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科普经典
特藏

The Double Helix

A Personal Account of the Discovery of the Structure of DNA

双螺旋

发现DNA结构的故事

〔美〕J.D.沃森　著
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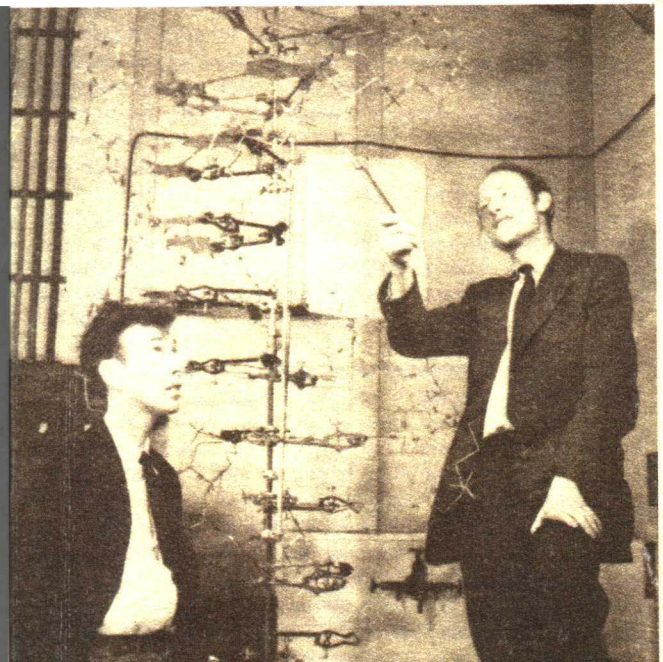

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序

20世纪在科学发展史上是一个辉煌的世纪，以物理学和生物学的创新性成果为标志的科学成就，极大地改变了世界的面貌，改变了人类的认知水平、生产方式和生活方式。20世纪也是科学史上的一个英雄世纪，一大批别具一格的科学大师风云际会，相继登场，使科学的舞台展现出前所未有的绚丽风采。20世纪发生了两次世界大战，二战催生的原子弹，使社会公众了解了科学的巨大威力，也促使人类认真地审视科学，了解到科学必须要与人类的良知，与人文精神结合在一起，只有合理地利用，才能造福于人类，才能有利于和平，有利于人类社会的可持续发展。进入20世纪80年代，人类更进一步认识到必须携起手来保护生态，控制环境污染，探索可持续发展的道路。可持续发展理念的形成，是20世纪阶级社会发展观进步的一个重大的事件。

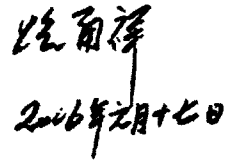
回顾20世纪科学走过的道路，从突飞猛进的科学创造，到科学与人文伦理的深度撞击，形成与人文精神交融并进的局面，最终在人类文明史上留下了不同寻常的篇章。

20世纪诞生的科学和思想大师所取得的非凡的科学成就、创造的充足科学和思想养分，孕育了一批优秀的科普作品，为公众提供了丰富的精神食粮。人们可以跟着爱因斯坦、薛定谔、伽莫夫、沃森、温伯格、霍金等等科学大师的生花妙笔去领略科学创造的历程、登攀一个个科学顶峰的征程和科学高峰的神奇景观；可以跟着卡逊在寂静的春天里思考知更鸟的命运；可以跟着萨根去观察宇宙和生命……。今天这些科学大

师和思想大师大部分都已离开了我们，但那些优秀科普作品是他们留给后代的不朽的精神财富。

20世纪已经过去，21世纪已经肯定是一个全球化、知识化的世纪，也是科技国际化、网络化的一个时代。可持续发展依然是人类唯一的发展道路，自然科学、社会科学、人文精神将交叉融合，世界的文化环境会发生很大的变化，东西方文化将会在激荡过程中进一步融合升华，创造出具有国际化，又有民族特色的新文化。在未来15年，中国要基本完成向一个创新型国家过渡。建立创新体系、创新机制配套的基础是要大幅度提高国民的文化教育水平和科学素质，把我国庞大的人口负担真正转化为无可比拟的创新人力资源。

在中国这样一个大国传播普及科技知识、科学精神是一个宏大的系统工程，需要政府组织倡导和社会各界的积极努力。中国科学院也承担着光荣而艰巨的任务，我们有义务整合全院资源努力把科普工作做大、做好，为国家和社会发挥更大的作用。科学出版社是科普图书出版的一支战略方面军，应该大有作为。《20世纪科普经典特藏》把原汁原味的经典科普大餐奉献给新时代读者，辅之以中文点评是一个很好的尝试。希望这些经典著作能给读者以启发，开拓读者的科学视野，更希望这些经典著作能起到示范的作用，推进我们自己的原创科普和科学文化作品的创作和出版。



