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Practice: A Missing Link in History and Philosophy of Science

The mythology of science in the twentieth century often conveys the impression that the distinction of pure from applied research should be understood in the sense of idealistic versus materialistic, the clean versus the unclean. Giants of pure thought are heroes, while giants of technology are mere engineers who apply the abstract results of their betters to concrete problems. To enoble a scientist, therefore, he or she must be portrayed as an isolated thinker uninterested in practical matters. Einstein serves as exemplar. The difficulty with this story-line is not that it is entirely false. Like all effective mythologies it captures a part of lived experience. But therein lies the problem. It isolates out of the reality of everyday science a particular component, sets that component up for idealization, and assumes that it is *the* fundamental motor of scientific progress. It thereby ignores, and often dismisses, other major factors in the creative life of science. It becomes an ideology rather than a description.

This ideology deeply infects present-day history and philosophy of science, with its emphasis on theory over experimentation and pure over applied research. Scientific knowledge is often presented as though it consisted in a set of theoretical sentences, that is, in a set of articulated ideas from which deductions are made. Consequently, only those factors which can affect ideas, namely other ideas, can play a positive role in the creation of knowledge. Experimenters provide data for their theoretical colleagues, or test their visionary predictions. Technology makes such testing possible and provides many practical applications of theory. But neither experimenters nor instrument makers nor engineers can play an independent science-forming role. This hierarchy of creation has a moral corollary. With their hands soiled by the furniture of the material world, the world of action and interest, practical people cannot be trusted to guard the gates to truth and virtue, the realm of disinterested contemplation of ideas, where curiosity and the love of truth are the springs of action.

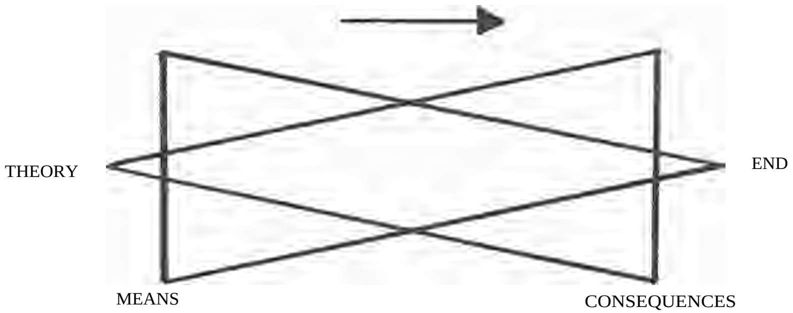
Theoretical and Practical Strategies

I have put the problem in polemical form for emphasis. Probably no one would admit to holding such crudely negative views of practical life. But more sophisticated versions produce the same effect. They systematically fail to recognize that the capacity regularly to carry out an intended practical action is itself a form of knowledge, even if unarticulated, and that such capacities often function productively in the formulation of more theoretical knowledge. I am contending, then, that curiosity and the love of truth, while real and critically important, will not suffice for describing knowledge production, particularly not if they are identified with the ideology of pure, as opposed to applied, science. First, they are as much the possession of industrial as of university researchers, and second, as many fundamental discoveries are rooted in practical as in theoretical knowledge. To illustrate, I will mention three subjects which appear on a single page of the *Los Angeles Times* for 3 October 1988. Two of the hottest topics in science at present are high temperature superconductors and human retro-viruses (e. g. the AIDS virus). Both discoveries contradicted reigning theoretical wisdom and they did not depend on alternative theories. They depended on analytical techniques, instrumentation, and knowledge of materials. Such research violates the theory-driven image. It is practice-driven. A third example disrupts related conventions. The researcher who has come closest to discovering how to "fingerprint" the relation between specific carcinogenic chemicals and damage to DNA molecules has a surprising title. He is professor of industrial hygiene at the University of California, Berkeley. Despite his industrial title and despite his obvious goal of finding a way to identify carcinogens directly by their molecular action, he emphasizes that his research is pure, not applied, not intended to yield immediate medical conclusions.

