PERSPECTIVES LECTURE

Recent Trends in the Historiography of Science

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Current Historiography of Science

The history of science has become a professional discipline only in the professional lifetime of people who are just reaching retirement. It's still a relatively new and exciting field, and welcome aboard. Professor Davenport has been good enough to provide a copy of the program you are following, and I wish I could take your course. I was a student of chemistry once upon a time and have ventured to write a few things concerning its history. It's gone a bit rusty, however; it will be better to leave history of chemistry proper to Professor Davenport and the other members of your faculty, and to say something of the current posture of scholarship in the historiography of science generally and illustrate it with a few examples related to your theme.

This is not something that will come out of the readings in which you are engaged. They are addressed to history of chemistry proper, as they need to be for your purpose, which is to develop historical material to be used in the teaching of science itself. There is not a lot of current writing in the field that would be applicable to that purpose, a most important purpose. In the last several decades, the emphasis has changed. Partington, Ihde, and I, when writing the chapter in *The Edge of Objectivity* you are now reading, were concerned with development of knowledge of the forces and structure of nature (1). That was the central thrust of the historiography of science down through the 1960s. The concern now is with the relation of science, not to nature, but to the forces and structure of society. This is not merely a question of what used to be meant by social history, that is to say, studies of the place of scientists in society at large together with the sociological characteristics of the scientific community. The term now is "Social Construction of Science." It signifies that, in the view of practitioners of this approach, the content of science is determined, not by technical consideration, not by its success in giving an accurate account of natural phenomena, but by interactions of the scientists who produce it, both among themselves and in the larger society (2).

The question is not whether a finding is right or wrong, nor whether a theory is true or false. In this view that is as meaningless as saying that a work of art or literature, or political theory is right or wrong, true or false. What determines whether a finding prevails is the persuasiveness, the style, the influence, the professional manipulations, the power ultimately of the scientists who developed it. Facts are not found. They do not reside in nature. They are constructed in the laboratory. All facts are, in a word, artifacts. Thus, the reason that the oxygen theory prevailed over the phlogiston theory was not that it yielded a more rational account of the phenomena of combustion. Not at all. The success of oxygen was owing to the authority Lavoisier exercised in the scientific establishment, and beyond that in the French state. He was administrator of the munitions industry. His laboratory was an official installation in the

Arsenal of Paris. His disciples got the best jobs. Around him gathered an entire team—Fourcroy, Guyton de Morveau, Berthollet, and others. They collaborated in extending the defeat of phlogiston into a general reformation of the system of chemical knowledge, creating a new language in which chemical agents were denoted and classified in accordance with their relations in nature. They devised the nomenclature we still use. Against this powerhouse, what chance was there for a forlorn and unorganized set of preachers, pharmacists, and amateurs like Priestley, Kirwan, Baumé, and the diminishing gaggle of phlogistonists? Lavoisier's Achilles heel was not that he overextended oxygen to make it the principle of acidification as well as combustion, though as you know, he did do that. It was that in the French Revolution, he was hopelessly compromised by his identification with the power structure of the old regime, and went to the guillotine. His overbearing, almost mathematical approach thereupon gave way to the more physical chemistry developed by Berthollet, which prospered in keeping with the activist spirit of the revolutionary Republic and the Napoleonic Empire.

It is not, in short, the structure of nature that determines the findings of science. It is just the other way about. The procedures of science are what construct our notion of nature, in whole and in part. Different procedures would produce different notions, and what determines which ones prevail reduces in the last analysis to a question of the power of its proponents and of the interests they serve. Now, I do not wish to leave the impression that all or a majority of historians of science subscribe to this outlook, or even that all those drawn to study the sociological reaches of science take the extreme position I have stated. Still, in any discipline there is usually a certain set of problems or an approach that is at the forefront at any juncture, and there is no doubt that the hot topics in the history of science nowadays are its sociological and cultural dimensions. I might just say a word about the reasons for that. It is the manifestation in our small discipline of the general movement of scholarship in the humanities that goes by the term deconstruction in studies of art and literature. There the point is no longer to develop appreciation of the picture or the text, nor to judge of its merit aesthetically, morally, or otherwise. Instead, the purpose is to exhibit how any expression of a culture, scientific, literary, artistic, whatever, serves the interest of the power groups that define the society. Thus, all study of culture reduces at bottom to a question of politics.

