# 从流代数到量子色动力学

From Current Algebra
to Quantum Chromodynamics
A Case for Structural Realism

## 结构实在论的一个案例

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范岱年 吴忠超 校

中国科学技术出版社
CHINA SCIENCE AND TECHNOLOGY PRESS

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# 总　序

　　科技是人类智慧的伟大结晶，创新是文明进步的不竭动力。当今世界，科技日益深入影响经济社会发展和人们日常生活，科技创新发展水平深刻反映着一个国家的综合国力和核心竞争力。面对新形势、新要求，我们必须牢牢把握新的科技革命和产业变革机遇，大力实施科教兴国战略和人才强国战略，全面提高自主创新能力。

　　科技著作是科研成果和自主创新能力的重要体现形式。纵观世界科技发展历史，高水平学术论著的出版常常成为科技进步和科技创新的重要里程碑。1543 年，哥白尼的《天体运行论》在他逝世前夕出版，标志着人类在宇宙认识论上的一次革命，新的科学思想得以传遍欧洲，科学革命的序幕由此拉开。1687 年，牛顿的代表作《自然哲学的数学原理》问世，在物理学、数学、天文学和哲学等领域产生巨大影响，标志着牛顿力学三大定律和万有引力定律的诞生。1789 年，拉瓦锡出版了他的划时代名著《化学纲要》，为使化学确立为一门真正独立的学科奠定了基础，标志着化学新纪元的开端。1873 年，麦克斯韦出版的《论电和磁》标志着电磁场理论的创立，该理论将电学、磁学、光学统一起来，成为 19 世纪物理学发展的最光辉成果。

　　这些伟大的学术论著凝聚着科学巨匠们的伟大科学思想，标志着不同时代科学技术的革命性进展，成为支撑相应学科发展宽厚、坚实的奠基石。放眼全球，科技论著的出版数量和质量，集中体现了各国科技工作者的原始创新能力，一个国家但凡拥有强大的自主创新能力，无一例外也反映到

其出版的科技论著数量、质量和影响力上。出版高水平、高质量的学术著作，成为科技工作者的奋斗目标和出版工作者的不懈追求。

中国科学技术协会是中国科技工作者的群众组织，是党和政府联系科技工作者的桥梁和纽带，在组织开展学术交流、科学普及、人才举荐、决策咨询等方面，具有独特的学科智力优势和组织网络优势。中国长江三峡集团公司是中国特大型国有独资企业，是推动我国经济发展、社会进步、民生改善、科技创新和国家安全的重要力量。2011年12月，中国科学技术协会和中国长江三峡集团公司签订战略合作协议，联合设立"中国科协三峡科技出版资助计划"，资助全国从事基础研究、应用基础研究或技术开发、改造和产品研发的科技工作者出版高水平的科技学术著作，并向45岁以下青年科技工作者、中国青年科技奖获得者和全国百篇优秀博士论文奖获得者倾斜，重点资助科技人员出版首部学术专著。

由衷地希望，"中国科协三峡科技出版资助计划"的实施，对更好地聚集原创科研成果，推动国家科技创新和学科发展，促进科技工作者学术成长，繁荣科技出版，打造中国科学技术出版社学术出版品牌，产生积极的、重要的作用。

是为序。

# 序　一

1960 年至 1980 年的 20 年间是基本粒子物理学发展的黄金时期。这一时期最重要的进展是强作用力理论的发展。在这一时期之初，几乎很难想象在 20 年间会对这种作用力有如此全面的理解，即现今用量子色动力学给予的简明表达。

这本书非常准确娴熟地追溯了这一发展。作为物理学家，我更感兴趣这一主题的"内部细节"，超出其哲学含义，因此，我特别看重，作者以高水准的科学精确性和对"历史细节"的全面把握，对物理学强有力的掌控。同样对我重要的是，这部著作超越了历史细节探讨的层面，极好地讨论了科学是如何演进的，以及其成果的科学哲学含义。虽然这不是我熟悉的领域，但是作者在最后几章中提出的议题还是很吸引我。

正如这本书详细描述的，量子色动力学的演化涉及几场概念革命。在 1960 年，S 矩阵理论强烈地影响了当时的理论背景（第 2 章）。正如回顾往事时我所理解的，这一研究方法的诞生是因为人们对强作用力的基本理论——像 QCD 那样的——何时会出现感到绝望。这一研究方法设法巧妙地开发了一些一般原理（例如"微观因果性""解析性""幺正性"）。这一研究方法使得强作用力理论变得稳固，并产生了真正的进展。但是，在我看来，最重要的进展是，S 矩阵理论为盖尔曼提出流代数做好了准备（第 2、第 3 章）。虽然流代数本身从过去就扎根于 S 矩阵概念中，但是流代数指向了通往未来的道路。的确，正如我们现在寻找超越标准模型的未来理论一样，可能从这一历史性的记述中能极好地获得今天我们应该注意的重要教训。

