Comments on the Culture of the Force

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All the English-language accounts we could find mentioned the original bridge construction in the 1840s and the reconstruction after World War II. We don't doubt Marton's additional historical elements about the reconstruction in 1913–1915, but we couldn't find an English account of them.

References

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Comments on the Culture of the Force

One of Frank Wilczek's main themes in "Whence the Force of $F = ma$? I: Culture Shock" (PHYSICS TODAY, October 2004, page 11) appears to be that although the force is, in Wilczek's words, "vaguely defined," it "continues to flourish" because the microscopic details it conceals are not really relevant for the scale of the phenomena it serves to describe. Further, it "survives the competition" because "it is much easier to work with." To this second point one might add that nothing succeeds like success. Let me explain.

The concept of force had been under attack much before the comments of Peter Tait and Bertrand Russell. Even some of Isaac Newton's immediate successors, most notably Joseph-Louis Lagrange and Jean Le Rond d'Alembert, were critical of the concept. D'Alembert regarded it as "useless to mechanics" and said that it "ought therefore to be banished from it."[1] However, the use of Newton's idea that force is a primary, nonderived concept, which was pursued steadfastly by Leonhard Euler,[1] led to the greatest successes in continuum mechanics in the two centuries immediately following publication of Newton's *Principia*. That period culminated in the 1820s in Augustin-Louis Cauchy's stress principle,[1] which unified the seemingly disparate fields of fluid mechanics and elasticity. This approach, commonly attributed to Newton rather than to Euler or Cauchy, is chosen over its main competitor, the variational formulation of Lagrange, to be taught in a typical fluid mechanics course. The stress has also been given a microscopic interpretation in kinetic theory and in more general statistical mechanics.

Wilczek mentions some assumptions about forces. Newton regarded mechanics as "the science of motions that result from any forces whatever."[1] Thus, he did not exclude contact forces, the dominating concept in continuum mechanics. Nor did he demand that all forces be central, which has particular relevance to the derivation of the angular momentum principle.

Albert Einstein to Max Born[1]

Translated by Irene Born Newton-John

In this letter to his old friend, now at Edinburgh University, Einstein first responds to Born's description of a local physician "who wouldn't hurt a fly, but [states] that no sacrifice is too great [for] the realization of Marxist ideals, not even the destruction of millions of human lives." And then it's back to the old quarrel about quantum mechanics. (See Born's comment below.)

Princeton, 15 September 1950

Dear Born,

. . . People such as your Bolshevik doctor come by their fantastic attitude as a result of their objection to the harshness, injustice and absurdity of our own social order (escape from reality). If he happened to be living in Russia, no doubt he would be a rebel there as well, only in that case he would take care not to tell you about it. Nevertheless it seems to me that our own people here [in the US] make an even worse job of their foreign policy than the Russians. And the idiotic public can be talked into anything. And they really are very shortsighted, for technological superiority is transitory, and if it comes to an all-out conflict, the decisive factor is sheer numerical superiority.

There is nothing analogous in relativity to what I call incompleteness of description in the quantum theory. Briefly it is because the ψ-function is incapable of describing certain qualities of an individual system, whose "reality" none of us doubt (such as a microscopic parameter). Take a (macroscopic) body that can rotate freely about an axis. Its state is fully determined by an angle. Let the initial conditions (angle and angular momentum) be defined as precisely as the quantum theory allows. The Schrödinger equation then gives the ψ-function for any subsequent time interval. If this is sufficiently large, all angles become (in practice) equally probable. But if an observation is made (e.g. by flashing a torch), a definite angle is found (with sufficient accuracy). This does not prove that the angle had a definite value before it was observed—but we believe this to be the case, because we are committed to the requirements of reality on the macroscopic scale. Thus, the ψ-function does not express the real state of affairs perfectly in this case. This is what I call "incomplete description."

So far, you may not object. But you will probably take the position that a complete description would be useless because there is no mathematical relationship for such a case. I do not say that I am able to disprove this view. But my instinct tells me that a complete formulation of the relationships is tied up with complete description of its factual state. I am convinced of this although, up to now, success is against it. I also believe that the current formulation is true in the same sense as e.g. thermodynamics, i.e. as far as the concepts used are inadequate. I do not expect to convince you, or anybody else. I just want you to understand the way I think.

I see from . . . your letter that you, too, take the quantum theoretical description as incomplete (referring to an ensemble). But you are, after all, convinced that no (complete) laws exist for a complete description, according to the positivistic maxim: *esse est percipi* [to be is to be perceived]. Well, this is a programmatic attitude, not knowledge. This is where our attitudes really differ. For the time being, I am alone in my views—as Leibniz was with respect to the absolute space of Newton's theory.