Apparently our usual idealistic, hierarchical ordering of the several pairs: theory-experiment, pure-applied, and university-industrial, is badly askew. The hierarchy captures little more than the class structure of science. Within a physics department, for example, theoreticians rank higher than experimentalists, who supposedly do nothing but test theory. Between departments, mathematicians often regard theoretical physics as little more than applied mathematics, while physicists regard electrical engineering as merely applied physics. Between institutions, finally, university research is more elevated than industrial research. No doubt this social stratification along the pure-applied axis carries deeply rooted cultural values (mentioned below). **But** does it represent a valid judgment of the relative epistemological significance of pure and applied research?

Numerous historical case studies, like contemporary developments in supercomputers and genetic engineering, suggest not.

In the interests of a more realistic understanding of how scientific knowledge is produced, I am attempting to reformulate the pure-applied distinction so as to avoid the connotation of noble vs. common, high vs. low, and to make it reflect a difference in activities. It ought to connote a complementarity of goals and methods, rather than the class structure of science. The basic idea, by no means new, is to differentiate pure and applied in terms of theoretical versus practical strategy. By these terms I mean especially deductive strategy versus means-ends strategy. The difference can be pictured as a difference in orientation of two arrowheads, as in the accompanying figure.



Practical activity aims at the solution of a particular problem by concentrating on it whatever tools and materials are available, typically a wide variety and including diverse theoretical ideas. Practicality is therefore oriented in the normal direction of arrowheads, from the wide base to the point. Theoretical strategy, on the other hand, aims at logical entailment of a wide variety of particular results under a single theory, thus moving from the point of the arrowhead to the base. It is the strategy of unity under deduction.

This scheme adds a practical abstraction to the formerly monolithic theoretical one. Both are still abstractions, however, and do not adequately represent either theoretical or practical activity, which in any given case are highly interactive. The arrowheads represent only what one hopes to end up with, either a single problem solution or a range of deductions from a single theory. The actual process of constructing a theoretical deduction often involves finding a theory from which the deductions can be made, and that process can look very much like a means-ends procedure, requiring that one assemble a variety of sorts of information to obtain the kind of theory that will work. Even when the theory is known in advance its application in any empirically realizable case always requires conjoining with it a set of phenomenological laws, mathematical techniques, and bits and pieces of practical knowledge all suitably chosen for the particular conditions. As in plane geometry, one typically works backward and forward at the same time to find what theorems and tricks to use in what order to construct a satisfactory proof, the point being that the proof is literally *constructed.* Only after it has been constructed does it take on its a priori deductive character. Similarly for the practical arrowhead. A particular problem is usually fully defined only in the process of solving it, and its incomplete prior definition is likely to be given partly or even largely in theoretical terms. Finding the solution also typically requires a variety of theoretical deductions. To reiterate, theoretical and practical refer to idealized strategies, abstracted from real life, where a constant adaptation of strategies one to another is always going on.

Universalization

Consistent with the theory-dominated approach to scientific knowledge, the problem of universalization is usually taken to be self-evidently a problem of universal validity. A theory is more universal if a wider variety of observable consequences can be subsumed under it, and this guarantees (or ought to guarantee) its universal acceptance in the scientific community. But that understanding conflates an epistemological notion of universality, e. g. , applicable to all propositions, with the geographical-social notion of universal acceptance. That is, it once again eliminates from consideration all questions of who, where, and for what immediate purposes. It is a teleological account based on the assumption that historical process follows the dictates of an abstract scheme of rationality which has been (or at least can be) specified at the outset. Ideas spread by their own force.

The most common such idealist scheme takes unity under deduction as both the goal of all science — that which establishes a "background consensus" — and a sufficient criterion of universalization. The problem, of course, is that people in different circumstances, with different interests, neither share the same view of unity nor count the same deductions as equally significant. Except as retrospective reconstructions, we have no good examples of universal acceptance based on this principle. Empirically, no background consensus has ever existed.