创造历史的人

科学实际上是一种历史活动，是由许许多多失败和成功组成的人类认识自然的过程。在这个过程中，“名垂青史”通常是科学家追求的最高境界。《双螺旋》的作者詹姆斯·D·沃森(James D. Watson)就是科学世界众多成名者中一位耀眼的“明星”。他在25岁时就做出了永垂生命科学史册的工作——与英国科学家弗朗西斯·克里克一道提出了DNA双螺旋模型，在34岁时因这一工作获得了1962年度诺贝尔生理学医学奖。

纵观沃森的学术生涯，追求卓越一直是他的工作标准。沃森的博士生导师卢里亚对他有过这样的评价：“沃森把值得做的事情做得十分出色。如果事情根本不值得做，也就不值得做好。”沃森肯定是一个聪明人，15岁时就进入了芝加哥大学，以我们中国的说法可以称得上是“神童”。大学时的他醉心于大自然研究、尤其是鸟类的研究，其雄心是做美国自然历史博物馆的馆长。当他读到物理学家薛定谔《生命是什么》以后，其研究兴趣就转移到了遗传学。在读博士的申请没有得到加州理工学院批准后，沃森申请了印第安纳大学，计划在经典遗传学的奠基人马勒教授处学习。在进入学校的第一个学期，沃森遇到了噬菌体小组的核心人物卢里亚，于是沃森立刻改变了注意，转投到卢里亚门下。这一步为沃森未来的科学事业打下了一个重要的基础，因为噬菌体小组的目标就是要寻找生命遗传活动的物理本质。从此后，不论其具体的研究课题如何变换，沃森始终将揭示生命遗传活动的本质作为自己科学事业的根本目标。

拿到博士学位后，沃森首先到哥本哈根大学卡

尔喀实验室做博士后，从事核酸化学研究。很快沃森就失去了研究核酸化学的兴趣；他通过各种努力，转到了剑桥大学卡文迪什实验室做博士后，于是也就有了《双螺旋》这本书中的种种故事。回到美国后，沃森和他的老搭档克里克以及新伙伴物理学家伽莫夫等人组织了“RNA领带俱乐部”，试图破译遗传密码。1980年代末期，沃森宝刀不老，作为“人类基因组计划”的第一个主要负责人，推动了这项揭示人类所有遗传信息的宏伟工程的启动。可以看到，沃森所从事的这几项主要研究都属于生命科学发展中里程碑性的工作。那么，沃森是如何从众多平常的、琐碎的研究中选出他认为值得做的事情？读完了《双螺旋》，你可能就会找到答案。

怎样进行自己选定的研究，并取得突出的成绩?这一问题显然也是科研人员必须回答的基本问题。沃森交出来的又是一份独特的答卷。包括沃森本人在内的许多科学家都认为沃森比较懒惰。沃森自己说，在芝加哥大学做大学生时，“设法免修任何看来是中等难度的化学或物理学课程”；在印第安纳大学做研究生时，“希望不必学习任何化学就能解决基因问题”；而选择做博士后的地点时，沃森又是这样考虑：“在卢里亚.德尔布鲁克的圈子里，对早期生活的不断回忆使我对欧洲产生了一种清晰的印象，觉得欧洲慢节奏的传统更有利于产生一流主意”。

看来，沃森喜欢的是要有足够的时间从事思考，而不是整天在实验室里忙忙碌碌。那个时代的研究者似乎都很看重这一点。有一次沃森向德尔布鲁克抱怨其博士论文乏味，德尔布鲁克是这样回答沃森：论文的乏味是他的运气，不然的话他就只会跟着论文转而没有时间去思考。有些研究者对沃森取得的成绩不服气，认为沃森在剑桥所做的不过是打网球和追女孩子。卡文迪什实验室的负责人佩鲁茨作为沃森的指导者，对这种指责给予了精彩的反驳：“他做出这个重大发现的部分原因是他从不会错误地混淆努力工作和努力思考，他总是拒绝将它们相互取代，这样他当然

有时间打网球和追求女孩子。”那么，沃森又是如何把自己的“白日梦”变成了科学的突破?读完了《双螺旋》，你可能就会找到答案。

吴家睿

注：本文中的引文来自《双螺旋——发现DNA结构的故事》(科学出版社，1984年版)和《创世纪的第八天——20世纪分子生物学革命》(上海科学技术出版社，2005年版)。

Foreword by Sir Lawrence Bragg

This account of the events which led to the solution of the structure of DNA, the fundamental genetical material, is unique in several ways. I was much pleased when Watson asked me to write the foreword.

