_2$, with $CaCl_2$	18
Without dATP	897
Without dCTP	1,780
Without dGTP	2,190

Virus treated with 'Nonidet' and dithiothreitol at 0° C was incubated in the standard polymerase assay with the substitutions listed.

The kinetics of incorporation with disrupted virions are shown in Fig. 1. Incorporation is rapid for 1 h. Other experiments show that incorporation continues at about the same rate for the second hour. Preheating disrupted virus at 80° C prevents any incorporation, and so does pretreatment of disrupted virus with crystalline trypsin.

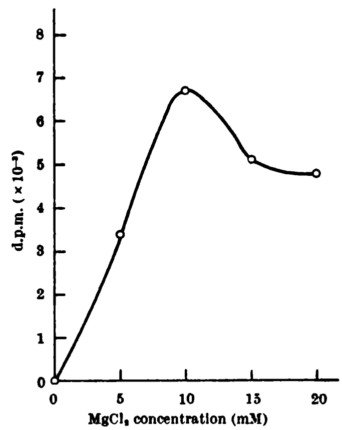


Fig. 2. $MgCl_2$ requirement. Virus treated with 'Nonidet' and dithiothreitol at 0° C was incubated in the standard polymerase assay with different concentrations of $MgCl_2$.

Fig. 2 demonstrates that there is an absolute requirement for $MgCl_2$, 10 mM being the optimum concentration. The data in Table 2 show that $MnCl_2$ can substitute for $MgCl_2$ in the polymerase assay, but $CaCl_2$ cannot. Other experiments show that a monovalent cation is not required for activity, although 20 mM KCl causes a 15 per cent stimulation. Higher concentrations of KCl are inhibitory: 60 per cent inhibition was observed at 80 mM.

When the amount of disrupted virions present in the polymerase assay was varied, the amount of incorporation varied with second-order kinetics. When incubation was carried out at different temperatures, a broad optimum between 40° C and 50° C was found. (The high temperature of this optimum may relate to the fact that the normal host of the virus is the chicken.) When incubation was carried out at different pHs, a broad optimum at pH 8–9·5 was found.

Table 2 demonstrates that all four deoxyribonucleotide triphosphates are required for full activity, but some activity was present when only three deoxyribonucleotide triphosphates were added and 10–20 per cent of full activity was still present with only two deoxyribonucleotide triphosphates. The activity in the presence of three deoxyribonucleotide triphosphates is probably the result of the presence of deoxyribonucleotide triphosphates in the virion. Other host components are known to be incorporated in the virion of RNA tumour viruses[18,19].

Table 3. RNA DEPENDENCE OF POLYMERASE ACTIVITY

Treatment	^{3}H-TTP incorporated (d.p.m.)
Non-treated disrupted virions	9,110
Disrupted virions preincubated with ribonuclease A (50 μg/ml.) at 20° C for 1 h	2,650
Disrupted virions preincubated with ribonuclease A (1 mg/ml.) at 0° C for 1 h	137
Disrupted virions preincubated with lysozyme (50 μg/ml.) at 0° C for 1 h	9,650

Disrupted virions were incubated with ribonuclease A (Worthington) which was heated at 80° C for 10 min, or with lysozyme at the indicated concentration in the specified conditions, and a standard polymerase assay was performed.

The data in Table 3 demonstrate that incorporation of thymidine triphosphate was more than 99 per cent abolished if the virions were pretreated at 0° with 1 mg ribonuclease per ml. Treatment with 50 μg/ml. ribonuclease at 20° C did not prevent all incorporation of thymidine triphosphate, which suggests that the RNA of the virion may be masked by protein. (Lysozyme was added as a control for non-specific binding of ribonuclease to DNA.) Because the ribonuclease was heated for 10 min at 80° C or 100° C before use to destroy deoxyribonuclease it seems that intact RNA is necessary for incorporation of thymidine triphosphate.

Table 4. SOURCE OF POLYMERASE

Source	^{3}H-TTP incorporated (d.p.m.)
Virions of SRV	1,410
Disrupted virions of SRV	5,675
Virions of AMV	1,875
Disrupted virions of AMV	12,850
Disrupted pellet from supernatant of uninfected cells	0

Virions of Schmidt-Ruppin virus (SRV) were prepared as before (experiment of Table 2). Virions of avian myeloblastosis virus (AMV) and a pellet from uninfected cells were prepared by differential centrifugation. All disrupted preparations were treated with 'Nonidet' and dithiothreitol at 0° C and assayed in a standard polymerase assay. The material used per tube was originally from 45 ml. of culture fluid for SRV, 20 ml. for AMV, and 20 ml. for uninfected cells.

To determine whether the enzyme is present in supernatants of normal cells or in RNA leukaemia viruses, the experiment of Table 4 was performed. Normal cell supernatant did not contain activity even after treatment with 'Nonidet'. Virions of avian myeloblastosis virus (AMV) contained activity that was increased ten-fold by treatment with 'Nonidet'.

The nature of the product of the polymerase assay was investigated by treating portions with deoxyribonuclease, ribonuclease or KOH. About 80 per cent of the product was made acid soluble by treatment with deoxyribonuclease, and the product was resistant to ribonuclease and KOH (Table 5).

Table 5. NATURE OF PRODUCT

Treatment	Residual acid-insoluble ^{3}H-TTP (d.p.m.) Experiment A	Experiment B
Buffer	10,200	8,350
Deoxyribonuclease	697	1,520
Ribonuclease	10,900	7,200
KOH	—	8,250

A standard polymerase assay was performed with 'Nonidet' treated virions. The product was incubated in buffer or 0·3 M KOH at 37° C for 20 h or with (A) 1 mg/ml. or (B) 50 μg/ml. of deoxyribonuclease I (Worthington), or with 1 mg/ml. of ribonuclease A (Worthington) for 1 h at 37° C, and portions were removed and tested for acid-insoluble counts.

To determine if the polymerase might also make RNA, disrupted virions were incubated with the four ribonucleotide triphosphates, including ^{3}H-UTP (Schwarz, 3·2 Ci/mmole). With either $MgCl_2$ or $MnCl_2$ in the incubation mixture, no incorporation was detected. In a parallel incubation with deoxyribonucleotide triphosphates, 12,200 d.p.m. of ^{3}H-TTP was incorporated.

These results demonstrate that there is a new polymerase inside the virions of RNA tumour viruses. It is not present in supernatants of normal cells but is present in virions of avian sarcoma and leukaemia RNA tumour viruses. The polymerase seems to catalyse the incorporation of deoxyribonucleotide triphosphates into DNA from an RNA template. Work is being performed to characterize further the reaction and the product. If the present results and Baltimore's results[12] with Rauscher leukaemia virus are upheld, they will constitute strong evidence that the DNA provirus hypothesis is correct and that RNA tumour viruses have a DNA genome when they are in cells and an RNA genome when they are in virions. This result would have strong implications for theories of viral carcinogenesis and, possibly, for theories of information transfer in other biological systems[20].

This work was supported by a US Public Health Service research grant from the National Cancer Institute. H. M. T. holds a research career development award from the National Cancer Institute.

HOWARD M. TEMIN
SATOSHI MIZUTANI

McArdle Laboratory for Cancer Research,
University of Wisconsin,
Madison,
Wisconsin 53706.

Received June 15, 1970.

1 Temin, H. M., *Cancer Res.*, **28**, 1835 (1968).
2 Murray, R. K., and Temin, H. M., *Intern. J. Cancer* (in the press).
3 Temin, H. M., *Virology*, **20**, 577 (1963).
4 Baluda, M. B., and Nayak, D. P., *J. Virol.*, **4**, 554 (1969).
5 Temin, H. M., *Proc. US Nat. Acad. Sci.*, **52**, 323 (1964).
6 Baluda, M. B., and Nayak, D. P., in *Biology of Large RNA Viruses* (edit. by Barry, R., and Mahy, B.) (Academic Press, London, 1970).
7 Temin, H. M., *Nat. Cancer Inst. Monog.*, **17**, 557 (1964).
8 Kates, J. R., and McAuslan, B. R., *Proc. US Nat. Acad. Sci.*, **57**, 314 (1967).
9 Munyon, W., Paoletti, E., and Grace, J. T., *Proc. US Nat. Acad. Sci.*, **58**, 2280 (1967).
10 Borsa, J., and Graham, A. F., *Biochem. Biophys. Res. Commun.*, **33**, 895 (1968).
11 Shatkin, A. J., and Sipe, J. D., *Proc. US Nat. Acad. Sci.*, **61**, 1462 (1968).
12 Baltimore, D., *Nature*, **226**, 1209 (1970) (preceding article).
13 Altaner, C., and Temin, H. M., *Virology*, **40**, 118 (1970).
14 Robinson, W. S., Pitkanen, A., and Rubin, H., *Proc. US Nat. Acad. Sci.*, **54**, 137 (1965).
15 Furlong, N. B., *Meth. Cancer Res.*, **3**, 27 (1967).
16 Vogt, P. K., *Adv. Virus. Res.*, **11**, 293 (1965).
17 Bauer, H., and Schafer, W., *Virology*, **29**, 494 (1966).
18 Bauer, H., *Z. Naturforsch.*, **21b**, 453 (1966).
19 Erikson, R. L., *Virology*, **37**, 124 (1969).
20 Temin, H. M., *Persp. Biol. Med.* (in the press).

Continuous cultures of fused cells secreting antibody of predefined specificity

THE manufacture of predefined specific antibodies by means of permanent tissue culture cell lines is of general interest. There are at present a considerable number of permanent cultures of myeloma cells[1,2] and screening procedures have been used to reveal antibody activity in some of them. This, however, is not a satisfactory source of monoclonal antibodies of predefined specificity. We describe here the derivation of a number of tissue culture cell lines which secrete anti-sheep red blood cell (SRBC) antibodies. The cell lines are made by fusion of a mouse myeloma and mouse spleen cells from an immunised donor. To understand the expression and interactions of the Ig chains from the parental lines, fusion experiments between two known mouse myeloma lines were carried out.

Each immunoglobulin chain results from the integrated expression of one of several *V* and *C* genes coding respectively for its variable and constant sections. Each cell expresses only one of the two possible alleles (allelic exclusion; reviewed in ref. 3). When two antibody-producing cells are fused, the products of both parental lines are expressed[4,5], and although the light and heavy chains of both parental lines are randomly joined, no evidence of scrambling of *V* and *C* sections is observed[4]. These results, obtained in an heterologous system involving cells of rat and mouse origin, have now been confirmed by fusing two myeloma cells of the same mouse strain, and provide the background for the derivation and understanding of antibody-secreting hybrid lines in which one of the parental cells is an antibody-producing spleen cell.

Two myeloma cell lines of BALB/c origin were used. P1Bul is resistant to 5-bromo-2′-deoxyuridine[4], does not grow in selective medium (HAT, ref. 6) and secretes a myeloma protein, Adj PC5, which is an IgG2A (κ), (ref. 1). Synthesis is not balanced and free light chains are also secreted. The second cell line, P3-X63Ag8, prepared from P3 cells[2], is resistant to 20 μg ml^{-1} 8-azaguanine and does not grow in HAT medium. The protein secreted (MOPC 21) is an IgG1 (κ) which has been fully sequenced[7,8]. Equal numbers of cells from each parental line were fused using inactivated Sendai virus[9] and samples continIng 2×10^5 cells were grown in selective medium in separate dishes. Four out of ten dishes showed growth in selective medium and these were taken as independent hybrid lines, probably derived from single fusion events. The karyotype of the hybrid cells after 5 months in culture was just under the sum of the two parental lines (Table 1). Figure 1 shows the isoelectric focusing[10] (IEF) pattern of the secreted products of different lines. The hybrid cells (samples *c–h* in Fig. 1) give a much more complex pattern than either parent (*a* and *b*) or a mixture of the parental lines (*m*). The important feature of the new pattern is the presence of extra bands (Fig. 1, arrows). These new bands, however, do not seem to be the result of differences in primary structure; this is indicated by the IEF pattern of the products after reduction to separate the heavy and light chains (Fig. 1*B*). The IEF pattern of chains of the hybrid clones (Fig. 1*B*, *g*) is equivalent to the sum of the IEF pattern (*a* and *b*) of chains of the parental clones with no evidence of extra products. We conclude that, as previously shown with interspecies hybrids[4,5], new Ig molecules are produced as a result of mixed association between heavy and light chains from the two parents. This process is intracellular as a mixed cell population does not give rise to such hybrid molecules (compare *m* and *g*, Fig. 1*A*). The individual cells must therefore be able to express both isotypes. This result shows that in hybrid cells the expression of one isotype and idiotype does not exclude the expression of another: both heavy chain isotypes (γ1 and γ2a) and both V_H and both V_L regions (idiotypes) are expressed. There are no allotypic markers for the C_κ region to provide direct proof for the expression of both parental C_κ regions. But this is indicated by the phenotypic link between the *V* and *C* regions.

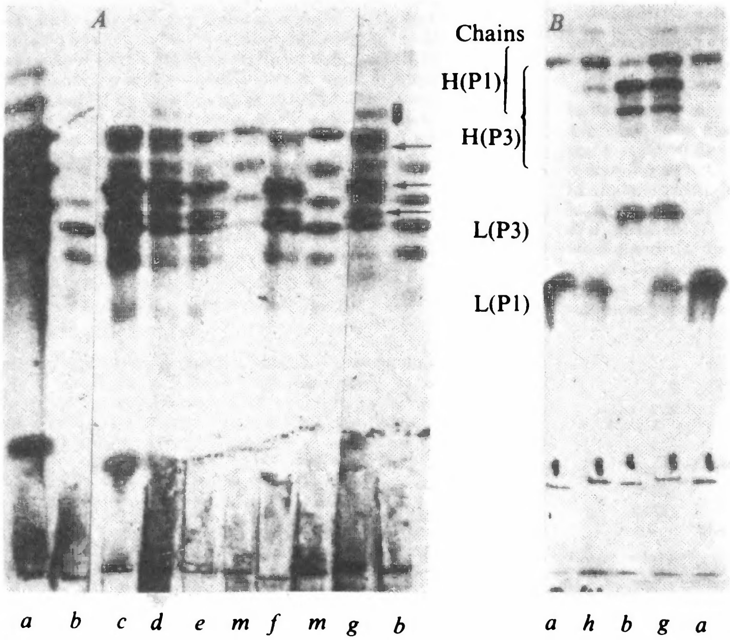


Fig. 1 Autoradiograph of labelled components secreted by the parental and hybrid cell lines analysed by IEF before (*A*) and after reduction (*B*). Cells were incubated in the presence of ^{14}C-lysine[14] and the supernatant applied on polyacrylamide slabs. *A*, *p*H range 6.0 (bottom) to 8.0 (top) in 4 M urea. *B*, *p*H range 5.0 (bottom) to 9.0 (top) in 6 M urea; the supernatant was incubated for 20 min at 37 °C in the presence of 8 M urea, 1.5 M mercaptoethanol and 0.1 M potassium phosphate *p*H 8.0 before being applied to the right slab. Supernatants from parental cell lines in: *a*, P1Bul; *b*, P3-X67Ag8; and *m*, mixture of equal number of P1Bul and P3-X67Ag8 cells. Supernatants from two independently derived hybrid lines are shown: *c–f*, four subclones from Hy-3; *g* and *h*, two subclones from Hy-B. Fusion was carried out[4,9] using 10^6 cells of each parental line and 4,000 haemagglutination units inactivated Sendai virus (Searle). Cells were divided into ten equal samples and grown separately in selective medium (HAT medium, ref. 6). Medium was changed every 3 d. Successful hybrid lines were obtained in four of the cultures, and all gave similar IEF patterns. Hy-B and Hy-3 were further cloned in soft agar[14].
L, Light; H, heavy.

Figure 1*A* shows that clones derived from different hybridisation experiments and from subclones of one line are indistinguishable. This has also been observed in other experiments (data not shown). Variants were, however, found in a survey of 100 subclones. The difference is often associated with changes

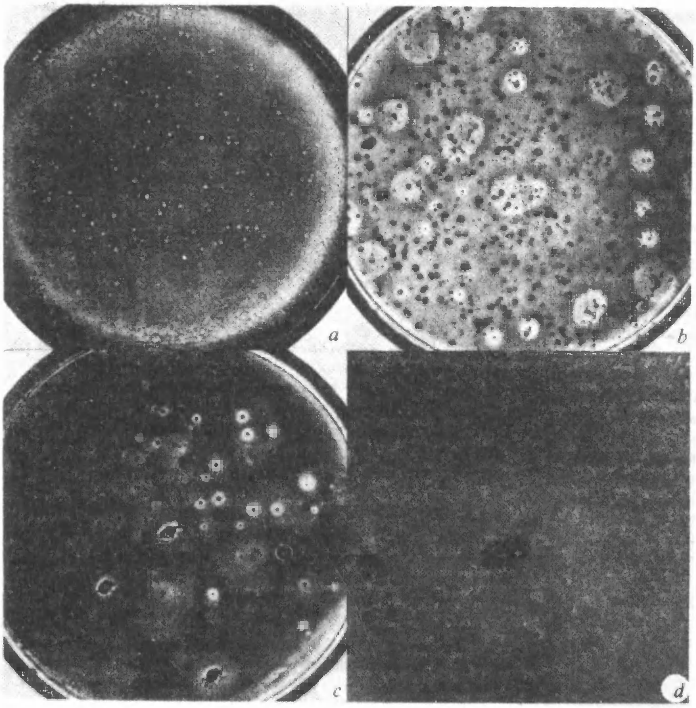


Fig. 2 Isolation of an anti-SRBC antibody-secreting cell clone. Activity was revealed by a halo of haemolysed SRBC. Direct plaques given by: *a*, 6,000 hybrid cells Sp-1; *b*, clones grown in soft agar from an inoculum of 2,000 Sp-1 cells; *c*, recloning of one of the positive clones Sp-1/7; *d*, higher magnification of a positive clone. Myeloma cells (10^7 P3-X67A g8) were fused to 10^8 spleen cells from an immunised BALB/c mouse. Mice were immunised by intraperitoneal injection of 0.2 ml packed SRBC diluted 1:10, boosted after 1 month and the spleens collected 4 d later. After fusion, cells (Sp-1) were grown for 8 d in HAT medium, changed at 1–3 d intervals. Cells were then grown in Dulbecco modified Eagle's medium, supplemented for 2 weeks with hypoxanthine and thymidine. Forty days after fusion the presence of anti-SRBC activity was revealed as shown in *a*. The ratio of plaque forming cells/total number of hybrid cells was 1/30. This hybrid cell population was cloned in soft agar (50% cloning efficiency). A modified plaque assay was used to reveal positive clones shown in *b–d* as follows. When cell clones had reached a suitable size, they were overlaid in sterile conditions with 2 ml 0.6% agarose in phosphate-buffered saline containing 25 μl packed SRBC and 0.2 ml fresh guinea pig serum (absorbed with SRBC) as source of complement. *b*, Taken after overnight incubation at 37 °C. The ratio of positive/total number of clones was 1/33. A suitable positive clone was picked out and grown in suspension. This clone was called Sp-1/7, and was recloned as shown in *c*; over 90% of the clones gave positive lysis. A second experiment in which 10^8 P3-X67Ag8 cells were fused with 10^8 spleen cells was the source of a clone giving rise to indirect plaques (clone Sp-2/3-3). Indirect plaques were produced by the addition of 1:20 sheep anti-MOPC 21 antibody to the agarose overlay.

in the ratios of the different chains and occasionally with the total disappearance of one or other of the chains. Such events are best visualised on IEF analysis of the separated chains (for example, Fig. 1*h*, in which the heavy chain of P3 is no longer observed). The important point that no new chains are detected by IEF complements a previous study[4] of a rat–mouse hybrid line in which scrambling of *V* and *C* regions from the light chains of rat and mouse was not observed. In this study, both light chains have identical C_κ regions and therefore scrambled V_L–C_L molecules would be undetected. On the other hand, the heavy chains are of different subclasses and we expect scrambled V_H–C_H to be detectable by IEF. They were not observed in the clones studied and if they occur must do so at a lower frequency. We conclude that in syngeneic cell hybrids (as well as in interspecies cell hybrids) *V–C* integration is not the result of cytoplasmic events. Integration as a result of DNA translocation or rearrangement during transcription is also suggested by the presence of integrated mRNA molecules[11] and by the existence of defective heavy chains in which a deletion of *V* and *C* sections seems to take place in already committed cells[12].

The cell line P3-X63Ag8 described above dies when exposed to HAT medium. Spleen cells from an immunised mouse also die in growth medium. When both cells are fused by Sendai virus and the resulting mixture is grown in HAT medium, surviving clones can be observed to grow and become established after a few weeks. We have used SRBC as immunogen, which enabled us, after culturing the fused lines, to determine the presence of specific antibody-producing cells by a plaque assay technique[13] (Fig. 2*a*). The hybrid cells were cloned in soft agar[14] and clones producing antibody were easily detected by an overlay of SRBC and complement (Fig. 2*b*). Individual clones were isolated and shown to retain their phenotype as almost all the clones of the derived purified line are capable of lysing SRBC (Fig. 2*c*). The clones were visible to the naked eye (for example, Fig. 2*d*). Both direct and indirect plaque assays[13] have been used to detect specific clones and representative clones of both types have been characterised and studied.

The derived lines (Sp hybrids) are hybrid cell lines for the following reasons. They grow in selective medium. Their karyotype after 4 months in culture (Table 1) is a little smaller than the sum of the two parental lines but more than twice the chromosome number of normal BALB/c cells, indicating that the lines are not the result of fusion between spleen cells. In addition the lines contain a metacentric chromosome also present in the parental P3-X67Ag8. Finally, the secreted immunoglobulins contain MOPC 21 protein in addition to new, unknown components. The latter presumably represent the chains derived from the specific anti-SRBC antibody. Figure 3*A* shows the IEF pattern of the material secreted by two such Sp hybrid clones. The IEF bands derived from the parental P3 line are visible in the pattern of the hybrid cells, although obscured by the presence of a number of new bands. The pattern is very complex, but the complexity of hybrids of this type is likely to result from the random recombination of chains (see above, Fig. 1). Indeed, IEF patterns of the reduced material secreted by the spleen–P3 hybrid clones gave a simpler pattern of Ig chains. The heavy and light chains of the P3 parental line became prominent, and new bands were apparent.

The hybrid Sp-1 gave direct plaques and this suggested that it produces an IgM antibody. This is confirmed in Fig. 4 which shows the inhibition of SRBC lysis by a specific anti-IgM

Table 1 Number of chromosomes in parental and hybrid cell lines

Cell line	Number of chromosomes per cell	Mean
P3-X67Ag8	66,65,65,65,65	65
P1Bu1	Ref. 4	55
Mouse spleen cells	—	40
Hy-B (P1-P3)	112,110,104,104,102	106
Sp-1/7-2	93,90,89,89,87	90
Sp-2/3-3	97,98,96,96,94,88	95

antibody. IEF techniques usually do not reveal 19S IgM molecules. IgM is therefore unlikely to be present in the unreduced sample *a* (Fig. 3*B*) but μ chains should contribute to the pattern obtained after reduction (sample *a*, Fig. 3*A*).

The above results show that cell fusion techniques are a powerful tool to produce specific antibody directed against a predetermined antigen. It further shows that it is possible to isolate hybrid lines producing different antibodies directed against the same antigen and carrying different effector functions (direct and indirect plaque).

The uncloned population of P3–spleen hybrid cells seems quite heterogeneous. Using suitable detection procedures it should be possible to isolate tissue culture cell lines making different classes of antibody. To facilitate our studies we have used a myeloma parental line which itself produced an Ig. Variants in which one of the parental chains is no longer expressed seem fairly common in the case of P1–P3 hybrids (Fig. 1*h*). Therefore selection of lines in which only the specific antibody chains are expressed seems reasonably simple. Alternatively, non-producing variants of myeloma lines could be used for fusion.

We used SRBC as antigen. Three different fusion experiments were successful in producing a large number of antibody-producing cells. Three weeks after the initial fusion, 33/1,086 clones (3%) were positive by the direct plaque assay. The cloning efficiency in the experiment was 50%. In another experiment, however, the proportion of positive clones was considerably lower (about 0.2%). In a third experiment the hybrid population was studied by limiting dilution analysis. From 157 independent hybrids, as many as 15 had anti-SRBC activity. The proportion of positive over negative clones is remarkably high. It is possible that spleen cells which have been triggered during immunisation are particularly successful in giving rise to viable hybrids. It remains to be seen whether similar results can be obtained using other antigenes.

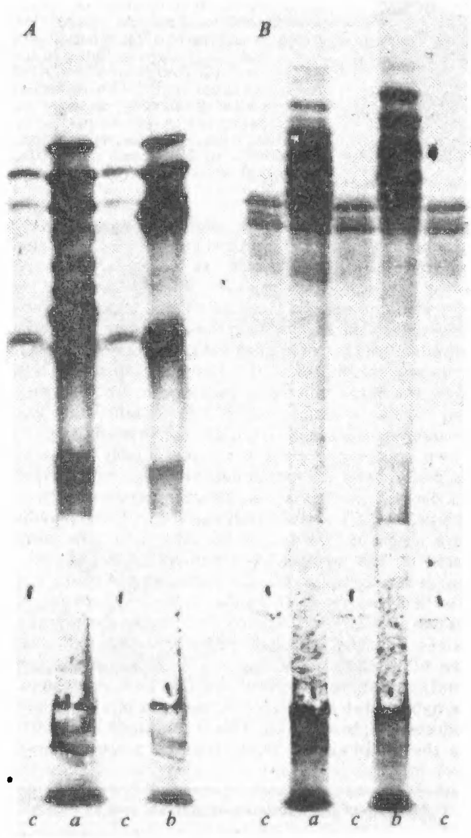


Fig. 3 Autoradiograph of labelled components secreted by anti-SRBC specific hybrid lines. Fractionation before (*B*) and after (*A*) reduction was by IEF. *p*H gradient was 5.0 (bottom) to 9.0 (top) in the presence of 6 M urea. Other conditions as in Fig. 1. Supernatants from: *a*, hybrid clone Sp-1/7-2; *b*, hybrid clone Sp-2/3-3; *c*, myeloma line P3-X67Ag8.

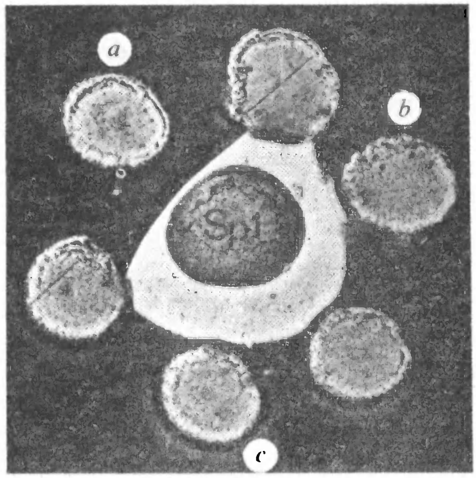


Fig. 4 Inhibition of haemolysis by antibody secreted by hybrid clone Sp-1/7-2. The reaction was in a 9-cm Petri dish with a layer of 5 ml 0.6% agarose in phosphate-buffered saline containing 1/80 (v/v) SRBC. Centre well contains 2.5 μl 20 times concentrated culture medium of clone Sp-1/7-2 and 2.5 μl mouse serum. *a*, Sheep specific anti-mouse macroglobulin (MOPC 104E, Dr Feinstein); *b*, sheep anti-MOPC 21 (P3) IgG1 absorbed with Adj PC-5; *c*, sheep anti-Adj PC-5 (IgG2a) absorbed with MOPC 21. After overnight incubation at room temperature the plate was developed with guinea pig serum diluted 1:10 in Dulbecco's medium without serum.

The cells used in this study are all of BALB/c origin and the hybrid clones can be injected into BALB/c mice to produce solid tumours and serum having anti-SRBC activity. It is possible to hybridise antibody-producing cells from different origins[4,5]. Such cells can be grown *in vitro* in massive cultures to provide specific antibody. Such cultures could be valuable for medical and industrial use.

G. KÖHLER
C. MILSTEIN

MRC Laboratory of Molecular Biology,
Hills Road, Cambridge CB2 2QH, UK

Received May 14; accepted June 26, 1975.

1 Potter, M., *Physiol. Rev.*, **52**, 631-719 (1972).
2 Horibata, K., and Harris, A. W., *Expl Cell Res.*, **60**, 61-70 (1970).
3 Milstein, C., and Munro, A. J., in *Defence and Recognition* (edit. by Porter, R. R.), 199-228 (MTP Int. Rev. Sci., Butterworth, London, 1973).
4 Cotton, R. G. H., and Milstein, C., *Nature*, **244**, 42-43 (1973).
5 Schwaber, J., and Cohen, E. P., *Proc. natn. Acad. Sci. U.S.A.*, 71, 2203-2207 (1974).
6 Littlefield, J. W., *Science*, **145**, 709 (1964).
7 Svasti, J., and Milstein, C., *Biochem. J.*, **128**, 427-444 (1972).
8 Milstein, C., Adetugbo, K., Cowan, N. J., and Secher, D. S., *Progress in Immunology*, II, 1 (edit. by Brent, L., and Holborow, J.), 157-168 (North-Holland, Amsterdam, 1974).
9 Harris, H., and Watkins, J. F., *Nature*, **205**, 640-646 (1965).
10 Awdeh, A. L., Williamson, A. R., and Askonas, B. A., *Nature*, 219, 66-67 (1968).
11 Milstein, C., Brownlee, G. G., Cartwright, E. M., Jarvis, J. M., and Proudfoot, N. J., *Nature*, **252**, 354-359 (1974).
12 Frangione, B., and Milstein, C., *Nature*, **244**, 597-599 (1969).
13 Jerne, N. K., and Nordin, A. A., *Science*, **140**, 405 (1963).
14 Cotton, R. G. H., Secher, D. S., and Milstein, C., *Eur. J. Immun.*, 3, 135-140 (1973).

A conserved DNA sequence in homoeotic genes of the *Drosophila* Antennapedia and bithorax complexes

W. McGinnis, M. S. Levine*, E. Hafen*, A. Kuroiwa & W. J. Gehring

Department of Cell Biology, Biocenter, University of Basel, Klingelbergstrasse 70, CH-4056 Basel, Switzerland

A repetitive DNA sequence has been identified in the Drosophila melanogaster *genome that appears to be localized specifically within genes of the bithorax and Antennapedia complexes that are required for correct segmental development. Initially identified in cloned copies of the genes* Antennapedia, Ultrabithorax *and* fushi tarazu, *the sequence is also contained within two other DNA clones that have characteristics strongly suggesting that they derive from other homoeotic genes.*

MANY of the homoeotic genes of *Drosophila* seem to be involved in the specification of developmental pathways for the body segments of the fly, so that each segment acquires a unique identity. A mutation in such a homoeotic gene often results in a replacement of one body segment (or part of a segment) by another segment that is normally located elsewhere. Many of these homoeotic loci reside in two gene complexes, the bithorax complex and the Antennapedia (Antp) complex, both located on the right arm of chromosome 3 (3R).

The bithorax complex is located in the middle of 3R, and its resident genes impose specific segmental identities on the posterior thoracic and abdominal segments[1]. For example, inactivation of the *bithorax* gene of the complex causes a transformation of the anterior half of the third thoracic segment into the anterior half of the second thoracic segment, resulting in a fly having wing structures in a site normally occupied by haltere. Other recessive mutations in the complex cause analogous transformations of posterior body structures into structures normally located in a more anterior position. Embryos having a deletion of the entire bithorax complex show a transformation of all the posterior body segments into reiterated segments with structures of the second thoracic segment. Based on the above results and others, Lewis has proposed a model in which segmental identity in the thorax and abdomen is controlled by a stepwise activation of additional bithorax complex genes in more posterior segments[1].

The Antp complex is localized nearer the centromere of 3R than the bithorax complex. The genes of the Antp complex appear to control segmental development in the posterior head and thorax, in a manner analogous to the way in which the bithorax complex operates in the more posterior segments[2–5]. A dominant mutation in the *Antp* locus, for example, can result in the transformation of the antenna of the fly into a second thoracic leg[6,7].

The homoeotic genes of both the bithorax and Antp complexes can be thought of as selector genes, using the nomenclature of Garcia-Bellido[8], that act by interpreting gradients of positional information. Based on their location in the gradient, a specific combination of selector genes are expressed, and thus different regions of the developing fly become selected to proceed down specific developmental pathways. Although the available evidence supports this model[1,9,10], the real situation appears to be more complex as there is also evidence that regulatory interactions between different homoeotic selector genes have a role in limiting their region of expression[10–12].

* Present address: Department of Biochemistry, University of California, Berkeley, California 94720, USA.

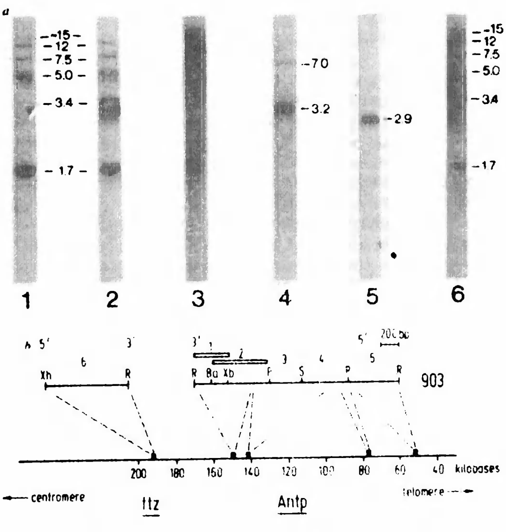


Fig. 1 Repeated sequences in the *Antp* and *ftz* genes. *a*, Individual lanes from *Drosophila* whole genomic Southern blots. The genomic DNA in each case was digested with *Eco*RI. The number below the lanes designates the fragment number used as a probe. The fragment number designations are shown above the respective fragments in *b*. The numbers alongside the lanes indicate the sizes in kilobases (kb) of the hybridizing genomic fragments. All the blots were hybridized and washed in the reduced-stringency conditions described below. Note that the two bands in lane 4 are due to the number 4 probe containing a sequence from each of two *Antp* exons. *b*, Map of the portions of the *Antp* and *ftz* genes used as hybridization probes for the genomic blots. *Antp* 903 is a cDNA clone described by Garber *et al.*[13] which contains the regions from the *Antp* locus marked by solid blocks. The broken lines indicate the approximate extent of the cDNA in each genomic location. The bottom line is a representation of the *Antp* region of chromosome 3, as taken from Garber *et al.*[13]; the numbers reflect the distance (in kb) from the *Humeral* chromosomal breakpoint. The 5′ and 3′ labels show the direction of transcription for the two loci (ref. 17 and A.K., unpublished results). Xh, *Xho*I; R, *Eco*RI; Ba, *Bam*HI; Xb, *Xba*I; P, *Pvu*II; S, *Sph*I.

