

**FURTHER RESEARCHES
INTO INDUCED CELL-
REPRODUCTION AND
CANCER**

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Further Researches Into Induced Cell-reproduction and Cancer by Various

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CONSISTING OF PAPERS BY

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WITH ILLUSTRATIONS

THE McFADDEN RESEARCHES

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PREFACE

THE following papers outline a continuation of the results of the researches which were published by me eight months ago in a book entitled *Induced Cell-Reproduction and Cancer*.¹

The book described a method by which it was found that human white blood-corpuscles and other cells can be made to divide when they are absorbing certain chemical agents from a film of jelly set on a microscope slide. The chemical agents evidently cause the divisions; and this fact formed the basis of a theory as to the possible causation of benign and malignant growths within the body, and led to the elaboration of experiments to try to prove the theory in question.

I was fully aware when I wrote it that the book was compiled in rather an unusual manner, and that the results which it mentioned were revolutionary to the older ideas; but it described a new method of research which has undoubtedly revealed new facts; and I considered that the way in which it was written might render the details of the complex experimental methods more readable, and facilitate their consideration—which I think it has done. Unfortunately, a group of gentlemen, whom I had thanked in the preface

¹ London, John Murray, 1910.

TO VINDICATE
ANTHONY D. PREFACE

for assistance and all of whom with one exception formed a committee to associate me officially with one of the hospitals in Liverpool, apparently became alarmed, and, before any one had had time to study the work fully, wrote a letter to the medical journals disclaiming all responsibility for my statements. The disclaimer was unfortunately worded, because, since those who signed it omitted to mention that most of them formed my committee, it seemed to imply that I had deliberately exploited their names without any authority or without knowing them—which, of course, was not correct. Those who signed the letter are gentlemen eminent in Science, and it has therefore had a most detrimental effect on the trial of the new method by others, and on the consideration of the researches by those who have not yet seen the actual experiments themselves. In the United States especially, a country which I have recently visited and where I was privileged to describe and demonstrate some of the experiments at the most important Research Institutes at Philadelphia, New York, Buffalo, and Chicago, and where every facility was afforded to me to explain the methods, while I was received most cordially, yet I found at the outset a tendency to scepticism in many cases attributable to the disclaimer; in fact, some of the medical journals went so far as to refer to the words of my committee and then to condemn our work.

It seems that anything written about the cancer problem is inclined to excite suspicion. Apart from the extremely complex nature of the scientific problem itself, a worker in it is beset

with the difficulty that people think he is trying to produce another speculation in the hope that it may ultimately turn out to be correct, or that he may increase his practice. As a matter of fact, we believe that the cancer problem is only a detail in the solution of the problem of the cause and control of cell-reproduction—or rather of the phenomenon of the reproduction of living matter. The person who solves this problem will, I think, have a far greater field in front of him than the mere practice of a “cure for cancer.” Besides, we none of us practise.

This suspicion, coupled I think with the disclaimer, has led to the publication of our work in its present form. When our experiments merely dealt with problems concerning well-known cell phenomena we met with no difficulty in getting the results published in scientific and medical journals; but now editors of scientific journals appear to be less inclined to insert our papers. After careful consideration, therefore, I think that it will be better to continue, as I have always meant to continue, to take all the responsibility, and to print the work independently for ourselves. We must publish our experiments, because others may be—in fact, we know that they are—working on similar lines; and nothing is gained by clashing in research.

Judging by the published reviews and the criticisms which I have received both in England and America, the question to be answered is whether my interpretation of the facts seen by the new method of *in-vitro* staining is correct or not; the main point turns on whether the

figures which were described are really those of cell-division or are merely some accidental distortion. It is difficult to discuss the *pros* and *cons* of this point, because it is one about which we are absolutely certain. Any one who has seen the number of figures in all their phases which we have seen cannot possibly dispute them; and several persons have agreed that they are cell-division.¹ Unfortunately, as I have said before, one cannot always induce the division-figures to order in such a manner that they resemble the well-known aspects pictured so frequently in text-books. There are many factors to be taken into consideration, such as the temperature, the consistency of the jellies, the vitality of the cells, the alkalinity, the degree of putrescence of extracts, and lastly the attitude which the cells may happen to present to the observer, over which we have no control whatever. Owing to scepticism people will only come with reluctance to see the experiments, and when they do arrive they frequently expect us to command living cells to undergo typically on every occasion the most complex physiological function of reproduction in the few moments which the visitors can spare us. In work of this nature it is necessary to spend many hours of careful observation, making control experiments and searching through many films. A cursory examination often leads to disappointment. If a fairly typical specimen happens to appear before the objective, however, many people become convinced, although their ardour is sometimes damped when they realise that the

¹ See letter in *The British Medical Journal* of January 11th, 1911.