That problems are posed this way derives in large part from the sea-change that came over sensibility a generation ago, in the so-called cultural revolution of the late 1960s and early 1970s. It took its impetus from the extreme skepticism which then set in among young people generally concerning the rationale of a liberal society and the possibility of objective and disinterested knowledge or actions. The more radical among the coming generation of scholarship found their prophets in the writings of the French philosopher, Michel Foucault, and the literary critic, Jacques Derrida. With respect to science there is the added incentive of showing that its pretentions to abstract truth or validity or objectivity because of its correspondence to natural reality are illusory and that its statements are no less uniquely derivative from socio-political processes than those of economists, novelists, or painters. The message can seem the more plausible in the light of publicity given in recent years to the scandals of science, the Rochester affair, the Gallo investigation, etc.

Whatever one thinks of this program in general, the emphasis on social processes of science has at least one signal merit. It has focussed attention on the actual conduct of experiment. Among the criticisms leveled at the older historiography is a just one, which is that it concerned itself almost entirely with the history of theory—the Copernican theory, the theory of gravity, the oxygen theory, the theory of natural selection, whatever; and theory is at best half the story. In the view of sociological historians, theory is much the less revealing half. Their concern is with the making of science, and they lose interest in it once it's made. Still, the summons to give experiment its due has been stimulating throughout the discipline, and I thought I would give an account of two pieces of work, concerning respectively Boyle and Lavoisier, one by a pair of scholars who represent the social constructionists at their most extreme, the other by an historian of moderate temper, both focussing on the role of experiment in the fabrication of science.

A Social Constructist Example

The first, I have to say, is the more amusing. *Leviathan and the Air Pump* by Simon Schaffer and Steven Shapin is a very entertaining book, even if I find it a bit exasperating (3). Published in 1985, it has had a great success. The topic is the dispute between, on the one side, Boyle and the circle of experimental philosophers who set the agenda for the Royal Society, and on the other, the philosopher Thomas Hobbes, author of the classic justification of absolutist monarchy, which he entitled

Leviathan, and which comprised a philosophy of materialism.

The centerpiece is Boyle's famous air pump, which initiated the century-long British research program in pneumatic physics and chemistry. He designed it for the purpose of performing experiments in vacuum as a refinement on the cumbersome hemispheres with which Otto von Guericke, Bürgermeister of Magdeburg, had dramatized the reality of air pressure by failing to drag them apart with two teams of horses. This was a mere stunt. The apparatus was good for nothing else. Boyle's machine was a table-top affair. It consisted of two main parts: a receiver from which the air was to be exhausted and a pumping apparatus. The receiver was a glass globe with a capacity of about 30 quarts and an aperture with a tight seal at the top through which experimental objects could be installed. It was connected through a valve at the bottom with the pump, and a brass cylinder about 14 inches high and three inches in diameter fitted with a piston of tanned shoe leather oiled so as to be practically air-tight. It was operated by a rack and pinion device and sucked the air out of the receiver in successive strokes, each requiring greater effort than the last.

Boyle published his first series of trials in 1660, *Experiments Physico-mechanical Touching the Spring of the Air.* We all know the effects he demonstrated: you see a bell rung and hear nothing; a puff of smoke collapses into powder; feathers fall to the bottom like lead weights; the candle gutters out as the air thins; the mouse slowly expires. With each stroke of the piston, the column of mercury descends in a Torricelli barometer until it is almost level with the reservoir in which it stands—but not quite, for the perfect vacuum is unattainable.

Our authors are concerned only incidentally with all this, however, and they take no interest in the corollary enunciation of Boyle's Law (which, to be sure, Boyle demonstrated in compressing a volume of air by increasing the amount of mercury that confined it in a J-tube, and not in the receiver of the air pump). For it is not the use of experimental method in discovery and demonstration of laws of nature that interests them. What interests them about experimentation is the way in which it serves a public relations campaign designed to establish the authority of the new philosophy, which is to say, science.