流代数的建筑砖块——流及其相伴电荷，既对那些称作强子（质子、中子、介子等）的强相互作用的粒子，也对那些称作轻子（电子、中微子等）的其他粒子给出了唯象的定义。轻子流和电荷的属性是相当明晰的。盖尔曼巧妙地提出了强子流和电荷可能有类似属性的想法。归根到底，流代数层面上这种轻子-强子对应关系导致了一组特别丰富的结果。

最丰富的一个领域证明是所谓的深度非弹性散射过程（第4～6章），这是我本人参与的一个领域。在本书中，作者对我在这段历史中的作用给予了十分慷慨的评价，我很高兴也很自豪自己已成了那场真正的革命中的一分子。这场革命源起于对盖尔曼的流代数方案在短程范围内的重要意义的关注，从而对高能过程，特别是那些涉及轻子和强子相互作用的过程提供了关键性的新洞见。到这场革命结束时，质子和中子中类点组分的思想已从这一结合中涌现出来。在这一发展的中途，费曼参与到这一图像的构造中。他的贡献极大地丰富了这一描述，此后导致了快速的进展。

这场革命为盖尔曼发起的另一场平行的革命所补充。这场平行的革命起源于对实验家们所发现和编目的令人眼花缭乱的丰富的强子共振态的谱系分类。寿命最长的组织原则是盖尔曼在1964年引入分数电荷的夸克，作为强子的基本建筑砖块（第2章）。在那时，这是一个相当奇怪的假说，每个人都花了相当长的时间来熟悉它。但是在这一假说引入后的10年间，它成为了深度非弹性现象和谱系的一个支撑点。而且，夸克的引入也导致了在相同的10年间强相互作用的场论（量子色动力学）的引入。

因此，仅仅15年的跨度，理论视角就发生了极大的转变。1960年否定了把定域场论［诸如量子电动力学（QED）］作为强力理论的原型。到1975年人们已经相当确信，量子电动力学的推广版本，即量子色动力学，实际上是关于强作用力的正确的基本理论。

诚然，理论视角的这一迅速改变，极大地受益于谱系和深度非弹性过程的实验进展。但是，同等重要的是纯理论的进展。这些进展不但起源于强作用力理论的语境，而且也起源于弱作用力理论及其唯象理论的进展。

我辨识出四个标志性的进展，它们在演化过程和含义方面有部分重叠。这四个进展是：（1）非阿贝尔规范理论；（2）自发对称破缺和希格斯机制；（3）这些理论的可重正化；（4）禁闭机制。这四个进展如何历史地演化是一个非常丰富的主题，在我看来，形成了这本书的核心部分（第7~8章）。虽然我想对这些观念如何相互交织作出更多的说明，但是我不打算这样做。这只是因为这是一个太丰富的主题，在这个简短的序中是无法作出清楚的表达的。

不过，我必须再次强调这些发展不仅仅是理论建构。在很大程度上，它们受实验数据的驱动。这些思想如此迅速发展的一个原因是实验的高速进展，而实验的高速进展本身是由一个技术的高速进展所驱动，特别是在粒子加速器物理学领域内。

无论如何，从1980年以来，用夸克、胶子以及一个类似于特别成功的量子电动力学的定域拉格朗日场论来描述强作用力，成为了这一领域的混合语。尽管回顾起来量子色动力学是量子电动力学的一个自然的逻辑推广，虽然量子电动力学本身在1960年已是一个相当成熟的主题，可是当时理解的东西完全不能预期这一概念革命。这些新概念——最著名的是夸克和胶子拥有的禁闭属性——已向科学史家和科学哲学家提出了有趣的问题。例如，假定原则上不能观察到孤立的夸克，那么能认为夸克是"真实的"吗？这本书的第9章极好地阐明了这一问题。我只想补充说"当然，它们是真实的，就如同基本粒子物理学中所有其他东西一样真实"。