To highlight the magnitude of the problem, consider Newton's three laws of motion in mechanics, which have often been taken to epitomize a universally valid and universally accepted theory. Prior to the last third of the nineteenth century no agreement existed as to what the theory was nor whether its laws were three. But in search of universal acceptance, let us assume that it refers in the first instance to acceptance of the view that some laws of mechanics were to be the foundation of all explanations of the physical world. This assumption will pick out a universe for the period 1700 to 1870 consisting of most of the people whom presentday physicists recognize as belonging to their heritage, even though it will exclude many famous chemists. We are already talking about a very limited group of people concerned with a limited domain of explanation, largely motions at the macroscopic level, certainly not extending down to intra-molecular interactions. And within this domain consensus was limited to the belief that mechanics would someday yield an adequate mathematical description of motions. Consensus ended where meanings and foundations entered.

The classic works of Lagrange, Laplace, and Poisson, as well as later Parisian texts, display no set of laws quite like Newton's, although they take the subject of mechanics to be the immediate action of force on mass. Cambridge texts from 1820 to 1850 do base the theory on three laws of motion but the laws are not Newton's. The two traditions differ also in that Cambridge authors often insist on a distinction in principle between forces that produce motions and those that do not. Thus on the banks of the Cam the two subjects of statics and dynamics required a special law to relate them, while on the Seine only the single subject of motions appeared, actual motions and mutually destroyed ones. Lagrange and company reduced dynamics to statics, or to the principle of virtual velocities, so that all forces acted as forces in equilibrium. Both traditions, finally, rejected the principle of least action of Maupertuis and Euler, which anchored the metyphysical necessity of the action of forces in the wisdom of God, as expressed in his economy of action.

The point to be stressed here is that no consensus existed as to what constituted the theory of mechanical action. The term "Newtonian mechanics" helps to differentiate all forms of pre-relativistic and pre-quantum mechanics from their forbears, but it by no means describes a universally accepted theory. "Classical mechanics" serves no better. It refers to the limited subject of non-relativistic and non-quantum motions as expounded in our contemporary textbooks, having become "classical" only after having been superseded. It is not Newtonian, since it replaces force with energy as the basic entity of mechanics, nor is it even Lagrangian, since it makes extremum principles like least action the basis of dynamics and subsumes statics under dynamics. As a foundational theory, it vied for acceptance in the period 1860-1900, just when mechanical philosophy in general was receiving severe blows from thermodynamics and electromagnetic field theory. And if it has attained universal acceptance since then, it is either as a beautifully self-consistent structure of thought or as an extremely valuable tool for obtaining practical results, but not as a theory of force, mass, and space. Unless we are content with the view that a theory is nothing other than the mathematical relations derivable from it, we have no ground in the history of mechanics for a universally accepted theory.

Once we have recognized that the set of all universally accepted theories is an empty set historically, the notion of universalization becomes one of a goal and a process. For describing this situation it is useful to differentiate the precondition for universalization, or universalizability, from the social process of universalization. Universalizability seems to be what philosophers of science are talking about when they look for general criteria of validity. Unity under deduction is one such criterion. I prefer a less idealized formulation, such as whether or not a theory works; can one use it to organize a stable body of practices in a wide range of situations? If so, it is valid, and therefore potentially universalizable. This criterion excludes mere quackery and pathologies such as the Lysenko affair, without building teleology into the preconditions of knowledge.

Whatever criterion of universalizability one chooses, however, it cannot explain the process of universalization, which is always a matter of degree. The degree depends on whether or not the unifying basis is widely acceptable and on whether or not the practices organized meet the interests of others. In the case of mechanics, Euler's least-action formulation epitomized the goals of science in the Berlin Academy but was un' acceptable in the Paris Academy. Some reasons are straightforward: it violated the dominant anti-metaphysical, anti-teleological, bias of rational mechanics in the French enlightenment; Lagrange and Laplace found that they could do without it; and its validity was restricted to conservative systems. (Laplace, as one who regarded conservation as the basis of the rule of natural law, as opposed to teleology, did not make much of the latter challenge, which actually constituted a challenge to universalizability.) On the other hand, least action offered a most useful mathematical technique for solving particular problems, and for that purpose it was widely employed. Only later, under different cultural conditions, different conceptions of an "economy", and the emergence of energy conservation, did the tool regain its status as a principle.