And it is true that these experiments do not read like an inquiry. Boyle knew ahead of time what the effects would be that he intended to demonstrate. When a trial came out differently, it was because something was wrong with the experiment. For example, a pair of thin marble squares with polished, lightly oiled surfaces would stick together under atmospheric pressure. When they did not separate in vacuo as they should have, Boyle described the experiment as a failure rather than taking it as reason to investigate phenomena of cohesion. In all this, his purpose was not to settle the dispute which had raged in the previous generation about the possibility of a void in nature. That he regarded as a merely metaphysical question, an empty question, so to say, which could never be settled by observation. He was interested in what could be seen and felt, in the action of the pump itself as well as in what happened in the receiver and in the reaction when the pump was operated, the "spring of the air," which any one feels who pumps up a tire by hand.

It might be, Boyle says, that the spring of the air can be explained by thinking of the air as corpuscular—"conceiving the air near the earth to be such a heap of little bodies, lying one upon the other, as may be resembled to a fleece of wool"—and clearly he did think in terms of atoms. But that goes beyond the evidence:

> I shall decline meddling with a subject, which is much more hard to be explicated than necessary to be so by him, whose business in this matter is not to assign the adequate cause of the spring of the air, but only to manifest, that the air hath a spring and to relate some of its effects.

The tactic is the same as Newton's, a little later on. The cause of gravity Newton does not pretend to know. What he demonstrates is the fact of gravity and of its effects. Such, indeed, is the condition that makes the new philosophy, or science, viable: that it have a boundary separating the establishment of physical matter of fact from speculation about causes, and indeed from everything else.

Staking out claims for scientific method, then: this was the real purpose of Boyle's operations with the air pump. Our authors see the campaign as a rhetorical one, transpiring in three stages, or as they say, making use of three technologies: mechanical, literary, and social. The mechanical technology is the air pump itself. Do not imagine experiments performed in the professional privacy of a laboratory. On the contrary, display was central to Boyle's strategy. The air pump was a very expensive instrument. In the eyes of the public, it became the emblem of the mechanical philosophy, the cyclotron of the 17th century. Only a wealthy man could afford one, and only six or seven were ever built. The demonstrations were spectacular. It was exhibited before

Charles II on the occasion of his first visit to the Royal Society. It was regularly trotted out for ceremonies honoring other dignitaries — the Danish ambassador; the Duchess of Newcastle, who was the first woman ever to attend a meeting. Representations of the air pump on title pages and in works of art make it a prime item in the iconography of science.

The motivation is to persuade opinion that experimental philosophy permits seeing, literally "seeing," into the operations of nature in a depth and detail inaccessible to the unaided senses. The telescope and the microscope merely enhance the senses. The air pump reaches further. It establishes (but does not explain) the fact of what is not immediately observable, the spring of the air, by exhibiting what happens in the absence of air. For opinion to be persuaded, there have to be witnesses. The demonstrations have to be public. It is not a question of convincing scientific colleagues of the cogency of some set of findings or the validity of a hypothesis. The target is public opinion, which is to be persuaded that such direct contact establishes irrefutable knowledge about the world, unadulterated by philosophical error, religious belief, or political influence.

For that purpose the relatively small number of witnesses who attend demonstrations in person would scarcely suffice. Experiments must be published to win their readers' assent as vicarious witnesses. The experimental style is not at all that of any prior philosophical or literary genre. The Royal Society enjoins a plain, natural way of writing. The writer effaces himself so as not to intrude upon direct contact between the reader and the facts. Boyle spares the reader no detail. No one could doubt that he actually performed, in just the way he said he did, all the hundreds, nay the thousands, of experiments he reports in this and other writings. His reporting is confident, since it is concerned with matters of fact, but unassuming, since the experimenter must keep out of it. When Boyle does go beyond reporting and ventures a possible explanation of the effects, the tone changes from confident to diffident. Such and such may be the cause, but the best that can be hoped for there is probability

Finally, and we come now to the social dimension, the experimental mode of constituting knowledge of facts has to be the affair of a community and not merely of individuals. It is a public matter. Rules are to be observed. A special kind of conduct is incumbent. Critics are to be countered. Recognition is needed from the authorities, that is from government. Recruits have to be attracted. Careers have to be accommodated. A forum has to be created and maintained. All this, of course, is the reason for founding the Royal Society, where the rules of discourse were explicit. Evidence could be discussed and criticized, but not persons. All discussion of religion, metaphysics, and politics was forbidden. The distresses of the world were not to intrude upon the serenity of science.