然而，我个人总是设法尽可能地避免断定在物理学中什么实体实际上为"真"的一般性问题。我对科学的定义或许太谨慎。但是，不管好坏，我把科学定义为"对自然界的简明描述"。我宁愿用"描述"一词而不用另一个可供选择的"说明"一词，原因很显然，当我们对任何对象探索得足够深时，最终会变得实际上不知道我们正在谈论的究竟是什么。但是，对我来说，好科学的本质还关系到我用以定义科学的另一个关键词——"简明"。粒子物理学的标准模型产生于几页纸的方程，却对大量极其复杂、精

确的观察结果的描述作出了说明。高度有用的物理实体通常具有使描述具有更大的简洁性的品性。只要随着研究对象的演变，这些实体始终保持这样的品性，它们就会一直保留为简明描述的中心要素，能够称作是"真实的"。就此而论，夸克和胶子完全够格具有"真实存在的"的属性。

历史洞察的一个明显好处是提供了从过去学习的机会。在我看来，这本书的内容有重要的教益，关系到产生物理学的下一次革命的问题。当然，关于这次革命如何发生，我必须诉诸猜想。在此，我愿意猜想希格斯玻色子的最新观察结果可能是催化剂。新的数据总是产生标准模型现存问题更明显的关注点。最重要的关注点极有可能是家族结构的问题（夸克和轻子为什么有三代这一令人困惑的问题）。在我看来，这个问题与理解希格斯区本性的问题纠缠在一起，似乎是合理的。

我认为，这个问题的一种解决策略未被充分使用。这种策略是把流代数的方法论应用于这一问题。有足够多的流算子和电荷算子，它们能建构成各式各样的夸克和轻子场的双线性乘积。从这些流出发，人们能建构关联函数并创造流代数的老的求和定则的类似物。甚至对于这种求和定则的最简单类型——温伯格求和定则，实际上有数以千计的考虑，特别是要仔细考虑它们可能饱和的方式。对这个问题的仔细审查只可能以不完全类似于本书已描述的方式结出果实。如果结果证明是这样，对年轻的研究者来说，一个极好的策略是从历史中汲取经验教训。我认为仔细研究这本极优秀的著作是最好的起点。

詹姆斯·比约肯

2013 年 7 月

斯坦福大学物理学教授（荣誉退休）

# Foreword

The decades between 1960 and 1980 represent a Golden Age for elementary particle physics. Central to the progress made during this period was the development of the theory of the strong force. At the beginning of this period, it was almost unthinkable that in twenty years there would be a comprehensive understanding of this force, now concisely expressed by the acronym QCD.

This book traces this development with great accuracy and skill. Important to me, a working physicist interested more in the inner details of the subject than in the philosophical implications, is that the author has a firm command of the physics, with a high level of scientific accuracy as well as a comprehensive grasp of the historical details. Equally important to me is that the subject matter goes well beyond that level, into issues of how science is done as well as the implications of the results for the philosophy of science. While this is less comfortable territory for me, I am very fascinated with the issues which are raised in the last chapters.

As is described in detail in this book, the evolution of QCD involved several conceptual revolutions. In 1960, the theoretical setting was strongly influenced by S−matrix theory ( Chapter 2 ). As I see it in retrospect, this approach was born out of a sense of despair that a fundamental theory for the strong force—like QCD— ever could appear. It made do with skillful exploitation of general principles ( e. g. "microscopic causality", "analyticity", "unitarity" ). This approach anchored the theory and produced real progress. But the most important consequence, in my

opinion, is that S – matrix theory set the stage for Gell – Mann's current algebra (Chapters 2 and 3). While current algebra was itself anchored in S – matrix concepts from the past, current algebra pointed the way far into the future. Indeed, as we now search for the future theory that extends beyond the Standard Model, there may well be important lessons from this historical account that should be heeded today.

The building blocks of current algebra – currents and their associated charges—were phenomenologically defined, both for the strongly interacting particles known as hadrons (protons, neutrons, mesons, etc.) and for others known as leptons (electrons, neutrinos, etc.). The properties of the lepton currents and charges were quite unambiguous. Gell – Mann skillfully exploited the idea that perhaps the hadron currents and charges might have analogous properties. In the long run, this lepton – hadron correspondence at the current – algebra level led to an especially rich set of consequences.

One of the richest areas turned out to be what is known as deep – inelastic scattering processes (Chapters 4–6). It is an area in which I myself participated, and thereby obtained some notoriety. In the book, the author is quite generous with regard to my own role in this story, perhaps overly so. That is not for me to judge. But no matter what, I am pleased and proud to have been part of the genuine revolution that took place. The origin of the revolution lay in concentrating on the implications of the Gell – Mann current algebra scheme in the limit of short distances, where high energy processes, especially those involving mutual interactions of leptons with hadrons, provided crucial new insights. By the end of the revolution, the idea of pointlike constituents within protons and neutrons had emerged from this mix. In the midst of this development, Richard Feynman entered the picture. His contributions greatly enriched this description, and led to

a rapid rate of progress thereafter.