Electromagnetic field theory offers similar lessons. Maxwell's theory had become a model of good theorizing for most British physicists by

1870. To them the reasons were good ones. It did away with forces acting at a distance by making energy differentials in a field the basis of dynamical action. It unified the physics of ether and matter by encompassing light waves as well as the entire range of known electrical and magnetic phenomena. But not all rational individuals, not even among field theorists in Britain, adopted the theory, despite its apparent advantages. So powerful a figure as William Thomson, Lord Kelvin, the leading spokesman of Victorian physics, opposed it for the remainder of the century. Its speculative and non-intuitive foundations violated his particular view of the unity of theory and practice.

In Germany, where energy was usually understood as an attribute of force — itself an abstract relation of space, time, and matter — the situation was more complicated. The reception process began with a major paper by Hermann von Helmholtz in 1870, which showed that Maxwell's equations for the propagation of light could be obtained from an action at a distance formulation if only space contained a medium polarizable by electric and magnetic distance forces. This theory, not Maxwell's and not the reformulations of his followers, provided the basis for both Heinrich Hertz's and H. A. Lorentz's development of field theory on the continent. Meanwhile, British Maxwellians paid equally scant attention to developments across the channel until after Hertz showed experimentally that electromagnetic apparatus could actually be used to transmit and receive waves propagating through space, and until Lorentz produced a new unification based on a strict differentiation of matter and fields, the electron theory. Most illuminating is the fact that Thomson and Helmholtz, who maintained a lively correspondence over other issues throughout this period, did not discuss electromagnetic theory. And in all his many publications on the subject, Thomson never cited Helmholtz's papers, which seemed so fundamental on the continent.

To summarize, the process of universalization cannot be understood in terms of the mere power of unifying ideas. That much is clear from the reception of the most powerful theories known. Physics does attain increasingly comprehensive theories. But is this the result of an Hegelian "cunning of reason", a world historical teleological force operating behind the scenes to guide the results of the interested activities of individuals? Or is it the natural result of efficient causes which can be understood in the terms of everyday life? In adopting the latter view, I do not seek to undermine the ideal of unity as an operative force in physical theory, but to ground that ideal in reality, to show that unities are multiple and that a given unifying idea will be honored only if it unifies phenomena in which people have an interest, in which ideas and interests merge.

This view suggests an alternative to idealist notions of how theories get disseminated. It is a market model. William Thomson sought to reconstruct the Lagrange-Laplace mechanics partly because he disliked the speculative metaphysics of forces acting at a distance between unobservable point particles, but simultaneously because it did not express the practices of steam engine engineering and did not open a route to improving those practices. The two objections merged into one in the industrial context of Glasgow where Thomson formed his identity. More generally, what is often referred to as the transmission of French mathematical physics to Britain in the period 1820-1840 bears considerable resemblance to an export-import market. Of the wide variety of products offered by French *savants,* only those sold well in Britain that matched the demand of the market. The British market slanted the transmission process sharply toward practicality. People like Thomson, who consciously set out to import French products, thoroughly transformed them in the process. He remade Poisson's mathematical theories of electricity and magnetism, for example, into field theory, thereby setting the stage for Maxwell's theory.