Methods: Reduced-stringency hybridizations were done as follows. Southern blots[31] were prehybridized in 5×SSC, 0.1% bovine serum albumin, 0.1% Ficoll, 0.1% polyvinylpyrollidone, 250 μg ml^{-1} sonicated, boiled herring sperm DNA, 50 mM $NaPO_4$ *p*H 7, 0.1% SDS, 43% deionized formamide at 37 °C for 2–3 h. The prehybridization buffer was removed from the bag and replaced with the same buffer containing 10^6 c.p.m. ml^{-1} of hybridization probe. Blots were hybridized at 37 °C for 25–48 h, then washed twice in 2×SSC, 0.1% SDS for 5 min at room temperature, followed by two washes for 15 min each at 45–50 °C. Stringent hybridization and wash conditions differed only in the hybridization buffer, which contained 50% instead of 43% formamide, and in the final wash which was done in 0.2×SSC, 0.1% SDS at 65–70 °C.

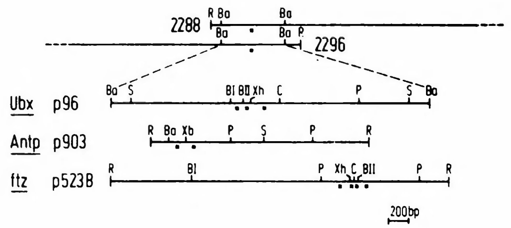


Fig. 2 Localization of the H repeat in clones from the *Ultrabithorax* (*Ubx*), *Antennapedia* (*Antp*) and *fushi tarazu* (*ftz*) genes. The overlapping region from the *Ubx* locus between λ clones 2288 and 2296 (gifts from P. Spierer) is shown at the top, as well as the *Bam*HI fragment common to both which hybridizes to fragments 2 and 6 (Fig. 1). Both these clones are described elsewhere[15], although λ2296 is incorrectly labelled as 2269. The indicated *Bam*HI fragment was subcloned into pAT153 and the resulting plasmid designated p96. A map of the p96 insert is shown. Restriction fragments containing the H repeat are marked by asterisks. *Antp* p903 is the same cDNA clone shown in Fig. 1. Restriction fragments to both sides of the *Xba*I site (Xb) show homology to the H repeat. *ftz* p523B is a *Drosophila* genomic clone from region 190 on the map in Fig. 1, an *Eco*RI fragment in pAT153. This fragment contains most of the *ftz* transcription unit (A.K., unpublished results). Again, the restriction fragments containing the H repeat are marked by asterisks.

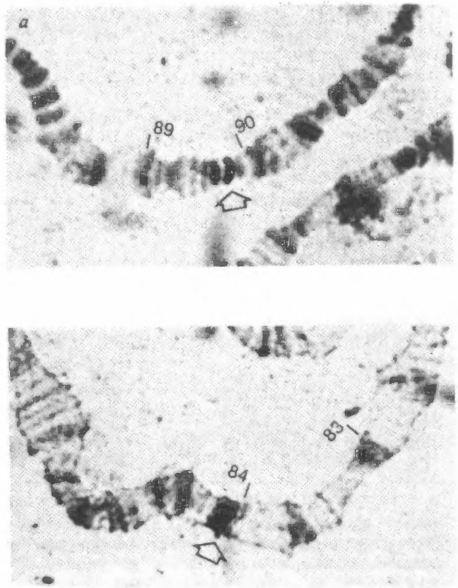


Fig. 3 *In situ* hybridization of H repeat clones p93 and p99 to polytene chromosomes. Clones p93 and p99 were nick-translated with biotinylated nucleotide, and hybridized to squashes of Ore-R chromosomes: p93 to *a*, and p99 to *b*. The hybridized probes were detected by an immunoperoxidase method[21] and the chromosomes stained with Giemsa and photographed. The chromosome 3 divisions are indicated and the sites of hybridization marked by arrowheads.

The physical proximity and similar but distinct functions of the bithorax complex genes led Lewis to propose that the genes of this cluster evolved by mutational diversification of tandemly repeated genes[1]. In the primitive millipede-like ancestors of *Drosophila*, an ancestral gene or genes would direct the development of repetitive segments having similar identities. The evolutionary transition to the Dipterans, with highly diverse segmental structures, might be achieved by duplication and divergence of ancestral genes. According to this model, null mutations in the present set of bithorax complex genes could result in a fly having a more primitive segmental array, that is, with legs on the abdominal segments, or with wings on the third thoracic segment, in addition to those on the second thoracic segment; both types of phenotype are known to result from reduction or loss of function of bithorax complex genes.

Although the bithorax and Antp complexes are widely separated on the third chromosome, their similar functions in specifying segmental identity suggests that both complexes might have evolved from a common ancestral gene or gene complex. A critical test for this hypothesis involves a test for conserved sequences in the genes of the two complexes. These conserved sequences could be relics of ancient gene duplications or regions specifically preserved by selection against mutational change. Here we show that there is DNA sequence homology between some genes of the bithorax complex and the Antp complex. We use this homology, which is imperfect and limited to small regions, to isolate other cross-hybridizing clones from the *Drosophila* genome. The cytogenetic map locations and spatial and temporal patterns of expression for the genes homologous to two of the clones suggest that they represent other homoeotic genes.

Repeated sequences

Genomic and cDNA clones from the *Antp* locus have been isolated and characterized by Garber *et al.*[13]. To test whether the *Antp* gene might be a member of a multigene family, we hybridized the 903 cDNA probe derived from the *Antp* locus to Southern blots of *Drosophila* genomic DNA. The 903 cDNA (see Fig. 1) is complementary to four non-contiguous chromosomal DNA regions spanning 100 kilobases (kb) at the *Antp* locus. Both normal- and reduced-stringency hybridization conditions were used with the 903 probe; in both types of hybridization conditions we detected many genomic fragments homologous to 903 that gave very strong signals, and many (>50) that were relatively weak. The weak signals were more prominent on the blot hybridized in reduced-stringency conditions (data not shown). A stringent wash of both blots (see Fig. 1 legend) removed the weak signals whereas the strong signals remained. The strongly hybridizing genomic fragments had the expected size for those portions of the genome represented in the 903 cDNA. The weakly hybridizing genomic fragments presumably possessed mismatched homology to one or more repeated sequences within the 903 cDNA.

To determine which region(s) within the 903 cDNA sequence were repetitive, the cDNA was subdivided into five restriction fragments of ~500 base pairs (bp) each, and these were individually hybridized to replica genomic Southern blots in reduced-stringency conditions (Fig. 1). The two left-most fragments, which overlap in the 3′ half of the 903 cDNA, detect the expected genomic fragments at *Antp*, and also cross-hybridize with seven other genomic fragments with less intensity. The next 903 fragment to the right (fragment 3) hybridizes to more than 50 genomic fragments. Finally, the two right-most 903 fragments (4 and 5) detect only their genomic homologues at the *Antp* locus.

Garber *et al.*[13] found weak homology between the 903 cDNA and a site to the left of the *Antp* locus, at position 190 on the map in Fig. 1. This site has subsequently been shown to be part of the transcription unit of the *fushi tarazu* (*ftz*) gene (A.K. and E.H., in preparation). The *ftz* gene is required for the determination of the correct number of segments in the *Drosophila* embryo[4,14]. Embryos that are homozygous for certain mutant alleles of *ftz* die early in development, and show deletions of alternate segment primordia. A 0.9-kb *Xho*I/*Eco*RI fragment (probe 6), containing a 3′ portion of the *ftz* transcription unit (A.K., unpublished results), was used as a probe of another Southern blot identical to those used for the five 903 fragments (Fig. 1). In addition to the strong signal contributed by the homologous genomic fragment from the *ftz*

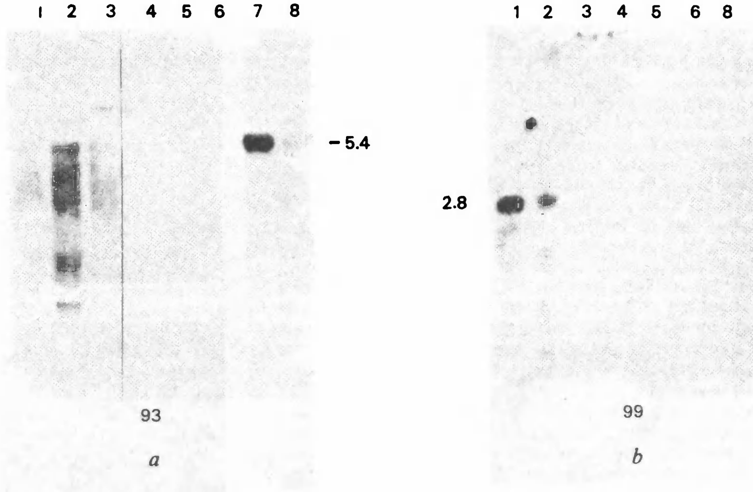


Fig. 4 Transcription from p93 and p99 during *Drosophila* development. Lanes 1, 2 and 3 in each panel contain embryonic RNA from 0–6, 6–12 and 12–18-h stages, respectively. Lanes 4 and 5 in each panel contain RNA from first and second instar larvae. Lanes 6 and 7 in each panel contain RNA from early and late third instar larvae, respectively. Lane 8 in each panel contains RNA from 1-day-old pupae. The numbers alongside the panels indicate the approximate size (in kb) of the largest hybridizing RNAs. On longer exposures a faint band of 2.8 kb was detected in the pupal lane (8) of the blot hybridized with p99 (not shown). Poly(A)$^+$ (10 μg) from successive stages of *Drosophila* development was run on formaldehyde agarose gels and blotted[32]. The blots were hybridized with nick-translated p93 and p99 in the stringent buffer described in Fig. 1 legend. After a stringent wash (also described in Fig. 1) the blots were used to expose X-ray film.

locus, eight other genomic fragments weakly cross-hybridized. Five of these weakly hybridizing genomic DNA fragments are identical in size to those detected by the two probes from the 3′ region of the 903 cDNA. Thus, the 3′ regions of the transcription units of the *Antp* and *ftz* genes share a common sequence, one that appears to be present at five or more locations in the *Drosophila* genome.

Subsequently, we will refer to this low-level repeat as the H repeat, and the high-level repeat in the middle of the 903 cDNA (fragment 3) as the M repeat. The M repeat is not detectable in the DNA of the *ftz* locus.

Presence of H repeat in bithorax complex

Next, we performed experiments to test for the presence of the H repeat in other homoeotic genes. We hybridized fragments 2 and 6 (from *Antp* and *ftz* respectively; see Fig. 1) in reduced-stringency conditions to Southern blots of recombinant clones from the *Ultrabithorax* (*Ubx*) unit of the bithorax complex, specifically λ2229, λ2269, λ2288 and λ2296 (ref. 15) (given by P. Spierer). A 3.2-kb *Bam*HI fragment common to λ2288 and λ2296 hybridized to both H repeat-containing probes (Fig. 2). This *Bam*HI fragment contains most or all of the 3′ exon of the *Ubx* transcription unit (refs 15, 16 and M. Goldschmidt-Clermont, personal communication). Cross-hybridization between the *Antp*, *ftz* and *Ubx* loci has been independently detected by M. Scott (personal communication). None of the *Ubx* region clones that we tested contained the M repeat.

Figure 2 shows more detailed restriction maps of the regions at each locus that contain the H repeat. The location of the H repeat within each of the *Antp*, *Ubx* and *ftz* clones was determined by *inter se* hybridizations to Southern blots of each clone digested with various restriction enzymes. The H repeat is the only detectable region of cross-homology between the three clones, and based on the intensity of signal the cross-homology is either very short (<100 bp), or larger but poorly matched. As the region of cross-homology overlaps both ends of the 90-bp *Xho*I/*Bgl*II fragments found in both *Ubx* and *ftz*, the latter possibility is more likely. The intensity of signal obtained from homologous hybridization compared with cross-hybridization due to the H repeat is shown in lane 1 of Fig. 1. Probe fragment 1, from the 3′ end of the *Antp* cDNA 903, hybridizes very strongly to its genomic homologue on a 1.7-kb *Eco*RI fragment, but with at least 10 times less intensity to the 3.4-kb and 7.5-kb genomic fragments in the same lane, which carry the *ftz* and *Ubx* H repeats respectively.

The cloned regions in Fig. 2 are shown with the same 3R chromosomal orientation: the centromere is to the left, and the telomere to the right. The transcriptional direction of the *Ubx* and *Antp* clones is from right to left, and for the *ftz* clone from left to right (refs 16, 17 and A.K., unpublished results). The relative orientation of the *Xho*I and *Bgl*II sites within the cross-homologous regions of *Ubx* and *ftz*, is consistent with the polarity of the H repeat being the same with respect to the direction of transcription. Preliminary DNA sequencing results from these two regions (W. McG., unpublished results) and of the *Antp* 903 cDNA (R. Garber, unpublished results) also indicate that the H repeat is in the same orientation with respect to transcription at each locus.

Isolation of clones containing H repeat

The observation that the H repeat was associated with three loci known to be crucial for proper segmental development in the fly suggested that it might be a common feature of many homoeotic loci in *Drosophila*. To test this, we first isolated *Drosophila* genomic clones that possessed homology to cloned H repeat sequences. Such clones were then subjected to the following three tests to implicate them as potential new homoeotic loci.

(1) Hybridization of the clones to *Drosophila* polytene chromosomes to determine whether their cytogenetic map locations corresponded to those of genetically characterized homoeotic loci.

(2) Hybridization of subclones containing the H repeat to Northern blots of RNA extracted from successive developmental stages to determine whether the regions were transcribed, and whether their transcription was developmentally regulated in a manner that might be expected for a homoeotic locus.

(3) Hybridization of subclones containing the H repeat to *Drosophila* embryonic tissue sections, to determine whether the transcripts homologous to the subcloned regions were distributed in a segmentally restricted manner, as has been shown for transcripts derived from the homoeotic loci *Antp* and *Ubx*[16,18,19].

Genomic library screen

Approximately 30,000 recombinant bacteriophages (three genome equivalents) from the Charon 4/*Drosophila* library of Maniatis *et al.*[20] were screened using H repeat probes from *Antp* and *ftz* (fragments 2 and 6 in Fig. 1) as probes of duplicate filters. The hybridizations and washes were done in the reduced-stringency conditions described in Fig. 1 legend. A total of 74 plaques from the original plates hybridized to both probes, with a wide range of signal intensity. All were picked and re-screened

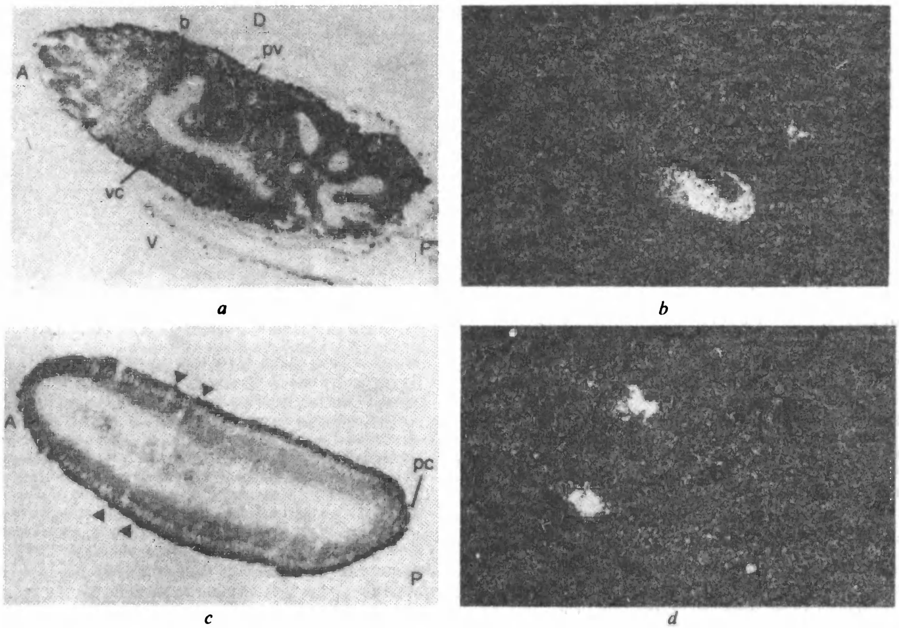


Fig. 5 Localization of transcripts homologous to p93 (*a*, *b*) and p99 (*c*, *d*) in embryonic tissue sections. The embryonic tissue sections were prepared and hybridized to ^{3}H-labelled probes as described by Hafen *et al.*[18]. Labelling of p93 is shown on a sagittal section of an 18–20-h embryo. Both brightfield (*a*) and darkfield (*b*) photographs of a 4-week autoradiographic exposure are shown. The p99 labelling is shown for an embryo 3 h old, just after the stage during which cell membranes envelop the nuclei at the periphery of the embryo. Both bright- and darkfield photomicrographs of a 4-week autoradiographic exposure are shown. The extent of hybridization of the brightfield view (*a*) is marked by arrowheads. A, anterior; P, posterior; vc, ventral nerve cord; b, brain; pv, proventriculus; pc, pole cells; D, dorsal; V, ventral.

with the same probes, and 24 were plaque-purified. DNA was extracted from each recombinant and digested with *Eco*RI, *Bam*HI and both enzymes together, then separated on an agarose gel, and transferred to nitrocellulose.

These blots were hybridized with probes 2 and 6 to determine which region of the insert contained the H repeat. In this way, we found that three of the clones were re-isolates of *ftz*, and two were re-isolates of *Antp*. The remaining 19 clones were divided into two classes on the basis of their extent of homology (as determined by signal intensity) with the H repeats from *Antp* and *ftz*. Seven clones cross-hybridized relatively strongly and by *inter se* hybridizations we found that these were derived from two different genomic regions. Representative clones from each of the two regions were designated 93 and 99.

The H repeat lies on a 4.5-kb *Bam*HI/*Eco*RI fragment for clone 93; this fragment was subcloned into plasmid vector pAT153 and is designated p93. The p93 insert is a single-copy sequence in the *Drosophila* genome, when tested by genomic Southern blot analysis in stringent hybridization and wash criteria, and derives from the 15-kb genomic *Eco*RI fragment previously identified by genomic blotting (Fig. 1, lane 1). The clone 99 H repeat lies on a 5-kb *Eco*RI fragment which was cloned into pAT153 and designated p99. The p99 insert is also a single-copy *Drosophila* sequence (in stringent conditions), and corresponds to the 5-kb *Eco*RI genomic fragment detected in Fig. 1, lane 1. Although p99 and p93 were selected by hybridization with the H repeat they also hybridize with the probe 3 shown in Fig. 1, and therefore probably contain the M repeat also.

In situ localization of p93 and p99

DNA from p93 and from p99 was labelled with biotinylated dUTP by nick-translation, and hybridized to squashes of salivary gland polytene chromosomes. The sites of hybridization were revealed by an immunoperoxidase detection protocol[21]. The stringency of hybridization was sufficient to allow only the detection of the genomic isologues of p93 and p99.

Clone p93 hybridizes to the 89E region on the right arm of chromosome 3 (Fig. 3), the cytogenetic location of the bithorax complex[1]. Bender *et al.*[15,22] have recently reported the isolation of the left half of the bithorax complex DNA, and Karch and Bender have subsequently isolated overlapping cloned genomic DNAs that include much of the right part of the complex (F. Karch and W. Bender, unpublished results); they provided us with a Southern blot containing cloned DNA from the entire cloned bithorax region. We find that the p93 insert is located within their cloned region, to the right of the *bithoraxoid*/*postbithorax* unit of the bithorax complex, near the *infra-abdominal-2* locus (data not shown). The same result has been obtained by S. Sakonju (personal communication) and M.S.L.

Clone p99 hybridizes to the 84A region on the right arm of chromosome 3; this is close to the cytogenetic locations of the *ftz* and *Antp* loci, both of which are in 84B1-2 (refs 2, 4, 5). The 84A region contains the Antp complex genes *proboscipedia*, *zerknüllt* and *Deformed*, all of which have been shown to affect the proper development of the posterior head segments of *Drosophila*[4,5,23].

Transcription

Genetic and developmental studies on the time of expression of homoeotic selector genes show that early to mid-embryogenesis (0–12 h after oviposition) and pre- and early metamorphosis (late third instar larval and early pupal stages) are possible periods of high levels of expression[24–26]. *Antp* transcripts exhibit their highest levels during both these periods, and *ftz* transcripts are most abundant in early embryogenesis (A.K., unpublished results).

To test whether the regions homologous to p93 and p99 were transcribed, the two clones were hybridized to Northern blots containing *Drosophila* poly(A)$^+$ RNA from successive stages of development. The blots were hybridized and washed using the stringent conditions described in Fig. 1 legend.

Clone 93 is homologous to multiple RNA species during embryonic stages, especially in embryos 6–12 h old (Fig. 4). The largest RNA (5.4 kb) homologous to p93 at the 6–12-h stage is also abundant in late third instar larvae, just before pupation. Clone p99 is homologous to an RNA species of 2.8 kb which is most abundant at early embryogenesis (0–6 h) and in the early pupal stage (Fig. 4). The RNA species homologous to p93 and p99 are both present at approximately the same levels as transcripts from the *Antp* locus (A.K., unpublished results).

Localization of transcripts

The most important experiment that we performed with the p93 and p99 clones was to test whether the transcripts homologous to these clones were spatially restricted during development. It has recently been shown that *Antp* and *Ubx* transcripts are restricted in a segmentally specific manner during embryonic development[16,18,19]. To a rough approximation, the embryonic segments and segmental anlagen that accumulate *Antp* and *Ubx* transcripts are those in which the function of these genes is believed to be required for proper development. Therefore, if the 93 and 99 clones represent other homoeotic selector loci, their transcripts should be restricted during embryonic stages to segments where 93 or 99 expression is required for proper development.

Figure 5 shows that the transcripts homologous to both p93 and p99 show spatial restrictions during development. We have shown only one developmental stage, when transcript localization is striking, for each clone. More detailed studies on the expression of these cloned regions will be reported elsewhere.

The localization of transcripts homologous to p93 is shown on a sagittal section of an 18–20-h embryo. At this advanced stage of embryogenesis, the central nervous system includes the two brain hemispheres and the condensed ventral nerve cord (Fig. 5). Before condensation the ventral cord consists of 12 paired ganglia, the sub-oesophageal ganglion, three thoracic ganglia and eight abdominal ganglia. Each ganglion of the ventral cord innervates its corresponding body segment[27]. The entire central nervous system of the embryo section hybridized with p93 appeared to be labelled above background levels, but a striking and reproducible concentration of label was observed over the posterior region of the ventral nerve cord, encompassing at least the posterior-most five or six abdominal neuromeres.

The labelling pattern of p99 is shown at the cellular blastoderm stage, ~3 h after oviposition. The cellular blastoderm consists of a monolayer of morphologically identical cells. It is at this stage that cells first become restricted in their developmental potential, with different regions of the blastoderm acquiring separate determinative fates[28,29]. Transcripts homologous to p99 were found to be concentrated in cells in about 60–65% of the embryo length from the posterior pole. Cell ablation experiments in this region of the cellular blastoderm result in embryos having defects in the first thoracic and posterior head segments[30].

Conclusions

Our analyses of the 93 and 99 clones, both isolated with the H repeat cross-homology, strongly suggest that they represent other homoeotic loci of *Drosophila*. Both clones fulfilled all three criteria that we applied for representing clones from homoeotic loci. First, both hybridize to cytogenetic locations of previously characterized homoeotic genes; 93 to the right half of the bithorax complex in the chromosome region 89E, and 99 to chromosome region 84A, which contains genes in the proximal half of the Antp complex. Second, both 93 and 99 are homologous to transcripts that are relatively abundant during embryogenesis and just prior to metamorphosis. These are the periods when transcripts homologous to the homoeotic locus *Antp* are most abundant (A.K., unpublished results). Third, and most importantly, the transcripts homologous to 93 and 99 show a striking spatial restriction during development. Transcripts homologous to p93 are most abundant in the posterior abdominal neuromeres of the embryo, as would be expected from a gene in the right half of the bithorax complex. The transcripts homologous to p99 are most abundant in a region of the cellular blastoderm that corresponds to the segmental anlagen of the posterior head or first thoracic segments. This is also consistent with its cytogenetic location in 84A, which contains genes that affect the development of these segments.

The basis for the cross-homology is of great interest. The position of the H repeat in the 3′ region of the transcription units of *Antp*, *Ubx* and *ftz* is consistent with a conserved protein-coding sequence. The DNA sequence of the H repeats of *Antp*, *ftz* and *Ubx* leaves no doubt that the sequence conservation is due to a conserved protein-coding domain (W.McG. and R. Garber, unpublished results). Since faithful copies of the H repeat are strictly delimited and found only in homoeotic genes, we now call the H repeat the 'homoeotic sequence'. However, it seems clear that not all homoeotic genes carry the homoeobox, for example, we have been unable to detect it in the *bithoraxoid/postbithorax* unit of the bithorax complex (W.McG., F. Karch and W. Bender, unpublished results). It is possible, of course, that another subset of homoeotic genes contains another repeat.

On the basis of these results, we propose that a subset of the homoeotic genes are members of a multigene family, highly diverged but nonetheless detectable by DNA cross-homology. This suggests a common evolutionary origin for some genes of both the Antp and bithorax complexes, as proposed by Lewis[1] for the genes of the bithorax complex. The conspicuous evolutionary conservation of the homoeobox sequence in some homoeotic genes of *Drosophila* suggests that it might also be conserved in other animal species; preliminary experiments strongly support this view (W.McG., unpublished results). It is possible that a fundamental principle in development is to duplicate a gene specifying a segment identity, allowing one of the copies to diverge and acquire new functions, or new spatial restrictions in expression, or both; this might allow, within the limits of natural selection, a striking polymorphism in the different segments of an animal, and the acquisition of highly specialized functions in different segments.

We thank Nadine McGinnis for experimental assistance; Rick Garber for helpful comments during the early phases of this work; Pierre Spierer for the gifts of cloned DNA; and Welcome Bender and Francois Karch for the bithorax complex blot. We also thank Erika Wenger-Marquardt for preparation of the manuscript. M.S.L. and W.McG. were supported by Jane Coffin Child fellowships. The work was made possible by a grant from the Swiss NSF and the Kanton Basel-Stadt.

Received 12 January; accepted 5 March 1984.

1. Lewis, E. B. *Nature* **276**, 565–570 (1978).
2. Kaufman, T. C., Lewis, R. & Wakimoto, B. *Genetics* **94**, 115–133 (1980).
3. Lewis, R. A., Wakimoto, B. T., Denell, R. E. & Kaufman, T. C. *Genetics* **95**, 383–397 (1980).
4. Wakimoto, B. T. & Kaufman, T. C. *Devl Biol.* **81**, 51–64 (1981).
5. Hazelrigg, T. & Kaufman, T. C. *Genetics* **105**, 581–600 (1983).
6. Le Calvez, J. *Bull. biol. Fr. Belg.* **82**, 97–113 (1948).
7. Hannah, A. & Strömnaes, O. *Drosoph. Inf. Serv.* **29**, 121–123 (1955).
8. Garcia-Bellido, A. *Am. Zool.* **17**, 613–629 (1977).
9. Duncan, I. & Lewis, E. B. *Developmental Order: Its Origin and Regulation*, 533–554 (Alan R. Liss, New York, 1982).
10. Struhl, G. *Proc. natn. Acad. Sci. U.S.A.* **79**, 7380–7384 (1982).
11. Struhl, G. *J. Embryol. exp. Morph.* **76**, 297–331 (1983).
12. Hafen, E., Levine, M. & Gehring, W. J. *Nature* **307**, 287–289 (1984).
13. Garber, R. L., Kuroiwa, A. & Gehring, W. J. *EMBO J.* **2**, 2027–2036 (1983).
14. Nüsslein-Volhard, C., Wieschaus, E. & Jurgens, G. *Verh. dt. zool. Ges.*, 91–104 (1982).
15. Bender, W. *et al. Science* **221**, 23–29 (1983).
16. Akam, M. *EMBO J.* **2**, 2075–2084 (1983).
17. Scott, M. P. *et al. Cell* (in the press).
18. Hafen, E., Levine, M., Garber, R. L. & Gehring, W. J. *EMBO J.* **2**, 617–623 (1983).
19. Levine, M., Hafen, E., Garber, R. L. & Gehring, W. J. *EMBO J.* **2**, 2037–2046 (1983).
20. Maniatis, T. *et al. Cell* **15**, 687–701 (1978).
21. Langer-Safer, P. R., Levine, M. & Ward, D. C. *Proc. natn. Acad. Sci. U.S.A.* **79**, 4381–4385 (1982).
22. Bender, W., Spierer, P. & Hogness, D. S. *J. molec. Biol.* **168**, 17–33 (1983).
23. Kaufman, T. C. *Genetics* **90**, 579–596 (1978).
24. Morata, G. & Garcia-Bellido, A. *Wilhelm Roux's Arch. dev. Biol.* **179**, 125–143 (1976).
25. Sanchez-Herrero, E. & Morata, G. *J. Embryol. exp. Morph.* **76**, 251–264 (1983).
26. Morata, G. & Kerridge, S. *Nature* **290**, 778–781 (1981).
27. Poulson, D. F. *Biology of Drosophila*, 168–274 (ed. Demereo, M.) (Wiley, New York, 1950).
28. Chan, L. N. & Gehring, W. J. *Proc. natn. Acad. Sci. U.S.A.* **68**, 2217–2221 (1971).
29. Wieschaus, E. & Gehring, W. J. *Devl Biol.* **50**, 249–263 (1976).
30. Underwood, E. M., Turner, F. R. & Mahowald, A. P. *Devl Biol.* **74**, 286–301 (1980).
31. Southern, E. *J. molec. Biol.* **98**, 503–517 (1975).
32. Goldberg, D. A. *Proc. natn. Acad. Sci. U.S.A.* **77**, 5794–5799 (1980).

The complete DNA sequence of yeast chromosome III

S. G. Oliver[1], Q. J. M. van der Aart[2], M. L. Agostoni-Carbone[3], M. Aigle[4], L. Alberghina[5], D. Alexandraki[6], G. Antoine[7], R. Anwar[1], J. P. G. Ballesta[8], P. Benit[7], G. Berben[9], E. Bergantino[10], N. Biteau[4], P. A. Bolle[9], M. Bolotin-Fukuhara[11], A. Brown[1], A. J. P. Brown[12], J. M. Buhler[13], C. Carcano[3], G. Carignani[10], H. Cederberg[14], R. Chanet[7], R. Contreras[15], M. Crouzet[4], B. Daignan-Fornier[11], E. Defoor[16], M. Delgado[17], J. Demolder[15], C. Doira[11], E. Dubois[18], B. Dujon[19], A. Dusterhoft[20], D. Erdmann[20], M. Esteban[17], F. Fabre[7], C. Fairhead[19], G. Faye[7], H. Feldmann[21], W. Fiers[15], M. C. Francingues-Gaillard[11], L. Franco[8], L. Frontali[22], H. Fukuhara[7], L. J. Fuller[23], P. Galland[6], M. E. Gent[1], D. Gigot[18], V. Gilliquet[9], N. Glansdorff[18], A. Goffeau[24,37], M. Grenson[25], P. Grisanti[22], L. A. Grivell[26], M. de Haan[26], M. Hasemann[27], D. Hatat[28], J. Hoenicka[8], J. Hegemann[20], C. J. Herbert[29], F. Hilger[9], S. Hohmann[14], C. P. Hollenberg[30], K. Huse[14], F. Iborra[11], K. J. Indge[1], K. Isono[31], C. Jacq[28], M. Jacquet[11], C. M. James[1], J. C. Jauniaux[25], Y. Jia[29], A. Jimenez[8], A. Kelly[32], U. Kleinhans[30], P. Kreisl[27], G. Lanfranchi[10], C. Lewis[23], C. G. van der Linden[33], G. Lucchini[3], K. Lutzenkirchen[30], M. J. Maat[26], L. Mallet[11], G. Mannhaupt[21], E. Martegani[5], A. Mathieu[7], C. T. C. Maurer[33], D. McConnell[32], R. A. McKee[23], F. Messenguy[18], H. W. Mewes[27], F. Molemans[15], M. A. Montague[32], M. Muzi Falconi[3], L. Navas[17], C. S. Newlon[34], D. Noone[32], C. Pallier[11], L. Panzeri[3], B. M. Pearson[23], J. Perea[28], P. Philippsen[20], A. Pierard[18], R. J. Planta[33], P. Plevani[3], B. Poetsch[27], F. Pohl[35], B. Purnelle[24], M. Ramezani Rad[30], S. W. Rasmussen[36], A. Raynal[11], M. Remacha[8], P. Richterich[35], A. B. Roberts[12], F. Rodriguez[5], E. Sanz[8], I. Schaaff-Gerstenschlager[14], B. Scherens[18], B. Schweitzer[20], Y. Shu[28], J. Skala[24], P. P. Slonimski[29], F. Sor[7], C. Soustelle[11], R. Spiegelberg[20], L. I. Stateva[1], H. Y. Steensma[2], S. Steiner[20], A. Thierry[19], G. Thireos[6], M. Tzermia[6], L. A. Urrestarazu[25], G. Valle[10], I. Vetter[21], J. C. van Vliet-Reedijk[33], M. Voet[16], G. Volckaert[16], P. Vreken[33], H. Wang[32], J. R. Warmington[1], D. von Wettstein[36], B. L. Wickstead[12], C. Wilson[22], H. Wurst[35], G. Xu[30], A. Yoshikawa[31], F. K. Zimmermann[14] & J. G. Sgouros[27]

The entire DNA sequence of chromosome III of the yeast *Saccharomyces cerevisiae* has been determined. This is the first complete sequence analysis of an entire chromosome from any organism. The 315-kilobase sequence reveals 182 open reading frames for proteins longer than 100 amino acids, of which 37 correspond to known genes and 29 more show some similarity to sequences in databases. Of 55 new open reading frames analysed by gene disruption, three are essential genes; of 42 non-essential genes that were tested, 14 show some discernible effect on phenotype and the remaining 28 have no overt function.

