Table 1b Concentration of ^{137}Cs in Females. Mean Values for Quarter-Year Periods (with s.e).

Year	No.	Total nCi	pCi/kg body wt.	pCi/g potassium
1967 (1)	47	4.67±0.33	84.2±6.5	49.2±2.6
(2)	41	3.99±0.28	67.0±4.4	45.0±3.3
(3)	67	3.33±0.10	58.4±2.1	38.8±1.1
(4)	28	3.09±0.15	54.9±3.9	36.1±1.8
1968 (1)	53	2.34±0.08	38.4±1.4	25.8±1.0
(2)	65	2.09±0.08	34.3±1.2	24.0±0.9
(3)	65	2.11±0.07	35.4±1.0	25.1±0.8
(4)	57	2.23±0.09	35.9±1.4	25.4±1.0
1969 (1)	31	1.68±0.08	28.9±1.2	18.3±0.8
(2)	24	1.71±0.11	30.9±2.8	17.6±1.0
(3)	19	1.68±0.14	33.5±1.6	21.8±1.7
(4)	29	1.87±0.09	33.6±1.7	21.9±1.1
1970 (1)	16	1.79±0.13	30.1±2.0	20.3±1.1
(2)	10	1.67±0.16	30.4±1.9	20.3±1.3
(3)	11	1.57±0.13	30.0±1.9	18.5±1.0
(4)	20	1.60±0.16	31.9±3.0	19.9±2.0

found to be 75 and 39 litres of milk per quarter year. The half-life value for men agrees very closely with 134 days reported for the slow turnover compartment[5] determined by retention of a single administration over a period greater than five hundred days. The agreement indicates that the assumptions underlying the model are not seriously in error. Lower calculated values of intake for the female population could possibly be due to a combination of two factors; smaller actual dietary intakes, and a larger fraction being rapidly excreted.

It is probably that the former factor is the more important. There are no published data on the relative caesium intakes of men and women, but Harries *et al.*[9] have reviewed work on energy intake and expenditure. Comparing results for groups of men and women matched according to age and occupation, they show that women have a consistently lower mean calorific intake than men. Therefore, assuming that women have a diet similar in composition to that of men, it is likely that they will also have a lower dietary intake of ^{137}Cs.

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Avery in Retrospect

Lederberg[1] and Olby[2] cite enough references to show that many biologists quickly recognized the significance of pneumococcal transformation by DNA. The history of such a subject has great intrinsic interest, and often acts as a cautionary tale that should suggest that there are oversights and false assumptions at the present time. It may therefore be permissible to add something more to the record.

In the spring of 1936, I worked for a few months with Landsteiner in the Rockefeller Institute. Avery often joined us at lunch-time and on several occasions conversation turned to the possible mechanism of the "Griffith phenomenon" as Landsteiner tended to call it. Landsteiner, undogmatically, tended towards a Darwinian explanation with differential survival of pre-existing bacterial types to which Avery pointed out many objections. At the Microbiological Congress in New York in 1939 I did not see Avery but asked Landsteiner whether any work was being done on the "Griffith phenomenon". He said he thought not.

As Lederberg points out, the paper by Avery, MacLeod and McCarty[3] was discussed both in public and private at the 1946 Cold Spring Harbor Symposium and at a Society for Experimental Biology Symposium also held in the summer of 1946[4]. It was the first paper cited by Kalckar. Stacey remarked "There is no doubt that this is an authentic case of a specific mutation caused by a chemical entity, and its importance cannot be over-emphasized". Reference to the work also appeared in a subsection called "Mutation theory of etiology of cancer" in a paper by Stowell. Darlington did not mention Avery but called nucleic acid "the molecular midwife of all reproductive particles". That symposium was also notable because the chemists at it showed that they had at last realized that the tetranucleotide hypothesis was baseless and implausible. A year later, at a symposium in Stockholm[5], Boivin mentioned Avery's work in a paper with the magnificent title "Le rôle des deux acides nucléiques dans la constitution et dans la vie de la cellule bactérienne, et plus généralement de toutes les cellules vivantes". In 1949 a Society for General Microbiology Symposium[6] was enlivened by intermittent discord between Harriett Taylor, who was working in Avery's laboratory, and Stacey. They disagreed profoundly on the interpretation of the work but agreed completely about its importance. The editors reported that discussion tactfully.

All those working on viruses, who were reasonably well informed, knew of the suggestion by Muller[7] and Duggar and Armstrong[8] that there were many analogies between viruses, and genes that had broken loose from their moorings. The suggestion is quoted in many papers published in the 1930s, and we followed with interest any genetical, or quasi-genetical, research that seemed relevant. It is not surprising therefore that Stanley and Knight[9], who were at that time on the staff of the Rockefeller Institute, referred to Avery's work in a review in 1945. I referred to it in a review in 1948[10] and Bawden in the third edition of *Plant Viruses and Virus Diseases* in 1950[11].

Bearing in mind that scientific communication and publication did not get properly restarted until 1947, and that scientists have never responded quickly to a change in fashionable assumption as great as that involved in the dethronement of protein and polysaccharide by nucleic acid[12], I feel that Avery's explanation of the "Griffith phenomenon" was incorporated into the general picture about as quickly as could have been expected.

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