Such, in the account of our authors, were the techniques through which Boyle and his contemporaries persuaded themselves that experimental research merely discovered and established matters of fact. In reality, we are told, they were constructing facts artificially, subject to constraints no different from those that bear on writers, theologians, philosophers, or thinkers in any domain. Boyle is thus an early example, and one of the perpetrators, of the illusion that science is a body of knowledge privileged by its correspondence to natural reality and untouched in content, if not in the civic role of its practitioners, by the play of political, economic, and ideological interest that determines other components of culture. Thomas Hobbes, again according to our authors, saw through that illusion at the outset.

Hobbes, in exile throughout the time of Cromwell and the Puritan Commonwealth, was the sharpest critic of the liberal, pluralistic, corrupt regime of the restored monarchy, the chrysalis of just the modern social and political order from which our authors take the scientific enterprise to be derivative. He published *Leviathan* in 1651 amid the disorders of the Civil War. His purpose was to construct a philosophical system that would guarantee civil order. The foundation of knowledge must be notions of cause. Hobbes's was a materialistic world in which the prime causes were matter and motion. Any philosophy worthy of attention must demonstrate causality on the model of the certain demonstrations of geometry. It must command assent to physical and to civic propositions as surely as does the demonstration of a theorem in Euclid. Assent must be total. It must be enforced. And what do we have in Boyle? An air pump that leaks purporting to establish physical facts. A pretense that a handful of amateurs looking over a physicist's shoulder are a guarantee of the factuality of what is going on in an inaccessable device. An argument that claims the status of knowledge for artificially produced appearances while segregating supposed facts from the physical causes underlying them and, worse, which withdraws the sanction of philosophy from civil order. For, so say our authors, the problem of generating knowledge is a problem in politics, and conversely the problem of political order always involves solutions to the problem of knowledge. About that, they hold, Hobbes was right—though they do not say what they think of the authoritarianism on which Hobbes makes order of both sorts depend.

The History of Experiment

Let us turn now to work of another sort, also focussing on the role of the experiment in the production of scientific knowledge, but not serving a sociological or political agenda. Two recent books have won a good deal of attention: Peter Galison's *How Experiments End* and Frederic L. Holmes' *Lavoisier and the Chemistry of Life* (4). Galison is concerned with team research in recent particle physics and with the criteria for deciding that a project is finished. It is an interesting book, but Holmes's seems to me the more successful of the two, and for us it has the further merit of pertinence. Holmes does have a program in his research, but it's not an ideological one. He considers that historians have paid too little attention to what scientists actually do in the laboratory because they have failed to go beyond the published accounts of the finished work. He has completed a series of studies based on the laboratory notebooks of three major figures important to the history of physiology in three centuries: Claude Bernard, Lavoisier, and Hans Krebs.

The book on Lavoisier is the second of this trilogy. Not all scientists have kept records detailed enough to permit reconstituting their experimental practice. Lavoisier did, fortunately. His laboratory registers—or most of them—in 12 great folios are preserved in the archives of the Academy of Science in Paris. Holmes's central interest is Lavoisier's study of respiration, but in the course of recovering that he has gone over the entire record and gives us a portrait which modifies our sense of Lavoisier the man and Lavoisier the scientist in very significant respects.

He is good enough to cite the conjecture in *The Edge of Objectivity* that it might have been in the laboratory that one could penetrate the facade of rationality and method to the human being underneath, and he gives us a far more sympathetic picture of Lavoisier than others do. Lavoisier's thinking is much more tentative than my book made it out to be. The progress from the research program he outlined in 1772 or 1773 to its realization in the *Reflections on Phlogiston* in 1785 is nothing like so preordained. His notes on the many experiments show the difficulty of distinguishing between fixed air, or carbon dioxide, the only gas known at the outset, and pure air, the still unidentified agent of combustion. This difficulty leads him sometimes to confuse oxidation with reduction. There are backslidings in which he refers to phlogiston, either out of habit, or because it avoided that confusion, all the way down to the memoir on acidification in 1778. His actual measurements are often less precise than the figures he reports in print, which are rounded off and sometimes adjusted a little to compensate for an error he knew he had made in some procedure that he didn't take time to repeat. Experiments he describes as repeated many times sometimes weren't, or at least he didn't record them, which is unlikely. He paid far more attention to the detail of Priestley's work than appears in the formal acknowledgments.