THE sequence of the DNA molecule of chromosome III from the budding yeast *S. cerevisiae* has been completed by a consortium of 35 European laboratories within the framework of the European Community's Biotechnology Action Programme. The sequence is 315 kilobases long and contains 182 open reading-frames (ORFs) encoding putative proteins of $\geq$100 amino acids. So far, 55 novel ORFs have been subjected to functional analysis by gene disruption. In addition to the putative protein-encoding genes there are 10 transfer RNA genes, of which four had previously been defined by suppressor mutations. Regions of sequence variation between chromosomes III of different strains of *S. cerevisiae* have been identified, as have the differences between the physical and genetic maps. These data indicate that systematic genome sequencing projects can reveal new functions that have been missed by more traditional approaches and also illuminate the mechanisms of genome evolution. Most importantly, they reveal that our knowledge of molecular genetics is far from complete and that we are ignorant about the biological function of the majority of genes in a eukaryotic genome as small and well-defined as that of yeast.

The bakers' yeast *S. cerevisiae* is one of the most important experimental organisms for studying eukaryotic molecular genetics[1]. This yeast has a very small nuclear genome which, at about 14 megabases (Mb), is less than four times the size of that of the bacterium *Escherichia coli*. Like all eukaryotes, yeast divides its nuclear genome between a number of linear chromosomes. The 16 yeast chromosomes have been defined by both genetic[2] and physical[3] analyses. Each contains a single duplex DNA molecule[4] whose average size is similar to that of the genome of a T-even bacteriophage and thus represents an achievable objective for nucleotide sequencing with current technology[5]. Such a sequence analysis would not only advance yeast molecular genetics but also provide information important to our understanding of the genomes of higher eukaryotes. The pattern of gene expression in yeast is essentially similar to that in higher organisms[1]. Moreover, a number of yeast genes have been identified that share both structural and functional homology with genes of multicellular eukaryotes.

Despite their small size, yeast chromosomes resemble their counterparts from higher organisms in structure and in their mechanisms of replication, recombination and segregation[6]. The ease with which the yeast genome can be manipulated by both classical and recombinant DNA techniques has enabled all the functional chromosome components to be isolated: centromeres[7], telomeres[8] and replication origins[9]. These advances in our knowledge of chromosome structure and function, gained by the use of gene cloning techniques, have been paralleled by the study of chromosome recombination using a combination of classical and molecular approaches[10]. The availability of the complete nucleotide sequence of a yeast chromosome permits

[1]Manchester Biotechnology Centre, UMIST, Manchester M60 1QD, UK; [2]Department of Cell Biology Genetics, Leiden University, The Netherlands; [3]Dipartimento di Genetica e di Biologia dei Microrganismi, Universita di Milano, Italy; [4]LBMS, F-33000 Bordeaux, France; [5]Dipartimento di Fisiologia e Biochimica, Universita di Milano, Italy; [6]Instituto Molecular Biology and Biotechnology, PO Box 1527, GR-71110 Heraklio, Crete; [7]Institut Curie, Centre Universitaire, F-91405 Orsay, France; [8]Centro de Biologia Molecular, E-28049 Madrid, Spain; [9]Faculté des Sciences Agronomiques, B-5030 Gembloux, Belgium; [10]Dipartimento di Chimica Biologica, I-35100 Padova, Italy; [11]Institut de Génétique et Microbiologie, Université de Paris-Sud, 91405 Orsay, France; [12]Department of Molecular and Cell Biology, University of Aberdeen, UK; [13]Service de Biochimie CEN Saclay, F-91191, France; [14]Institut für Mikrobiologie, D-6100 Darmstadt, Germany; [15]Lab. voor Moleculaire Biologie, B-9000 Gent, Belgium; [16]Katholieke Universiteit Lab. voor Gentechnologie, B-3001 Leuven, Belgium; [17]La Cruz del Campo S.A., PO Box 53, E-41080 Sevilla, Spain; [18]Research Institute of CERIA-COOVI, B-1070 Bruxelles, Belgium; [19]Institut Pasteur, F-75724 Paris Cedex 15, France; [20]Institut für Mikrobiologie und Molekularbiologie der Universität, 6300 Giessen, Germany; [21]Institut für Physiologische Chemie, Universität München, 8000 München, Germany; [22]University of Rome, Department of Cell and Developmental Biology, I-00185 Roma, Italy; [23]AFRC Institute of Food Research, Norwich Research Park, Colney NR4 7UA, UK; [24]Unité de Biochimie Physiologique, Université de Louvain, B-1348 Louvain-la-Neuve, Belgium; [25]Université Libre de Bruxelles, Lab. Cell Physiology and Yeast Genetics, B-1050 Bruxelles, Belgium; [26]University of Amsterdam, Section for Molecular Biology, NL-1098 SM Amsterdam, The Netherlands; [27]Martinsried Inst. for Protein Sequences, D-8033 Martinsried, Germany; [28]Génétique Moléculaire, École Normale Superieure, F-75005 Paris, France; [29]Centre de Génétique Moleculaire du CNRS, F-91190 Gif-sur-Yvette, France; [30]Institut für Mikrobiologie der Universität Dusseldorf, D-4000 Dusseldorf 1, Germany; [31]Department of Biology, Kobe University, Kobe 657, Japan; [32]Trinity College, Department of Genetics, Dublin 2, Ireland; [33]Department of Biochemistry and Molecular Biology, Vrije Universiteit, NL-1081 HV Amsterdam, The Netherlands; [34]Department of Microbiology and Molecular Genetics, New Jersey Medical School, NJ07103, USA; [35]Fakultät für Biologie der Universität, D-7750 Konstanz, Germany; [36]Carlsberg Lab., DK-2500 Copenhagen Valby, Denmark; [37]Commission of the European Communities, B-1049 Brussels, Belgium

a detailed comparison of genetic and physical maps and so helps to define local variations in the frequency of recombination[11]. *S. cerevisiae* also contains a number of transposons (called Ty elements) which show similarities to retroviruses and other mobile genetic elements found in multicellular eukaryotes[12] and which are a major source of chromosome polymorphisms.

All of this suggests that sequencing the yeast genome should provide a useful paradigm to guide our interpretation of the information gained from the sequences of larger genomes when these become available. Indeed, at 14 Mb (compared with 3,000 Mb for the human genome), the 16 chromosomes of yeast might be regarded as comprising a basic gene-set required for the maintenance of a eukaryotic mode of cell organization and propagation. There are also practical reasons why the determination of the yeast genome sequence should be a useful precursor to larger enterprises such as sequencing the genomes of higher plants or humans. First, the functional analysis of novel genes discovered from the sequence is facilitated by the easy methods for gene disruption and replacement[13] which are available in yeast. Second, high-capacity cloning vectors (yeast artificial chromosomes, or YACs[14]), propagated in yeast, are important for creating clone banks of human or plant genes. Thus, it is useful to establish in the first instance the genome sequence of the vehicle organism. Finally, sequencing the yeast genome can act as a pilot scheme for assessing new techniques designed to speed sequence determination and analysis[15]. The yeast chromosome III sequence reported here has been determined mainly by conventional techniques. In fact, only 25 kb of the 385-kb sequence from which the final 315-kb consensus was derived has been obtained using automated techniques (7%). The following approaches were used to divide the primary clones (indicated by upper bars in Fig. 1) into fragments of a size suitable for sequencing: directed subcloning (19 kb; 5%), shotgun cloning (82 kb; 21%), chromosome walking with synthetic oligonucleotides (137 kb; 36%), and nested deletions (147 kb; 38%).

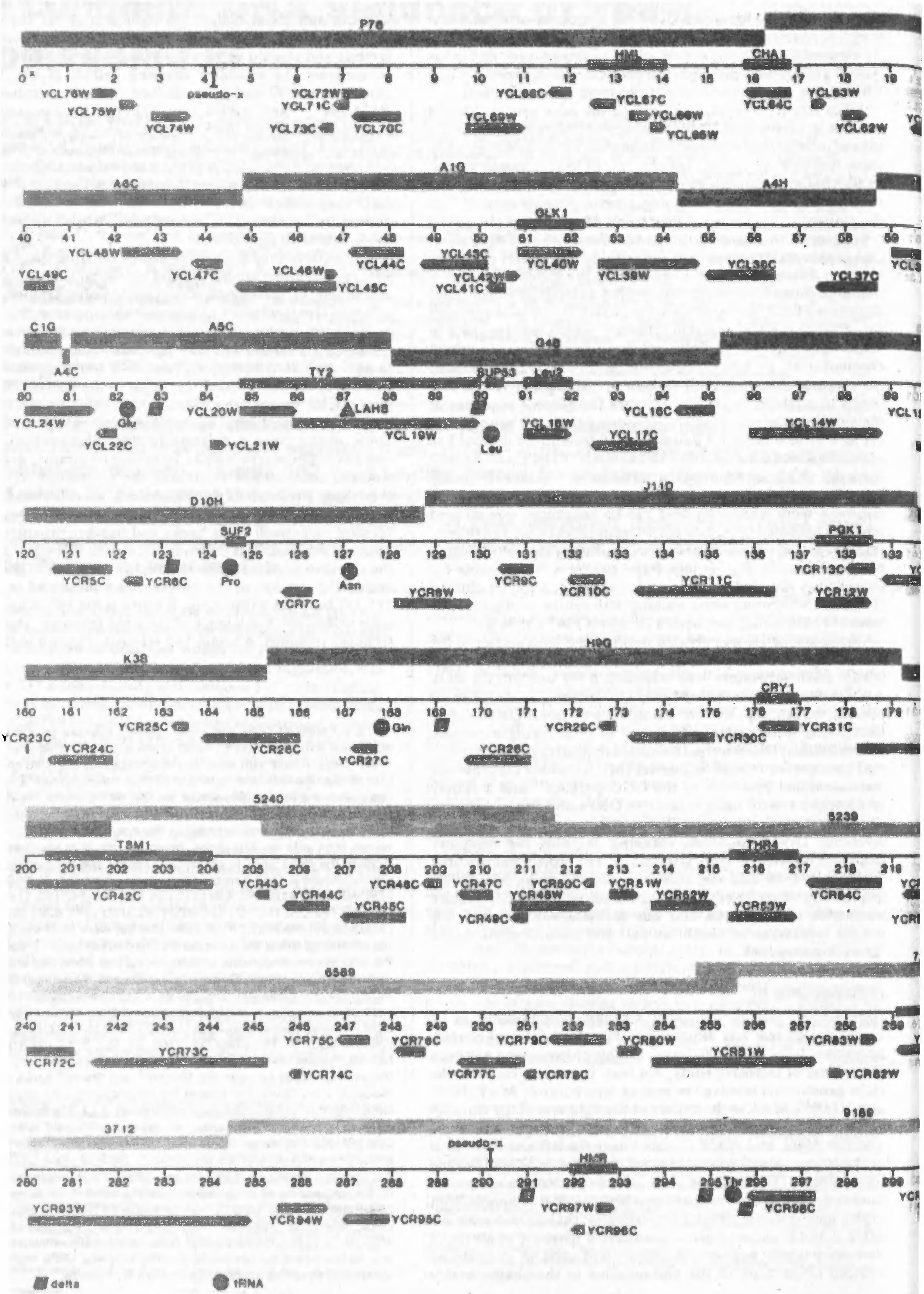
Chromosome III was divided between the laboratories of the Consortium and accredited clones were distributed to each group. Each laboratory was responsible for one or two work units (1 work unit is 8 kb of primary sequencing and 3 kb of overlap sequencing) and for disruptions of novel ORFs (some disruptions were performed instead of re-checking overlaps). The individual laboratories forwarded their data to the Martinsried Institute for Protein Sequences (MIPS), where the sequence was assembled by means of the GCG package[16] and a variety of algorithms were used to analyse ORFs and other sequence elements. MIPS also assembled a database for all extant *S. cerevisiae* DNA sequences, scanning not only the computer databases but also all published results. The project was initiated in January 1989 and the sequence completed by May 1991. Sequencing of entire eukaryotic genomes will probably require worldwide collaboration and our chromosome III sequence project represents an organizational and technological model for such enterprises.

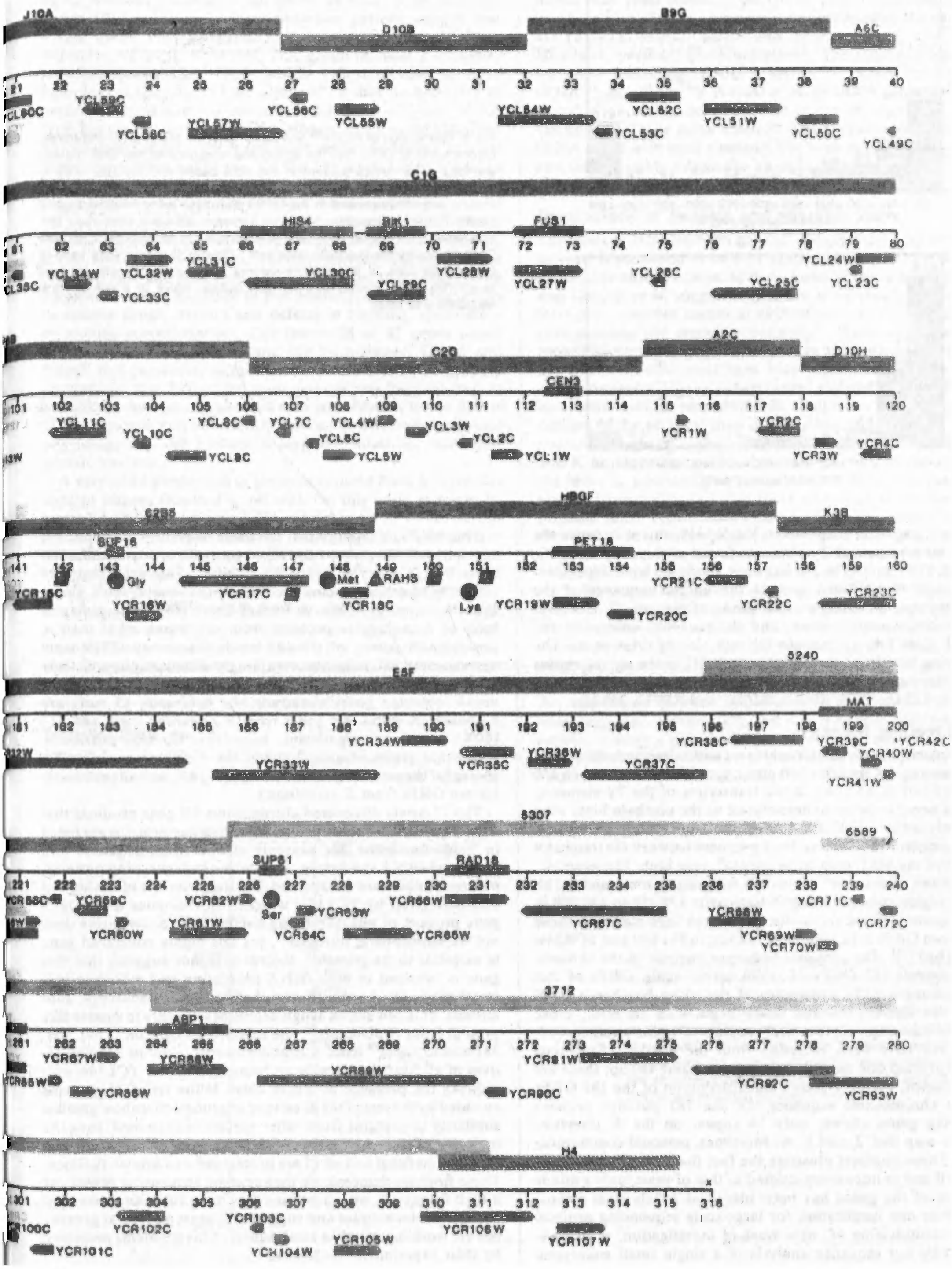
Chromosome III

Chromosome III is the third smallest chromosome in *S. cerevisiae*; its size was estimated from pulsed-field gel electrophoresis studies to be 300–360 kb[17]. This chromosome has been the subject of intensive study, not least because it contains the three genetic loci involved in mating-type control: *MAT*, *HML* and *HMR*[18]. *MAT*, in the middle of the right arm of the chromosome, is the expression locus that determines mating-type, whereas *HML* and *HMR* (located near the left and right ends of the chromosome) represent silent repositories of mating-type information. These three loci share nucleotide sequence homology, which means that intrachromosomal recombination events may generate circular derivatives of the chromosome. An *HML* × *MAT* recombination produces a 63-μm ('small ring') derivative which has been isolated[18] and used to generate an ordered clone bank of the chromosome in the shuttle vector YIp5 (ref. 19). This bank, necessarily, lacks a small part of the left arm and about half of the right arm of the chromosome; the latter was available from a series of clones constructed in lambda and cosmid vectors[20]. These two gene banks were used to generate the sequence covering 280 kb of the 315 kb of chromosome III and were derived from *S. cerevisiae* strains XJ24-24a[18,19] and AB972 (ref. 21). For the remaining 35 kb, clones derived from strains A364A[19] and DC5 (refs 22 and 23) were used. These strains are closely related (XJ24-24a, A364A) or isogenic (AB972, DC5) to the standard yeast laboratory strain S288C (ref. 24). The data are summarized in this article and a full listing of the sequence is available in the EMBL Data Library (accession number X59720) and will also be the subject of a more extensive publication.

A comparison of the chromosomes III from these strains, as well as from other laboratory and industrial isolates, permits an assessment of the degree of sequence polymorphism between different representatives of the same chromosome. Ty insertions in the chromosome are mainly confined to two regions that have been dubbed the left-arm and right-arm transposition hot spots (LAHS and RAHS, respectively[25-28]). Many strains (but not XJ24-24a) contain another Ty, distal to the RAHS, close to the gene *CRY1*. Interactions between this distal Ty and the RAHS seem to be a major cause of chromosome length polymorphisms. Some strains have a deletion in this region which causes a number of phenotypic effects, including respiratory deficiency[23], whereas other strains (including the progenitor S288C) have this region duplicated (ref. 19, and B.L.W. *et al.*, manuscript in preparation). We have sequenced a large overlap between the Newlon and Olson clone banks and this has provided detailed information about this polymorphic region as well as permitting the elucidation of the authentic unique sequence of the chromosome. The version of the chromosome presented in Fig. 1 is 315,357 base pairs (bp) long; it starts at the left telomere[29] and ends within the X sequence of the right telomere, about 400 bp from the terminus. A single Ty*2* element is shown in the LAHS,

FIG. 1 Location of ORFs and known genetic markers on chromosome III. ▶ Royal blue arrow-bars: ORFs (≥100 amino acids) are designated as follows: Y (for yeast) C (the third letter of the alphabet for the third chromosome); L or R (for the left and right arm of the chromosome); a number that designates the relative position of the ORF on its arm of the chromosome (the most centromere-proximal ORF being given the number 1); w or c (for Watson or Crick strand) to indicate the orientation of the ORF. All ORFs shown start with an ATG codon. Number scale is in kb. Upper blue bars indicate the clone used to obtain the primary sequence of that region of the chromosome. Dark turquoise bars: clones J10A [36, 6], D10B [16], B9G [23], A6C [25], A1G [18], A4H [17], C1G [30, 20], A4C [20, 1] A5C (1), G4B [1], D8B [1], C2G [1], A2C [2], D10H [4], J11D [24], 62B5-2D [28], HBGF [21], K3B [9], H9G [35, 14] and E5F [29, 19] were from the YIp5 bank of the small ring form of the chromosome (numbers in square brackets indicate the laboratories responsible for sequencing that clone, see addresses); all, except 62B5-2D (from DC021/DC022#62) and HBGF (from CN31c), were from strain XJ24-24a (ref. 19). The *Bam*HI junctions between these clones were checked using a λCh4A bank derived from strain A364A (ref. 19). Light turquoise bars: clones 7121 [12, 32], 3270 [5, 3], 5240 [11, 13], 5239 [26, 33], 5307 [7, 15], 6589 [8], 7260 [21, 20], 3712 [22, 10] and 9189 [33, 26, 7] were from strain AB972 (ref. 20). Clones 7121 and 3270 are in the overlap region between the Newlon[19] and Olson[20] banks and are not indicated in the figure. The ends of the chromosome were sequenced using clones (dark turquoise, cross-hatched) p78 [36, 2, 11, 19] derived from strain A364A (ref. 19) and (light turquoise, cross-hatched) H4 [25] from strain DC5 (ref. 22). The Sanger dideoxy-sequencing technique[49] was used for the entire chromosome with the exception of parts of clone D10B, for which the chemical procedure of Maxam and Gilbert[50] was employed. Full details of the sequencing strategy will be published (S.G.O. *et al.*, manuscript in preparation) and the full 315, 356-bp sequence has been deposited in the EMBL Nucleotide Sequence Data Library under the accession number X59720. Two ORFs from the *pet18* complex are <100 amino acids in length and so are not shown in the figure. The following ORFs represent newly discovered essential genes: YCL17c, YCL14c, YCR69w.


P76
HML
CHA1
YCL76W
YCL75W
YCL74W
pseudo-x
YCL72W
YCL71C
YCL73C
YCL70C
YCL69W
YCL68C
YCL67C
YCL66W
YCL65W
YCL64C
YCL63W
YCL62W
A6C
A1G
A4H
GLK1
YCL48W
YCL49C
YCL47C
YCL46W
YCL45C
YCL44C
YCL43C
YCL42W
YCL41C
YCL40W
YCL39W
YCL38C
YCL37C
C1G
A5C
A4C
G4B
TY2
SUP53
Leu2
YCL24W
Glu
YCL22C
YCL20W
YCL21W
LAH8
YCL19W
Leu
YCL18W
YCL17C
YCL16C
YCL14W
D10H
J11D
SUF2
PGK1
YCR5C
YCR6C
Pro
Asn
YCR7C
YCR8W
YCR9C
YCR10C
YCR11C
YCR12W
YCR13C
K38
H9G
CRY1
YCR23C
YCR24C
YCR25C
YCR26C
YCR27C
Gln
YCR28C
YCR29C
YCR30C
YCR31C
5240
5239
TSM1
THR4
YCR42C
YCR43C
YCR44C
YCR45C
YCR46C
YCR47C
YCR48W
YCR49C
YCR50C
YCR51W
YCR52W
YCR53W
YCR54C
6589
YCR72C
YCR73C
YCR74C
YCR75C
YCR76C
YCR77C
YCR78C
YCR79C
YCR80W
YCR81W
YCR82W
YCR83W
3712
9189
pseudo-x
HMR
Thr
YCR93W
YCR94W
YCR95C
YCR96C
YCR97W
YCR98C
delta
tRNA


J10A
D10B
B9G
A6C
21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40
YCL60C
YCL59C
YCL58C
YCL57W
YCL56C
YCL55W
YCL54W
YCL53C
YCL52C
YCL51W
YCL50C
YCL49C
C1G
HIS4
BIK1
FUS1
61 62 63 64 65 66 67 68 69 70 71 72 73 74 75 76 77 78 79 80
YCL35C
YCL34W
YCL33C
YCL32W
YCL31C
YCL30C
YCL29C
YCL28W
YCL27W
YCL26C
YCL25C
YCL24W
YCL23C
C2G
A2C
D10H
CEN3
101 102 103 104 105 106 107 108 109 110 111 112 113 114 115 116 117 118 119 120
YCL11C
YCL10C
YCL9C
YCL8C
YCL7C
YCL6C
YCL5W
YCL4W
YCL3W
YCL2C
YCL1W
YCR1W
YCR2C
YCR3W
YCR4C
H9GF
62B5
K3B
SUF16
PET18
141 142 143 144 145 146 147 148 149 150 151 152 153 154 155 156 157 158 159 160
Gly
Met
RAHS
Lys
YCR15C
YCR16W
YCR17C
YCR18C
YCR19W
YCR20C
YCR21C
YCR22C
YCR23C
E5F
5240
MAT
181 182 183 184 185 186 187 188 189 190 191 192 193 194 195 196 197 198 199 200
YCR34W
YCR33W
YCR35C
YCR36W
YCR37C
YCR38C
YCR39C
YCR40W
YCR41W
YCR42C
6307
6589
SUP61
RAD18
221 222 223 224 225 226 227 228 229 230 231 232 233 234 235 236 237 238 239 240
Ser
YCR58C
YCR59C
YCR60W
YCR61W
YCR62W
YCR63W
YCR64C
YCR65W
YCR66W
YCR67C
YCR68W
YCR69W
YCR70W
YCR71C
YCR72C
3712
ABP1
261 262 263 264 265 266 267 268 269 270 271 272 273 274 275 276 277 278 279 280
YCR87W
YCR88W
YCR86W
YCR85W
YCR89W
YCR90C
YCR91W
YCR92C
YCR93W
H4
301 302 303 304 305 306 307 308 309 310 311 312 313 314 315 316
YCR100C
YCR101C
YCR102C
YCR103C
YCR104W
YCR105W
YCR106W
YCR107W

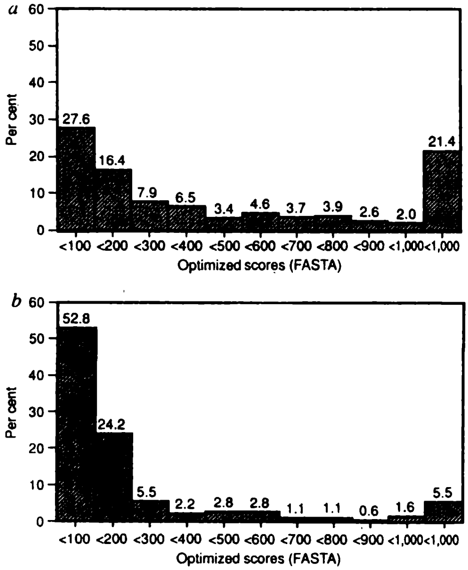


FIG. 2 Distribution of FASTA[51] scores for all yeast proteins (*a*) and for the chromosome III ORFs (*b*). The amino-acid sequence of the 968 protein-encoding yeast genes present in the data bases and the 182 ORFs in chromosome III were subjected to a similarity search against all known protein sequences present in the MIPSX data base using the Pearson and Lipman[51] FASTA algorithm. (MIPSX is a merged database comprising PIR-International, SwissProt and automatic translations of both the EMBL and GenBank nucleotide-sequence data sets.) Optimized scores were used to compile the data. In this analysis, scores due to matches with identical sequences were omitted and the next highest score to a non-identical sequence was taken.

but no transposon is shown in the RAHS; this is because the fused tandem pair of Ty elements found at this site in S288C, AB972, DC5 and XJ24-24a has been produced by a recombination event that deleted part of the unique sequence of the chromosome, including an ORF and a tRNA gene[27]. The form of the chromosome shown, and the sequence entered in the EMBL Data Library, permits the inclusion of these genes. The following lengths of the chromosomes III in the source strains have been calculated from the sequence data: XJ24-24a, 324 kb; A364A, 322 kb; DC5, 336 kb; S288C and AB972, 345 kb.

Open reading frames

The transcript analysis of Yoshikawa and Isono[22] predicted that chromosome III specifies 160 messenger RNA molecules (≥350 nucleotides) in addition to the transcripts of the Ty elements. This is likely to be an underestimate as the northern blots were run only on poly(A)$^+$ RNA extracted from cells growing in a rich medium. Nonetheless, the agreement between the transcript map and the ORF map is, in general, very high. For example, Yoshikawa and Isono[22] show two transcripts of 6.5 and 4.2 kb in the region corresponding to base pairs 178,250 to 188,900 in the sequence. These transcript sizes agree very well with those of the two ORFs in this region, YCR32w (6,501 bp) and YCR33w (3,678 bp)[30,31]. The complete sequence analysis of the chromosome reveals 182 ORFs of ≥100 amino acids. ORFs of this length have <0.2% probability of occurring by chance in *S. cerevisiae* DNA[32]. All 182 ORFs begin with an ATG; those ORFs whose sequence is entirely contained within another reading-frame have been excluded from the analysis. Seventeen ORFs overlap one another by between 7 and 449 bp; these are all included. Figure 1 shows the distribution of the 182 ORFs in the chromosome sequence. Of the 182 putative protein-encoding genes shown, only 34 appear on the *S. cerevisiae* genetic map (ref. 2, and R. K. Mortimer, personal communication). These numbers illustrate the fact that, even in a genome as small and as intensively studied as that of yeast, only a minor fraction of the genes has been identified by classical means. Therefore one justification for large-scale sequencing projects is the identification of new areas of investigation, as demonstrated by our sequence analysis of a single small eukaryotic chromosome.

The MIPS YEASTPROT database currently contains the sequence of 968 protein-encoding genes from *S. cerevisiae*. Of these 968, a total of 251 (26%) show amino-acid sequence similarity to other proteins encoded by the same species, giving FASTA scores of ≥200. A further 286 (30%) show a similar level of homology to proteins from organisms other than *S. cerevisiae*. A survey of the 145 newly discovered ORFs from chromosome III presents a strikingly different picture. Only ~10% (15/145) show significant similarity (FASTA≥200) to non-*S. cerevisiae* genes already in the databases, 15 more are homologous with other genes from *S. cerevisiae* itself, and 117 (80%) show no significant homology to any previously sequenced genes. (Figure 2 shows the distribution of FASTA scores for the set of 182 chromosome III ORFs and all previously known ORFs from *S. cerevisiae*.)

The 15 newly discovered chromosome III gene products that show significant similarity to non-*S. cerevisiae* proteins are listed in Table 1*a*. Some, for example alcohol dehydrogenase and asparagyl-tRNA synthetase, are routine housekeeping proteins, but many others are unexpected. For instance, one of the highest scores is given by YCL17c, which is homologous to the *nif*S gene product of nitrogen-fixing bacteria[33,34]. *S. cerevisiae* does not fix atmospheric nitrogen[35], yet this highly conserved gene is essential to its growth[36]. Recent evidence suggests that this gene is involved in both tRNA processing and mitochondrial metabolism (P. Leong-Morgenthaler *et al.*, manuscript submitted). YCL74w shows a high degree of similarity to a *copia*-like protein from *Arabidopsis*[37], the tobacco transposon (Tnt*1*; ref. 38) and to *copia*[39] itself. Chromosome III contains representatives of all four classes of yeast transposons[25-28]; YCL74w may indicate the presence of a fifth class. If the set of 15 proteins encoded by the rest of the *S. cerevisiae* genome that show greatest similarity to proteins from other species is examined, then the majority of matches are to proteins from other yeasts and filamentous fungi and all 15 are proteins of well-known function. These findings demonstrate that genome sequencing reveals, at a high frequency, new functions that have not been discovered by classical techniques and suggest that yeast molecular geneticists are working on only a small subset of the problems presented by their experimental organism.

The set of chromosome III ORFs that show significant similarity to previously characterized genes on other yeast chromosomes (Table 1*b*) reveals no particular pattern, except that kinase genes are overrepresented (5/15; YCL24w, YCR8w, YCR91w, YCR73c, YCR38c). This group includes similarities to some genes, like *YKR*, which were expected to have homologues elsewhere in the genome[40]. It may be necessary to perform several inactivations to reveal the role of genes of this type, but the task of assigning functions to novel genes identified by systematic sequencing is still more readily addressed in yeast than in any other eukaryote. Of the 145 novel ORFs found in the chromosome III sequences, 55 (38%) have been subjected to gene disruption[13]. For three genes this was a lethal event. Of the remainder, 42 disruptants have been tested for other effects on phenotype, using a standard battery of tests compiled by P.P.S., and in 14 cases some effect was observed. The phenotypes included heat and cold sensitivity, respiratory deficiency (or an enhanced level of mutation to that phenotype), hypersensitivity to various drugs, sterility and defects in budding, sporulation or meiotic recombination. This leaves 28 of 45 genes tested (62%) for which no phenotype can be assigned; Goebl and Petes[41] had previously suggested, based on a study of random disruptions, that 70% of the yeast genome was 'dispensable'. It is unlikely that these genes make no contribution to the fitness of the organism and this implies that our understanding of yeast physiology and cell biology is lagging behind our molecular genetic analysis.

A very small proportion of genes sequenced from *S. cerevisiae* contain introns (about 2%; ref. 42). On this basis, it might be expected that chromosome III would include three intron-containing genes. Apart from the mating-type loci[18], three have been detected (YCR31c and two others that are less than 100 amino acids long) by searching for the TACTAAC box and the 5′ and 3′ splice junctions[42]. One of these genes, *cry1* (YCR31c), was already known; it encodes the ribosomal protein rps59 (ref. 43). A large proportion of intron-containing genes in yeast encode ribosomal protein subunits[42], but neither the protein-encoding sequences nor the upstream regions of the two small intron-containing genes on the chromosome give any indication that they are ribosomal proteins as well.

Transfer RNA genes

Like most organisms, *S. cerevisiae* contains multiple gene copies encoding iso-accepting species of tRNA. Feldmann[44] has estimated that yeast contains 360 tRNA genes. Four such genes had been identified as suppressors and mapped to chromosome III (*SUP53*, *SUP61*, *SUF2* and *SUP16*; ref. 2, and R. K. Mortimer, personal communication). The sequence identifies six more (encoding $tRNA^{Asn}$, $tRNA^{Glu}$, $tRNA^{Gln}$, $tRNA^{Lys}$, $tRNA^{Met}$ and $tRNa^{Thr}$). A total of seven tRNA genes would be expected on a statistical basis; all of the 10 found are within 500 bp of a Ty or delta element. The frequent association of tRNA genes with such elements has been noted previously[45,46] and their possible role in the amplification and spread of such genes has been discussed[27].