每一个研究者都渴望着这样的评价。

There is in the first place its scientific interest. The discovery of the structure by Crick and Watson, with all its biological implications, has been one of the major scientific events of this century. The number of researches which it has inspired is amazing; it has caused an explosion in biochemistry which has transformed the science. I have been amongst those who have pressed the author to write his recollections while they are still fresh in his mind, knowing how important they would be as a contribution to the history of science. The result has exceeded expectation. The latter chapters, in which the birth of the new idea is described so vividly, are drama of the highest order; the tension mounts and mounts towards the final climax. I do not know of any other instance where one is able to share so intimately in the researcher's struggles and doubts and final triumph.

在这段话里，L. Bragg点出了这本书的特色和价值。

Then again, the story is a poignant example of a dilemma which may confront an investigator. He knows that a colleague has been working for years on a problem and has accumulated a mass of hard-won evidence, which has not yet been published because it is anticipated that success is just around the corner. He has seen this evidence and has good reason to believe that a method of attack which he can envisage, perhaps merely a new point of view, will lead straight to the solution. An offer of collaboration at such a stage might well be re-

优先权和独创性是研究者追求的核心目标。而在科学史

garded as a trespass. Should he go ahead on his own? It is not easy to be sure whether the crucial new idea is really one's own or has been unconsciously assimilated in talks with others. The realization of this difficulty has led to the establishment of a some-what vague code amongst scientists which recognizes a claim in a line of research staked out by a colleague—up to a certain point. When competition comes from more than one quarter, there is no need to hold back. This dilemma comes out clearly in the DNA story. It is a source of deep satisfaction to all intimately concerned that, in the award of the Nobel Prize in 1962, due recognition was given to the long, patient investigation by Wilkins at King's College (London) as well as to the brilliant and rapid final solution by Crick and Watson at Cambridge.

中，包括在DNA双螺旋的发现史中，可以看到这种目标是逐渐逼近的，是在与社会、与他人互动的过程中产生并形成的。因此没有纯粹的，与他人、社会无关的独创性观点。

Finally, there is the human interest of the story—the impression made by Europe and by England in particular upon a young man from the States. He writes with a Pepys-like frankness. Those who figure in the book must read it in a very forgiving spirit. One must remember that his book is not a history, but an autobiographical contribution to the history which will some day be written. As the author himself haue said, the book is a record or impressions rather than historical facts. The issues were often more complex, and the motives of those who had to deal with them were less tortuous, than he realized at the time. On the other hand, one must admit that his intuitive understanding of human frailty often strikes home.

科学内容是客观的、价值中立的；但是科学家和科学研究的过程则是主观的，有各种价值冲突的。这二者的抉择就构成了一部科学史。这也就是这本书值得我们一读之处。

这一段话在某种程度上试图去理解和解释本书为什么受到书中一些当事人的批评和哈佛大学出版社的拒绝。

The author has shown the manuscript to some of us who were involved in the story, and we have suggested corrections of historical fact here and there, but personally I have felt reluctant to alter too much because the freshness and directness with which impressions have been recorded is an essential part of the interest of this book.

W. L. B.

L. Bragg的心胸令人感动，正可谓"宰相肚里能行船"。

Preface

Here I relate my version of how the structure of DNA was discovered. In doing so I have tried to catch the atmosphere of the early postwar years in England, where most of the important events occurred. As I hope this book will show, science seldom proceeds in the straightforward logical manner imagined by outsiders. Instead, its steps forward (and sometimes backward) are often very human events in which personalities and cultural traditions play major roles. To this end I have attempted to re-create my first impressions of the relevant events and personalities rather than present an assessment which takes into account the many facts I have learned since the structure was found. Although the latter approach might be more objective, it would fail to convey the spirit of an adventure characterized both by youthful arrogance and by the belief that the truth, once found, would be simple as well as pretty. Thus many of the comments may seem one-sided and unfair, but this is often the case in the incomplete and hurried way in which human beings frequently decide to like or dislike a new idea or acquaintance. In any event, this account represents the way I saw things then, in 1951—1953: the ideas, the people, and myself.