What is, perhaps, most interesting, his ideas did not become clear simply in the course of analyzing and recombining mercuric oxide, nitric acid, sulfuric acid, metallic carbonates, and all the rest of it. At times he got so interested in the methods he was using that he lost sight of the problem he was investigating and seems to be concentrating on the means instead of on the ends, sometimes interchanging them. In the laboratory he showed a spontaneity that is carefully repressed in his publications. Also, it was in the actual writing of his memoirs, and not in the manipulation of retorts, distilling columns, and scales that he did some of his best thinking, seeing explanations that went beyond the data in the notebooks and adjusting his published account so as to support his conclusions. He was more apt to doctor the order than the results of his investigation. Clearly, then, he cleaned up his act in the printed memoirs. Now, all scholars and scientists do that to some extent, of course, but Lavoisier led the way toward modern practices in that respect too. He cared more for appearances than was characteristic of his time, perhaps because of his admiration for mathematical science, or perhaps that was the effect of his temperament, or both. At all events, the reality was a lot less programmed, a lot less logical than the appearance.

What do we learn about Lavoisier the scientist? The most important thing is that Holmes modifies the sense of the configuration of his career that has been widely accepted, and that appears in my chapter. According to that picture, Lavoisier gets straight the role of oxygen as the combining agent responsible for combustion, for calcination, and—over-reaching himself—for acidification. He thereupon generalizes the methods he has employed so as to work a reformation in the entire science, and the result is modern chemistry. It is the culmination of his career, completed to all intents and purposes in the framing of chemical nomenclature and in publication of the *Traité élémentaire de chimie* in 1789. True, along the way he occasionally mentioned the probability that oxygen is also the active agent in the respiration of animals, but the subsequent research on respiration, and on organic compounds and reactions, has been seen as an appendix, if not an afterthought, a next stage amputated by the guillotine before it could amount to much.

Not at all, we learn. Holmes's own primary interest is in history of physiology rather than chemistry, but his reconstitution of Lavoisier's physiological research puts the whole career in a new and broader perspective. Lavoisier made tests with animals very early on. In his first experiments on mercuric oxide, for example, he tried to differentiate between the air he got by reducing it with charcoal and the air he got by reducing it without charcoal, that is by heating it. The first test he made was on animals: a bird expired at once in fixed air. Another spent half a minute in the other air and flew away chirping happily when the jar was removed. From then on, he calls it sometimes pure air, sometimes eminently respirable air, sometimes vital air, and this before Priestley taught him that it was a distinctive gas. The eventual shift in terminology to oxygen conceals what is clear from the use of the older term in the notebooks. In fact, its role in sustaining life was always one of the defining properties. For a time, he thought that respiration was a process that separated the vital portion of the atmosphere from air in general. The trouble with that was, of course, that the product of respiration had exactly the wrong properties. Only gradually did he come to appreciate that breathing vitiates air in just the way a candle does. In 1775 he was enormously impressed by Priestley's experiments showing that coagulated sheep's blood changes from dark to bright red and back when transferred between jars of phlogisticated and dephlogisticated air. Where does the change occur in the body? At the surface of the lungs? In the bloodstream? Beyond that Lavoisier finds that there is a difference between the air produced by respiration and that remaining after calcination, but he's not yet clear about the difference between fixed air—carbon dioxide—and mephitic air—atmospheric nitrogen. The same amount of vital air yields a larger volume of the one than the other; the one precipitated limewater; the other did not.

Gradually, then, Lavoisier developed a theory of respiration concurrently with the development of his understanding of calcination and combustion, and his misunderstanding of acidification. He moved towards his completed oxygen theory along all four lines, and not just the three lines of inorganic chemistry. But none of the respiration experiments fitted into finished pieces of research. The main thing he published on respiration—apart from passing allusions—were the experiments he did with Laplace in the ice calorimeter. The subject appeared to be the heat generated by the breathing of the guinea pig in that icy jar rather than the creature's physiology, but in fact it is clear in the notebooks that Lavoisier was thinking very much about the process of respiration and that he had considerable experience in animal physiology. His comprehensive memoir on respiration was the first major work he published after *Reflections on Phlogiston* later in the same year, 1785.