Comparison of genetic and physical maps

The average ratio between genetic and physical map distances in yeast is estimated to be 0.34 centimorgans (cM) per kb (ref. 2). Smaller chromosomes, such as I and VI, have a higher ratio and Kaback *et al.* suggested that this is necessary to ensure at least one crossover occurs at each meiosis so that these small chromosomes can segregate correctly[47]. Within chromosomes, recombination frequency varies widely and both recombination hot spots and cold spots have been identified[11]. The sequence of chromosome III may suggest some molecular basis for these local variations in recombination frequency. A detailed comparison of the physical map of the chromosome (based on the sequence) and the current version of the genetic map (ref. 2; and R. K. Mortimer, personal communication) is unrealistic as the latter is, necessarily, a compromise between the results of several laboratories and also takes advantage of the emerging physical data. Nevertheless, some instructive generalizations may be made. The average ratio of genetic map distance to physical distance for the chromosome is 0.51 cM per kb; this is in line with the estimates for chromosomes I and VI (0.62 and 0.55 cM per kb, respectively) and supports Kaback's[47] postulate. The variation in the cM per kb ratio for different intervals along the chromosome is at least 10-fold; it is lowest close to the centromere and greatest midway down each arm. There is thus an approximate correlation between the pattern of genetic recombination and that of transcription along the chromosome[22]. An association between recombination and transcription has been suggested previously[48], and it may be that both processes require relatively naked DNA within the chromatin. A region where the frequency of recombination is particularly high is the interval between *MAT* and *thr4* (1.12–1.27 cM per kb); this

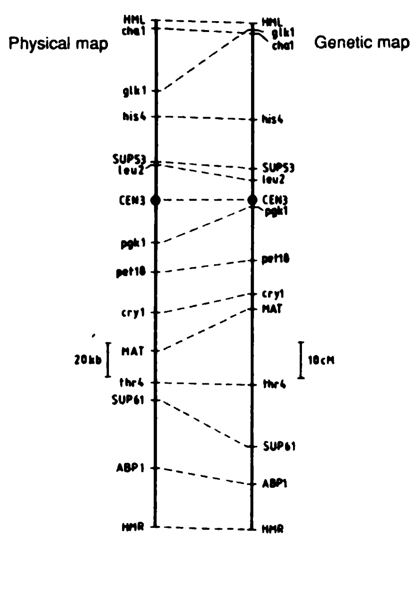


FIG. 3 Comparison of genetic and physical maps. The physical map has been derived from the sequence by assigning each locus to a point in the centre of the appropriate ORF, group of ORFs, or DNA element. Thus each locus, given as a point on this physical map, may represent a region of the chromosome from 0.1 kb to 6 kb in length. The genetic map is derived mainly from the analysis of meiotic recombination events[2]; fine structure mapping data (permitting estimation of genetic length) are available for only a few genes.

TABLE 1 Chromosome III gene products showing significant homology to extant protein sequences

ORF	Coordinates	Rational gene name	Similar sequence (database: accession no.)	FASTD score	Effect of gene disruption
			(a) Newly discovered ORFs showing similarity to non-*S. cerevisiae* proteins		
YCL57w	24,752-26,887		Rat soluble metalloendopeptidase (Genbank: M61142)	1,012	
YCR92c	278,640-275,500		Human DUC-1 protein upstream of DHFR (EMBL: J04810)	960	Non-lethal, no detectable phenotype*
YCL17c	93,881-92,391	*NFS1*	*Anabaena nifs* nitrogen-fixation protein (EMBL: J05111; M21840)	778	Lethal†
YCR2c	117,951-116,986		Mouse DIFF6 protein, regulated by gp90(MEL-14) (EMBL: X13303)	660	
YCR24c	161,932-160,457		*E. coli* asp-tRNA synthetase (EMBL: M33145)	550	
YCL74w	2,816-3,739		*Arabidopsis copia*-like protein (EMBL: M53975)	550	
YCR11c	136,474-133,328	*ADP1*	*Drosophila* white pigment protein (EMBL: X51749)	512	Non-lethal, no detectable phenotype‡
YCR63w	227,040-227,510		*Xenopus* G10 protein, developmentally regulated (EMBL: X15243)	480	Slow growth§
YCL43c	50,196-48,631	*PDI1*	Protein disulphide isomerase precursor (PIR: JX0182; EMBL: M62815, X52313)	2,501	Lethal‖
YCR105w	307,800-308,882		*Zymomonas* alcohol dehydrogenase I (EMBL: M32100)	366	
YCR27c	167,715-167,089		*Dictyostelium ras*-related protein (EMBL: X54291)	309	
YCR65w	228,031-229,626		*Drosophila fkh* homeotic gene (EMBL: J03177)	286	
YCR57c	221,442-220,126		Human transducin β-2 chain, GTP-binding (Gen Bank: M36429)	266	
YCL11c	102,964-101,684		*Xenopus* poly(A)-binding protein (PIR: S12000)	227	
YCR107w	312,620-313,708		Tobacco auxin-induced protein (EMBL: X56269)	218	
			(b) Newly discovered ORFs showing significant similarity to other *S. cerevisiae* proteins		
YCL25c	77,880-75,982		General amino-acid permease, GAP1 (EMBL: X52633)	1,527	
YCL64c	16,869-15,790		L-serine dehydratase, SDH1 (EMBL: X52657)	984	
YCL48w	42,140-43,528		Sporulation-specific SPS2 protein (EMBL: M13629)	941	
YCR67c	235,046-231,852		Sec12p membrane glycoprotein (Gen Bank: X13161)	889	
YCL24w	79,119-81,566		Sucrose non-fermenting SNF1 protein kinase (EMBL: M13971)	650	
YCR8w	128,072-129,880		Nitrogen permease reactivator, NPR1 (EMBL: X56084)	445	
YCR52w	213,725-215,173		Gene complementing petite type mutation (EMBL: X62430)	434	
YCR45c	208,342-206,870		Proteinase B precursor (EMBL: M18097)	429	
YCR91w	273,138-275,315		Protein kinase, YKR (EMBL: M24929)	397	
YCL35c	61,142-60,813		Glutaredoxin (PIR: A35492)	396	
YCR73c	245,320-241,379		Protein kinase, Ssp31 (PIR: JQ1118)	321	
YCR83w	258,312-258,692		Thioredoxin II (EMBL: M59169)	272	
YCR28c	171,115-169,580		Allantoate permease (EMBL: M24098)	255	
YCR89w	266,168-270,994		A-agglutinin core subunit, AGA1 (EMBL: M28164)	250	
YCR38c	197,967-196,354		Cell division cycle, CDC25 protein (EMBL: M15458)	201	

Full details of similarity search procedures are given in the legend to Fig. 2. For each ORF, the highest optimum FASTA score found is listed. References to the authors of the pre-existing sequence may be found in each database entry, the accession numbers for which are given in the figure. The term 'Yeast' here indicates the conspecific group, *S. cerevisiae*, *S. carlsbergensis* and *S. uvarum*. For the division between *S. cerevisiae* and non-*S. cerevisiae* matches (see text), first priority was given to similarities to other *S. cerevisiae* sequences; thus, if a match with a FASTA score ≥200 with another *S. cerevisiae* sequence was found, this was counted even if a match to a non-*S. cerevisiae* sequence produced a higher score.

TABLE 1—continued

ORF	Coordinates	Rational gene name	Similar sequence (database: accession no.)	FASTD score	Effect of gene disruption
			(*c*) Genes previously sequenced but not assigned to chromosome III		
YCR75c	247,550-246,771	*ERS1*	ER defect suppressor, ERS1 protein (EMBL: X52468)	1,455	
YCR5c	121,931-120,552	*CIT2*	Cytosolic citrate synthase (EMBL: Z11113)	2,241	
			(*d*) Genes previously sequenced and assigned to chromosome III but not mapped		
YCL50c	38,775-37,813	*DTP1*	Diadenosine tetraphosphate phosphorylase, DTP1 (EMBL: M35204)	1,602	
			(*e*) Genes previously mapped on chromosome III but not sequenced		
YCR53w	215,428-216,969	*THR4*	*Corynebacterium* threonine synthase (Gen Bank: X56037)	759	
YCR9c	131,144-130,350	*RVS161*	Reduced viability on starvation, RVS161 protein (PIR: Y01077)	1,280	
			(*f*) Genes previously sequenced and mapped on chromosome III		
YCL18w	90,935-92,026	*LEU2*	β-isopropyl-malate dehydrogenase (EMBL: X03840)	1,688	
YCL27w	71,768-73,303	*FUS1*	Cell fusion protein, Fus 1 (EMBL: M16717)	2,425	
YCL29c	68,568-69,867	*BIK1*	Nuclear fusion protein, Bik1 (EMBL: M16717)	1,304	
YCL30c	68,299-65,903	*HIS4*	Histidinol dehydrogenase (EMBL: V01310)	3,716	
YCL40w	50,810-52,309	*GLK1*	Glucokinase (EMBL: M24077)	2,422	
YCL66w	13,271-13,795	*HML*	Hmlα1 protein	896	
YCL67c	13,007-12,378		Hmlα2 protein (EMBL: V01315)	979	
YCR12w	137,347-138,594	*PGK1*	Phosphoglycerate kinase (EMBL: J01342)	1,911	
YCR19w	152,544-153,632	*PET18*	Maintenance of killer, MAK32 protein (PIR: 507695)	1,807	
YCRC20c	154,366-153,722	*PET18*	MAK31 protein (PIR: 507695)	1,155	
YCR31c	176,968-176,128	*CRY1*	Ribosomal protein s59 (EMBL: M16126)	617	
YCR40w	199,173-199,697	*MAT*	Matα1 protein	896	
YCR39c	198,909-198,280		Matα2 protein (EMBL: V01315)	979	
YCR42c	204,128-199,908	*TSM1*	Temperature-sensitive lethal TSM1 protein (EMBL: M60486)	6,911	
YCR66w	230,224-231,684	*RAD18*	DNA repair protein, Rad18 (EMBL: X125880	2,315	
YCR84c	261,186-259,048	*TUP1*	Repressor protein (EMBL: M35861)	3,156	
YCR88w	263,802-265,577	*ABP1*	Actin-binding (EMBL: X51780)	2,674	
YCR97w	292,568-293,051	*HMR*	Matα1 protein	896	
YCR96c	292,271-291,915		Matα2 protein (EMBL: V01315)	979	

* Region between *Cla*I site at 278,001 to the *Xba*I site at 277,004 replaced with *URA3*.
† Gene disruption experiments for this ORF are detailed in ref. 36, and in P. Leong-Morgenthaler *et al.*, manuscript submitted.
‡ *URA3* inserted into *Bgl*II site at 134,370.
§ *URA3* inserted into *Xba*I site at 227,218.
‖ Region between *Bst*EII site at 48,846 to the *Bst*EII site at 50,887 replaced with *URA3*. The techniques used for the other 51 gene disruptions may be summarized as: 23 insertion mutations (9, *URA3*; 6, *HIS3*; 5, MiniMu; 2, *TRP1*; 1, *HIS4*) and 28 deletion/replacement mutations (15, *URA3*; 6, *HIS3*; 5, *LEU2*; 1, *TRP1*; 1, *LYS2*).

partly explains the apparent paucity of markers in this segment of the genetic map. There is only a single discrepancy in the gene order derived from the sequence and that in the genetic map; the order of *glk1* and *cha1*, on the far left arm of the chromosome, is reversed. These data are summarized in Fig. 3.

The sequence data also provide evidence of past recombination events. A 245-bp sequence between residues 290,656 and 290,901 on the right arm (Fig. 1) shows a high degree of homology with the X element found in all yeast telomeres[29]. A similar sequence, 252 bp long, is found between residues 4,065 and 4,317 at the left end of the chromosome. It may be that chromosome III was once even shorter than it is now and that it has grown by recombination events involving its telomeres. Both the instability of linear plasmids and small artificial chromosomes[6] in yeast, as well as the Kaback rule[47], suggest that there may be selection against short chromosomes in *S. cerevisiae*. Moreover, a region near the middle of the left arm of chromosome XI has been shown by our analysis to contain sequences homologous to both the X and Y′ telomere elements (B.D. *et al.*, unpublished results).

Discussion

In sequencing the entire unique 315 kb of yeast chromosome III, we have generated 385 kb of total sequence. Thus 22% of the chromosome has been independently sequenced by at least two laboratories. The level of agreement between these sequence determinations was very high, the mismatch frequency being 0.4 per kb for clones from the same strain and 6.2 per kb for clones from different strains. In the latter case, 65% of the mismatches in ORFs occur in the third base of the codons. A re-check of the mismatch regions by the different pairs of laboratories reduced the discrepancies to zero, where clones came from the same strain, but had no significant impact on the number of differences between strains. In an independent check of the sequence, the restriction map predicted from the sequence has been compared with that derived by experiment (I. Collins and C.S.N., unpublished results). Of 504 6-bp restriction sites sampled, only four showed a discrepancy between the predicted and experimental maps (a putative error rate of 1.3 per kb). All four discrepancies represent predicted sites that could not be identified by experiment, therefore these sites may not be recognized by the appropriate restriction enzyme owing to context effects or DNA modifications. The chromosome III sequence is the largest data set available that has been subjected to such independent checking and so indicates the level of accuracy that larger genome projects may achieve using current technology.

The chromosome III sequence has revealed 145 novel protein-encoding genes and a start has been made on their functional analysis. The results so far indicate that there are vast areas of yeast genetics of which we are completely ignorant and emphasize the need for molecular genetics and physiological studies to proceed hand-in-hand. The data also call for a radical reappraisal of our view of the yeast genetic map. The availability of the sequence establishes unequivocally the locations of the different genes on the chromosome. In consequence, the genetic map acquires a different emphasis; it becomes much more a tool with which to study recombination and the dynamics of chromosome evolution. The goal of sequencing the entire yeast genome is achievable with present technology and this sequence will prove at least as important to the future development of eukaryotic molecular biology as the classical *S. cerevisiae* genetic map has in the past. The complete sequence of the yeast genome will open up new areas of molecular genetics and establish a foundation for the interpretation of sequence data from higher organisms. □

Received 30 December 1991; accepted 27 March 1992.

1. Strathern, J. N., Jones, E. W. & Broach, J. R. (eds) *The Molecular Biology of the Yeast* Saccharomyces: *Metabolism and Gene Expression* (Cold Spring Harbor Laboratory, New York, 1982).
2. Mortimer, R. K. *et al. Yeast* **5**, 321-403 (1989).
3. Carle, G. F. & Olson, M. V. *Nucleic Acids Res.* **12**, 5647-5664 (1984).
4. Petes, T. D. *et al. Cold Spring Harb. Symp. quant. Biol.* **38**, 9-16 (1973).
5. Baer, R. *et al. Nature* **310**, 207-211 (1984).
6. Newlon, C. S. *Microbiol. Revs* **52**, 568-601 (1988).
7. Clarke, L. & Carbon, J. *Nature* **287**, 504-509 (1980).
8. Szostak, J. W. & Blackburn, E. H. *Cell* **29**, 245-255 (1982).
9. Stinchcomb, D. T., Struhl, J. & Davis, R. W. *Nature* **282**, 39-43 (1979).
10. Klar, A. & Strathern, J. N. (eds) *Mechanisms of Yeast Recombination* (Cold Spring Harbor Laboratory, New York, 1986).
11. Petes, T. D., Malone, R. E. & Symington, L. S. in *The Molecular and Cellular Biology of the Yeast Saccharomyces: Genome Dynamics, Protein Synthesis and Energetics* (eds Broach, J. R., Pringle, J. R. & Jones, E. W.) 407-521 (Cold Spring Harbor Laboratory, New York, 1991).
12. Boeke, J. D. in *Mobile DNA* (eds Berg, D. E. & Howe, M. M.) 335-374 (ASM, Washington DC, 1989).
13. Rothstein, R. J. *Meth. Enzym.* **101**, 202-211 (1983).
14. Burke, D. T., Carle, G. F. & Olson, M. V. *Science* **236**, 806-812 (1987).
15. Martin, W. J. *Genome* **31**, 1073-1080 (1989).
16. Devereux, J., Haeberli, P. & Smithies, O. *Nucleic Acids Res.* **12**, 387-395 (1984).
17. de Jonge, P. *et al. Yeast* **2**, 193-204 (1986).
18. Strathern, J. N., Newlon, C. S., Herskowitz, I. & Hicks, J. B. *Cell* **18**, 309-319 (1979).
19. Newlon, C. S. *et al. Genetics* **129**, 343-357 (1991).
20. Olson, M. V. *et al. Proc. natn. Acad. Sci. U.S.A.* **83**, 7826-7830 (1986).
21. Link, A. J. & Olson, M. V. *Genetics* **127**, 681-698 (1991).
22. Yoshikawa, A. & Isono, K. *Yeast* **6**, 383-401 (1990)
23. Toh-e, A. & Sahashi, Y. *Yeast* **1**, 159-172 (1985).
24. Mortimer, R. K. & Johnston, J. R. *Genetics* **113**, 35-43 (1986).
25. Warmington, J. R. *et al. Nucleic Acids Res.* **13**, 6679-6693 (1986).
26. Pederson, M. B. *Carlsberg Res. Commun.* **51**, 163-183 (1986).
27. Warmington, J. R. *et al. Nucleic Acids Res.* **15**, 8963-8982 (1987).
28. Stucka, R., Lochmuller, H. & Feldmann, H. *Nucleic Acids Res.* **17**, 4993-5001 (1989).
29. Button, L. L. & Astell, C. R. *Molec. cell. Biol.* **6**, 1352-1356 (1986).
30. Jia, Y., Slonimski, P. P. & Herbert, C. J. *Yeast* **7**, 413-424 (1991).
31. Rodriguez, F. *et al. Yeast* **7**, 631-641 (1991).
32. Sharp, P. M. & Cowe, E. *Yeast* **7**, 657-678 (1991).
33. Mulligan, M. E. & Haselkorn, R. *J. biol. Chem.* **264**, 19200-19207 (1989).
34. Arnold, W. *et al. J. molec. Biol.* **203**, 715-738 (1988).
35. Cook, A. H. *The Chemistry and Biology of Yeasts* (Academic, New York, 1958).
36. Symington, S. & Petes, T. D. *Molec. cell. Biol.* **8**, 595-604 (1988).
37. Koniec[illegible] *et al. Genetics* **127**, 801-809 (1991).
38. Grandbastien, M.-A., Spielman, A. & Caboche, M. *Nature* **337**, 376-380 (1989).
39. Mount, S. M. & Rubin, G. M. *Molec. cell. Biol.* **5**, 1630-1638 (1985).
40. Ohno, S. *et al. FEBS Lett.* **222**, 279-285 (1987).
41. Goebl, M. E. & Petes, T. D. *Cell* **46**, 983-992 (1986).
42. Oliver, S. G. & Warmington, J. R. in *The Yeasts* Vol. 3 (eds Rose, A. H. & Harrison, J. S.) 117-160 (Academic, London, 1989).
43. Larkin, J. C., Thompson, J. R. & Woolford, J. R. *Molec. cell. Biol.* **7**, 1764-1775 (1987).
44. Feldmann, H. *Nucleic Acids Res.* **3**, 2379-2386 (1976).
45. Eigel, A. & Feldmann, H. *EMBO J.* **1**, 1245-1250 (1982)
46. Genbauffe, F. S., Chisholm, G. E. & Cooper, T. G. *J. biol. Chem.* **259**, 10518-19525 (1984).
47. Kaback, D. B., Steensma, H. Y. & de Jonge, P. *Proc. natn. Acad. Sci. U.S.A.* **86**, 3694-3698 (1989).
48. Keil, R. L. & Roeder, G. S. *Cell* **39**, 377-386 (1984).
49. Sanger, F., Nicklen, S. & Coulson, A. R. *Proc. natn. Acad. Sci. U.S.A.* **74**, 5463-5467 (1977).
50. Maxam, A. & Gilbert, W. *Meth. Enzym.* **65**, 499-559 (1980).
51. Pearson, W. R. & Lipman, D. J. *Proc. natn. Acad. Sci. U.S.A.* **85**, 2444-2448 (1988).

ACKNOWLEDGEMENTS. The Consortium of 35 European laboratories was aided by the provision of clones from laboratories in the USA and Japan and was established by the Biotechnology Division of the Biology Directorate of the CEC funded by their Biotechnology Action Programme (BAP). The overall organizer and leader of the Consortium was A. Goffeau (Université Catholique de Louvain, Belgium), assisted by S. Oliver (UMIST, UK) as DNA Coordinator, and J. Sgouros and W. Mewes (MIPS, Germany) as Informatics coordinators. We thank M. Olson and L. Riles for AB972 clones and for advice, and R. Mortimer for communicating genetic map data prior to publication. The graphic for Fig. 1 was prepared by Edeltraud Hanesch (Klinikum, Grosshadern) and that for Fig. 2 by Gertrude Schitteck (Universität Greifswald). This study represents the first phase of a major yeast genome sequencing project initiated by the EC and was carried out within the framework of the Biotechnology Action Programme (BAP) of the CEC under the general coordination of A. Vassarotti (Biotechnology Directorate) and the Université Catholique de Louvain. In addition, the following national agencies provided support:- Belgium: Service of the Prime Minister-Science Policy Office; Germany: Bundesminister für Forschung und Technologie; Greece: Ministry of Industry, Research and Technology; Italy: Consiglio Nazionale delle Ricerche, Comitato per le Biotecnologie e la Biologia Moleculare; Spain: Comision Interministerial de Ciencia y Tecnologia.

Science in Russia.

IT is gratifying to learn from a correspondent that, throughout the troubled period of the past few years, the splendid premises and wonderful collections of the famous Zoological Museum of the Academy of Sciences in Leningrad have scarcely felt the breath of war, famine, pestilence, and revolution which has passed over them. When the English traveller walks in, and is greeted by the famous young mammoth from Siberia, preserved like a recently killed specimen, and sees the rich collections illustrating the fauna of the vast steppes and deserts of Russian Asia, he feels that he is entering into a new world.

During the cold winter of 1919–1920, when fuel was unobtainable, it was impossible to heat the Museum premises, but the staff suffered more than the collections. Little or no looting was done during the disorders, except that the director had some difficulty in preventing the valuable collection of skins from being taken to be used as furs by the shivering population. Far more damage was done during the severe floods last autumn, when the waters burst into the basement and ground floor, and at least one member of the staff actually saved his own life and that of others by swimming: the library was badly damaged, many valuable specimens were ruined by the water and damp, and great inconvenience was caused by the smashing of the stores of alcohol, which is difficult to obtain to-day in Russia, as the supply is under strict Government control and very limited.

The entomological collections were enriched in 1914 by the generous gift by A. P. Semenov-Tian-Shansky of an immense collection of Central Asiatic Coleoptera, consisting of no less than 800,000 specimens: the same donor last year presented his own collection of Hymenoptera, Diptera, Neuroptera.

The staff of the Zoological Museum consists of ten "senior zoologists," who form a "soviet" and elect their own director, ten "keepers," and eight assistants. The present director is A. A. Bialitsky-Birula, well known from his work upon Arctic zoology, who is also editor of the *Annuaire*. Birds are under the charge of P. P. Sushkin, Member of the Academy, who is well known in Great Britain and the United States. Another name well known outside his own country is that of the entomologist A. P. Semenov-Tian-Shansky, whose many friends will regret the sad news of his failing eyesight. Fortunately, his general health leaves nothing to be desired, and it is to be hoped that he will be spared with capacity for useful work for many years. His father, P. P. Semenov, was a distinguished explorer, who surveyed the Tian Shan mountains, receiving the authority of the Tsar to add the title Tian-Shansky to his surname. Other well-known members of the staff are N. J. Kuznetsov the lepidopterist, G. G. Jacobson the coleopterist, A. K. Mordviko the aphidologist, P. I. Schmidt the ichthyologist, A. N. Kirichenko and A. M. Diakonov, entomologists.

The staff of the Museum are, of course, State officials, and paid at least a living wage: the salary of a senior zoologist is 47 gold roubles a month, equivalent to about 5*l.*: this, of course, leaves no margin for luxuries, but they are at least happy in their devotion to science. Their chief complaint has been the shortage of modern foreign literature, but this is now to a certain extent being made good. There is, however, considerable leeway to make up, and as the postal arrangements are now working satisfactorily, zoologists in England will be doing good work if they bear this in mind.

During recent years it has not been possible to publish the results of research work in agriculture in Russia, since the scanty funds for agricultural publications have been used, in the first place, for publishing popular handbooks and pamphlets. At the same time, research work has been carried on, often under most unfavourable conditions, and a considerable amount of new facts is awaiting publication. The new journal (*Journal für landwirtschaftliche Wissenschaft*, vol. i. Nos. 1-6; Moscow, 1924 (in Russian)), edited by a group of leading professors and research workers of the Moscow Agricultural Academy, aims at becoming a medium for publishing results of research work in all branches of agricultural science. The five numbers (one double) before us now include a great variety of papers on different subjects.

One of the most interesting papers is by A. G. Dojarenko, on the utilisation of solar energy by plants (No. 1, pp. 7-21), which describes the methods used in the author's experimental work for exact measurements of solar energy both received and utilised by cultivated plants, and gives interesting, though only preliminary, conclusions. Of considerable general interest is a paper by A. R. Minenkov (No. 1, pp. 29-47) dealing with the problem of chemical determination of sex in plants and in animals; the results of his experimental work are that both in plants and in animals there is a definite sexual difference in the fermenting properties of extract (plants) or blood (animals) which enables one to determine the sex. A. D. Prianishnikov (No. 3, pp. 179-190) describes experiments on the transformation of nitrogen compounds in plants and in animals, the author's conclusion being that the analogies in this respect are very far-reaching and suggest a close similarity of processes in plants and in animals. In a paper by V. Israilsky and E. W. Runov the question of the action of vitamins on bacteria is discussed and experiments described, which tend to show that bacteria are very sensitive to vitamins. G. D. Karpetchenko (No. 5-6, pp. 390-410) describes hybrids between two plants of different genera, *Raphanus satinus* L. and *Brassica oleracea* L.; an exhaustive study of the morphology and cytology of hybrids is given. These are only a few of the more interesting papers from the journal, which represents, on the whole, an important step in the development of agricultural science in Russia. The value of the journal to Russian agricultural research workers is greatly enhanced by abstracts of current literature.

ANTIQUITIES FROM THE RUSSIAN ALTAI.—Dr. Alexis Zakharow, of the Russian Historical Museum, Moscow, describes in the Journal of the Royal Anthropological Institute, vol. 55, pt. i., some of the antiquities from two cemeteries on the Katanda Steppe, which were discovered by W. Radloff so long ago as 1865. These antiquities, however, have never been described. The first cemetery contained 30 to 40 tombs covered with mounds of round pebbles, and a row of flat barrows, which, however, Radloff considered to be places of sacrifice. In this cemetery were found skeletons of horses with bits of iron, human remains, male and female, knives of iron, and arrowheads of iron and bone. Spearheads with tubular sockets, and fragments of a bow, were among the objects found with male burials; copper ear-rings of spiral form were with the female skeleton. In the eighth and last kurgan excavated were fragments of garments. In the second cemetery, the most interesting objects came from a large kurgan more than seven feet high and 100 feet in diameter. The skeletons of at least six horses were found in this mound. *

*Abridged

The Closed Circuit—a Record of Soviet Scientific Life

In May this year Dr Zhores A. Medvedev, once head of the Department of Molecular Radiobiology at the Institute of Medical Radiology, Obninsk, was detained by the Soviet authorities in a psychiatric hospital, but was released in mid-June after protests from several distinguished Russian scientists. This account of Dr Medvedev's earlier unsuccessful attempt to accept an invitation to speak at the Ciba Symposium on Ageing at Sheffield in September 1966 is taken from his book *The Medvedev Papers: The Plight of Soviet Science*, due to be published in English early in 1971 by Macmillan and Co. (and St Martin's Press, New York, in the United States). The Russian text was published by Macmillan this week. The circumstances of this publication are recounted elsewhere in this issue.

My first visit to the Ministry of Health of the USSR showed that the attitude of the Foreign Section towards my trip to England had undergone a sharp change. The Deputy Head of this section, M. A. Akhmetelli, began to talk very vaguely about the fact that the Agreement on Cultural and Scientific Exchange with England had not yet been signed for 1966, and hence it was not clear how many man-days their section would have for official visits in 1966. Naturally, he said, if the plan for man-days in 1966 was not very large, then they would be able to arrange only the most important trips, agreed at the very highest level. Another rank and file employee of the same section, who had previously been given the materials from our Institute for safe keeping, hunted in his papers but could not find the file with the correspondence about the lecture and the resolutions of his relevant superiors. This file, which he had formerly had in his possession, seemed to have disappeared somewhere. I encountered the same lack of certainty in the Academy of Medical Sciences. Furthermore, it was necessary to start preparing an Exit Dossier again. The forms for the Exit Dossier are considered to be strictly accountable papers and are issued by the Foreign Sections of departments only to individuals whose trip has been agreed upon by the department. An Institute cannot prepare Exit Dossiers for capitalist countries on its own initiative.

After waiting a couple of weeks without obtaining any clear-cut decision, nor any official instructions to the Institute to prepare an Exit Dossier, I let Dr Wolstenholme know fairly clearly about the uncertainty which had arisen. I told him that the process of getting this kind of trip approved was a multi-staged one and that a first favourable reaction might be changed at various levels on its way to an actual decision. I could not tell him straight out the nature of the difficulties which had arisen, all the more so because I still did not know precisely what they were. It was necessary to speak in hints.

> The process of arrangements here could be compared [I wrote] with multi-stage chromatography and ion-exchange. Imagine a set of columns with different absorbents and ion-exchangers. A definite substance must pass through all of them. Those molecules which have no (+) or no (−), which are chemically inert, have more chances.

Anyone acquainted with chemistry and biochemistry, and this must include Dr Wolstenholme, would understand that no compound could pass through a system of both cation and anion exchangers if it had either a positive charge or a negative charge or both types of charges simultaneously on different groups. Only substances which are completely neutral, uncharged, without pluses or minuses, can get through.

> I am not completely certain [I wrote] that I can by my own efforts ensure my trip to England, nor whether the relevant people will consider the personal invitations which I have received. Taking into account all the existing difficulties, I want to warn you that my trip to England is still problematic. There are two courses open to you to ensure the traditional lecture on ageing: first, to invite some other scientist whose personal wish and acceptance is sufficient guarantee of his attending the Symposium in Sheffield; or, secondly, to transfer the discussion of matters relating to this lecture to the official level, which would apparently have to be not lower than that of the Minister of Health. . . .

Again, as in 1960, I did not want to have to take the sins of others upon myself. If the responsible authorities did not reach a favourable decision, then let them conduct the correspondence and let them take the responsibility for the breakdown of this matter. In all events, I would do my duty, the lecture would be written, translated into English and sent to England, whatever the situation. The tenth traditional annual lecture on the problem of ageing would take place, whether or not I myself could go to England. With this aim, I began to prepare my first draft of the lecture; it was still too long, but I proposed to cut it later, keeping what was most significant. The lecture could not last more than seventy to eighty minutes.

The programme which I had received earlier was a preliminary one, which had been sent out only so that those attending could fix their plans for going to Sheffield in September, particularly since half of them were from other countries. The programme had not yet been published, and the Ciba Foundation would still be able to ask some other scientist to write the lecture. But, to speak frankly, I was sure that Dr Wolstenholme would choose the second course and would procure the visit of the lecturer he had already invited by top-level discussions. A short time later, I received confirmation of this. Dr Wolstenholme had drawn up an official letter to the Ministry of Health of the USSR, and had sent copies of his letter to the Director of our Institute and to me. The letter read as follows:

Professor B. V. Petrovskii,
Ministry of Health,
Rachmanovskii, Str. 3,
Moscow,
USSR.

Dear Professor Petrovskii,

It is my pleasant duty to write formally, on behalf of the Trustees of the Ciba Foundation, to ask you to give your official approval and assistance to enable a distinguished Russian scientist, Dr Medvedev, to come to England to lecture about his research.

The Ciba Foundation in London is an independent scientific and educational Trust which has been working in the field of international cooperation in medical research for the last sixteen years. We are assisted in this work by eminent scientists in many countries, who serve as our Scientific Advisory Panel. The representatives for the USSR are Academicians V. A. Engelhardt, A. I. Oparin and M. M. Shemyakin. Any one of these scientists would undoubtedly be willing to give you further information about the activities of the Ciba Foundation.

Some ten years ago, as part of our programme of medical research, we made a special effort to encourage fresh research, internationally, in relation to the problems of ageing. We have held a number of conferences on this subject, and each year we invite a distinguished scientist to visit England and lecture about his own special line of research in relation to ageing processes.

We now wish to invite Dr Zh. A. Medvedev (Chief of the Laboratory of Molecular Radiobiology, Obninsk) to give the Ciba Foundation's tenth Annual Lecture on Ageing Research. We should very much like him to tell us about his work, which we believe to be of international importance, and we should like him to do so in conjunction with a symposium on Ageing Research. This symposium will be held in Sheffield early in September 1966 and is being organized jointly by the Ciba Foundation, the Society for Experimental Biology and the British Society for Research on Ageing. We hope that Dr Medvedev will come and take part in this five-day symposium, with his own lecture (on the second day) being the main function of the week. Preliminary letters have been sent to Dr Medvedev and we have ascertained his willingness to prepare a lecture on his special subject for this occasion. We now hope that his visit to England can be given official sanction.

The Ciba Foundation undertakes to provide all the necessary travelling expenses, accommodation and living expenses for Dr Medvedev in connexion with this visit to England.

A copy of this letter is being sent to Professor G. A. Zedgenidze (Director of the Institute of Medical Radiobiology of the Academy of Medical Sciences in Obninsk).

We should be most grateful for your early, favourable attention to this matter, which we believe will strengthen the mutual respect and cooperation of Soviet and British scientists.

Yours sincerely,
G. E. W. Wolstenholme
OBE, FRCP, FIBiol.

Three weeks after receiving this letter, I was summoned by telegram to see the Deputy Head of the Foreign Section of the Ministry of Health, M. A. Akhmetelli. I went in haste to Moscow, almost sure that Dr Wolstenholme's letter had produced the necessary action. By this time I had read up about the wide scale of the Ciba Foundation's international activity, and I knew that the USSR received considerable benefits from this organization, particularly in the field of public health.

My hopes, however, were unfounded. Akhmetelli did not receive me in too friendly a manner. He told me that the Minister of Health had decided against my trip to England. Akhmetelli hinted quite clearly that the English had done me too great an honour and that I ought not to be striving so hard to bring it off. When I asked who, in his opinion, was more worthy for such a mission, Akhmetelli was evasive. Akhmetelli also said that it was not quite convenient for the Minister to send a refusal to Dr Wolstenholme's letter, and that therefore I should send a reasonable refusal to England. If I did not do this, then their Section would hardly be able to do any serious business with me in the future. This was an obvious, although polite, threat, and it was only left to me to tell Akhmetelli my candid opinion of him and his department.

A few days later, the Director of our Institute was summoned to the Ministry. He was advised, in a more official manner, to send a short letter to Dr Wolstenholme, which would save the Minister from having to deal with the problem. I did not learn about this until later, when Dr Wolstenholme sent me a photocopy of this letter and a copy of his own reply to it.

The refusal, written in good English, read:

9.3.1966.

