On Feynman’s Discussion of Classical Physics Failing at Specific Heat

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I provide the backstory on how a historical error in the Feynman Lectures on Physics was corrected.

Don Groom’s recent account [1] of editing the transcripts that became the Feynman Lectures on Physics prompted me to reflect on how strange it is that I have an acknowledgment in a more recent edition, while people who did much more work and who were much more important in the process of making the books happen have remained anonymous. (The name of at least one woman in the story, an editor at Addison-Wesley, has been lost to history [2].) I am of the generation that learned from physicists who knew Feynman; my first encounter with the Feynman-industrial publishing complex was the warts-and-all portrayal in Gleick’s estimable biography [3]. Consequently, it is a little odd that I have any involvement to write about. But there is a (thankfully short) story to be told, and one with a moral or two.

On occasion, somebody voices the idea that in year $N$, physicists thought they had everything basically figured out, and that all they had to do was compute more decimal digits. This turns out to be more myth than fact.

One classic illustration of how the old guys with the beards knew their understanding of physics was incomplete involves the specific heats of gases. How much does a gas warm up when a given amount of energy is poured into it? The physics of the 1890s was unable to resolve this problem. The solution, achieved in the next century, required quantum mechanics, but the problem was far from unknown in the years before 1900. And here we come to the Feynman Lectures on Physics (1964), volume 1, chapter 40:

The first great paper on the dynamical theory of gases was by Maxwell in 1859. On the basis of ideas we have been discussing, he was able accurately to explain a great many known relations, such as Boyle’s law, the diffusion theory, the viscosity of gases, and things we shall talk about in the next chapter. He listed all these great successes in a final summary, and at the end he said, “Finally, by establishing a necessary relation between the motions of translation and rotation (he is talking about the $\frac{1}{2}kT$ theorem) of all particles not spherical, we proved that a system of such particles could not possibly satisfy the known relation between the two specific heats.” He is referring to $\gamma$ (which we shall see later is related to two ways of measuring specific heat), and he says we know we cannot get the right answer.

Two years later, in a lecture, he said, “I have now put before you what I consider to be the greatest difficulty yet encountered by the molecular theory.” These words represent the first discovery that the laws of classical physics were wrong. This was the first indication that there was something fundamentally impossible, because a rigorously proved theorem did not agree with experiment. About 1890, Jeans was to talk about this puzzle again. One often hears it said that the physicists at the latter part of the nineteenth century thought they knew all
the significant physical laws and that all they had to do was to calculate more
decimal places. Someone may have said that once, and others copied it. But
a thorough reading of the literature of the time shows they were all worrying
about something.

The lesson is right, but either the name or the date is wrong. “Jeans” can surely only be
Sir James Jeans, but then “1890” cannot be correct. Jeans was born in 1877 and did not go
to Cambridge until 1896.

A better date, in the next century but still in the earliest years of the old quantum theory,
would be 1904, the first publication of his book *The Dynamical Theory of Gases*, which has
a lengthy discussion of how kinetic theory fails to account for gases’ specific heats. The
nub is on p. 173: “Our theory has, then, led to a result which is in flagrant opposition to
experiment.” Jeans’ own attempt at a solution was basically to deny that all the degrees of
freedom were in statistical equilibrium [4]. Additional references can be found in Hudson [5].

If you want a statement of this issue which really does date to “about 1890”, see Peter
Guthrie Tait’s “On the Foundations of the Kinetic Theory of Gases”, *Transactions of the
Royal Society of Edinburgh*, 14 May 1886. Tait writes of Maxwell,

He obtained, in accordance with the so-called *Law of Avogadro*, the result that
the average energy of translation is the same per particle in each system; and
he extended this in a Corollary to a mixture of any number of different sys-
tems. This proposition, if true, is of fundamental importance. It was extended
by Maxwell himself to the case of rigid particles of any form, where rotations
perforce come in. And it appears in such a case that the whole energy is ulti-
mately divided *equally* among the various degrees of freedom. It has since been
extended by Boltzmann and others to cases in which the individual particles are
no longer supposed to be rigid, but are regarded as complex systems having great
numbers of degrees of freedom. [...] This, if accepted as true, at once raises a
formidable objection to the kinetic theory. For there can be no doubt that each
individual particle of a gas has a very great number of degrees of freedom besides
the six which it would have if it were rigid:—the examination of its spectrum
while incandescent proves this at once. But if all these degrees of freedom are
to share the whole energy (on the average) equally among them, the results of
theory will no longer be consistent with our experimental knowledge of the two
specific heats of a gas, and the relations between them.

This passage can be found on p. 135 of the 1900 reprint collection of Tait’s papers.
“On the Foundations of the Kinetic Theory of Gases” was actually a five-part series which
appeared in the *Transactions* from 1886 to 1892, so a slightly vague dating makes a kind of
sense, and it would be easy to swap Jeans for Tait while speaking.