. . . I have not changed my attitude to the Germans, which, by the way, dates not just from the Nazi period. All human beings are more or less the same from birth. The Germans, however, have a far more dangerous tradition than any of the other so-called civilized nations.

Kind regards,

Yours,

A. E.

Born's 1969 comment:[1] This is probably the clearest presentation of Einstein's philosophy of reality. . . . He calls my way of describing the physical world "incomplete." In his eyes, that is a flaw which he hopes to see removed, while I am prepared to put up with it. I have in fact always regarded it as a step forward, because an exact description of the state of a physical system presupposes that one can make statements of infinite precision about it, and this seems absurd to me.

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1. M. Born, *The Born–Einstein Letters 1916–1955: Friendship, Politics and Physics in Uncertain Times*, Macmillan, New York (2005), p. 184. Original letter © Hebrew University of Jerusalem.

In 1776, Euler, guided by his research on elasticity, came to regard the balance of angular momentum as an independent, second principle of mechanics,[1] the first principle being the balance of linear momentum. When Euler arrived at the rigid-body equations of motion in 1752 using the first principle, he had to invoke hypotheses about internal forces. However, once he saw the balance of the moments as an independent principle, he had no need of such hypotheses. In special cases such as that of a perfect fluid, the second principle follows from the first. In fact, the second principle leads to the symmetry of the stress tensor when all torques may be obtained as moments of forces.[2] The status of the third law has been clarified by the work of Walter Noll, who gives a precise mathematical interpretation of Newton's verbal statement of the law.[3]

Even in applications of quantum mechanics, Richard Feynman emphasized the importance of forces.[4] He commented that "many of the problems of molecular structure are concerned essentially with forces," that "forces are almost as easy to calculate as energies are," and that "the quantities are quite as easy to interpret." Another application of the concept of force is found in nonequilibrium statistical mechanics. Just as the contact force had to be found as the appropriate force to describe the dynamics of the continuum, the physically realistic short-time force derived from the mean instantaneous potential had to be discovered as the force that describes typical chemical dynamics in liquids,[5] in contrast to the traditional concept of the potential of mean force, which is more appropriate for slow or diffusion processes.

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I think Frank Wilczek is too harsh when he implies that the use of equations like $F = ma$ is a matter of intellectual inertia. In practical terms, in engineering, and even in the design of physics instruments, we are interested in the values taken by certain variables x_i and the known dependence is in the form of differential equations $dx_i/dt = v_i$ and $dv_i/dt = f(x_i, v_i)$. When x_i is some position, the last is a form of $F = ma$. Many physicists—David Bohm and John Stewart Bell, for example—have argued that position is the fundamental variable . . . hence the importance of $F = ma$.

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Frank Wilczek's column exposes a delicate point in physics teaching. Good teachers avoid implanting misconceptions to be overwritten later. Yet Newtonian mechanics courses do just that! During 20 years teaching I've maintained that Newton's three laws are neither good laws nor independent. Students enjoy hearing the first law is just as circular as it seems. Textbook apologies that falsely limit physics to inertial frames contradict later teaching that physics can be used in any coordinates. Perhaps the first law was just a political device to start discussion, and to divide Newton's detractors. The third law is necessary for beginning physics of ropes and pulleys, but is wrong as "principle": Momentum conservation via translational symmetry has myriad solutions. The second law is okay, but it is indefensible to promote Newton's emphasis on "force" as primary, only later to revise it with Hamilton's equations of greater scope. Eventually I evolved a refreshing approach to non-calculus physics with energy and conservation laws as primary, and it works well.

Students happily accept that Newton sometimes guessed wrong. A timid teaching culture and careless textbook writing create the intellectual inertia Wilczek observes. Good physics teachers need to demonstrate critical thinking, distribute their own notes, and have the courage not to brainlessly repeat what is written in the book.

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Wilczek replies: Ramamurthy Ravi's letter is an excellent, scholarly supplement to my October column, which emphasizes that some classical masters of mechanics had logical and aesthetic misgivings about the force concept, even before modern physics began to push us strongly toward different ones.

Regarding Brent Meeker's letter, my critique was meant to be directed at foundational issues including, specifically, which principles should be regarded as primary, and which as derived. There are some significant problems with using $F = ma$ as a primary principle, as I discussed. They could be avoided, perhaps advantageously, by focusing on momentum and energy. Of course, in that approach it would still be appropriate and extremely useful to have $F = ma$ as a derived equation, with its limitations indicated. Some intellectual inertia isn't necessarily a bad thing, if it keeps you moving in the right direction and allows you to remain in sync with long-established flows.

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