Dear Professor Wolstenholme,

I was very pleased to receive your kind letter in which you ask my assistance to enable Dr Zh. Medvedev to come to England to lecture about his research.

I express my regret, but I suppose Dr Zh. Medvedev will not be able to come to England this year because of a great pressure of work he has to do in his laboratory.

Yours sincerely,
Professor G. A. Zedgenidze

Director of the Institute of
Medical Radiology of the Academy of
Medical Sciences of the USSR,
Member, AMS of the USSR.

Dr Wolstenholme's reaction was almost instantaneous.

Dear Professor Zedgenidze,

Your letter of 9th March comes as a very great disappointment not only to all of us at the Ciba Foundation, but also to the many people gathering in Sheffield in September who were particularly looking forward to an opportunity of learning at first hand about this work on Molecular Aspects of Ageing.

Dr Medvedev was also invited to the International Congress of Gerontology in Vienna, but had decided, correctly, that the lecture and meeting in Sheffield would give him a better opportunity to have the work discussed by appropriate scientists. It is a very great pity if the work cannot now be heard either in Vienna or Sheffield.

So far as the Ciba Foundation is concerned, this annual lecture has been given by people from the USA, France, Holland, Israel, etc., and it will be a matter of very great regret to us if the USSR is not to appear in this series.

Since Dr Medvedev would have to be in this country for only one week, and indeed a visit of 2–3 days would certainly be better than nothing, is it really impossible to spare him from his duties for so short a time, particularly as this is presumably at a time when he might be on vacation?

I should be most grateful if you would reconsider the matter, and hope that there may still be the possibility of a favourable reply.

I am sending copies of this letter to Professor Petrovskii at the Ministry of Health, and also to Academicians Engelhardt, Oparin and Shemyakin who represent the Ciba Foundation in your country.

Yours sincerely,
G. E. W. Wolstenholme
OBE, FRCP, FIBiol.

Together with the copy letters sent to Engelhardt and the other two academicians, Dr Wolstenholme wrote to each of them asking him to use his influence to obtain a favourable decision about the Ciba Annual Lecture.

Academician Oparin, as might have been expected, completely ignored the request and did not even answer Dr Wolstenholme's letter, but both Engelhardt (Director of the Institute of Molecular Biology) and Shemyakin (Director of the Institute of the Chemistry of Natural Compounds) actively sought to do what had been asked. I found out about this much later when Academician Engelhardt's campaign was over and he sent me copies of the correspondence on this matter and told me of the measures which had been taken.

At the end of March, Engelhardt and Shemyakin had prepared a special memorandum to the Ministry of Health of the USSR and, having obtained an interview with the Minister, they handed it to Professor Petrovskii personally, urging him to take a favourable decision. It ran as follows:

Zhores A. Medvedev.

Dear Boris Vasil'evich,

Please permit us to turn to you with a request that you take note of the question of granting to the Head of the Laboratory of Molecular Radiobiology at the Institute of Medical Radiology in Obninsk, Dr Zh. A. Medvedev, the opportunity of taking part, as principal lecturer, in the International Symposium on the Biology of Ageing to be held in England in the autumn of this year.

The Symposium is being held by the scientific organization 'The Ciba Foundation'. We are both members of the Council of this organization, as representatives of the Soviet Union. This Council includes scientists from a great number of countries, since the Ciba Foundation is of an international nature. It is a very solid organization with a high international repute. It undertakes a broad range of scientific and organizational work, it holds many specialized conferences and publishes a large number of monographs and collections of papers. It serves as an active centre of scientific contact between representatives of different branches of medical science, with scientists from related fields—chemists, biophysicists, pharmacologists, etc. In particular, one of the fields to which the Ciba Foundation pays special attention is gerontology, in its various aspects. The Symposium we are concerned with is the next event of the current year in this line. Preparations for the Symposium began over a year ago, and the organizers nominated as the principal lecturer, to give the annual lecture, Zh. A. Medvedev, the author of a number of important papers on problems of the biological principles of ageing.

However, at present, the situation regarding Zh. A. Medvedev's trip to England has taken a considerable turn for the worse.

The Director of the Ciba Foundation, Dr Wolstenholme, has approached us, as members of the Advisory Panel, with the request that, if possible, we should lend our efforts to prevent the breakdown of this long-planned Symposium, since the Annual Lecture is the central point of the whole programme.

In asking you to reconsider this problem, we should like to stress that the Ciba Foundation has always shown great courtesy to Soviet scientists, inviting this one or that to take part in its activities. Our scientists frequently visit the Ciba centre, where there are excellent facilities for scientific work, and avail themselves of the various forms of assistance offered by the Ciba administration. The difficulties which would arise out of a refusal to contribute to the success of this symposium would certainly not be in the interest of the expansion and strengthening of our scientific contacts abroad.

Academician V. A. Engelhardt
Academician M. M. Shemyakin

The Minister of Health certainly paid attention to this letter, and turned his attention to the question it raised, but on quite a different plane. I was summoned again, this time by the Head of the Foreign Section of the Ministry of Health (this Section for some reason calls itself, in the list of Sections, the External Relations Section), Comrade Novgorodtsev, who advised me, in a categorical and high-handed manner, to stop all correspondence and activity connected with taking part in the Symposium.

Clearly, the trip could be written off as impossible. However, I decided "no surrender", and I tried to solve the problem one way or another. There were still four months left, quite long enough.

I wrote once again to Dr Wolstenholme, telling him that the Ministry of Health still had not reconsidered their decision. "Since," I wrote, "the programme of the Symposium has already been fixed and since my lecture forms part of that programme, then whatever the circumstances the Ciba Foundation will receive the text of this lecture in English not less than two weeks before the Symposium." However, I told Dr Wolstenholme, it would clearly be advisable to attempt another try, this time a contact between the Ciba Foundation and the Chairman of the State Committee on Science and Technology, who, being the Deputy Chairman of the Council of Ministers of the USSR, was on a still higher level, and was fully empowered to consider and reconsider any decision of this kind.

I no longer believed in the success of such a contact, but the experiment had to be made, all the more since the Head of the State Committee was Academician V. A. Kirillin who, back in 1960, had been in charge of the Science Section of the Central Committee of the CPSU, and later had been a Vice-President of the Academy of Sciences of the USSR. In 1963, as Vice-President of the Academy of Sciences, he had read my manuscript on the history of the genetic controversy, and, so I was told, had been favourable to it.

It was less than two weeks before I received from Dr Wolstenholme a copy of his letter to the State Committee on Science and Technology.

> Dear Academician Kirillin,
>
> I have the honour to address you as Head of the State Committee on Science and Technics of the Council of Ministers of the USSR, and would greatly appreciate your kind consideration of the following matter.
>
> In March 1965 I wrote formally, on behalf of the Trustees of the Ciba Foundation, to Dr Zh. A. Medvedev (Chief, Laboratory of Molecular Radiobiology, Institute of Medical Radiology, Obninsk) to invite him to give the tenth in a series of annual lectures on research relevant to the problems of ageing, which are given by people of international importance. . . . The lecture [is to be] part of an international symposium . . . concerned with many aspects of the problem of ageing. Dr Medvedev, from his personal point of view, kindly accepted our invitation. . . .

Dr Wolstenholme then briefly outlined the history of his correspondence with the Minister of Health and the Director of our Institute, and also explained the problems and aims of the Ciba Foundation. At the end of his letter he wrote:

> It is our strong wish to encourage wider recognition of Soviet scientific achievements, and, at the same time, to do everything possible to improve official and personal friendship. We greatly hope, therefore, that you would be so courteous as to use your influence to overcome whatever minor obstacles there may be which so far prevent Dr Medvedev from confirming his ability to give our lecture and participate in the international symposium in Sheffield.

Academician Kirillin used his influence, but, like the Minister, in an entirely different sense from what Dr Wolstenholme had hoped for. At the end of May, a member of the State Committee for Science and Technology, D. Pronskii, who was Head of the International Section of this Committee, forwarded Dr Wolstenholme's letter to the Ministry of Health of the USSR, with a covering letter of his own. In this letter, he stated insistently and in an insulting manner that Zh. A. Medvedev had broken the regulations about "intercourse with foreign firms" and recommended that these regulations should be explained to Medvedev. Judging from this letter he did not even understand that the Ciba Foundation was not a firm but was a scientific organization. But, nevertheless, his letter found the necessary response in the Ministry of Health.

In the middle of June, I was summoned by the Head of the Special Section of our Institute, and, on the instructions of the Director, I was made familiar with a letter (of 10 June 1965) which had come to our Institute from the Head of the Foreign Section of the Ministry of Health, Comrade Novgorodtsev, to which was attached a copy of Pronskii's letter to the Ministry of Health. Repeating in part the expressions of Pronskii, Novgorodtsev wrote in his letter that Zh. A. Medvedev had "broken the regulations about correspondence with foreign firms", that "he was striving to make this trip by any means, and was involving scholars who had nothing to do with it". Here he had in mind Engelhardt and Shemyakin. Further, Novgorodtsev expressed his doubt that Medvedev was sufficiently competent in the problems which it was proposed he should raise in his lecture, and he also recommended that the necessary administrative measures should be taken to let Medvedev know that his behaviour and his position of opposition to the decisions of the Ministry were not acceptable.

It should be noted that Novgorodtsev's letter was sent to the Institute via the Special Section deliberately, for the psychological effect. A copy of this letter from the Ministry of Health was also sent to the Academy of Medical Sciences of the USSR.

Ten days later, via the same route, an enquiry came from the International Section of the AMS, signed by a new head of this section, Academician Kovanov of the Academy of Medical Sciences. In this letter, he told the Institute to let him know what concrete measures had been taken by the Party Organization and the Institute authorities about Zh. A. Medvedev, in connexion with his breach of the rules on correspondence and contact with foreigners, and a reference to Novgorodtsev's letter on this matter followed.

The Special Section of our Institute demanded an explanatory memorandum from me on this matter. I wrote them one, clearly pointing out the misunderstanding which had arisen both in the Academy of Medical Sciences and the Ministry of Health. I also clearly indicated my opinion on Comrade Novgorodtsev's power to judge my competence in the field of gerontology. So far as I know, my explanatory memorandum was sent to the AMS and the Ministry of Health by the same channels which had brought their letters, but neither an answer nor a "sorting out" of the matter followed.

There was now little more than two months until the opening of the Symposium. The lecture was written, and the translation into English almost finished,. At the end of July, my old friend Ralph Cooper was arriving for the International Microbiology Congress in Moscow. I was counting on him to help me edit the translation of the text. Dr Cooper and I had worked together in the biochemical laboratory of the Timiryazev Agricultural Academy in 1958–59, when he visited the USSR under a scientific exchange programme. He was at that time a young biochemist who had just finished his postgraduate course at Oxford and had been working for about a year at the Rothamsted Experimental Station. When he arrived in Moscow, not knowing Russian, he was almost helpless, particularly since our working conditions are so different from those in England. He was at first assigned to the Department of Microbiology, but then he was transferred to our Department of Agrochemistry and Biochemistry. I shared a small study with him, and in the end we used to coach each other in our respective languages. During the year, we became good friends, and afterwards we had kept up a constant correspondence. In 1961, Ralph Cooper was a biochemistry lecturer at Hatfield College.

Ralph Cooper was coming to the Microbiology Congress with his son Paul, who was twelve years old. During the Congress Paul was to stay with us in Obninsk, with my sons, the elder of whom, Sasha, could speak a little English.

At the beginning of July, another letter came from Dr Wolstenholme.

> Dear Dr Medvedev,
>
> . . . We have received no further news from you or from the Soviet Authorities, and greatly hope that in this interval arrangements are going ahead favourably for your visit to give this important lecture for the Ciba Foundation, and for your participation in the very interesting symposium in Sheffield. I leave for my holidays on 28th July and will be back in England only a day or two before the Sheffield meeting, so that I hope there will be good news before I leave. In any case my office will be in touch with me while I am away.
>
> A room is being held for you at the Ciba Foundation for the nights of 2nd and 3rd September. . . . A room has also been reserved for you in Sheffield . . . for the nights of 4–9th September inclusive. . . .
>
> If the worst comes to the worst, and you are refused permission to come to England—although I cannot imagine what valid reason there could be for this—then we should hope to receive your manuscript, with any slides, in good time so that we could arrange for Dr Strehler to present the lecture for you.

By this time I had already sent the Russian text of the lecture, some forty pages, for the necessary official approval, without which not a single post office in the country would accept it for dispatch. The procedure for such approval had hardly changed at all since 1960. Obtaining approval for posting, with the special form 103A, required the preliminary consideration of the text by the Academic Council (with two representatives), a decree of the Commission that it was non-secret, and decisions by a certain department of the Academy of Medical Sciences of the USSR, the International Section of the AMS and Glavlit. After permission had been granted for the text to be sent abroad, the English translation had to be considered, and the Institute itself had to verify that this was identical with the Russian text.

The failure of the attempt to get a reversal of the previous decision on the trip by way of the State Committee for Science and Technology left me only one possibility of further action—to go to the Central Committee of the CPSU, the final and highest of all departments. for all decisions on foreign travel to capitalist countries.

The Secretariat of the CPSU, unlike all other bodies, has the right to send people abroad for short periods, even at short notice. I knew of a case of a footballer who was suddenly required for an international match; he was summoned and rushed by air from the resort where he was on holiday, approved by all departments, including the visa section, delivered from Moscow to England, driven straight to the stadium from the airport, and all this within twenty-four hours. He was to play for the Rest of the World against an All-England side. The idea of the match had come up unexpectedly, and no preparations had been made for it. So far as I remember, England won. But this, of course, was a special case; football, sport, the glamour, the prestige! No doubt the Chairman of the Exit Commission watched the match himself in the English stadium or at home on TV. It was not a lecture on gerontology.

A preliminary discussion with some employees of the Central Committee of the CPSU, mainly by telephone, led me at last to the section which had it in its power to be most helpful in the solution of the problems which had arisen in contact with other departments. This was the International Section for Capitalist Countries of the CPSU. (There was yet another International Section which was responsible for problems connected with socialist countries of the East and West.)

At the very end of June, I was received by the First Assistant to Comrade Ponomarev, the Secretary of the Central Committee of the CPSU who was in charge of this Section. Comrade Ponomarev himself was away at this time, accompanying the French President, De Gaulle, on his tour of our country. His assistant, Comrade V. S. Shaposhnikov, met me in a most friendly manner, and our conversation lasted more than an hour. He was interested in gerontology, its achievements and the reason why our country was lagging behind in a number of directions in biology. He had made himself thoroughly familiar with my case; he promised to take all necessary measures to set the matter right and make the visit possible, and, in case of necessity, to interest Comrade Ponomarev in the matter personally. After this visit to the Central Committee of the CPSU, I had the first feeling of hope that something would be done and the trip to England would take place.

However, no results were apparent in the course of the next two weeks. When I received Dr Wolstenholme's letter, quoted above, I once again applied to Comrade Shaposhnikov, in writing, and at the same time sent him a photocopy and translation of the letter from England. In a telephone conversation, Shaposhnikov said that he was very sorry but he had not yet had time to deal with my business, but he promised once again to do everything possible in the very near future.

At the end of July, my friend Ralph Cooper arrived in Moscow. Naturally, I told him about my problems, and, when we had considered the situation carefully, we decided that my best plan would be to send a copy of the English translation of the lecture by him, for safety, since the approval to send it by official channels was taking so long. The International Section of the AMS had sent the manuscript of the lecture first for review to the Institute of Gerontology in Kiev, and Glavlit, which is not to be hurried, was still to come. Cooper promised that when he got home he would edit the translation from the language point of view, have it retyped if there were a lot of corrections, and deliver it to the Ciba Foundation by mid-August. He would send the copy of the lecture to Dr Strehler. Bernard Strehler was taking part in the Symposium and I asked him if he would read my lecture in case I could not go myself. I knew from the Biochemistry Congress in Moscow in 1961 that Strehler was a very fine speaker and could carry out this task excellently.

To take advantage of a convenient opportunity such as this for sending the text of a lecture was not, of course, in the bureaucratic sense, in accordance with the "rules for intercourse with foreign firms". But no other way out was left to me. If I could not get the text of the lecture to my English colleagues in good time, I should be to blame in their eyes, I should have defaulted. No reasonable Englishman could ever believe in the reality of the procedure for sending a scientific text which I have described above. He would not believe it, but would think that I had made it all up to cover the fact that I had been unable to write the lecture, although I had already written to England that the lecture was now ready.

The course of subsequent events proved that I had acted correctly. The official procedure (and this only under pressure from me) was as follows. The report from Kiev, from the Institute of Gerontology, was received in August. Fortunately, the Kiev experts did not recommend any changes. Glavlit's permission to send the lecture abroad was granted on 4 September, and the text of the lecture was sent to England on 5 September, the day before the session when the lecture was to be delivered. It was received in England on 11 September, when the Symposium was already over.

In 1939, when Nikolai Vaivlov was refused permission to go to Scotland for a Genetics Congress of which he had been elected President, he sent the text of his speech unofficially via his Bulgarian friend, Doncho Kostov, who was going to the Congress from Leningrad. At that period, Vaivlov was risking far more than I was. I regard this as an example for scientists to follow, and in this case I find no moral problem in breaking rules when they have become ridiculous.

In the middle of August, I received from England several leaflets announcing my lecture. These leaflets were being distributed to the science centres, universities and colleges of Great Britain. They announced in large type:

THE 10th (ANNUAL)
CIBA FOUNDATION LECTURE
ON
RESEARCH ON AGEING
WILL BE GIVEN BY
DR Z. A. MEDVEDEV
(USSR)
SUBJECT:
"MOLECULAR ASPECTS OF AGEING"
on Tuesday, 6 September 1966, at 8.15 pm
in Lecture Theatre No. 1, University of Sheffield
Chairman: Professor H. N. Robson
Open without fee or ticket to all interested

It was not open only to the lecturer himself.

At the same time as he sent the leaflets, the Deputy Director of Ciba told me that as a final means of ensuring my trip to England, they, together with the British Society for Cultural Relations with the USSR, had approached the Soviet Embassy in London and asked for their help, and had written to the British Embassy in Moscow, so that, in case the Soviet authorities did request a visa for my visit to England, it could be issued without the delays in correspondence usual in such cases.

Once again, I sent all this material and one of the leaflets to Comrade Shaposhnikov, enclosing a covering letter. There was, however, no answer.

At the end of August, a letter came from Ralph Cooper. He told me that the correction to the text of the lecture had been completed and that it was now in the hands of Ciba.

> The Deputy Director of Ciba [he wrote], Dr de Reuck, sent a telegram to Bernard Strehler, to confirm that he will be present. He also suggested that I should go to Sheffield as your representative, and, if necessary, give an explanation of your absence. Thus everything necessary has been arranged....

September drew near, and my hopes of being able to make the trip finally died. Comrade Shaposhnikov was away on leave, and it was now too late to start another round of high-level discussions. I began to be very concerned about the absence of any legal guarantees of so important an aspect of the rights of man and of the scientist which the freedom to travel abroad and to engage in international cooperation is recognized throughout the world to be. When it was clear to me that the lecture would take place without its author, I decided to send a special explanation to England, making the reason for my absence clear. I prepared this "explanation" six days before the lecture, and sent it from Moscow, from the International Post Office, by registered express airmail, requesting an "advice of delivery" and sending it, for safety, to three people: Dr Wolstenholme, who was by now in Sheffield; the Chairman of the lecture, Professor Robson; and Ralph Cooper. I asked them to read this text either before or after the lecture, whichever was more convenient. I was not sure whether or not these letters would get through to England, but, as it turned out, only one of them went astray. The other two reached Sheffield on 5 September, and the "advice of delivery" notes were returned to me.

And so came the day of 6 September, a day which should have been a day of triumph for any scientist. Early in the morning, I set my watch to British time. My lecture was to take place at the evening session. The morning was given over to two sessions on problems of the ageing of plants. In Obninsk, however, a sterner task awaited me.

In the autumn, as is well known, all city organizations take part for a couple of months in the potato harvest. It just had to happen that the turn of our section, that of Radiobiology and Genetics which comprised four laboratories, to go potato picking came precisely on 6 and 7 September. That morning, with my colleagues from our laboratory, I travelled 25 kilometres by bus, out to the State Farm. In Sheffield, they were just getting ready for the first morning session, someone else was in the Chairman's seat instead of me, while we were carrying baskets and starting to sort the potatoes, moving back and forth along the furrows. The section was assigned to a field of some two hectares. The potatoes had to be collected, sorted into large and small, and the large ones loaded into the backs of the lorries. The small ones went to the farmyard. We finished our working day at around four o'clock. By this time the second morning session in Sheffield was over.

The dinner in Sheffield was at 18.30, British time. In Obninsk, I had a few of my friends coming to my home at this time. The dinner in Sheffield did not last long, the lecture followed it. But we, in Obninsk, had no need to hurry, and we sat over it, talking. When, by my reckoning, my explanation might well be being read in Sheffield, I too read it aloud to my own guests at home.

Translated by Vera Rich.

Soviet Union

Sakharov arrest threatens East-West scientific exchange

Vera Rich reports on the reactions of the scientific community in the West to the arrest of Dr Andrei Sakharov (right) in Moscow last week and his subsequent internal exile

BANISHING Dr Andrei Sakharov, physicist and leader of the human rights movement in the Soviet Union, to Gor'kii is not the way to promote international scientific exchange. This is the clear consensus of the western scientific community.

Referring to Sakharov's "internal exile" as "a shocking" and "a blatantly punitive" act, Dr Philip Handler, President of the US National Academy of Sciences, noted that "in recent years the scientists of both nations have carefully built structures of communication and good will", and that the move against Sakharov "can only be regarded as a challenge to further cooperation and an act of deliberate ill will. . . . I find it difficult to imagine scientific exchange continuing in the spirit we had created heretofore."

"The exile of Professor Sakharov", said Kenneth Boulding, Chairman of the Board of Directors of the American Association for the Advancement of Science, and Frederick Mosteller, President of the AAAS, "deprives the people of the Soviet Union and the world of a brilliant voice in support of mutual understanding and the defense of human freedom". His banishment, according to 25 US Nobel laureates, who sent their own personal telegram to Brezhev "will silence an eloquent voice of wisdom desperately needed in our troubled world".

For the US Committee of Concerned Scientists, Drs Max Gottesman and Mark Kac, expressed their concern that the move against Sakharov coming "as the aftermath of Afghanistan" might "portend a change which threatens the continuance of cultural and scientific relations between our two countries." Telegrams from the French Academie des Sciences and Société des Physiciens expressed similar shock and concern.

In London, the "President and Officers" of the Institute of Physics, expressed "shock and dismay" fearing that "this action . . . will damage both Soviet physics and the relations between Soviet and British physicists". The Royal Society, with its usual diplomacy, expressed the view that "voices sound louder through doors that are open than through those that are locked".

This is not the first time that western colleagues have felt obliged to leap to Sakharov's defence. In September, 1973, an ominous *Pravda* article, entitled "To be a Russian scientist means to be a patriot" was widely seen as a first move towards expelling Sakharov from the Academy of Sciences. No expulsion in fact took place, perhaps due to the storm of foreign protests the article provoked, but also to the reluctance of the Soviet Academy to permit undue government and Party meddling in its internal affairs. (The Academy is the last remaining Soviet body to have elections by secret ballot with a plurality of candidates). A short while later, when a similar attempt was made to deprive Veniamin Levich, the Jewish "refusnik" of his "Corresponding" (i.e. Associate) membership of the Academy, the attempt failed, his son Aleksandr told *Nature*, "because even some members who were quite anti-Semitic had decided to vote against the motion, feeling that their own position was threatened by any attack on the Academy".

Perhaps as a pre-emptive measure against such academic solidarity, Soviet "explanations" of the Sakharov affair have stressed how much he is out of step with the ideal of a Soviet scientist. The initial allegation (in *Izvestiya*) was that he had abandoned science for "the political walk of life" following a crisis in his own creativity. (No mention was made of his sudden dismissal from his government job — following the *samizdat* publication in 1968 of his "thoughts on Progress, Coexistence and Intellectual Freedom").

This approach, however, was negated when the western media stressed that Sakharov had been arrested on his way to a physics seminar at the Academy of Sciences — and by confirmation from close friends that he was still, in spite of the demands on his time made by his human rights activities, keeping up his theoretical work on physics, though with little hope of publication. Indeed, the *Izvestiya* accusation, that Sakharov was being "used as a channel for passing on important Soviet secrets to the . . . imperialist powers" was incongruous with their allegation that he had abandoned science.

The next stage came with an attack on Sakharov by individual members of the Academy. Academician Evgenii Fedorov spoke of the committment of Soviet scientists to "social work, the struggle for peace, cooperation with our colleagues abroad in science and technology" (almost echoing the title of Sakharov's first essay!). A "large community" of Soviet scientists, he said, are "fed up with Sakharov's remarks and his general attitute . . . contrary to everything for which Soviet people, including representatives of Soviet science, have fought". Academician Mark Mitin spoke of the "humanitarian" action of the Soviet government, which in spite of Sakharov's slanders of the Soviet way of life had simply deprived him of his state honours and sent him out of the capital. Nevertheless, Mitin observed unctuously, "as a member of the same Academy of Sciences I cannot but feel sorry about the downfall of this man."

Some, it would seem, felt more than mere sorrow. The resignation of Vladimir Kirillin from his posts as Deputy Prime Minister and head of the State Committee for Science and Technology was almost certainly connected with the Sakharov affair — though speculations vary as to whether Kirillin resigned voluntarily in protest or was manoeuvred into a situation where resignation became inevitable. Of recent years he has been a well known visitor to Britain to negotiate extensions to exchange agreements in technology and trade. (The next round of talks, due in London in May, will almost certainly now be at least postponed).

During the past few months, there have been a number of indications of conflict between the State committee (which comes directly under the government) and the Academy, which still shows a remarkable independence of spirit, paticularly where its own autonomy is concerned.

Although the move against Sakharov must be seen against a whole wave of repressions against a wide spectrum of human rights activists over the past few months, the fact that the authorities felt able to renew their challenge to the Academy may suggest that the strength of the Academy has somewhat waned in comparison with 1973. Certainly, it is Dzherman Gvishiani, a representative of the State Committee who will lead the Soviet delegation to next month's Hamburg Forum — an event which, incidentally is still going ahead, although as one of the organisers told *Nature* "current political tensions will naturally be reflected there in some way". □

Andrei Sakharov

US cuts back on official exchanges with USSR

THE US scientific community has been almost unanimous in condemning recent moves by the government of the USSR, in the occupation of Afghanistan and the internal exile of physicist Andrei Sakharov.

But while many support the legitimacy of individual protest, there remains uncertainty over how far official reaction should undermine the network of bilateral exchange agreements which have, in recent years, begun to produce mutual benefits.

On the one hand, Congressman George Brown, last week introduced a bill calling for a one-year moratorium on US-Soviet exchange except those essential to national needs or a matter of extraordinary circumstance of individual conscience.

The Carter administration, however, while keen to make clear to the Soviet government that it is not 'business as usual' for scientific exchanges, is also concerned to maintain what it can of the existing framework for scientific cooperation.

Thus all high-level meetings between scientists and scientific administrators from the two sides are being deferred. Three such meetings have already been postponed since the occupation of Afghanistan, and all current exchange arrangements are undergoing close review.

But the administration is at present keeping the door open for low-level contacts between scientists which it considers to have a purely scientific or humanitarian purpose. For example, a scientific delegation from the USSR studying the biological control of pests has been allowed in — but a delegation coming from the USSR to discuss science policy has not, since it was lead by a deputy minister.

The administration's determination to keep open channels of communication, even if limiting activities in these channels, was supported by Academy President Dr Philip Handler, who will be leading the US delegation to the scientific forum in Hamburg later this month to discuss the progress of the Helsinki accords of 1975. But he emphasised the Academy's position that the main responsibility lay on the shoulders of individual scientists.

Uncertainty over how tough the administration should be is reflected in the debate over what type of strategy the US delegation should pursue at the Hamburg science forum.

Few currently support a complete boycott of the meeting. And there seems general agreement that human righs considerations are central to international scientific communication and exchange.

But given agreement that the forum should discuss, in Dr Press's words, "the context within which scientific cooperation takes place, not simply those scientific subjects which are amply discussed in many other settings", opinions are divided on what such discussions should aim.

Dr Paul J Flory, for example, Emeritus Professor of Chemistry at Stanford University and a member of the US delegation, suggested that the scientific community should "reshape its criteria for participation" and lay down a set of minimum conditions for future international collaboration.

These might include the condition that "negotiations should be in the hands of scientists, not governments", and that participants be selected "without regard for their political conformity, race or ethnic background".

Others have suggested more modest goals. In particular Dr John T Edsall of Harvard University, chairman of the American Association for the Advancement of Sciences' Committee on Scientific Freedom and Responsibility, has suggested that the forum should establish a working group to collect and review reports about obstacles to international scientific cooperation. **David Dickson**

Soviet Union responds to western reaction

WITHIN a few minutes of the news of Sakharov's exile reaching the West, the Royal Society sent a cautious telegram to the Soviet Academy of Sciences, deploring the action and asking for the Academy's comment. Six days later, the Academy made its opinion known in a formal statement by the Presidium.

The statement condemned the activities of Academician Sakharov as being directed "against the interests of our country and the Soviet people, actions that help to build up international tension and bring into disrepute the exalted title of Soviet scientist". Ironically, the Presidium's statement accuses Sakharov of "undermining" precisely those ideals of peace, arms limitation and detente that underlie his whole involvement with human rights. Last year, indeed, he spoke out resolutely against overenthusiastic efforts to link US willingness to sign SALT-2 with human rights in the USSR.

Nevertheless, the statement contained no indications that moves would be made to expel Sakharov from the Academy. While feelings of solidarity may so far have prevented Sakharov's fellow-Academicians from expelling him from their ranks, there is little doubt that scientists of dissident outlook and lesser prestige now feel themselves under increasing threat. Andrei Tverdokhlebov, the young physicist, who had formerly worked closely with Sakharov in the human rights movement was expelled to the West just in time to testify before last week's congressional hearing in preparation for the Hamburg scientific forum. And a number of Jewish refusniks, notably Naum Meiman the mathematician and Aleksandr Lerner the cybernetician, have expressed fears for their own liberty.

Meanwhile, the Soviet authorities are attempting to counter western concern by denying that Sakharov has been, in any sense, exiled. "Administrative banishment", said one Tass statement "is neither 'arrest' nor 'exile'." Gor'kii, said comentator Yaroslav Khabarov on the foreign service of Moscow radio, "is one of the most beautiful towns in Russia, as well as a major industrial, cultural, and scientific centre with its own university". By simply stripping Sakharov of his awards and titles, and "moving him outside the city of Moscow", Khabarov explained, Sakharov has been given "the opportunity to continue to work in keeping with his profession". **Vera Rich**

Sakharov

SIR—Your journal of 11 September contained a short article by Vera Rich about a conference in the Hague devoted to the "Sakharov affair". On the basis of this article (and I have no grounds for doubting its accuracy, so my grievances are not directed at the author and editors) I was left with the impression that the participants of the conference had received distorted information. This information then reached the pages of your journal. Factual inaccuracies in the description of my situation at the conference and other significant facts and a kind of general demobilizing trend of these inaccuracies aroused perplexity and anxiety, causing me to send this letter to your journal.

I suppose that the participants of the conference and its organizers sincerely wished to help me and others who are suffering from repression, but excessive haste and other errors in its organization have almost cancelled its usefulness.

In my time I have taken part in defending the organizer of the conference, Dr Shtern, but I have never made his acquaintance. I have had no communication with him during the preparation for the conference or at any other time. I am also not acquainted with Dr Lozansky and did not meet him at the Jewish seminar of "refusnik" scientists in Moscow. Shortly before the start of the conference Dr Shtern contacted my representative abroad, Efrem Yankielevich, with a request for him to participate in the work of the conference. Yankielevich declined, expressing some apprehension concerning his unpreparedness, though he submitted some concrete advice about desirable witnesses (which, as far as I know, was not used). Yankielevich also commented on the excessively pretentious (in his, and my, opinion) title of the conference.

The article, however, does not refer to Efrem Yankielevich but to his wife, my adopted daughter Tanya Yankielevich (erroneously called Tamara), who it seems objected to "garish" actions, as if they might interfere with the plans of the Soviet authorities to return me to Moscow. This is not simply an inaccuracy, but something outrageous. A stance that is completely alien to us is ascribed to a person close to me, and thereby, indirectly, to myself also.

I, and persons of a kindred spirit, regard openness as the only weapon in the fight for justice. The plans of the authorities are known to no-one. Our actions and statements must be based on fundamental considerations as we perceive them, without accommodating ourselves to these plans, which are unknown to us, or to anyone's ambitions or other considerations of the political situation. This applies both to my defence, and to the defence of other victims of repression, and to general questions such as disarmament and international security, human rights, environmental protection and nuclear power.

Now to some concrete inaccuracies. In Moscow, publication of preprints of my articles in English was forbidden, and the manuscripts were removed by the KGB from the office of the Institute, evidently to preclude any possibility of acquaintance with my work in the West. The preprints were published at Stanford University, and I am deeply grateful to my colleagues there. I am also very grateful to my colleagues in the USA and in other countries who send me their preprints. The Russian texts of my articles are published in *Zh. éksp. teor. Fiz.* in June, August and September, according to normal schedules. I am confident that the Politbureau has had nothing to do with these publications, and, on the whole, references to the Politbureau without stating the sources of information seem unfounded.

My isolation in Gor'kii is of an entirely different nature than appears from the conference. There is no scientific library opposite my home, to which I might have access. Opposite there is only mud and piles of rubbish. I have no contact with scientists in Gor'kii, not because there are only secret institutes here, but because I am in a state of almost total isolation, deprived of the possibility of meeting anybody at all apart from my wife and two people from Gor'kii, who obtained permission for this from the KGB, and one visit from my university colleague, also by permission of the KGB. Any others are kept away by a militiaman, on duty round the clock, one metre from the door.

I don't even get to know about the majority of visitors, and they have great trouble. After some time I merely learn of people who are close to me. Our friend and doctor, who travelled from Leningrad, was not admitted, nor was our 82-year old aunt from Moscow. They do not even admit my son's fiancée, who has lived with us nearly three years — Liza Alekseeva. The authorities will not allow her out of the country to join the person she loves, she is subjected to persecution, threats of physical and legal reprisals. Fear for her, for her life compels my wife to divide her time between Gor'kii and Moscow. Liza Alekseeva has become a hostage of my public activity, and now her defence is, perhaps, the most realistic form of supporting me.