Slightly later, in a very different context, Maxwellian field theory failed to meet the demands of the German market. Closely tied to the requirements of long-distance ocean telegraphy in a world-wide empire, British theory made much of concepts like displacement current, which seemed incomprehensible in Germany. Helmholtz had to radically reformulate Maxwellian theory in order to adapt it to his own tradition and interests. Put starkly, the ideal of unity meant something different in the new German Reich than in the British Empire. Recent case studies are beginning to explore the look of those differences when worked out in terms of interests. They are revealing the underside of justification and legitimation, where the political economy of science looms large as a nexus of efficient causation in the universalization of knowledge.

In contemporary terms, we would misunderstand the all-out race to construct an adequate theory of superconductivity if we supposed it to be driven either by the love of unity or by the high probability of a Nobel prize and industrial wealth for the winner. As pure breeds, dewy-eyed idealists and crass materialists are both rare. The new theory is being sought for its practical and theoretical value simultaneously. It will help to organize, limit, and extend a variety of practices and it will provide a newly generalized concept of electron interactions. If history is any guide, these two motivations are inseparable. We may conclude that the universalization of any candidate theory will extend little farther than does interest in the practices it organizes.

Values

Debates about the nature of scientific activity are much more vehement outside the natural sciences than inside. That is partly because the term "science" carries strong values. Universal truth, for example, is not primarily a value of science; it is more importantly a cultural value, an ideal symbolized by "science". The symbol represents knowledge that is imagined to be independent of any particular human situation, human interests, or individuals. It therefore represents autonomy, both the autonomy of nature and the autonomy of behavior grounded in nature, whether external nature or human nature. Rationalists have often associated this autonomy with action guided by deductive reasoning from universal principles. Thus the pursuit of rational truth is an autonomous activity. The rational individual is an autonomous individual. Scientific knowledge and scientists, when behaving properly, are not motivated by personal or political interests and are not subject to their manipulation.

The view that the disinterested pursuit of truth precludes political manipulation has become particularly prominent in the twentieth century, and for good reason. It is closely connected with the desire to avoid the perversions of Hitler and Stalin. If the successful pursuit of scientific knowledge depends on its being disinterested, then it cannot be in the interest of any state to manipulate science, or even to attempt to direct pure research, otherwise it would be neither pure nor fruitful.

This is a seductive view. It promises an escape from distressing concerns with ambition, wealth, power, ideology, and the like. But disinterest does not typify basic research, not even theoretical research, as noted above. In fact, I cannot think of many major figures in the history of science who would be candidates for such disinterest, while it is surely the case that personal, industrial, and political motivations have often been highly productive forces in science. The issue, therefore, cannot be anything like a contradiction between the pursuit of interests and the pursuit of truth. A contradiction enters when scientific validity is sacrificed to other interests. Such cases are pathologies.

But suppose the disinterested pursuit of truth did characterize science. It would still do nothing to prevent political manipulation. On the contrary, the myth of disinterest has typically been used by scientists to justify their participation in state projects of questionable moral standing, while at the same time avoiding responsibility for them. The scientist pursues pure knowledge; what application the state makes of that knowledge is the business of politicians. This argument became standard among physicists after WWII who went to work on the hydrogen bomb project. It is now ubiquitous among the large proportion of all physicists in the United States who work on university and industrial projects funded by the Department of Defense. Recruitment for Star Wars research, which has been so widely attacked among physicists, depends on selling the argument to recruits and on their repeating it to themselves and their friends.

The point here is not that Star Wars research is morally wrong. Whether right or wrong, it is interested research, research for political ends. The myth of disinterested science serves to hide that fact from the consciousness of researchers by setting up an absurd separation between the knowledge they aim to produce and the aims for which they know the knowledge is intended, as though the one were clean even though the other might be tainted. This absurdity allows their cooptation and manipulation — even if self imposed — in the interests of the state. I would far rather see scientists pursuing the theoretical and experimental truths of X-ray lasers and guidance and control systems with an honest commitment to the purposes of those who fund the research or to their own ambition. They would then be accountable. In general, if we wish to avoid political manipulation, we should examine with our eyes open the conditions of interested activity under which knowledge is regularly produced. The myth of disinterest does not serve its intended purpose.