Another candidate who fits the dating less well, but should be noted nevertheless, is
Josiah Willard Gibbs, writing at the end of 1901:

In the present state of science, it seems hardly possible to frame a dynamic theory
of molecular action which shall embrace the phenomena of thermodynamics, of
radiation, and of the electrical manifestations which accompany the union of
atoms. Yet any theory is obviously inadequate which does not take account
of all these phenomena. Even if we confine our attention to the phenomena
distinctively thermodynamic, we do not escape difficulties in as simple a matter
as the number of degrees of freedom of a diatomic gas. It is well known that while theory would assign to the gas six degrees of freedom per molecule, in our experiments on specific heat we cannot account for more than five. Certainly, one is building on an insecure foundation, who rests his work on hypotheses concerning the constitution of matter.

This from the Preface to his Elementary Principles in Statistical Mechanics.

As my friends will testify, I am a complainer who masquerades as a historian [6–9]. So, in 2013, I wrote a letter to the people in charge of revising the Feynman red books, and the passage was quickly corrected [10].

What morals can we draw from this little excursion? First, of course, there is the lesson that Feynman himself was laying out: the importance of peeling back what Stephen Jay Gould called the “textbook cardboard” of a subject [11]. (I am indebted to Riley Black for teaching me this term, back in the halcyon days of science blogging.) The real history is inevitably more intricated than the caricature we receive from books that rush past it to get to topics on which homework can be assigned. Another lovely example of this is that Pauli derived the correct quantum-mechanical energy levels of the hydrogen atom before Schrödinger found the Schrödinger equation, by deducing the quantum counterpart of the Laplace–Runge–Lenz vector [12, 13]. This is never how we teach undergraduate quantum mechanics, because second-year undergraduates do not have the background in classical physics that Pauli did!

A second lesson loops us back around to where we began, with the production process of the Feynman Lectures. This erratum is indicative of the type of glitch that can slip through proofreading by students and by physicists, precisely because we are naturally weak on history.

The Feynman Lectures are widely regarded to be unsuitable as textbooks, sometimes I think for better reasons than others. The difficulties that Feynman’s own afterword describes sound to me like the start-up challenges of any new course that does not yet have a standard text — challenges that I’ve had more than one encounter with. The Lectures were distributed without the accompaniment of problems, which degraded their utility. And while a separate book of exercises was printed much later [14], I find that it illustrates, above all, how hard it is to devise problems that provide a clear gradation of difficulty to guide the student (or the instructor) into the material of each chapter.

So, it is only right that we dream big, and wonder how we might do it all differently! (For life is more fragile by the day, and if our profession cannot deliver on its promises of either utility or beauty, why should we even stay in it?) But let us anchor that dream in practicality. The next time we attempt a panoramic survey of modern physics, we should pay attention to how we might incorporate the experience of intervening decades, in matters as mundane as version-tracking, assigning authorship [15], and getting the eyeballs with the right expertise in front of the text.

[1] D. Groom, “Commentary: The making of Chapter 46 of The Feynman Lectures,” Physics Today (9 December 2021).
[2] M. Sands, “Capturing the Wisdom of Feynman,” Physics Today 58 (2005), 49.
[3] J. Gleick, Genius: The Life and Science of Richard Feynman (Pantheon, 1992).
[4] J. H. Jeans, “On the Laws of Radiation,” *Proceedings of the Royal Society A* **76** (1905), 545–52.

[5] R. Hudson, “James Jeans and radiation theory,” *Studies in History and Philosophy of Science A* **20** (1989), 57–76.

[6] B. C. Stacey, “Von Neumann Was Not a Quantum Bayesian.” *Philosophical Transactions of the Royal Society A* **374** (2016), 20150235, arXiv:1412.2409.

[7] B. C. Stacey, “Misreading EPR: Variations on an Incorrect Theme,” arXiv:1809.01751 (2018).

[8] B. C. Stacey, “QBism and the Ithaca Desiderata,” arXiv:1812.05549 (2018).

[9] B. C. Stacey, “Ideas Abandoned en Route to QBism,” arXiv:1911.07396 (2019).

[10] M. A. Gottlieb, “Errata for The Feynman Lectures on Physics Volume I New Millennium Edition (4th printing),” https://feynmanlectures.caltech.edu/info/errata/FLP_New_Millennium_Edition_4th_printing_Vol_I_Errata.pdf (2014).

[11] S. J. Gould, *Time’s Arrow, Time’s Cycle: Myth and Metaphor in the Discovery of Geological Time* (Harvard University Press, 1988).

[12] W. Pauli, “Über das Wasserstoffspektrum vom Standpunkt der neuen Quantenmechanik,” *Zeitschrift für Physik* **36** (1926), 336–63.

[13] B. L. van der Waerden, *Sources of Quantum Mechanics* (Dover, 1968). Contains introductory material and a translation of Pauli’s 1926 article among others.

[14] R. P. Feynman, R. Leighton, M. Sands et al. *Exercises for the Feynman Lectures on Physics.* Edited by M. A. Gottlieb and R. Pfeiffer (Basic Books, 2014).

[15] J. Sokol, “The Hidden Heroines of Chaos,” *Quanta Magazine*, (20 May 2019).