To describe my situation, I might add that I have no telephone and it is not possible to make a call from a post office; I am deprived of the medical aid of those doctors who used to treat me; my correspondence is carefully inspected by the KGB and only a fraction of correspondence reaches me; in the house where I live there is a personal radio jamming device, which was even in operation before jamming of radio transmissions was resumed in the USSR.

In July my wife found two KGB agents in the flat, who had entered through a window and, without the knowledge of the militiaman on duty, rummaged through my papers, and erased tape recordings. Illegal entry like this, the purposes of which may be even more dangerous, has happened before. I have not received a reply to a single one of my letters or telegrams to officials. Two months ago I sent a letter to the vice president of the Academy of Sciences of the USSR, E. P. Velikhov, and would like to hope for a reply.

The Soviet press, Soviet representatives abroad and some of my Soviet colleagues during foreign missions, in contacts with people in the West who are concerned about my fate, in an attempt to disorganize my defence, assert that I am against détente, have spoken out against SALT, and have even permitted the divulgence of state secrets; they also emphasize the mildness of the measures taken against me. My attitude, and open way of life and actions are well known and show how absurd these accusations are.

I have never infringed state secrecy, and any talk of this is slander. I regard thermonuclear war as the main danger threatening mankind, and consider that the problem of preventing it takes priority over other international problems; I am in favour of disarmament and a strategic balance, I support the SALT-II agreement as a necessary stage in disarmament negotiations. I am against any expansion, against Soviet intervention in Afghanistan, but in favour of aid to refugees and the starving throughout the world. I regard as very important an international agreement on refusal to be the first to use nuclear weapons, concluded on the basis of a strategic balance in the field of conventional weapons. I am convinced of the interrelatedness of international security and defence of human rights, and am in favour of freedom of convictions and exchange of information, freedom to choose one's country of domicile and place of domicile within that country, and freedom of religion. I am deeply anxious about the fate of political prisoners in the USSR, unjust courts, illegal repression. The most important aim for me is the release of prisoners of conscience throughout the world, including the USSR, the countries of Eastern Europe, and China.

I do not make it my task to give special support to the viewpoint of Western governments, or anyone else, but express precisely my own viewpoint on matters causing me anxiety. As for the mildness of the measures taken against me, they are not as severe as the terms of imprisonment lasting many years for my friends and scientists— prisoners of conscience Sergei Kovalev, Yurii Orlov, Toli Shcharanskii (Anatoli Shcharansky), Tanya Velikanova, Viktor Nekipelov, nor as the fate of those awaiting trial — Aleksandr Lavut, Leonard Ternovskii, Tanya Osipova and many others.

But my banishment, without trial in infringement of all constitutional guarantees, the isolation measures applied, interference of the KGB in my life, are completely illegal and inadmissible as an infringement of my personal rights, and as a dangerous precedent of the actions of the authorities, who are casting aside even that pitiful imitation of legality in the persecution of dissidents that they displayed in recent years. Only a court has the right to establish that a law has been infringed and to define the manner of punishment. Any deliberations about culpability and mercy without a trial are inadmissible and against a person's rights.

Therefore I insist on a public trial, and attach fundamental importance to this. It is this that should have been heard at the conference in my defence, and not the greater or lesser mildness of the measures taken against me. Unfortunately, albeit unwittingly, the conference adopted the formulation of the problem that is most advantageous to the Soviet authorities, but even then there were the factual inaccuracies stated above.

I do not know how the fate of others was described (this is not mentioned in the article), but the report concerning the only other person apart from myself mentioned in the article — Dr Gol'fand — is incorrect. He was not dismissed from his work in connection with an application to leave for Israel, but long before he decided to emigrate. Those colleagues of his in the West who are helping him in his struggle for the right to emigrate will, I fear, be misinformed and will cease their efforts after reading such a cheerful paragraph about his reinstatement.

In conclusion I should like to draw attention to the possibility of obtaining accurate information on my situation and attitude directly from my statements and from the reports originating from my wife and from my representative abroad, Efrem Yankielevich.

ANDREI SAKHAROV

6 October 1980,
Gor'kii 137, Gagarina 214 kv.3, USSR

Reforming Soviet research

Soviet research is in good shape and even good spirits, but would be more effective if its managers paid more attention to the needs of its researchers and the framework in which they operate.

THIS brief survey of the state of science in the Soviet Union is informed by several prejudices, of which the more obvious are now listed.

First, science is international in the sense that even researchers of different nationalities and with different short-term goals (publishing a paper, making a bomb) have identical long-term interests (finding out). That is why it is has been a tragic loss for the West as well as for the Soviet Union that, for half a century, links between the Soviet enterprise in science and that elsewhere have been needlessly attenuated.

Second, the election of Mr Mikhail Gorbachev as General Secretary of the Communist Party of the USSR in March 1985 is the best thing to have happened in the Soviet Union, 70 years old next week, for at least 60 years; it may also the best thing to have happened to the world elsewhere since the end of the Second World War. Mr Gorbachev wants to reconstruct the Soviet Union and to make it prosperous, but to keep it socialist. Can anybody elsewhere pretend that the world would be a safer place if the Soviet Union were bankrupt and socialist?

Third, nobody can tell whether Mr Gorbachev's venture will succeed. The odds of history are against him; Russia changes only slowly. The fact that the intellectual community in the Soviet Union seems to be solidly behind him does not assure his success.The next few months, before the *ad hoc* party conference next June, will be critical.

Fourth, the Soviet government is (or should be) downcast that the attention given since 1917 to higher education, science and technology has left the Soviet Union with a civil industry which is inefficient and incapable of meeting social needs.

Science and technology have been cherished from the outset of the Soviet state precisely in the belief that only science and technology would create the resources required for the sustenance of a socialist state. Why has that calculation gone so wrong?

The achievements of Soviet science are a curious mixture of success and failure. Even before the revolution, Soviet science had made a distinctive mark (remember Mendeleev?). Since, there has been a long succession of distinguished Soviet practitioners and a long roster of practical achievements. Especially in mathematics and physics, but more recently in other fields as well, Soviet researchers have won great respect from their colleagues elsewhere. There are many ways in which the temper of Soviet science as practised at the best institutes and universities is directly in the tradition of old-fashioned European scholarship; undistracted by the scramble for research grants, unworried by doubts about their tenure of office and secure in the knowledge that their institutions will survive, able men and women devote themselves wholeheartedly to scholarship and the intellectual welfare of their students (also a part of scholarship).

Other Soviet researchers, equally distinguished and sometimes as well-known academically, have shown the Soviet government the recipes for making several startling machines that function successfully — nuclear weapons of various kinds, rockets that send satellites into orbit reliably and nuclear submarines apparently no less functional than those of other manufacturers (of which there are not many). Even those who wish these machines did not exist must agree that they play a crucial and legitimate part in the defence of the Soviet Union. Why cannot a technical enterprise so technically competent arrange for Moscow to have a reliable supply of toothpaste?

The failure to deliver the economic growth the Soviet state and now (with *glasnost* increasingly) the Soviet people look for is not the fault of Soviet science but of the economic environment. By declining to make science a scapegoat for obvious economic failure, all Soviet governments since the Second World War have implicitly acknowledged that. Mr Gorbachev's strength is that he has gone further than his predecessors in his appraisal of the magnitude of the Soviet Union's economic failure and in recognizing the dangers inherent in its continuation. He puts much of the blame on the shoulders of the 15 million bureaucrats, planners and decision-makers, intervening between the two halves of civil industry — production plants and their customers. What must worry him, as it does his supporters, is the difficulty of creating a link between the two halves that will give the former an incentive to produce what the latter want that will not be mistaken for ideological backsliding.

If Soviet science is not to blame, what can it do to help? Readers of the pages that follow will recognize that the Soviet research community abounds with talented people working imaginatively on important problems, just as in many other places. That impression is deliberately intended. But it should also be clear that the organization and management of Soviet science, unfamiliar to Westerners, also leave a great deal to be desired in Soviet terms. It is, for example, strange that a system attaching great importance to planning should allow the distribution of its talent among institutes and even geographically to be determined largely by the chance relationships of young men and women still at university with their teachers. There is indeed a danger that if the economic climate in which it operates were more efficient, Soviet science would not be able to meet the challenges that would face it. Moreover, the academy's programme of modest reform (see p. 781) is entirely insufficient to avert that risk.

Here is a brief list of some of the obvious defects of the present system, and some suggestions as to how, in Soviet terms, they could be remedied.

Mobility. Immobility within the Soviet research enterprise is stultifying. Outstanding people may be able to force their way to other institutes or even to persuade the authorities to let them create new institutes of their own, but most working researchers must reconcile themselves to staying for the remainder of their careers at the institutes to which they are first appointed. The University of Novosibirsk is even proud that half of all its physics graduates are still working in the town (most of the rest having been shuffled off into less exalted jobs). There is every reason to expect (and plenty of anecdotal evidence to confirm) that people thus placed are unlikely to be best placed, and are in danger of going to sleep, becoming malcontents or both. Academy administrators, saying there is "no problem", list the schemes by which people wishing to change jobs can, for example, look for others willing to swap apartments (a lottery that may succeed in 3 to 5 years or never). Within its own terms, it is mystifying that the academy does not follow other large employers of skilled labour in advertising all vacancies at all levels throughout its system and arranging that they are filled by merit from among all applicants, providing necessary assistance with relocation.

Deprivation. The days have gone when all practitioners of Soviet science were privileged. Now, only academicians and the similarly placed are privileged. Young people embarking on their careers are especially encumbered by the hardships of daily

Soviet life, but the establishment seems curiously indifferent to their hardship. Those who succeed against the odds are likely to be exceptionally able people (although nepotism may also do the trick, and often does, for friendships forged in adversity endure). But even those ground into the dust by adversity remain on the books. The official view is that, with last year's 25 per cent increase of university salaries (on a par with those of full-time researchers), "we are hoping it is our turn next". Should not a large employer of scientific labour anxious to get the best out of its workforce be more active in seeking the removal of impediments to its efficacy?

Injustice. Thoughtful enthusiasts for *perestroika* say that economic reform may be less urgent than social reform, especially that of the Soviet judicial system. A similar truth obtains within Soviet science. The most obvious cases of injustice are those in which Jewish people who apply for visas to emigrate to Israel usually (but not always) lose their jobs, in flat contradiction of the provisions of the Soviet constitution guaranteeing national equality (where a person's nationality is usually defined by the republic in which his parents were born, but for Jewish people is defined as 'Jew' and so recorded in his internal passport). A more common source of trouble is the arbitrary administration of the system for deciding who shall travel abroad, especially to the West. Although it seems that other government agencies and even organs of the party influence these decisions, they are administered (for academy staff) by the academy, which seems not to appreciate how arbitrariness can corrode morale. Few will deny the Soviet government's right to deny exit visas to some of those who apply for them, or the academy's right to decide when its employees can be spared to travel, but surely the government could insist that its nondiscriminatory laws are obeyed and the academy could arrange that its administration of its relations with the outside world is transparent and fair. The matter is the more urgent because of the shabby treatment of Sakharov in his exile.

Isolation. Much could and should be done to relieve the self-imposed isolation from which Soviet science suffers, and which explains why Soviet science is elsewhere less well respected than it should be. Simple stratagems would help, such as a courier service to the West to circumvent the delays that impede communications with Western journals. (Inward mail can take up to a month to reach its destination.) The bumbledom of the procedures for winning permission to publish abroad (increasingly a formality with *glasnost*) could surely be substituted by an institute director's fiat without compromising the Soviet Union's legitimate interest in commercial and military secrecy. More flexible policies on travel would plainly also help, although it is becoming clear that the cost of travel will be a handicap unless Mr Gorbachev can find a formula for currency reform that gives rubles real value. Other urgent problems of liaison are not even considered yet: what, for example, can be done to connect under-computerized Soviet science with the electronic data networks now well-established elsewhere?

Impoverishment. The performance of Soviet science is sadly hampered by equipment deficiencies on a scale too great to be cured by purchases from abroad, at least while rubles are what they are. Moreover, while the interest of individuals in research institutes is as variable as at present, it would be wasteful to arrange that everybody has the most modern tools for research. In the long run, this serious problem will be solved only when the ills of Soviet industry are cured. Meanwhile, there is an urgent need that the Soviet scientific enterprise should have more natural access than is provided by the often arbitrary allocations of hard currency to the international market in instruments and equipment. Plans that academy institutes with a present interest in the development of equipment should be free to make deals with companies overseas will unfortunately not produce results quickly. The capitalist solution of the problem would be for the Soviet Union to buy an instrument company elsewhere, using it as a means of selling its own designs internationally and using the proceeds for buying what it does not manufacture. Should not the academy be pushing for some solution of that kind?

Mismanagement. The sheer scale of the Soviet research enterprise means that its management must be a complicated task. The need that research institutes must be more self-reliant than in the West, ensuring that there is housing for their staffs for example, is a further complication. In the circumstances, it is inevitable that most attention should be spent on major projects, as in space research, plasma physics or high-energy physics.

By contrast, the management of more routine endeavours tends to be skimped. This, at least, is the best explanation why second-rate projects appear to survive indefinitely once begun, while there is a great deal of needless duplication within the system. (Most biology institutes, for example, support programmes of cell membrane research not easily distinguishable from each other.) It is asking too much of the academicians who function as heads of the academy's 17 divisions to deal with all these issues decisively. For the long run, there may be more promise in the scheme now being tried for supporting research in high-temperature superconductivity by asking interested laboratories to compete for funds. It will be interesting to see how this first national competition for research grants is adjudicated; as things are, the Soviet Union is the only major scientific enterprise in which such competitions do not exist.

Two other structural issues need attention, of which the chief is the conflict of interest arising within the academy's own operations. As things are, the academy is both an agent of research and a group of distinguished people which, naturally, has an important (and apparently welcome) collective influence within the Soviet government. Both functions are important, but not easily reconciled with each other. Saddled as it is with the research organization as it has evolved, the academy is not well placed to diagnose its faults and to remove them. It is also hamstrung by the plain truth that people chosen for their distinction in research are not necessarily able managers of the sprawling research enterprise. In the late 1960s, some academicians argued vigorously against the temptation to let the academy become the Soviet government's ministry of science, but events have gone against them. Now the academy is veering towards the election as academicians of people whose skills are managerial, which in the long run may undermine its influence and its usefulness. Either the academy or its masters should ask whether it would not be more effective if it shed its managerial responsibilities, concentrating instead on shaping the pattern of research, perhaps by the administration of a research grants scheme. Such a move would also diminish the often deadening influence of grand men in the Soviet scheme of things.

A better balance between the universities and institutes in the conduct of research is also urgently needed. The Soviet Union is unique (even compared with China) in its dependence on formal institutes rather than universities as the agents of research. Nobody would suggest that universities should have a monopoly of basic research, but there is now good reason why Soviet universities should have a better crack at the whip. The potential benefits for the quality of students' education should be evident, but it would help enormously to loosen the over-rigid framework of Soviet research if a measure of plurality were built into the system. Part of the present trouble is that the system is literally self-perpetuating; the numbers of young people qualifying as researchers is determined by the academic influence of those already working in the same field. In the circumstances, breaking new academic ground is bound to be difficult. It is remarkable that Soviet science has done so well. But would it not be even better placed if young men and women were not committed to some specialized field of science more or less from the beginnings of their careers? And is this not what the spirit of *glasnost* requires? □

A man of universal interests

Andrei Sakharov

The death of Ya. B. Zel'dovich robs physics of one of its guiding lights of the twentieth century. Here Andrei Sakharov reflects on his life and his science, while on p.673 appears a report of two meetings written by Zel'dovich, in collaboration with A. A. Starobinskii, shortly before his death.

On 2 December, 1987, Academician Yakov Borisovich Zel'dovich died suddenly from a heart attack. He was an outstanding physicist, who made enormous contributions to many branches of science and technology. I am not a specialist in every sphere of his activity (indeed, he was probably unique in the breadth of his interests), and hence I will touch on certain sides of his work only in broad outline. From 1948 to 1968, however, the two of us worked closely together, and I know a great deal of about that period of his life.

In his memoirs, Zel'dovich states that in 1931 (when he was a 17-year-old laboratory technician in the Institute of Processing of Useful Ores) he went on an excursion to the laboratory of chemical physics of the Leningrad Physical-Technical Institute and got into discussion with some of the staff on the forms of crystallization of nitroglycerine. He was invited to work in the laboratory in his free time, and soon afterwards he transferred there officially. So began his path in science, which was to last for 56 years. In 1936 he defended his dissertation for the degree of Candidate of Sciences; later he wrote of the "happy times when permission to defend [a Candidate's dissertation] was granted to people who had no higher education".

In the 12–15 years following his move to the Physical-Technical Institute, Zel'dovich carried out outstanding work on the theory of combustion and detonation, adsorption and catalysis, the fixation of nitrogen, chemical chain reactions and (before, during and after the war) reaction technology. His interest in chemical physics lasted his entire life, and his last work now seems to have been a return to this first love.

The discovery of uranium fission changed Zel'dovich's scientific destiny, as it did that of many other scientists, years before — on a far wider scale — it changed the destinies of us all. His pioneering researches, in collaboration with Yu. B. Khariton, on the theory of explosive and controlled fission chain reactions, were simultaneously the last to be published in the open literature until the veil of secrecy was lifted from the subject. They had a great influence on everyone working in this field. From the very beginning of Soviet work on the atomic (and later the thermonuclear) problem, Zel'dovich was at the very epicentre of events. His role there was completely exceptional.

In the middle of the 1950s, Zel'dovich found himself a new area of activity — first of all, the theory of elementary particles (in which, soon after the end of the Second World War, there had been a breakthrough, and which has developed up to the present time), and then, in the 1960s, the no less dynamic and captivating field of astrophysics and cosmology.

Novosti

Ya. B. Zel'dovich, 1914–1987.

In 1955, Zel'dovich and S.S. Gershtein together put forward the hypothesis of the conservation of the weak charged vector current. This idea (which was discovered independently by Feynman and Gell-Mann) played an important part in the formulation of the theory of weak interactions and the unified theory of weak and electromagnetic interactions, and also in what became known as 'current algebra'. In another paper of the same period, Zel'dovich predicted the existence and certain properties of the Z^0 boson.

For the last 25 years of his life, astrophysics and cosmology had a central place in the thinking of Zel'dovich and his pupils. He was universally acknowledged as a world leader in the field — for the exceptional clarity and concreteness of his physical thinking; for his intellectual daring as a theoretical physicist, which was applied with equal facility to physical laws and theoretical methods, to the formation of Liesegang rings in test-tubes, to the grandiose processes of the explosion of a supernova with the formation of a neutron star or black hole, and to even more extreme processes of the cosmology of the early Universe; and for his closeness to observations. I shall enumerate in no particular order a few of his contributions to astrophysics and cosmology.

In Zel'dovich's work of 1967, there is a formulation of the problem of the cosmological constant. According to Zel'dovich, it followed from the theory of elementary particles that this constant is small or equal to zero. At the present time, the cosmological constant is one of the central problems in attempts to construct a unified theory of all fields and interactions. In publications with Ya.A. Smorodinskii and S.S. Gershtein, Zel'dovich considered the cosmological limits on the masses of the electron and muon neutrinos. These contributions are examples of the new directions in science that arose during the 1960s, that lie at the junction of cosmology, astrophysics and the theory of elementary particles, and that to a considerable extent are associated with Zel'dovich. Here, the entire Universe plays the role of a gigantic laboratory. Zel'dovich's works on the generation of particles in a gravitational field are of great importance, and include a joint paper with Pitaevskii which contains a remarkable discussion with S. Hawking. The effects of polarization of a vacuum make possible the generation of particles by a classical field. Soon after this discussion, Hawking himself published his famous theory of radiation evaporation of black holes. The effects of polarization of a vacuum (quadratic conformal anomaly), according to the theory of Zel'dovich's colleague, A.A. Starobinskii, lead to cosmological solutions without singularity. (I don't give this formulation my wholehearted support and would propose instead that the main causes of inflation are 'false vacuum' effects.)

Closer to classical astrophysics, but nonetheless important, are Zel'dovich's publications on neutron stars and black holes, and accretion and radiation in solitary objects and binary stars. In his first paper on black holes (1964), he put forward the idea of observing black holes by the radiation from material moving in its gravitational field (simultaneously and independently, a similar proposal was published by Salpeter). Soon afterwards, there followed his work in collaboration with O.Kh. Gusseinov on radiation in binary systems, one component of which is a black hole. In a work of 1964 (in collaboration with M.A. Podurets), and in a number of subsequent papers, he considered the dynamics of neutron emission during the formation of black holes. As a result, black holes became accepted as really observable objects, and appropriate observational programmes were worked out and began to be implemented. These programmes, as is well known, have already yielded very interesting results.

Another important aspect of Zel'dovich's interests (in which A.G. Doroshkevich, I.D. Novikov, R.A. Sunyaev, S.F. Shandarin and others also took part) was the formation of galaxies and galaxy clusters. He established the nature of the spectrum on initial perturbations of density, which have observable consequences, and he predicted singularities of the large-scale structure of the Universe — according to that theory, there are gigantic 'black regions' free from galaxies and filled with hot low-density gas of 'pregalactic' composition. There are data suggesting that this is really the case, although the question must still be considered open.

Zel'dovich and his colleagues analysed cosmic electromagnetic radiation and proposed experiments to be carried out in a range suitable for the discovery and investigation of relict radiation. These ideas were not generally known abroad and were not properly exploited by those who did know them. Immediately after the discovery of relict radiation, Zel'dovich recognized it to be of enormous importance, not only as a confirmation of the hot model of the Universe but also as a powerful means of investigating many other important questions in cosmology and astrophysics. In a number of publications (in collaboration with R.A. Sunyaev and others) he examined the effect of various cosmological factors on the anisotropy of relict radiation. As is well-known, this line of investigation has acquired great significance.

In his last years, Zel'dovich published works in which there was an especially profound reflection on the interconnection of the theory of elementary particles and cosmology, considering cosmic domains, and cosmic structures, the astronomical consequences of the rest mass of the neutrino and other postulates of the theory of elementary particles. He attempted to indicate the outlines of what he called 'full cosmological theory' (corresponding to the question of the character of the pre-classical, that is, the quantum-gravitational, stage of development of the Universe, and the origin of the qualitative and quantitative characteristics of the classical stage, including the polytropy spectrum of 'initial perturbations').

All his life Zel'dovich was at the leading edge of science, and he was always surrounded by people. His effect on his pupils was remarkable; he often discovered in them a capacity for scientific creativity which without him would not have been realized or could have been realized only in part and with great difficulty. An essential factor here was his scientific style and personality — his immense energy, his sensitivity to what was new, his intuition, his striving for theoretical simplicity and elegance, his scientific honesty, and his readiness to admit his own error or acknowledge the priority and correctness of another. In science, Zel'dovich was a humble man (although the manner in which he sometimes took part in discussions, defending what he considered to be the scientifically irrefutable, could give a somewhat different impression). He was almost childishly delighted when he had managed to achieve some important piece of work, or had overcome a methodological difficulty by an elegant method, and felt failures and errors keenly.

To Zel'dovich, it often seemed that he was not professional enough in this or that field, and he summoned up greater efforts to fill in these gaps. Here, too, his approach was creative. He often found new, more comprehensive ways of describing and dealing with a problem. So there came into existence numerous papers and articles of a pedagogic nature, and monographs and books (more than 20 of them) which always included much that was original. His books *Relativistic Astrophysics*, *Theory of Gravitation and the Evolution of Stars* and *The Structure and Evolution of the Universe* (jointly with I.D. Novikov) acquired great renown. It is impossible to overestimate the importance of this side of his work, which helped a great many people to come to science by the most direct route.

Special note should be made of his book *Higher Mathematics for Beginners*. In one of his papers, Zel'dovich wrote: "The so-called 'strict' proofs and definitions are far more complicated than the intuitive approach to derivatives and integrals. As a result, the mathematical ideas necessary for an understanding of physics reach school-pupils too late. It is like serving the salt and pepper, not for lunch, but later — for afternoon tea". I whole-heartedly agree with him on this point.

As I have already said, for many years I worked closely with Zel'dovich. This was a relationship of comradely goodwill, mutual readiness to help and strenuous work towards a common aim. There was no negativism, hostility or signs of unhealthy competition (this, moreover, although the group of I.E. Tamm, to which I belonged, had 'fallen in' to an already well-established team from outside). In the 1950s and 1960s, our offices and homes were next to one another, and several times a day we would get together to consider a basic theme; we would also talk about general scientific problems. Often we discussed interesting physical and mathematical trifles (what I call 'amateur problems'). Sometimes we played games, as it were, competing in the speed and elegance of solutions (the one who solved it first ran to tell the other at any hour of the day or night).

Much of my own work on fundamental science had its beginning in these contacts. Here is one example. Once Zel'dovich rang me up and said that he had been lecturing to one of the Moscow scientific seminars on his work on the cosmological constant (described above) and had met with puzzlement. I immediately appreciated the importance of the problem, and a few days later I rang him up with a further development, the idea of induced gravitation. Zel'dovich received the idea enthusiastically, and, in his turn, wrote a paper which treated electrodynamics in an analogous way. I, too, understood the role played in the problem of the cosmological constant by the compensation of the boson and fermion contributions — unfortunately, neither of us managed to think the problem through as far as supersymmetry.

I should also like to put it on record that Zel'dovich helped a great number of people in a purely personal way, even in matters of everyday life. Of course, one should not assume that in every case Zel'dovich appeared in the best possible light. He was no angel.

My relations with Zel'dovich were not always unclouded. In the 1970s and 1980s, especially in the Gor'kii phase of my life, hurt feelings and mutual coldness crept in. Zel'dovich strongly disapproved of my social work, which irritated and even frightened him. He once said "People like Hawking are devoted to science. Nothing can distract them". I did not understand why he could not give me the help which, given our relationship, I considered myself justified in asking for. I know that all this tormented Zel'dovich. It tormented me too, as described in my memoirs. Today, the events of those years seem like foam, carried away on the stream of life. Unfortunately, after my return from Gor'kii I met Zel'dovich only once or twice, and then in company, and was hardly able to communicate with him in human terms. Yet another lesson is that it is not always possible to put things off to another day.

Now, when Yakov Borisovich Zel'dovich has departed from us, we, his friends and colleagues in science, understand how much he himself did, and how much he gave to those who had the chance to share his life and work. □

Andrei Sakharov is at the Lebedev Physical Institute, Academy of Sciences of the USSR, Moscow, USSR.

New light on the Lysenko era

Valery N. Soyfer

Previously unpublished material shows how Trofim Lysenko both deceived and won the support of Stalin, and destroyed genetics in the Soviet Union.

TROFIM Denisovich Lysenko's life and work have been much analysed and discussed in the world's literature. It is well-known that his activities were among the factors leading to the breakdown of biological, agricultural and, to some extent, medical science in the Soviet Union. They contributed to the transformation of Russia from a country that exported grain and other agricultural products to the whole world into an importer of grain, butter and meat. So the study of the life and activity of Lysenko may illuminate the flaws which, even now, cripple Soviet agriculture — and Soviet science.

Another reason for returning to the history of lysenkoism is that much has so far been unclear and even mysterious[1]. In particular, there is much to be learned from the relationship between Lysenko and Vavilov in 1930–35 and Lysenko and Stalin in 1947–48.

The question of whether Lysenko's earliest works were independent and original contributions to science has often been asked[2,3]. It is generally accepted that they were original and pioneering in character. More recently, Roll-Hansen pointed out how widely this idea is held by remarking[4] that "some of his [Lysenko's] physiological work was highly praised even by his strongest critics among the geneticists". So how independent of previous researchers was Lysenko in his early work?

Lysenko published two articles of little significance as early as 1923, when working at a small plant-breeding station in the town of Belaya Tserkov' in the Ukraine[5]. In 1925, while still at Belaya Tserkov', Lysenko graduated by correspondence from the Kiev Agricultural Institute, but, once he had received his agronomist's diploma, he unexpectedly left for the Caucausus (Azerbaijan), where he found a job as a junior specialist on the selection of legumes, at an experimental station in the town of Gandzha (now Kirovobad). The director of the station, the well-known Russian agronomist N. F. Derevitsky, set Lysenko a problem: to try to acclimatize beans for Azerbaijan as a green-manure crop. In the autumn of 1925, Lysenko for the first time sowed peas as a winter crop. Thanks to the mild winter of 1925–26, they yielded a good vegetative mass. This result was promising. The work should have been repeated on a wider scale, to determine appropriate dates for autumn sowing, to choose the best cultures, to work out the agrotechnical details and to settle on the most appropriate varieties. But at this point, an event occurred that drew Lysenko away from this work and directed his activity into a very different field.

To understand this event, it is important that, from the point at which Soviet power was established in Russia, Lenin and, after him, Stalin did everything possible to train cadres of a new intelligentsia, intended to replace the specialists trained under the tsarist regime. Since the new leaders adhered to the class approach and considered that the so-called "bourgeois specialists" were for the most part hidden enemies of the new order, there was a drive to train persons of worker or peasant origin (the so-called "red intelligentsia"). All paths were opened to members of these classes; special attention was paid to bringing them forward.

Trofim Lysenko satisfied the new criteria perfectly. He had been born in 1898 into a peasant family, and was late in learning to read and write; only at thirteen did he enter a two-form village school. Afterwards, he attended the Poltava Lower Horticultural School for rather more than two years. Only in 1917 did he enter the Uman' Horticultural School, where he was a student for about four years.

The course of Lysenko's studies was far from normal. Between 1917 and 1920, Uman' was in the battle zone between warring armies and changed hands several times between the Whites, the Greens (Ukranian peasant insurgents) and the Reds. Taking advantage of his peasant origin, Lysenko then enrolled at the Kiev Agricultural Institute, although he studied there externally and, as a result, was not able to attend regular higher education courses. But the circumstance that he graduated not from a bourgeois, but from a Soviet, higher-education institution ensured that he continued to benefit from all the advantages guaranteed by the Soviet leadership to persons of peasant origin.

So it is hardly surprising that when, in 1927, the well-known Moscow journalist V. Fedorovich visited the Gandzha experimental station, he was presented to the budding agronomist Lysenko. In his article in *Pravda* on 7 August 1927, Fedorovich lauded Lysenko's first experiment with the peas as a substantial discovery, and announced that Lysenko (whom he hailed as a "barefoot professor") had made an outstanding discovery that would allow the peasants of Azerbaijan to avoid all future difficulties due to shortage of fodder. (As it happens, green-manure crops are not cultivated on any scale in Azerbaijan.)

Lysenko's first work thus remained unfinished; no publications other than Fedorovich's article mentioned it. But Lysenko himself considered that the matter had been dealt with and switched to new work. Quick as lightning, he "solved" another problem, namely the

Lysenko, newly appointed president of the Soviet Academy of Agricultural Sciences, surrounded by colleagues at Odessa.

effect of low temperature on plants.

From the autumn of 1926 to the spring of 1927, several of Lysenko's trainees collected data on the germination, development and fruiting of cereals (wheat, rye, oats and barley) and cotton. In Russia at that time, the same problem was being studied by Vavilov's collaborators N. A. Maximov and G. S. Zaitsev[6]. Lysenko's data were presented in long monotonous tables which he furnished with primitive commentaries. They were published in 1928 as a booklet titled *Proceedings of the Gandzha Experimental Station*[7]. Instead of working out his results statistically and presenting the data in summarized form, he devoted 110 of the 169 pages in the book to tables with raw data.

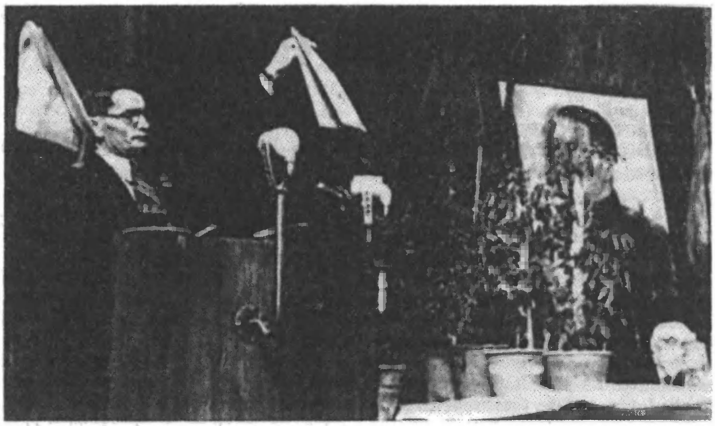
Lysenko finally set out his nine-point conclusion, which can be reduced to the single observation that, for the initial phases of plant development to be completed, definite quantities of heat are required. The method he used was close, even in its detail, to that developed by the talented Russian physiologist G. S. Zaitsev[6] who studied the effects of various environmental factors, not simply temperature, on the phases of development of cotton.

It is relevant and important that, in January 1929, Lysenko attended the First All-Union Congress on Genetics, plant breeding, seed growing and pedigree stock rearing in Leningrad. There he announced that, on the basis of his work, he could recommend the cold treatment of winter-wheat germinants so that it would be possible to sow winter instead of spring wheat[8]. But in the discussion, his proposal that cold germination (he called it 'vernalization') should be put into practice immediately was criticized by Maximov.

Despite the criticisms and the fact that his method had not been tested scientifically, Lysenko managed, in the summer of the same year, to organize a noisy press campaign in favour of his 'vernalization'[9,10]. The agricultural bosses were captivated by the novelty[10] and, in the winter, the official Party organ *Sel'skokhozyaistvennaya Gazeta* (*Agricultural Gazette*) launched a discussion of Lysenko's ideas. At that stage an article by the outstanding Soviet scientist N. M. Tulaikov stated[11]:

> At the beginning of 1927 [at the inception of Lysenko's work on vernalization] at the experimental station at Gandzha, I happened to have a number of discussions with Lysenko ... who was working on the application to various plants of the region of the laws established by Professor G.S. Zaitsev on the sums of the temperatures necessary for the various phases of development of cotton.

Tulaikov wrote that by then, Lysenko had "already outlined ... a certain rule", but the reference to the fact that Lysenko knew Zaitsev's results is quite definite.



Lysenko offers herbarium samples as proof of his claimed transformation of wheat into rye (1948).