This revolution was supplemented by a parallel revolution, also initiated by Gell-Mann. It originated in the spectroscopy and classification of the bewildering plenitude of hadron resonant states that had been discovered and catalogued by the experimentalists. The organizing principle that turned out to have the greatest longevity was his introduction in 1964 of fractionally charged quarks as the basic building blocks of the hadrons (Chapter 2). At the time this was a rather bizarre hypothesis, and it took everyone quite some time to get used to it. But within a decade of its introduction, it became an anchor point for the deep-inelastic phenomenology as well as for the spectroscopy. Furthermore, the introduction of quarks led as well to the introduction of the field theory of the strong interaction, QCD, within the same decade.

Thus in the span of just 15 years, the theoretical perspective shifted greatly. In 1960 there was the dismissal of local field theories such as quantum electrodynamics (QED) as a prototype for the theory of the strong force. In 1975 there was already considerable confidence that a generalized version of QED, namely QCD, was in fact the correct, fundamental theory of the strong force.

This rapid change in perspective was indeed greatly aided by the experimental developments, both in spectroscopy and in deep – inelastic processes. But equally important were purely theoretical developments. These originated not only in the context of the theory of the strong force, but also in the developments occurring within the theory and phenomenology of the weak force. I identify four landmark developments, all of which overlap in their evolution and their implications. They are (1) non-Abelian gauge theories, (2) spontaneous symmetry breaking and the Higgs mechanism, (3) renormalizability of such theories, and (4) the confinement mechanism. How these four developments

evolved historically is a very rich subject, and forms in my opinion the centerpiece of this book (Chapters 7−8). While I am tempted to say more about how these ideas interlock, I will not do so. It seems simply to be too rich a subject to be addressed in this brief foreword.

Nevertheless, I must again emphasize that these developments were not mere theoretical constructions. They were to a large extent data−driven. And one reason the ideas developed so quickly was that there was a high rate of experimental progress, which itself was driven by a high rate of technological progress, especially within the field of particle−accelerator physics.

In any case, from 1980 onward the description of the strong force in terms of quarks, gluons, and a local Lagrangian field theory (QCD) akin to the extraordinarily successful quantum electrodynamics (QED) became the lingua franca of the field. Although QCD is in retrospect a natural and logical generalization of QED, which itself was a rather mature subject in 1960, the conceptual revolution was not at all anticipated by what was understood at that time. And these new concepts—most notably the confinement property possessed by quarks and gluons − have raised interesting issues for the historians and philosophers of science. For example, can quarks be regarded as "real", given that they cannot in principle be observed in isolation? This question is neatly addressed in Chapter 9 of this book. And I only will add "of course they are as real as anything else in elementary−particle physics."

However, I personally try to avoid as much as possible the general question of deciding what entities in physics are truly "real". My definition of science is perhaps too modest. But, for better or worse, I define science as "a concise description of nature." I prefer the word "description" to the alternative "explanation", for the obvious reason that, when we probe any subject deeply enough, we end up not really knowing what we are talking about. But for me the

essence of good science lies in the other crucial word in my definition—"concise". The standard model of particle physics originates in a few pages of equations, but accounts for the description of a vast variety of extremely detailed, precise observations. Highly useful physical entities usually have the property of allowing greater conciseness of description. As long as such entities continue to do so as the subject evolves, they will remain as central elements of the concise description, and can be designated as "real". In this regard, the quark and the gluon richly deserve the attribution "real".

One of the clear benefits of historical insight is the opportunity to learn from the past. In my opinion, there exist important lessons within the content of this book which are very relevant to the problem of creating the next revolution in physics. I must of course resort to guesswork as to how this revolution will occur. I will here suppose that the recent observation of the Higgs boson will be the catalyst. It is fresh data which already creates a sharper focus on the extant problems of the Standard Model. And the most important focus may well be on the question of family structure ( the puzzling problem of why there are three generations of quarks and leptons). It seems reasonable to me that this problem is entangled with that of understanding the nature of the Higgs sector.