Meanwhile, the notebooks show, he had tried experiments on combustion of plant materials, on the organic acids, especially acetic in connection with acidification, on alcohol and its relation to sugar and water; and on what he saw as processes of nature, especially fermentation and putrefaction. Gradually, he arrived at a definition of plant substances as containing carbon, hydrogen, and oxygen in three-way combinations, and a definition of animal substances as containing in addition azote or nitrogen, and in some cases sulfur and phosphorus. With the oxygen theory completed with respect to inorganic chemistry by 1785, he turned attention mainly to what we would call organic chemistry.

It's not a new departure, however, but rather an extension of his reformation of chemistry to problems of life. Five chapters are devoted to them in the *Traité élmentaire*. He fits them into the same scheme of classification that he employed for organic compounds. Plant substances are acids or oxides of double and triple bases composed of varying proportions of carbon and hydrogen. Vegetable acids contain additional proportions of oxygen. The bases of animal substances are triple and quadruple composites, etc. It's true that the tone of these chapters is different. They read like provisional stages in a research program rather than like the finished system of science in the body of the treatise. Lavoisier uses the same balance sheet method of equating input of the substance undergoing analysis, whether by fermentation or combustion, with the yield in gases, liquid, and solid matter. The classifications don't really work. Still, he arrives at very nearly the modern values for the proportions of carbon, hydrogen, oxygen, and nitrogen in many instances. The techniques may properly be considered the point of departure of organic chemistry. The famous final memoirs of the 1790s, in collaboration with a new assistant, Armand Séguin, are addressed to the animal economy. The purpose is to extend the analysis from respiration to all physiological processes, digestion, excretion, transpiration, that maintain the steady state of matter and heat in the animal body. That work was never finished. What the notebooks show, however, is that it was continuous with the whole course of Lavoisier's reformation of chemistry and also of agriculture, and not a new departure once the chemical revolution was finished.

Critical Conclusion

In conclusion, and for whatever my opinion may be worth, it is obvious that I have a great deal of sympathy for Holmes's treatment of Lavoisier in particular, and more generally for the current historical emphasis on recovering the detail of experimental practice. At the same time, I do have fundamental reservations about the more extreme approach of the social constructionists, the "strong program," as they call it. For one thing, it seems to me hoist by its own petard. If the findings of scientists are thought to be determined, not by the structure of nature but by the sociology of the investigators, is it not equally or still more probable that the same is true of statements by sociologists concerning the structure of science?

That is merely a debating point, however. More substantively, it seems to me that the micro-sociology of research projects can be very illuminating when it is a question of the making of science. But the sociology of research fails when it becomes a question of explaining the success of the outcomes that prevail. Technical considerations and the fit with nature are therein paramount. Though undoubtedly framed by individual persons and groups of persons in a social environment, science has the capacity to transcend the personalities and circumstances of those who produce it. The relation of the finished piece of work to its creator is not the same in science as it is in art or literature. It is obvious that Hamlet and the Mass in B-Minor would not exist if Shakespeare and Bach had never lived. It is otherwise in science. The planets would still move subject to the inverse square law of gravity if Newton had died in infancy or (as he threatened) suppressed the *Principia*. No one else would have composed it, but it is clear that everything in it that really mattered to classical physics would soon have been written down by others in some way. Much the same is true of all, or nearly all, the significant contributions to modern science. The well-known phenomenon of simultaneous and usually independent discovery is all the evidence needed.

Moreover, although the introduction of a piece of science will bear the mark of its creator and of the circumstances in which he worked—of Lavoisier's clarity of mind, in the case of the oxygen theory, and of his interactions with the people around him—still, the personal and social elements that went into the original formulations make no difference to the practice of workaday science once a contribution has left the hand of its creator, and that happens immediately. No one has to retrace the road he took to discovery. Instead, the discovery must be verifiable and workable by any qualified scientist if it is to be science at all. So it is with the cultural context. Until very recently, science was uniquely a product of western civilization. Nowadays, however, the Japanese and Indians, among others, work it in the same way as Europeans and Americans do, and in some instances more effectively. The same cannot be said of the legacy of art, poetry, religion, or political theory originating in the West.

In my view, not perhaps a very fashionable one at the moment, that is because science, though produced in society, is about nature.

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