Indeed, Lysenko himself, in a book published in 1928, mentioned Zaitsev at three crucial points, comparing Zaitsev's results with his own and acknowledging that they were similar (see ref. 7, pages 16, 136, 142). But all references of this kind afterwards disappeared from Lysenko's works, and he began to insist on his own unconditional priority in the matter. Apparently the chief reason for this behaviour was that, in January 1929, while travelling from Central Asia to Leningrad to the First All-Union Congress on Genetics and Plant Breeding at which Lysenko first put forward his hypothesis, Zaitsev became ill and died in Moscow without reaching Leningrad. Thereafter, Lysenko decided never to mention his precursor again.

The sources of Lysenko's first work have been forgotten with time. In 1937, N. M. Tulaikov, the only living witness of Lysenko's use of Zaitsev's work, was accused of hostile activity. (The charges were given by Lysenko himself in an article published in *Sotsialisticheskoe zemledelie* (*Socialist agriculture*) on 4 April 1934; a week later, his close collaborator V. N. Stoletov published in *Pravda* an enormous article accusing Tulaikov of wrecking.) Soon afterwards, Tulaikov was shot.

M. A. Popovsky in the Soviet Union[12] and D. Joravsky in the West[13] were the first to point out that N. Vavilov was Lysenko's scientific patron. The most authoritative Western biography of Vavilov supports this view[14]. Popovsky cited extracts from Vavilov's statements proposing Lysenko for membership of the Ukrainian Academy of Sciences, for the Lenin Prize and as a corresponding member of the USSR Academy of Sciences.

But Medvedev[15] categorically denies that Vavilov was the one who mainly advanced Lysenko in scientific circles, saying that, as an important administrator, Vavilov might have signed the documents in support of Lysenko without having read them. But the facts unearthed by Popovsky contradict that. Moreover, I have been able to find many previously unknown statements of Vavilov that confirm that it was Vavilov who played the chief role in Lysenko's scientific advancement[1].

Why did Vavilov succumb to this fatal blindness and not recognize Lysenko as a dangerous upstart from the outset? Popovsky takes the view that the explanation lies in the social conditions of the times, and specifically in the demand of the Party of Bolsheviks that people "from the furrow and the bench" should be advanced in all spheres of society, science included. As Vavilov sincerely accepted the revolution, putting himself at its service, he would have acknowledged this "social imperative" and would have seen Lysenko as an obvious candidate to be "promoted from the people".

The logic is tempting, but the proposition only partly explains Vavilov's motives. A more likely explanation lies in the nature of the field in which he worked. In 1925, he had become director of the Institute of Applied Botany and New Crops (later the All-Union Institute of Plant Industry) and had begun to assemble a collection of seeds of wild and cultivated plants from all over the world. The motives for collecting this "gene-bank of world flora" were not exclusively scientific; Vavilov intended to make extensive use of the seeds collected so as to increase the rate at which new varieties of cultivatable crops were introduced. This would be done by the transfer of valuable genes to strains extant in the Soviet Union. Not himself an expert on plant breeding and without much experience of fieldwork, Vavilov could only hope the plan was feasible.

In practice, Vavilov and his colleagues encountered problems that were not easily soluble. The physiological requirements of the seeds collected from elsewhere were different from those of the local varieties. They germinated, sprouted, flowered and fruited in different ways, and many would not germinate at all in the Russian climate. Crossing the "foreign" with the local varieties proved a practical impossibility in most cases. And then, suddenly, there appeared a man who confidently declared that he could solve a far more complex problem — the conversion of winter into spring crops by vernalization (cold treatment).

Lysenko was then working at the Gandzha station, a part of Vavilov's institute, so that it is not surprising that the director soon learned of his work. This apparently explains why Lysenko, whom nobody had heard of, suddenly received the prestigious invitation to take part in the First All-Union Congress on Genetics and Plant Breeding that Vavilov himself had organized. Vavilov was apparently convinced that if the complex transformation of winter to spring crops were feasible, the problem of synchronizing germination and flowering of other plants might be easily solved.

This interpretation is confirmed, in particular, by the fact that, several days before the meeting, Vavilov gave an interview to the Leningrad newspaper *Smena* in which he spoke highly of the idea of transformation[16]. It was also especially important for Lysenko's later career that Vavilov put the utmost value on the experiment for the vernalization of winter wheat, calling Lysenko an outstanding scientist at several sessions in 1930 and 1931 of the Presidium of the Academy of Agricultural Sciences and of the Collegium of the State Commissariat for Agriculture of the Soviet Union[17]. Thus in September, 1931, at a meeting of the State Commissariat for Agriculture, Vavilov said:

> Of especial interest ... is the work of Lysenko, who has actually managed in practice to change late-ripening into early-ripening strains and to convert winter into spring varieties. The facts which he has established are indisputable and are of considerable interest ... Lysenko's experiments show that, with special pre-sowing treatment, late Mediterranean varieties of wheat may be converted into early varieties in our conditions. Many of these varieties surpass our ordinary varieties in quality and productivity ... Rapid organizational collective persistent work is required in order to realize the most interesting facts established by Lysenko[18].

My case is further strengthened by the fact that Vavilov, delighted at the possibility of synchronizing the flowering of different plants, gave instructions for the reserve of wheat at the Institute of Plant Industry to be vernalized and sown near Leningrad and Odessa. (From 1929, Lysenko worked at the Odessa Institute of Genetics and Plant Breeding.) A proportion of the plants of the varieties that did not normally yield ears in the Odessa region gave ripe ears. Lysenko promptly exaggerated this result and presented it as proof that all varieties could then be sown in regions that were abnormal for them. This categorical conclusion greatly delighted Vavilov who, taking it literally, on many occasions hailed the method of vernalization. For example, in 1934, in a paper at a conference on planning genetic-selection work, Vavilov said:

> Perhaps in no other division of plant physiology have there been such profound advances as in this field ... In this respect, we consider the work of T. D. Lysenko to be outstanding.
>
> The relatively simple procedure of vernalization and the possibility of its broad application are opening up broad horizons. Investigations of a world-wide range of wheat and other crops under the action of vernalization have revealed facts of exceptional significance. The world-wide range of wheat varieties has been entirely transformed by this simple procedure[19].

But there had been no *complete* alteration of the worldwide range. What had happened was entirely different — the general acceptance of Lysenko's inflated estimation of his own work. Even in 1935, after five years of trials of vernalization by scientific institutions, scattered over different regions of the vast Soviet Union (vernalization was tested on 35 varieties of wheat), the head of the trials, Academician P. N. Konstantinov, came to this conclusion:

> On average, over the years, what we have observed is that there is here a decrease and there an increase due to vernalization, but on the average, over five years, vernalization has given almost no increment of yield[20].

Vavilov nevertheless continued to place a high value on Lysenko's work. For example, on 17 June 1935, at a meeting of the Presidium of the Academy of Agricultural Sciences, Vavilov said[21], "Lysenko is a careful and highly talented researcher. His experiments are irreproachable". Similarly in 1935, he took part in two meetings in the Kremlin at which Stalin was present and put an overwhelmingly high value on Lysenko's contribution to science and agricultural production[22].

Only in 1936–37 did Vavilov realize what sort of a person his protégé really was. He began an active campaign against Lysenko, but by then it was too late. The "people's scientist" had become Stalin's pet, had acquired enormous power and had begun the methodical annihilation of his scientific opponents. Lysenko had declared war not for the life but for the death of genetics, and of the study of plant hormones and other biological disciplines based on the principles of genetics. He was preaching the doctrine of the inheritability of acquired characteristics. Lysenko even launched an attack on Vavilov himself. As early as 1935, in Stalin's presence, he publicly named Vavilov as an enemy of his own "science". These attacks ended in tragedy; on 6 August 1940, Vavilov was arrested and in 1943 he died of exhaustion in Saratov prison.

The Second World War led to the destruction of agriculture in much of the European territory of the USSR, with the result that the ineffective system of collectivized agriculture could not cope with the ever-increasing difficulties. Worse, in 1946, the Soviet Union suffered enormous crop-failures across all its agricultural zones — Russia in Europe, the Ukraine, Siberia and Kazakhstan. The famine of 1947 is still remembered in the Soviet Union.

Because practically all the major leaders of agriculture had been arrested in the late 1930s, Lysenko became the autocratic 'boss' in biology and agronomy in the Soviet Union. In famine conditions, the government and Party required from him scientific projects that would allow immediate relief from the problems of food and fodder shortage.

But Lysenko was unable to propose anything realistic, and his former promises to produce a sharp increase in yield by vernalization, summer planting of potatoes, intra-strain crossing, changing the selection criteria and the rapid introduction of new varieties were ineffectual. So too were his promises to introduce, in between one and two years, new varieties of wheat for the Ukraine and Siberia, to introduce new strains of cotton, to destroy insect predators by "pasturing" poultry in the fields (the poultry were supposed to peck up the grubs and caterpillars of the insects) and other similar proposals.

Lysenko had also harmed his own cause

Lysenko (right) with party general secretary Nikita Khrushchev and M. Suslov (left) at an experimental farm in 1955.

by his resolute repudiation of plant hormones, and his reputation was undermined when the Dane Vent and the Soviet physiologist N. G. Kholodny were honoured for their discovery.

At the personal level, Lysenko suffered much unpleasantness as a result of his brother's decision to go over to the Nazis and then to defect to the West. In the Soviet Union, the relatives of those who left the country were considered traitors. (There was a special article in the Criminal Code to that effect.)

Lysenko's relationship with the authorities was also affected, immediately after the war, by the degree to which Western society was shocked by the fate of N. I. Vavilov, G. D. Karpechenko, N. V. Timoféeff-Ressovsky and other outstanding scientists whom Lysenko and lysenkoists had publicly attacked and who had then mysteriously disappeared. At that time, British and American leaders, in particular Churchill, made repeated personal appeals to Stalin asking what had happened to Vavilov (Yurii Zhdanov, personal communication, 22 December 1987). Stalin knew, of course, that this outstanding Soviet scientist had been arrested precisely because he had opposed Lysenko. Finally, it is also relevant that, in 1947–48, the major Soviet biologists who were still alive openly declared themselves against Lysenko's new ideas, particularly his assertion that he had found serious errors in the work of Darwin. According to Lysenko, there is no intra-species struggle in nature, but mutual cooperation between plants of a single species.

Lysenko's dethronement was further assisted by Yurii Zhdanov, head of the Science Section of the Central Committee of the Party and son of the influential Secretary of the Central Committee of the Party, A. A. Zhdanov. The younger Zhdanov was openly sympathetic to the geneticists, expressing his scepticism about Lysenko. Among the many letters from geneticists, plant breeders and agronomists, criticizing Lysenko that reached the Central Committee was a long manuscript by Vladimir Efroimson which set out in concise form the details of Lysenko's scientific and organizational errors.

The post-war change of attitude towards Lysenko was reflected in the following events:

■ In 1945, A. R. Zhebrak who, after the arrest of Vavilov and the other plant geneticists, had become the most important specialist in this field, and who also worked in the office of the Party Central Committee apparatus, published an article called "Soviet biology" in the journal *Science*[23]. This amounted to a careful criticism of Lysenko. That there should be an article had been suggested to Zhebrak by the then Politburo member A. N. Voznesenskii, but Zhebrak obtained permission for the project from another member of the Politburo of the Central Committee of the All-Union Communist Party (Bolsheviks), A. S. Sherbakov, head of the Sovinformburo (A. R. Zhebrak, personal communication).

■ Steps were taken to set up a new Institute of Genetics and Cytology within the framework of the Soviet Academy of Sciences, as an alternative to Lysenko's institute. The new president of the Academy, Sergei Vavilov, brother of the Nikolai Vavilov who had perished in Stalin's torture chambers and who had been elected president of the academy on 17 July 1945, helped[24] in this project. The project progressed quite a long way — it was approved by the Central Committee, the appropriate documents were forwarded to the Soviet government, the recruitment of staff began and a source of funds was opened up.

■ Some of the new members elected to the Soviet Academy of Sciences in 1946 were mostly people who had openly expressed their disagreement with Lysenko. There was even a geneticist among them. Furthermore, in 1946, Stalin prizes were awarded to two specialists who were well-known for their negative views of Lysenko: V. S. Nemchinov, for his work *Agricultural Statistics*, and V. I. Edel'shtein for his textbook *Vegetable Growing*.

■ On 9 February 1947, at a plenary session of the Central Committee devoted to agriculture, Lysenko's taboo on hybrid maize was revoked, his proposal to replace winter by spring wheat in the Ukraine was reassessed as "incorrect" and it was noted that there were no good varieties of winter wheat for Siberia (although Lysenko had been trumpeting that such varieties had either been created already, or else were in the process of being created by him personally).

■ According to Zhores Medvedev[2], A. A. Zhdanov went to the Organizing Bureau of the Central Committee with a proposal that the leadership of the Academy of Agricultural Sciences should be strengthened. This, in bureaucratic language, was a proposal to find someone to replace Lysenko as president of Lenin's Academy of Agricultural Sciences.

■ In 1947 and 1948, scientific conferences at Moscow University were attended by enormous audiences (up to 1,000 people at the last of them) at which Lysenko's views on the reform of darwinism were subjected to searching criticism.

■ The final chord of the whole anti-Lysenko symphony was a speech by Yurii Zhdanov, head of the Division of Science of the Central Committee, at a seminar of district committee and provincial committee lecturers in Moscow. Zhdanov's lecture was almost entirely devoted to criticism of Lysenko[27]. Zhdanov said that Lysenko "to a great extent is struggling against shadows of the past", and that, based on his

> very narrow-minded conceptions, he definitely delayed the usage of new hybrid corn in our country. I consider that in this example we have new theoretical narrow-mindedness overgrowing into definite material detriment. And we are compelled to consider critically one or another new supposition proposed by a school of Lysenko.

Zhdanov spoke about the failures of

> . . . the attempts by Lysenko to 'hunt out' new plant varieties for 2 or 3 years, as well as his promise to select for Siberia winter wheat which will be resistant to frost and which will not differ in their stability from domestic varieties.

Zhdanov characterized Lysenko's claims as "undesirable accusations" which brought harm to Soviet agriculture. One of the most important requirements of the lecture was the denial of Lysenko's favourite thesis: the wish to divide the Soviet scientists into bourgeois and socialist specialists.

> It is wrong to think [Zhdanov said] that there is a struggle between two bourgeois schools, one of which represents Soviet views and another bourgeois Darwinism. I think that such a contradiction should be rejected, because discussion takes place between the scientific schools, both of which belong to Soviet science, and neither of these schools can one call as bourgeois.

The final sentence of this lecture was as follows:

> It is necessary to liquidate the attempts to establish the monopoly in one or another division of science, because every monopoly leads to stagnation . . . Trofim Denisovich tried to do many things, and we say to him: Trofim Denisovich, you also did not do many important things. Moreover, you close your eyes on the whole set of new forms and methods of the reorganization of nature.

Such lectures were then of exceptional significance, for they brought to the attention of those responsible for the current Party line the latest resolutions of the leadership, as well as providing ideological directions for the immediate future. Zhdanov's sharp criticism was taken as clear evidence of the inevitable and imminent downfall of Lysenko[25].

Immediately after this lecture, Lysenko decided on a desperate step. Once again, he showed that he was by no means a coward and was an excellent psychologist. Lysenko wrote to Stalin and to the senior Zhdanov saying that he was willing to give up the presidency of the Academy of Agricultural Sciences, not because there were fatal errors in his work, but because all his life geneticists and other opponents of his science ("Michurinist science") had prevented his proposals from being put into practice, had slandered him and were now thriving to such an extent that they had even persuaded Yurii Zhdanov to support them.

Lysenko plainly planned his reply to

Zhdanov's criticism carefully. He wasted few words on the principal offender, Yurii Zhdanov. The chief emphasis in his letter is his undisguised request to be allowed to deal with those who held dissenting views. That was completely in Stalin's style.

Lysenko's letter embodies a double distortion of the truth. He represents himself as an unfortunate little lamb against whom the reactionary geneticist-wolves are sharpening their fangs. (This, of course, was a blatant lie; the geneticists were still reeling from the arrest of many of their leaders, the death of N. K. Kol'tsov and, after the 1936 and 1939 discussions, the general weakening of their position.) But Lysenko also sought to convey the impression that, because of this situation, it was difficult for him to "press forward" with the application of his proposals.

The self-pitying tone of the letter is deliberate. His self-abnegation was calculated to demonstrate his humility and his readiness to carry out instructions unquestioningly. This, he would have known, would have been the safest way of obtaining *carte blanche* to deal with potential and declared enemies.

Significantly, Lysenko did not conceal his intention of dealing with those with dissenting views. That is the relevance of the concatenation of two of his complaints. First, he said, he had been "accused more than once" of having applied "administrative blocks" in the interests of the "Michurinist direction to which [he] subscribe(d)" to the "other, contrary, direction". But then, he continued, "for reasons outside [his] control, this, unfortunately, was far from being the case".

Lysenko's suggestion was that he was not in a position to launch a pogrom against his opponents. His calculation was that the leaders of the Central Committee, Stalin and Zhdanov, who had a propensity for crushing their opponents, would not fail him. He was seeking their agreement for a pogrom in genetics. He understood quite well how to approach Stalin and Zhdanov senior.

Although Yurii Zhdanov's lecture created a considerable stir, it led to no real unpleasantness for Lysenko. But, equally, there was no immediate response to his letter to Stalin and Andrei Zhdanov. So Lysenko resolved on a second course of action. Through the Minister of Agriculture of the USSR, I. A. Benediktov, he obtained a transcript of Yurii Zhdanov's speech. On 11 May 1948, he returned it to Benediktov, attaching yet another note which ended with him suggesting that he should resign as President of the Academy of Agricultural Sciences[25].

Lysenko began by saying that the transcript had been "somewhat toned down" compared with what he had heard, and continued:

Lysenko's appeal to Josef Stalin

To the Chairman of the Council of Ministers of the Union of SSR, Comrade Josef Vissarionovich STALIN "To the Secretary of the Central Committee of the VKPV, Comrade Andrei Aleksandrovich ZHDANOV"
from Academician T. D. Lysenko

It has become very difficult for me to go on working, both as President of the Lenin All-Union Academy of Agricultural Sciences and even as a scientist. Therefore, I have decided to turn to you for help. A most abnormal situation has arisen in agrobiological science.

That there has been, in this science, and still continues a struggle between the old metaphysical line and the new Michurinist line is well-known; that I consider normal.

At present, for obvious reasons, the Weissmanists and neo-Darwinists have adopted a new strategem. Without changing anything in their science, they have proclaimed themselves to be supporters of Michurin and have accused us, who subscribe to Michurinist science and who are developing it, of having restricted and perverted that science. It is also obvious why all this pressure from the Weissmanists and neo-Darwinists is aimed first and foremost at me personally.

Under these conditions, it is extremely difficult for me, as head of the Academy, to go on working.

Up to a point, all this has seemed to me normal and understandable. The test of the validity of the objectives and methods of scientific work in our country is the degree to which they assist socialist agricultural practice. This was the principle from which I, as head, drew the strength to develop Michurinist science and to assist practice as much as possible. This was also the best means of combatting metaphysical arguments in biology.

In spite of the lack of scientific objectivity and, often, the downright slanders to which the opponents of the Michurinist persuasion have resorted, and although it has been difficult for me, drawing inspiration from collective and state-farm practice, I have found the strength in myself to resist the pressure and to go on improving my work in theory and practice.

But now a situation has arisen which has really made me lose heart. On 10 April this year, the head of the Science Division of the Propaganda Section of the Central Committee of the All-Union Communist Party (bolsheviks), Comrade Yurii Andreevich Zhdanov, delivered a lecture at a seminar of Party provincial committee lecturers on the subject "Debatable questions in contemporary Darwinism".

In this lecture, the lecturer poured out against me personally, by name, the false accusations of my anti-Michurinist opponents. It is clear to me that the allegations put forward by this lecturer — the Head of the Science Division of the Propaganda Section of the Central Committee of the All-Union Communist Party (bolsheviks) — were accepted as the truth by the large audience of Party regional committee lecturers. So the falsehoods perpetrated by the anti-Michurinists and neo-Darwinists will have even greater effect in the provinces, both among scientific workers and among agronomists and leaders of agricultural practice. And for the scientists whom I lead, the road to the practical application of their results will be beset with great difficulties. This has come as a great shock to me; it is difficult for me to endure it.

Thus I am turning to you with a request which is very important for me: if you think it appropriate, give me your help in this matter, which seems to me to be of no small importance for our work in agricultural and biological science.

It is untrue to say that I cannot take criticism. This is so far from the truth that, in the present case, I will not discuss it in detail. I have always submitted all my work, both theoretical and practical, to criticism; from that, I have learned to draw what is useful for the cause, for science. All my scientific life has been conducted under criticism, which has been a good thing.

The lecturer had never sent for me, and had never spoken to me personally, although the whole criticism of his lecture was basically aimed at me. I was refused a ticket for the lecture and I listened to it carefully, not in the auditorium, but in another room, on an extension-speaker in the office of Comrade Mitin, the Deputy President of the All-Union Society for the Propagation of Political and Scientific Knowledge.

The essence of the lecture, in my understanding, may be judged even from the very fragmentary notes I made of the concluding part of the lecture. These I enclose separately.

I have been accused more than once of putting — in the interests of the Michurinist persuasion in science to which I subscribe — administrative impediments in the way of the other opposite persuasion. But in fact, for reasons outside my control, this, unfortunately, is far from being the case. The contraints that have been imposed have been on the persuasion which I support, the Michurinist persuasion.

I do not believe it is an overstatement if I say that personally, as a scientific worker and not as president of the Academy of Agricultural Sciences, I have by my own scientific and practical work given no little assistance to the growth and development of Michurinist science.

The principal sorrow and difficulty of my work as president stems from an obligation placed on me which I am profoundly convinced is incorrect — to ensure the development of different persuasions in science. (It is not a matter of different branches of science, but specifically of different persuasions.)

For me, this obligation cannot be undertaken. But I have not been able to block the opposing view, first because such questions in science are not solved by administrative means and, second, because the neo-Darwinist defence has been so strong that I could not have blocked it.

In reality, I have not been the president of the Academy of Agricultural Sciences, but the lone defender and head of the Michurinist persuasion, which is still in a clear minority in higher scientific circles.

The difficulty has been that, as president of the Academy, I have had to present the scientific and practical work of the representatives of the Michurinist persuasion (a clear minority in the Academy) as the work of the entire Academy. *But the anti-Michurinists have not spent their time on creative work as much as on random attacks and calumnies.*

I can assist the development of the most diverse branches of agricultural science, but only within the context of the Michurinist persuasion, that which acknowledges the transformation of living nature arising from the conditions of life and which acknowledges the inheritance of acquired characteristics.

Long ago I accepted and I now subscribe to and am developing Vil'yam's studies on agriculture and Michurin's studies on the development of organisms. Both these studies belong to a single persuasion.

I should be happy if you could find it possible to provide me with the opportunity of working only in this field. Here I sense is my strength and I would be able to make a useful contribution to our Soviet science, to the Ministry of Agriculture and to our Collective-and-State farm experience in its different fields.

Forgive my clumsy style. It is due largely to my current situation.

ACADEMICIAN T. D. LYSENKO
President of the Lenin All-Union Academy of Agricultural Sciences
17 April, 1948

...the lecturer presents as from him personally the old calumnies of the anti Michurinists-Morganists-neo-Darwinists.

This criticism was made in secret from me, so that I would not be able to object and refute it either verbally or in print.

In the corrected transcript, no references are made to the titles of my works, nor to the pages from which the quotations are taken. Hence the reader cannot compare what the lecturer said on each question with what I said. I have already stated more than once that, in these conditions, into which I have been thrust, it is impossible for me to work as President of the Lenin All-Union Academy of Agricultural Sciences.

For the good of agricultural science and its application I seek to raise the question of my release from the post of President and to be given the opportunity of carrying out scientific work. I should then be able to accomplish considerably more to the benefit both of our agriculture and for the development of biological science of the Michurinist persuasion in various branches, including the training of scientific workers.

The tone of this letter is quite different, from which one may cautiously conclude that, by then, Lysenko had some information of Stalin's reaction to his first letter, and that he wished to speed up events. His categorical accusation of Yurii Zhdanov as incompetent and even unscrupulous is a sign of that.

There were good reasons for hurrying events. The Academy of Agricultural Sciences needed to fill several vacancies left by those of its members who had perished in prisons and labour-camps as well as those who had simply died naturally. These were to be the first elections in the history of the Academy of Agricultural Sciences, and many of Lysenko's scientific opponents had been nominated unopposed. If they were elected, Lysenko would certainly lose control over the Academy of Agricultural Sciences.

Two years earlier, Stalin had summoned Lysenko and given him a little bag containing the seeds of an unusual branchy-eared wheat, asking him to consider whether the wheat could be improved. The burst of attention paid to this wheat in the Soviet Union, and Stalin's own sudden interest in it, were typical of that time. Once again, there gleamed the prospect of solving the complex and urgent problem of supplying the population with food simply and cheaply — by introducing just one miracle-variety.

Interest in branching wheat had flared up even before the Second World War, when the news was carried all over the country that a simple woman working on a collective farm in Central Asia, Muslima Begieva, had obtained a wondrous harvest by sowing branching wheat. But after the war, according to Begieva herself, things had gone wrong, and the wheat had stopped branching. However, to make up for this, in Georgia, Stalin's own birthplace, the seeds of this wheat had, it was said, been renewed and improved.

The moment at which Lysenko received from Stalin's own hands the packet of seeds of branched wheat is crucial for the understanding of Lysenko's personality. When Lysenko heard Stalin's wish that branched wheat should be improved, he ought to have refused the request; he already knew perfectly well that this was an impossible task. Stalin, naturally, did not know what a real expert on wheat, as Lysenko always made himself out to be, would have known — that interest in branching forms went back more than a century, both in Russia and abroad.

The Russian scientists N. Shcheglov (1828), M. Spafariev (1837) and many others had described this wheat in detail and had shown that it could not be used to increase the yield per unit area, because its plants would develop normally only if they were sown further apart. So great an authority on wheat as Professor A.M. Bazhanov wrote in 1856 that, "multi-eared wheat, like a pampered child, requires, in addition to rich soil, well-spaced sowing. It cannot endure even slight frost and suffers more than other simple kinds from smut and rust. Although each individual ear yields more grains, compared to the ears of simple grains, when one considers the total yield of all the ears for a known area of land, it is always found that multi-ear wheat has no advantage over the ordinary kinds"[26].

Lysenko, of course, most probably did not know this. He was not conspicuous for his erudition, and had failed to read not only earlier but even much later works. Nor had he paid attention to a number of publications by the pupils of Vavilov during the 1930s. But nevertheless, there are data to show that Lysenko knew quite well that the advantages of branched wheat were purely superficial.

First, the journal *Yarovizatsiya* (*Vernalization*), which Lysenko himself edited, had published in 1940 an article on the properties of branching forms[27]. Second, in 1938 the newspaper *Sotsialisticheskoe zemledelie* (*Socialist Agriculture*) had published a photograph of Denis Lysenko, Trofim's father, holding ears of branching wheat in his hand. The caption made it quite clear that the two Lysenkos, father and son, had set their sights on this wheat, and had studied its properties, but understood that nothing useful would come out of it.

Yet when Stalin approached Lysenko, his affairs were in such a bad state that he had nowhere else to turn, and he decided to resort to bluff. So as not to have to refuse straight out to carry out the task, he promised Stalin that he would start working on branched wheat. When the earth began to shake under his feet, he turned to direct deception and informed Stalin that the seeds he had been handed two years earlier had been improved so much that, in the coming year, the production of grain would be increased fivefold.

This promise gave Stalin grounds for putting an end to Lysenko's downfall. Only recently I have learned that, in May 1948, Stalin urgently summoned the whole staff of the Politburo of the Central Committee and in very sharp form criticized the organizers of Yurii Zhdanov's lecture (D. T. Shepilov, basically) and Zhdanov himself (Y. A. Zhdanov, personal communication, 24 December 1987). Stalin repeated again and again one phrase: "Who dared to offend such a good person!" (D. T. Shepilov, personal communication, 8 January 1988). In July, 1948, the innovator was summoned to an audience with Stalin, during which Lysenko persuaded the leader that the work with branched wheat was almost concluded, and that it remained only to produce more stocks of the variety before distributing it into the boundless fields of Russia. Lysenko sought and obtained permission to name the new varieties "Stalin branching".

At the same time, Lysenko also won Stalin's consent for the final administrative elimination of all his critics, for a final ban on genetics in the USSR and for the cancellation of the elections in the Academy of Agricultural Sciences. Instead of the elections, Stalin simply co-opted 35 new members to the academy, most of them supporters of Lysenko. The appointments were announced in *Pravda* on 28 July 1948.

Among the new members were S. N. Muromtsev, an officer of the KGB (NKVD) who had previously been head of the special prison for arrested scientists. He took his job so seriously that he personally took part in the beating of two professors, Zdrodovsky and Zilber. Two days later saw the beginning of the August 1948 session of the academy, which destroyed genetics in the Soviet Union.

A week later, *Pravda* published a letter from Yurii Zhdanov to Stalin renouncing his earlier position and saying his errors were due to political immaturity and a tendency to substitute "scientific objectivity" for "Party principledness", which was a great sin. Yurii Zhdanov wrote[28]:

> Lenin stated more than once that a recognition of the necessity of this or that phenomenon conceals in itself the danger of falling into objectivism. To a certain measure, I too did not avoid this danger.

Immediately after the session, about 3,000 scientists were dismissed from work. Russian genetics, which had produced splendid research recognized by the entire world, ceased to exist. Darkness fell on biological sciences in the Soviet Union. Nobody at that time could know how long that night would last — or whether he would survive until dawn. □

Valery N. Soyfer is at Ohio State Biotechnology Center, Ohio State University, Columbus, Ohio 43210-1002, USA.

1. Soyfer, V. N. *Lysenko* to be published in Russian (Hermitage, Ann Arbor), and abridged in English (Rutgers University Press).
2. Medvedev, Zh. *The Rise and Fall of T.D. Lysenko* (Columbia University Press, 1969).
3. Joravsky, D. *The Lysenko Affair* (Harvard University Press, 1970).
4. Roll-Hansen, N. *Science* **237**, 1329 (1985).
5. Lysenko, T.D. *Bull.sort.-semenovod. Upravl.*, No. 4, 72–76; 77–80 (1923).
6. Zaitsev, G.S. *Influence of Temperature on Cotton Development* (Proc. Turkestan Plant Breeding Station) Moscow-Leningrad. Promizdat (1927). (In Russian with English summary).
7. Lysenko, T.D. *Trudy Azerbaidzhanskoi Tsentr opytno-selektsionnoi stantsii imeni tovarishsha Ordzhonikidze*, Part 3, Baku, 1–169 (1928).
8. Lysenko, T.D. & Dolgushin, D.A. *Trudy Vsesoyus. s'ezda po genetike, selektsii, semenovodstvu i plemennoumu zhivotnovodstvu* Vol. 3, 188–189 (Leningrad, 1929)
9. Grigor'ev, V. *Pravada* No. 165 (4299), 21 July, p.4, (1929).
10. Shlikhter, A. *Pravada* No. 232 (4366), p.3, (1929).
11. Tulaikov, N.M. *Sel'skokhozyaistvennaya gaseta*, No. 212, p.3 (12 November 1929).
12. Popovskii, M.A. *Prostor, Alma-Ata*, No. 7 4–27; No. 8 98–118 (1966).
13. Joravsky, D. *Slavic Rev.* **24**, (3), 381–394 (1965).
14. Adams, M.B. *Dictionary of Scientific Biography*. Vol XV Suppl. 1, 505–513 (Scribner's, New York, 1978).
15. Medvedev, Ah.A. *Novyi Mir*, No. 4, 226–234 (1967).
16. Vavilov, N.I. *Smena*, Leningrad. 11 January (1929).
17. Vavilov, N.I. *Sotsial. zemledelie*, No. 54, (616), p.1 (24 February, 1931).
18. Vavilov, N.I. No. 253 (815), (13 September, 1931).
19. Vavilov, N.I. Theoretical principles of the selection of plants. Moscow-Leningrad vol. 1, p. 865 (1935).
20. Konstantinov, P.N., Lysitsyn, P.I., Kostov, D. *Yarovizatsiya*, No. 5 (8), pp.15–19 (1936).
21. Vavilov, N.I., Speech of 17 June 1938 to the Presidium of the Lenin All-Union Academy of Agricultural Sciences. Archives of the LAAAS, document 450, p.192, point 3.
22. Vavilov, N.I., *Pravda*, No. 2 (6608), p.2, 2 January (1936).
23. Zhebrak, A.R., *Science*, 102, 2649 (1945).
24. Vavilov, S.I. *Vestnik AN SSSR* No. 9, 26 (1948).
25. Soyfer, V.N. *Kontinent* (Paris), No. 47, pp. 267–305, No. 48, pp.263–297 (1986).
26. Bazhanov, A.M. *On the Cultivation of Wheat, with a Description of the Crops grown in Russia*, Moscow. Izd.Moskovskogo Imperataskogo Universiteta (1856).
27. Kuperman, F.M., *Yarovizatsiya*, No. 2, pp.101–105 (1940).
28. Zhdanov, Yu. A. *Pravada*, No. 220 (10961), p.5, 7 August (1948).

The physicist and the Soviet citizen

E. L. Feinberg

Few people have had such wide and deep influence as Andrei Dmitrievich Sakharov. Here E. L. Feinberg reflects on Sakharov's life and work. On page 13 Maxim Frank-Kamenetskii recounts the circumstances of a first encounter with 'a man to remember', and on page 14 Valery Soyfer tells of Sakharov's battle against lysenkoism.

SAKHAROV — an outstanding scientist, a man of the highest moral standards, and a courageous fighter for human rights, and for a better, peaceful world. One cannot say which of these sides was predominant.

Biography

Andrei Dmitrievich Sakharov was born in Moscow on 21 May 1921, a hungry year. His father taught physics at high school and then university, and published several text books. Father, mother and no less grandmother greatly influenced his spiritual development; they belonged to the finest layer of the Russian intelligentsia and imparted its traditions to him.

Sakharov started formal educaion only at the age of 12, before that being taught at home with an annual examination at school. In 1938 he entered the physics faculty of Moscow University, graduating in 1942 when the university was evacuated to Ashkhabad in Middle Asia. Like all the Soviet people, he suffered from hunger during the war, and moreover went through a dangerous bout of dysentery. In 1942–44 he worked in a laboratory of a military plant on the Volga. Here he came up with four invenions for production control (one of which was patented) and, even though he had no contact with other physicists, began his research in theoretical physics. At that time he married Klavdiya Alexeevna Vikhereva, a chemist whose education in Leningrad had been interrupted by the war after four years of study. They had two daughters and a son.