One strategy for dealing with this question has been, in my opinion, underutilized. It is to apply the methodology of current algebra to the problem. There is no shortage of current operators and charge operators which can be constructed as bilinear products of the sundry quark and lepton fields. And from these currents, one can construct correlation functions and create analogues of the old sum rules of current algebra. Even for the simplest class of such sum rules, the Weinberg sum rules, there are literally thousands to consider, and in particular to contemplate the way in which they might be saturated. Serious examination of this problem just might bear fruit in a way which is not totally dissimilar to what has

been recounted in this book. If this were to turn out to be the case, a very good strategy for the young researcher would be to take a lesson from history. And I can think of no better starting point than the careful study of this excellent book.

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James Bjorken
Professor of Physics
Emeritus, Stanford University

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# 序 二

　　曹天予教授的《从流代数到量子色动力学》，就和他的第一部著作《二十世纪场论的概念发展》一样，是一部划时代的杰作。

　　他的第一部著作是回顾经典牛顿-爱因斯坦模型和量子"标准模型"的演化，以及与此相对应的物理世界的观念从机械本体向场本体的变革。此后，他很关注 20 世纪 40 年代后期到 70 年代后期强作用物理方面激动人心的发展，无论是在物理还是在科学哲学的层面上。他的研究成果以两本专著来发表，其中即将出版的《量子色动力学的形成过程》将侧重于各种概念演化的历史研究，而我们面前的这部书则从结构实在论的观点，对从流代数方法到量子色动力学的主要进展作哲学分析。

　　本书探源溯本，首先描述强作用力的理论发展史，尤其是 1962 年至 1972 年间的概念发展。1935 年汤川秀树提出强作用介子理论，但介子理论是不成功的，那是由于介子核子耦合常数太大，后来模仿量子电动力学微扰展开和重正化的技巧无法施展。1953 年，泡利试图推广量子电动力学的思想，提出用非阿贝尔规范理论来描写核相互作用，但他认识到了规范粒子零质量与相互作用的短程性相冲突的困难，这个矛盾只有在自发对称破缺提出后才得以解决，那是很后来的事了。朗道及其同事认为，由于深层的原因，诸如定域场算符，微观因果律，以及没有可以接受的拉格朗日函数作为出发点等，量子场论在处理强作用时陷入困境，不能简单地由拓广量子电动力学的框架来描述强作用。

　　在这种背景下，人们转向海森堡和朗道倡导过的，丘等策划的解析 S 矩阵研究纲领（色散理论、靴袢、最大解析性、雷吉轨迹）。由于不能确定哪

些强子是基本的，邱甚至反对基本粒子的存在，拒绝承认复合粒子和基本粒子的差异，这在某种意义上倡导了粒子的民主性。解析 S 矩阵研究是唯象的，后来让位于盖尔曼的流代数纲领。

由于受费米弱作用理论，守恒矢量流和部分守恒的轴矢量流的启示，盖尔曼干脆把强子流当成首要的实体，由此提取其可能的属性，尤其是实验数据所隐含的对称性。1964 年，他和茨威格提出了重子和介子是假想的 SU（3）群的三重态粒子，所谓的"夸克"的束缚态的想法，认为夸克模型可以得到这些近似对称性，但他并没有将其当作强子的真正建筑砖块。曹天予强调，盖尔曼起初把他提出的夸克（流夸克）当成纯粹的数学实体，而茨威格的组分夸克模型的支持者则认为组分夸克是真实的粒子或强子的真实组分。这两种夸克观之间的关系在物理学上、哲学上都极为复杂，双方争论不休。直到 1972 年春天，才由盖尔曼的学生对此作出了初步厘清，并由此为量子色动力学的诞生提供了一个必要的前提。

由流代数结果界定的夸克结构特征的实在性，是经由和诸如在斯坦福直线加速器中心的深度非弹性电子–质子散射实验等一致而建立的，这些实验发现夸克的最重要特征是它们的类点性质和短程无相互作用。1971 年，因为泡利不相容原理，盖尔曼和弗里奇引进了新的称为"色"的量子数。当他们以 SU(3)色对称为规范群给出正确的量子色动力学拉氏量后，普利策、格罗斯和维尔泽克等利用重正化群中的卡伦–希曼齐克方程证明，相关的非阿贝尔规范理论导致所谓的渐近自由，即短程无相互作用。渐近自由还解释了比约肯的标度无关性和对数破坏。

渐进自由的发现使我们在高能端能做微扰计算，并受到实验的验证，使量子色动力学被当成实际的物理理论而接纳。不过直到今日，仍然无法做低能端束缚态计算。

尽管作者的主要动机是这些发展背后的哲学含义，然而仅从科学史，即人类智力奋斗史的角度，这已是一部渊博而翔实的量子色动力学史。此前从未有过描述这个激动人心的探索历程的类似的专著，在可见的将来恐