任何一个试图真正理解科学家、理解科学研究工作的人都应该仔细地理解这一段话。即使在今天，仍然有许多人，包括许多研究者，总是把科学家视为不食人间烟火的圣人，认为科学发现是一个纯粹客观的活动。显然，这些看法与真实的科学界相去甚远。

I am aware that the other participants in this story would tell parts of it in other ways, sometimes because their memory of what happened differs from mine and, perhaps in even more cases, because no two people ever see the same events in exactly the same light. In this sense, no one will ever be able to write a definitive history of how the structure was established. Nonetheless, I feel the story should be told, partly because many of my scientific friends have expressed curiosity about how the double

纯客观的历史是不存在的。那么，什么是历史，什么是真正的反映科学进程的历史？如果有，应该如何再现这种历史？如果没有，那么我们应该怎样看待过去？

这就是Watson写这部书的动机，也是我们阅读这部书的主线条。

helix was found, and to them an incomplete version is better than none. But even more important, I believe, there remains general ignorance about how science is "done". That is not to say that all science is done in the manner described here. This is far from the case, for styles of scientific research vary almost as much as human personalities. On the other hand, I do not believe that the way DNA came out constitutes an odd exception to a scientific world complicated by the contradictory pulls of ambition and the sense of fair play.

今天可能很少有人在做发现时同时拥有相类似的想法。更少有人将此想法付诸实施。当然，也没有几个人能有发现DNA双螺旋这样的运气。

The thought that I should write this book has been with me almost from the moment the double helix was found. Thus my memory of many of the significant events is much more complete than that of most other episodes in my life. I also have made extensive use of letters written at virtually weekly intervals to my parents. These were especially helpful in exactly dating a number of the incidents. Equally important have been the valuable comments by various friends who kindly read earlier versions and gave in some instances quite detailed accounts of incidents that I had referred to in less complete form. To be sure, there are cases where my recollections differ from theirs, and so this book must be regarded as my view of the matter.

Some of the earlier chapters were written in the homes of Albert Szent-Györgyi, John A. Wheeler, and John Cairns, and I wish to thank them for quiet rooms with tables overlooking the ocean. The later chapters were written with the help of a Guggenheim Fellowship, which allowed me to return briefly to the other Cambridge and the kind hospitality of the Provost and Fellows of King's College.

As far as possible I have included photographs taken at the time the story occurred, and in particular I want to thank Herbert Gutfreund, Peter Pauling, Hugh Huxley, and Gunther Stent for sending me some of their snapshots. For editorial assistance I'm much indebted to Libby Aldrich for the quick, perceptive remarks expected

from our best Radcliffe students and to Joyce Lebowitz both for keeping me from completely misusing the English language and for innumerable comments about what a good book must do. Finally, I wish to express thanks for the immense help Thomas J. Wilson has given me from the time he saw the first draft. Without his wise, warm, and sensible advice, the appearance of this book, in what I hope is the right form, might never have occurred.

J. D. W.

Harvard University
Cambridge, Massachusetts
November 1967

For Naomi Mitchison

In the summer of 1955, I arranged to join some friends who were going into the Alps. Alfred Tissieres, then a Fellow at King's, had said he would get me to the top of the Rothorn, and even though I panic at voids this did not seem to be the time to be a coward. So after getting in shape by letting a guide lead me up the Allinin, I took the two-hour postal-bus trip to Zinal, hoping that the driver was not carsick as he lurched the bus around the narrow road twisting above the falling rock slopes. Then I saw Alfred standing in front of the hotel, talking with a long-mustached Trinity don who had been in India during the war.

Since Alfred was still out of training, we decided to spend the afternoon walking up to a small restaurant which lay at the base of the huge glacier falling down off the Obergabelhorn and over which we were to walk the next day. We were only a few minutes out of sight of the hotel when we saw a party coming down upon us, and I quickly recognized one of the climbers. He was Willy Seeds, a scientist who several years before had worked at King's College, London, with Maurice Wikins on the optical properties of DNA fibers. Willy soon spotted me slowed down, and momentarily gave the impression that he might remove his rucksack and chat for a while. Bu all he said was, "How's Honest Jim?" and quickly in creasing his pace was soon below me on the path.

Later as I trudged upward, I thought again about our earlier meetings in London. Then DNA was still a mystery, up for grabs, and no one was sure who would get and whether he would deserve it if it proved as exciting as we semisecretly believed. But now the race was over

and, as one of the winners, I knew the tale was not simple and certainly not as the newspapers reported. Chief it was a matter of five people: Maurice Wilkins, Rosalin Franklin, Linus Pauling, Francis Crick, and me. And as Francis was the dominant force in shaping my part, I will start the story with him.

Francis Crick and J. D. Watson during a walk along the backs. In the distance, King's College Chapel.

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