Sakharov sent the results of his early researches to the well-known physicist I. E. Tamm, head of the theory department of the P. N. Lebedev Physical Institute of the USSR Academy of Sciences in Moscow. In January 1945 he became a postgraduate fellow át the institute. It was then that I first met him. Living conditions were hard; with a newly born child and a tiny grant, and without a permanent place to live (his parents' apartment had been bombed and destroyed), he helped to support his family by lecturing at the Moscow Power Institute. He defended his thesis for the degree of candidate (the equivalent of the Western doctorate) in November 1947 and joined the department as a permanent member. After that his living conditions improved considerably.

At that time Tamm formed a small group of talented members of the staff, including Sakharov, to investigate the possibility of building a weapon based on the fusion of light nuclei. In 1948 Sakharov proposed a most important idea

which, together with another essential concept generated in the group, made a solution to the problem seem practical. Sakharov and Tamm were transferred to a special institute where Sakharov worked until 1968 (Tamm returned in 1953). Later he himself wrote that he was "the author or coauthor of several key ideas". Almost 20 years — of a period of life most valuable for any theorist — were consciously sacrificed mainly to develop and improve the weapon, although at the same time Sakharov did a great deal towards the peaceful use of nuclear fusion. He was generously rewarded by the government — with three Golden Stars of the Hero of Socialist Labour, among other orders and prizes — and in 1953 was elected a full member of the USSR Academy of Sciences. He earned the admiration and affection of all his colleagues and of everyone else who knew about his work.

In 1961 Sakharov started to come into conflict with the government, in particular with N. S. Khrushchev, over further bomb testing. By and by, his political views were changing. In 1968 he published abroad his first famous 'manifesto' ("Meditations on Progress, Peaceful Coexistence and Intellectual Freedom") which brought him the enmity of the nation's rulers. He was dismissed from his position. Shortly afterwards, in March 1969, his wife died unexpectedly from cancer at the age of 49. This was a hard blow for him. Sakharov donated almost all of his money to an oncological hospital and to the Red Cross; he was frugal in his ways, and thus accumulated large savings. The same year he returned to Tamm's department at the Lebedev Institute with which he had renewed scientific contacts in the early 1960s, a time when he had begun to work on pure physics.

In 1972 he married Elena Georgievna Bonner, a physician, who also had years of dissident activiies behind her. Persecution by the security service, and by the press and other agencies of official propaganda, was on the increase. Nevertheless Sakharov continued to have a leading part in the struggle for human rights, and in 1975 was awarded the Nobel peace prize. After his open protest against the Soviet invasion of Afghanistan he was exiled to Gor'kii and put under 24-hour surveillance by the security service. Any contact with him was forbidden (he even had no telephone), save for his wife, children and colleagues from the Lebedev Institute, of which he formally remained a member of staff. We were permitted to visit him from time to time for scientific discussions. Later his wife was sentenced to exile with him.

In December 1986 M. S. Gorbachev called from Moscow (the telephone was specially installed) and invited Sakharov to return, together with his wife, "to renew his patriotic activity". He was then free (except to travel abroad) and became a leading political figure, still trying to continue his scientific work. But there was less and less time for it, especially when in

1989 he was elected a people's deputy. His heart, which had been in bad shape even before his exile and his hunger strikes, could not withstand the strain, and on 14 December he died suddenly from a heart attack.

As was his will, Sakharov was buried in an 'ordinary' cemetery, Vostryakovo, where his first wife and the mother of his second wife had been buried. His death spiritually uplifted people throughout the country, uniting them in mourning and reawakening the sleeping consciences of many

Science

Throughout his life, science was an abiding passion for Sakharov. His changes of direction — from research in pure physics to technological problems, as in the period of work on nuclear fusion in 1948–68, or to humanitarian and political activity from 1968 onwards — were each time a necessary sacrifice to the urgent needs of humanity as he understood them. But at all times he tried to carry on with fundamental physics. In these conditions it is remarkable how many important ideas he put forward, although he undoubtedly did not fully realize his extraordinary talent — a talent, I should add, that was manifest equally in his work on pure physics and on its technological applications,

'Preliminary' period. Among the four papers he wrote during his time at the Volga plant, one deserves especial notice. Perhaps after reading a report on chain reactions in a uranium–moderator mixture, he realized that uranium must be prepared in blocks instead of being homogeneously mixed. This principle, which makes the chain reaction with natural uranium possible, had already been discovered but was classified.

This episode was typical of Sakharov — throughout his life he used to formulate a problem and solve it without publishing the result (maybe because he was not aware of its novelty). He called them 'amateur problems'. The subjects varied from number theory to one that attracted his interest when he helped his wife in the kitchen: in chopping cabbage there arise polygons of different sizes and with different numbers of vertices. He found, first, that the average number of vertices is four; and, second, that the ratio of the square of the average perimeter to the average area is 4π, exactly as for a circle. Solution of some of these problems is far from simple. For him they constituted a special kind of relaxation, the equivalent of playing chess.

During his postgraduate fellowship, Sakharov published three papers: on high-energy pion production, on optical determination of the temperature of hot plasmas, and on the transition between states of zero angular momentum in the nucleus. They show a sound knowledge of the theoretical techniques of the time, and the last one — a summary of his thesis — deserves special attention because it is a fundamental contribution and bears the glittering marks of his intellect.

The fruits of this early period may seem rather modest, but one should mention that the papers were produced during only two years of work. I can testify that his extraordinary abilities emerged during numerous discussions with colleagues. The very trend of his thought was unusual. Moreover, when expressing his ideas he often used to omit some intermediate elements which seemed obvious to himself. Accordingly, at times his arguments at first seemed inconceivable and even plain wrong. Only after further, sometimes rather lengthy reasoning would it became clear that he was right.

Nuclear fusion. I have already described how Sakharov came to work on the hydrogen bomb. He himself greatly disliked being called 'the father of the Soviet thermonuclear weapon', saying that many outstanding scientists took part in the enterprise, that they put forward many of the most important ideas and solved innumerable complicated physical and technological problems. But at the same time everyone who worked on the project acknowledged his outstanding, if not leading, role.

Sakharov's work at that time was far from being limited to the weapon. His pioneering ideas laid the foundations for all further research into the peaceful use of fusion. Among them is the concept (and detailed theoretical elaboration) of the magnetic thermonuclear reactor (MTR), proposed by Sakharov and Tamm in 1950. In the MTR, both heating of an ionized gas of light atoms (deuterium and tritium) necessary to overcome mutual repulsion of nuclei, and its confinement (preventing collisions of ions with the walls of the huge toroidal 'vessel'), are effected by a system of magnetic fields. This is the 'tokamak', upon which the subsequent attempts to obtain controlled energy-producing fusion have largely centred. As proposed by Sakharov in 1951, the toroid can be covered by uranium in which neutrons liberated in the process of fusion can produce plutonium and fissionable uranium. Another way to the same end is Sakharov's idea of 'mu-catalysis'. It is possible to overcome the electrostatic repulsion of, say, deuterium and tritium without heating by using a peculiar phenomenon, first discussed in quite a different context by F. C. Frank: a negative muon in combination with deuterium or tritium forms a system akin to a hydrogen atom, but some 200 times smaller. Being neutral it is not repulsed by another nucleus and can approach it so closely that fusion becomes possible. Again, the uranium blanket can be applied, as for the tokamak. Recently a committee of experts in the United States concluded that successes obtained with this scheme make it as promising as the traditional approaches to fusion.

Finally, the third method which is now developing — laser-induced fusion — was also first proposed by Sakharov, in a classified talk.

In 1951–52 Sakharov came up with 'magnetic cumulation', an ingenious method of obtaining extremely strong magnetic fields. The technique involves compressing a moderate magnetic field (say, within a cylinder) by 'imploding' the enveloping substance (chemical or atomic). In this way a group of his colleagues obtained record magnetic fields of 16 million gauss (in some experiments, 25 million gauss).

Return to pure physics. In the early 1960s, when his involvement in fusion was becoming less necessary, and his controversy with Khrushchev accelerated the change in his political views, Sakharov turned to pure physics. Elementary particles, field theory and cosmology were topics that had always interested him above all. The years spent on other problems had left gaps in his knowledge, however. He began to come to Moscow, to participate in Tamm's seminars more and more often, and overcame his drawbacks surprisingly quickly. His most important papers appeared after 1965.

As an example, I will describe the striking idea of the origin of so-called baryon asymmetry in the Universe; that is, why the Universe contains only matter — both baryons (protons and neutrons), and leptons (electrons and photons) — and practically no antimatter. This is surprising because in the beginning of its expansion the Universe was so hot that particles and antiparticles had to be produced in equal quantities. The answer, proposed by Sakharov in 1966, is that because of violation of CP symmetry (symmetry in respect to simultaneous change of particle charge and parity), discovered two years before, decay of a baryon is not a mirror reflection of decay of an antibaryon, and if we suppose a certain mechanism of baryon transformation the antibaryons decay faster. In a universe that is expanding fast enough, antibaryons could not survive until the present.

With extreme boldness, Sakharov proposed a hypothetical example of such a mechanism leading to instability of the proton (although with a very long life time). Such a construction seemed sheer fantasy then. But a decade later the development of the unified field theory led to the same conclusion, and the search for proton decay was proclaimed the 'experiment of the century'. Several groups have tried to find it but failed. This failure may mean that the accepted version of the unified theory must be modified. Nevertheless the entire scheme proposed by

A glimpse of Andrei Dmitrievich

I FIRST met — or, more precisely, saw — Andrei Dmitrievich in 1951 or 1952. Although I was still a child of 10 or 11, I have never forgotten that meeting.

At the time we were living far from Moscow, in an old Russian town that had been stripped of its name and was impersonally referred to (as it still is) as the 'facility', or the 'mail box'. The town itself wasn't large, and its surrounding fields and forests, rivers and villages were encircled by a ring of several rows of barbed wire and ploughed-up strips of ground. Specially trained, fierce Alsatians roamed between those rows of barbed wire, and watchmen stood guard on towers.

This was the 'zone', yet another name, in this case a local one, by which it was known. People would say, 'beyond the zone', and 'inside the zone'.

But it was not a camp, or even a special research-institute-type prison. Inside that large zone there were numerous small zones where prisoners lived and worked, since everything that was built at this facility was, naturally, built by their hands. But the people who were working on the atomic project were free people — to the extent that people could be free under Stalin — who were merely involved in strictly secret work. Among those who worked on the atomic project under the direction of Khariton and Kurchatov, and under the vigilant supervision of L. P. Beria and the overall guidance of J. V. Stalin, was my father, David Albertovich Frank-Kamenetskii, a physicist.

Our family had come there almost at the very beginning, when the facility had just sprung up, in 1947 or 1948, so at the time of the events I am describing I was already a long-term resident. In terms of living conditions, we were pretty well off. Our family had been given half of a special, two-storey, comfortable cottage. Although it was a wooden building and not very large, at the time it seemed big to me, as it did, I think, to many others. It was a luxury for those times: five rooms, a large yard and a garden where Papa raised flowers and Mama raised vegetables.

Father liked to have conversations with his fellow workers and colleagues outdoors when the weather permitted. He would lie in the hammock, his visitors would sit nearby, and they would all talk about something or other. Don't get the idea that these were seditious conversations. I think people at the time were afraid of carrying on seditious conversations even with themselves, much less with each other. (But there were exceptions. My father told me that when the 'doctor's affair' got started in 1953 he was on a business trip somewhere having something to do with the atomic project. He was lying in his hotel bed at night, and a full-length portrait of Stalin in his Generalissimo's uniform was hanging before his eyes on the wall at his feet. Father couldn't get to sleep the whole night and, gazing with hatred at the portrait, kept repeating to himself: "You ought to drop dead!". He would smile when he told about this later: "And he listened to me!".)

So, he would talk out in the yard, as I understand it, because the idiotic system of secrecy did not allow people to discuss their work anywhere away from their immediate work place. But scientists cannot help talking about their work, discussing it, thinking about it. They would call 'uranium' 'selenium' and use various other conspiratorial

tricks in order not to attract the attention of the state security officers, who were ever vigilant and ready to initiate a 'case' at any moment. In that way they would deceive them and do their work — even better if they did it in spite of Beria and his hangmen. They were convinced that they were engaged in a very important undertaking; besides, these were people who simply did not know how to work poorly.

And so once a visitor came to see my father. He was tall, very thin and ungainly, dark-haired and much younger than my father. They talked for a long time; meanwhile I was hanging around in the yard. When he left, my father, who had grown intensely pensive, called me over and said: "Remember that man. He's a genius". "Who is it?" I asked. "That's Andrei Dmitrievich Sakharov".

Maxim Frank-Kamenetskii

Maxim Frank-Kamenetskii is in the Institute of Molecular Genetics, USSR Academy of Sciences, Moscow 123182, USSR.

Sakharov still dominates consciousness of particle physicists.

The scale of Sakharov's thinking on cosmology can be illustrated by his idea of the 'reversal of time's arrow' and of the 'multisheet Universe'. Sakharov accepted the point of view according to which expansion of the Universe is only a stage in an infinite pulsation, with many turning points (on the time axis) of maximal density of matter. For one such point he assumed that time starts to increase not only in the adjoining stage of expansion but also in the adjoining stage of 'contraction' (and therefore this is actually not a contraction but also an expansion). Here, therefore, the arrow of time is reversed. Interchanging stages of contraction and expansion are connected with each other, thus forming a 'multisheet Universe'. This hypothesis, of course, makes one giddy.

Also, there is the important work of 1967 (further developed in 1975) in which gravitation is explained as a result of quantum fluctuations of the vacuum ('induced gravity' as it is called by many authors of subsequent papers). So altogether we have three cosmological papers, based on extremely bold hypotheses, which Sakharov himself included (in 1980) in the list of his six works which he considered most important.

Still another striking hypothesis, put forward in 1984 when he was in exile, assumes that the so-called signature of metric may arbitrarily differ from the one known to us (one dimensional time plus three dimensional space); that is, signatures may differ in different parts of the Universe because of 'phase transitions of metric'. This idea has elements in common with those to be found in papers by A. Vilenkin, and by J. Hartley and S. Hawking.

Sakharov did not include in his list his 1965 paper in which formation of inhomogeneities (stars, galaxies) is explained by quantum fluctuations of metric. The idea was not upheld at that time but a decade later it found supporters. But he did include in it a series of four papers on semi-empirical formulae for baryon and meson masses based on analysis of their quark structure. This is quite a different sphere — elementary particle physics.

Political evolution

Although Sakharov was never a member either of the communist youth organization or of the party, his political views were in accordance with official ideology almost until 1956, the year Khrushchev revealed Stalin's ferocious crimes. This may seem inconceivable to those in the West but such was the case. One should take into account, first, that the 'scientific'

Against Lysenko

SAKHAROV's name first achieved wide currency among biologists rather than physicists. These were the years of the bitter struggle against Lysenko who had set Soviet science back decades. Paradoxical as it may seem, the 'healing' of Soviet biology began from the outside, from a direction not under the control of Lysenko. The rebirth of genetics, and later of many other biological disciplines, was assisted by physicists. At this stage, Sakharov — with conviction, and with specifically sakharovian thoroughness — began his struggle against Lysenko.

In 1959 he published a large article entitled "The Radioactive Carbon of Nuclear Explosions and Nonthreshold Biological Effects" in which he criticized the view of Edward Teller, who had declared that the adverse effects of hydrogen-bomb testing were "the equivalent of smoking one cigarette twice a month". Sakharov made a precise mathematical calculation of the disturbance of the hereditary molecules from neutron action and estimated the various side effects of irradiation. In a very concise way he showed the role of mutations in the appearance of hereditary diseases, and the possibility of increase in cancer and leukaemia, of decrease in the immunological response of organisms, and of damage done to mankind because of an increase in the mutability of bacteria and viruses.

Having investigated the effect of radiation on heredity, Sakharov was able, in addition, to clarify for himself the damage being done by lysenkoism. That done, he boldly joined in the battle against Lysenko and the lysenkoites, especially against Lysenko's protégé, N. Nuzhdin. By this time Tamm and Sakharov had become legendary figures in biological circles.

It seems to me that the struggle by Andrei Dmitrievich for the interests of science was a turning point for him. It brought to light something in him that singled him out from among many colleagues: a capacity for public activity, fearlessness and adherence to the highest moral principles. In the years when he was speaking out against lysenkoism, he had not yet proved to be a fighter for the ideals of humanism — something that was to bring him world recognition. This was perhaps his first test of strength. But it was a test that clearly showed the character of this amazing man. Valery N. Soyfer

Valery N. Soyfer is in the Department of Molecular Genetics and the Ohio State Biotechnology Center, Ohio State University, Columbus, Ohio 43210, USA.

structure of marxist ideology had a great appeal for Russian intellectuals. Second, experience shows that families within the Soviet Union who themselves managed to avoid becoming Stalin's victims, would remain loyal to this ideology — the twentieth century has been not so much the century of the atom or the electronics industry, as that of the realization of the immense possibilities of mass propaganda. As Machiavelli knew, a ruler must "inspire terror in such a way that if he does not deserve affection, he does avoid hate". Third, since 1948 Sakharov had been working with enthusiasm behind the curtain of secrecy. He was involved in work of the utmost importance (according to his own views) for the preservation of world peace. It was only later, when he came to realize that the nation's cynical and politically unacceptable leaders had exploited him for their own ends, and when he learned more about the suffering of ordinary people, that his views began to change. It took a rather long time. But when his mind cleared he came to a new and deep understanding, and from then nothing could stop him immersing himself in political activity.

Disarmament

It is absolutely wrong to think (as many people do) that Sakharov's political activity was a kind of repentance for the 'sin' of participation in the bomb making. At that time, as well as later, most physicists believed that the world would not be safe if one power had a monopoly on nuclear weapons. Even in 1944, Niels Bohr was greatly troubled by this, as later were Albert Einstein and Bertrand Russell. I remember Landau (who himself had been jailed under Stalin, but who nevertheless took some part in the thermonuclear project) saying to me many times in the 1960s: "Good for physicists — they saved the world from war".

Sakharov was of the same opinion. There was perhaps a moment of doubt in the period when his political views were changing. I remember a conversation with him, some time in the early 1970s. He made a remark that caused me to exclaim: "What? You regret that you took part in making the bomb?". He answered: "You know, there are certain questions about which it is better not to think too much". But this doubt was temporary. Later his struggle for disarmament was intensified because of the danger of the accumulation of stocks of nuclear weapons and of their proliferation. I heard at second hand that during his meeting with Edward Teller about one year ago he said to him something of the kind: "Essentially, you and I were solving a common problem". On another occasion I myself heard him say, "Teller is a tragic figure".

Sakharov's belief was that one should struggle for non-proliferation, and for reduction of nuclear arsenals as well as of conventional armaments. But he saw this as being possible only in the open world with human rights truly secured. In sum, I am sure that his political and social activities would have followed the same course even if he had not been involved in the making of the bomb.

Human rights

The great significance of Sakharov's struggle for human rights lies not so much in the practical results of dissidence as in its tremendous moral and spiritual effect. For the first time in many decades people of my country saw a man of extreme honesty, sincerity and nobility who fearlessly faced the state machine — both in proclaiming large-scale political ideas and in defending individuals. He was devoted to his fellow fighters for human rights. He was ready to struggle for every one of them, and never missed a chance to call attention to those who were in jail or exile. The names of such people were included in his Nobel lecture and in his letters to the government. When Gorbachev told him that he was free he immediately responded that all of them also should be liberated. When later he was told that his honours (of which he had been deprived when sent to exile) were to be returned to him, he refused to take them back until all dissidents were rehabilitated.

This indivisible combination of deep feelings for humanity as a whole and for single individuals was the most remarkable feature of Sakharov's personality. Together with his absolute inability to say anything that did not correspond with his convictions, with what he actually thought and felt, it explains why both ordinary people and the political leaders of the world listened to and trusted him. At the same time he did not claim to be a deity. Once, in the middle of the 1970s, I said to him: "You know, Andrei Dmitrievich, some of your admirers nevertheless believe that neither yourself or Solzhenitzyn should give recommendations concerning concrete political and economic problems since you are not professionals". He exclaimed: "Of course, I am not a professional, of course, I make errors. But what am I to do if nobody else can or dares to say a word?".

A year or so ago, I asked him whether certain recollections about his younger years were true. He became indignant: "All this is a myth to make a superman of me. I am a man like others". It would be unjust to suspect that here he was posturing; he simply meant that although he knew his worth, he did not want to feel himself to be apart from other people. And he was not. We have lost one of the finest examples of humanity. □

E. L. Feinberg is in the P.N. Lebedev Physical Institute, USSR Academy of Sciences, Leninsky Prosp. 53, 117924 Moscow, USSR.

Recipe for ex-Soviet republics' science

The republics that were formerly part of the Soviet Union embark on a journey full of hazard: sooner or later they will have to transform the structure of science left by Mr Mikhail Gorbachev.

WHETHER Mr Mikhail Gorbachev, who left the Kremlin last week, was a greater success than a failure is a version of the old difficulty of comparing very large numbers: both his successes and his failures were huge. His use of the absolute power he enjoyed six years ago to dissolve the Soviet empire in Central Europe, his doctrine of *glasnost* (which has at least allowed his downfall last week to be fully chronicled) and his willingness to negotiate deals on arms control in Europe and more generally, driven though that may have been by the ruinous cost of sustaining global strategic power in the shambles of the old Soviet economy, are memorable. (The course of recent history, and even Gorbachev's career, might have been even more cheerful if President Ronald Reagan's advisers had let him hold to the plan for getting rid of all nuclear weapons to which he had briefly agreed at Reykjavik in 1987.) Gorbachev's outstanding claim on public attention is that he brought an unprecedented degree of personal intelligence to public life, and gave it almost free rein. That in the process he changed the world is not disputed.

Gorbachev's failures stem from two conspicuous errors, of which the chief was his failure to distance himself from the Communist Party of the old Soviet Union. Although his first power base was that of general secretary of the Party, by 1987 he was also president of the union; it is mystifying that such a clever person never sensed the conflict of interest between that national office and the supposition that the Party should have a constitutionally 'leading role'. Yet the regular bruising meetings of the Politburo and the Central Committee were the means by which brave plans were emasculated by tawdry compromise. (The enmity between Gorbachev and Boris Yeltsin, now the president of Russia, dates from such a meeting.) Gorbachev's other serious error has been his failure, over seven years, to grasp the nettle of economic reform. His consistent argument was that legal and constitutional reform should come first, but that has proved to be a recipe for bankruptcy.

Bankruptcy

Bankruptcy is the most perplexing of the legacies Gorbachev's successors will inherit. In Russia, the reform process is due to begin today (2 January) with the unfreezing of all prices in the retail trade and something like a fivefold increase in the cost of fuel. Unless this change unlocks previously unknown hoards of food and other supplies, it will exacerbate an already dangerous situation. Market prices are those that strike a balance between supply and demand, but if the supply is physically insufficient to keep the people alive through the winter now begun in earnest, those on the edge of starvation will not be comforted that their plight will send a strong economic signal to those responsible for next year's harvest. Yeltsin and his fellow-presidents will need more than mere nerve; they will need a convincing demonstration that they will be in better shape during the winters ahead, if they are to command the fortitude of their electors in the hard months ahead.

Ignorance

How far-sighted will they be? One danger is that the nationalist scramble for the assets of the old union (Ilyushin aircraft, for example) will obscure simpler economic truths (that capital assets are of literally no value if they cannot be used, for example). Another, more serious, is that the republics are bent on embracing what they call a market economy without a proper sense of what is entailed. After 75 years, understanding of the interplay between capital and labour seems to have been thoroughly obliterated. Despite *Das Kapital* and all that flowed from it, the notion that capital costs money remains mystifying (and, when understood, is as often resented).

The civilities indispensable to a stable market economy are non-existent. There is, for example, no way of collecting income tax except by deductions from the salaries of those who happen to be on government payrolls, the new sales tax (at a rate of 28 per cent) will test the honesty of Russia's retailers to the full — and there is no system of social security for relieving the hardship of those thrown out of work by economic change. The republics now rushing towards reform may be likened to a group of people embarking by light aircraft on a journey across an uncharted continent without navigational aids or even an understanding of aerodynamics. Let us hope they enjoy the luck that they will need.

The hazards of such a journey may require that many valuable things will have to be jettisoned on the way. The international scientific community should have a particular regard that an early casualty may be the distinctive Soviet scientific enterprise. The prospect seems to be (see *Nature* **354**, 499; 19 December 1991) that the structure of the Soviet Academy of Sciences will kept going, perhaps hand to mouth, as the Russian academy. As a stopgap, that is probably necessary, but outside help will be essential if able people are to remain productive (or even where they are). But the structure will not suffice for the long run if the Russian government endorses even a small part of the old Marxist rhetoric about the importance of science in the modern state. Radical upheaval will be required.

Here is why, and how. The present system in which a network of research institutes (now truncated by the departure of the Ukraine) is supported and managed directly by an academy embodies an intolerable conflict of interest. Especially in such rough times, the interests of the academy are indistinguishable from those of the directors of its institutes, traditionally personal fiefdoms in which the power of personal patronage, even if moderated by the notion that the institutes will be collectives, will now be greater than ever. These are hardly the circumstances in which basic research can flourish, nor will they help to create a research enterprise of the kind that a would-be modern state such as Russia needs. The academy's complacent assumption that only its own institutes are worthwhile is at once mistaken (many of its institutes are second-rate) and a recipe for the indulgence of self-interest. (The old academy's attempts to set up a system of competitive research grants might have been more successful if its own payroll pensioners had been less influential in its councils.) When this transitional year is over, it should withdraw from the direct management of research.

There is plenty of other work to be done. What, for example, is to happen to the universities which, despite the efforts of Gorbachev's able minister Yagodin, are still largely cast in their Stalinist mould, itself a means of keeping students away from knowledge judged irrelevant to their immediate needs as well as from the traditional subversiveness of teachers? An academy with the national interest (Russia's now) at heart could have a decisive influence on the overdue restructuring of the universities — and on the encouragement of research therein.

There is a similar need in the new republics' reconstruction of the apparatus of industrial research. During 75 years of central planning, production ministries have maintained research institutes to serve the needs of their own production plants, while the Soviet academy's institutes were regarded as a source of radical innovations that might, after negotiation within a labyrinthine bureaucracy, be scheduled for production at a factory whose managers might not previously have known what was in the wind.

There could hardly have been a mechanism for what is called technology transfer less suitable for matching the performance of new products to market needs. The notion that academy institutes, functioning as collectives, may now make deals with production enterprises may help a little, but that random process will do little to equip Russia or any of the other republics with the skills in research and development they will eventually need. What, in the meantime, can be done to keep alive and to improve a hard core of industrial research and development? The Russian academy could more easily bend its mind to that important issue if it were freed from the anxiety of keeping its own institutes in being.

To blame Gorbachev for not having resolved these problems while still in office would be unfair. He had other things on his mind. But it is a fact that nothing much was done in his time to change a structure of research so cumbersome and unsuited to its declared purposes that it is almost miraculous that so much talent has survived in spite of it. With all the social upheavals now in prospect, the decades ahead will not be so lucky unless reform comes soon.

Opening for US science

US scientific organizations want (and need) to find ways to help colleagues in the former Soviet Union.

THE crisis in the republics of what was the Soviet Union presents a special challenge to scientists in Western nations who want to see science not only survive but also improve as an enterprise during the coming years. The new Russian Republic, home to the majority of the most able Soviet scientists, is of special concern, particularly in the light of reports that as many as half of Russia's most cited scientists have left the country. What are the immediate needs? What can be done?

Although the organizational structure of Russian science is in disarray (former Soviet colleagues are no longer in positions of authority or lack resources), various groups of US scientists are trying to maintain contact. Recently, the US National Academy of Sciences (NAS) sent a delegation from its committee on dual-use technologies to talk about future industries. Concern about the fact that Russian scientists (like those in the Ukraine and elsewhere) have no currency to purchase essential reagents, spare parts and the like, has led to conversations between the NAS and the US government, as well as the Royal Society in Britain, about ways to help. But no decisions have been reached.

On 10 January, a group representing wealthy US foundations, including the Rockefeller, Carnegie, MacArthur and Ford Foundations, will leave for Moscow on an exploratory mission. With the ruble virtually worthless, contributions of even a few millions of dollars could have a major impact on the professional lives of Soviet scientists.

The American Academy of Arts and Sciences, whose headquarters are in Boston, is also thinking about ways to help. One plan under consideration is to form a clearing house for information on grants and potential collaborations. Provisions in certain US research grants, for instance, permit as much as 5 per cent of funds to be allocated to work with scientists from other countries.

These and other ideas for helping scientists in the new republics during this period of hard transition are in a nascent stage. It is essential that as many as possible come to fruition very soon. □

Редакция журнала «Природа»
благодарит за помощь
в подготовке и выпуске этого номера:

Чеховский полиграфический комбинат,
А. А. Замятнина, Б. Мэддокса, Г. Н. Петрову,
П. Поттера, Е. Д. Табакеева,
а также наших коллег
из журнала «Nature»

Уникальное предложение журнала «Природа»

Самый передовой в мире международный научный еженедельник предлагает Вам возможность совершенно БЕСПЛАТНО приобщиться к этому уникальному изданию!

Этот номер журнала «Природа» («Nature») оставил у Вас лишь самое общее – но незабываемое – представление о журнале, но наше предложение даст Вам возможность получить непосредственный личный доступ к единственной в своем роде подборке материалов, подготовленной редакцией журнала «Nature».

- **Самые последние и наиболее значительные результаты научных исследований.**
- **Новейшие новости из мира науки.**
- **Аргументированные комментарии и мнения ведущих специалстов по всевозможным направлениям научных исследований.**
- **Ни с чем не сравнимая сеть обмена научной информацией.**

Если Вы желаете получить БЕСПЛАТНО экземпляр журнала, заполните и направьте в наш адрес следующую форму.

Наш адрес Ross Sturley, Nature, 4 Little Essex Street, London WC2R 3LF, UK.

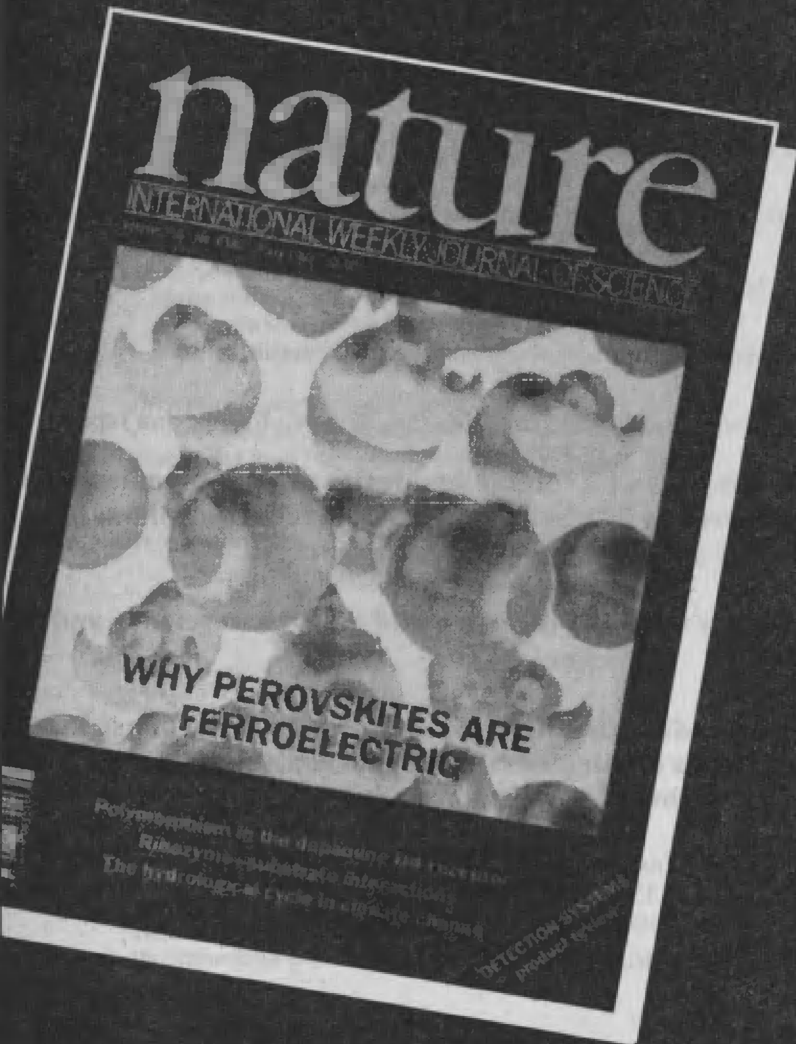
Вышлите мне, пожалуйста, мой экземпляр специального БЕСПЛАТНОГО номера журнала «Nature», самого известного в мире научного журнала. Я дал ответы на все нижеследующие вопросы.

Страна:

Почтовый индекс:

Адрес:

Ф.И.О:



Ответьте, пожалуйста, на следующие вопросы. Ответы должны быть полными, если Вы желаете воспользоваться этим уникальным предложением.

1. Что из нижеперечисленного ближе всего подходит к наименованию вашей должности?

- Профессор
- Ассистент профессора
- Доктор наук
- Генеральный директор, президент или председатель Правления фирмы
- Директор
- Технический работник
- Старший преподаватель вуза или доцент
- Руководитель лаборатории
- Заведующий кафедрой
- Библиотекарь или хранитель
- Учитель
- Студент вуза
- Аспирант
- Старший научный сотрудник
- Доктор медицины
- Преподаватель вуза, не занимающийся исследовательской работой
- Иное (уточните)

2. Что из нижеперечисленного лучше всего отражает сферу вашей деятельности?

Сфера деятельности

- Фармакология
- Иммунология
- Биохимия
- Молекулярная биология
- Экология
- Сельскохозяйственные науки
- Палеонтология
- Микробиология
- Клеточная биология
- Нейробиология
- Биология развития живых организмов
- Иная отрасль биологии
- Медицина
- Материаловедение
- Химия
- Науки о Земле
- Астрономия или астрофизика
- Физика твердого тела
- Иная отрасль физики
- Инженерные науки или математика
- Иное (уточните)

1 р. 80 к.
Индекс 70707

ISSN 0032—874X. Природа. №10. 1—128.