How to Justify Teaching False Science[1]

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We often knowingly teach false science. Such a practice conflicts with a prima facie pedagogical value placed on teaching only what's true. I argue that only a partial dissolution of the conflict is possible: the proper aim of instruction in science is not to provide an armory of facts about what things the world contains, how they interact, and so on, but rather to contribute to an understanding of how science as a human endeavor works and what *sorts* of facts about the world science aims to provide. Such an aim legislates for an increased prominence of history and philosophy in even secondary science education.

1. INTRODUCTION: A PEDAGOGICAL PUZZLE

Science education faces a deceptively simple question: How should we justify teaching false science? Take, for example, Newtonian Mechanics. It's false. But we teach it to countless of high school and college students nonetheless. On its face, that seems totally unacceptable: truth seems a necessary condition on acceptable teaching practices. This suggests a simple argument (I focus on Newtonian Mechanics as a representative example, but I believe the point can be extended to many other theories or models):

(NF) Newtonian Mechanics is a false scientific theory;

(F) We should not teach any false scientific theory; therefore:

(C) We should not teach Newtonian Mechanics.

As this argument is clearly deductively valid, rejecting the conclusion (C) requires rejecting either (NF) or (F). Of course, a simple way out (if we wished to aver the premises) would be to just accept the conclusion that we should not teach Newtonian Mechanics (or other false theories) — or anyway that we should not teach the parts of them which are false. Perhaps so; I am not convinced either way (*nota bene* that the fact that we *do* teach Newtonian Mechanics does not imply that we *should*). In this essay, however, I shall proceed under the assumption that the conclusion is false. I will try to show that the best way of denying the conclusion involves denying premise (F). I believe that teaching certain false theories from within their historical context is indeed defensible.

In denying (F), I do not mean to claim that we *should* teach false theories (even under the conditions I describe). Likewise, denying (C) amounts only to the claim that there are conditions under which it is permissible to teach Newtonian Mechanics. Perhaps those conditions are rarely (or never) realized, or perhaps realizing them incurs too significant a cost, and so on. As I do not attempt to specify how to put a proposal like mine into practice (as far exceeding the scope of this essay), I must remain neutral on this issue. Nor do I claim

[1] I've thought about the material in this essay off and on for going on four years now. During that time, I received some excellent discussion from David Albert, Andrea Borghini, Chris Haufe, Philip Kitcher, Achille Varzi, Neil Williams, and Zanja Yudell. More recently, I thank Jerine Pegg, David Duschl, and three anonymous reviewers for *Science Education* for their encouraging and challenging comments. The essay was much improved by their help.

that my particular way of denying (F) is the only plausible. However, I do think that many other *prima facie* plausible avenues of defense are not nearly as attractive; for two reasons. First, I believe that however we justify teaching false theories, we should ideally be able to *include* that justification alongside the theories themselves. Thus, second, this justification should not crucially depend on concepts or theories about which there is serious controversy that cannot be effectively related to students (either for reasons of complexity or time constraint). I think of these constraints as arising from concerns analogous to those John Rawls grappled with in the years after he wrote his *Theory of Justice*. He saw his project of "Political Liberalism" as attempting to answer the question: "How is it possible that there can be a stable and just society whose free and equal citizens are deeply divided by conflicting and even incommensurable religious, philosophical, and moral doctrines?" (1993, p. 133). How such deeply divided agents come to agree upon and justify a particular conception of justice? Just as Rawls believed that it was possible to reach an "overlapping consensus" from these various doctrines — that a "freestanding" conception of justice was possible — I believe that a freestanding justification of teaching false science is possible. While I cannot fully defend these justificatory principles here, I shall try to make them plausible and show how my defense of teaching false theories does not run afoul of them. Whether or not you agree they deserve our respect as *general* principles, perhaps you will agree with their application in at least this case and thus see why I believe my particular style of justification to be superior to other initially plausible contenders.

My claim that we may be justified in teaching strictly false theories from the perspective of their historical and philosophical contexts confirms what I believe many science education researchers have long held: that a central goal of science education is providing students with a realistic understanding of science, of the "nature and development of scientific knowledge" (National Research Council, 2007, p. 334). How specifically we should approach this challenge as teachers and scholars remains to my mind an open question. But it is a question that more philosophers and historians of science ought to take seriously. Teachers and scholars alike need to think seriously and carefully about how the historical and philosophical study of science might be integrated into classroom science, even at an early level. My approach in effect connects this goal with what I take to be another central goal of science education: relaying *factual* knowledge of particular scientific results and techniques (both in training future scientists and preparing responsible citizens).

I begin by an assessment of the presumptive importance of truth in science and science education (§2): is it a necessary or sufficient condition of permissible candidates of pedagogical focus? I shall argue that it is neither — far more important is the *significance* of what is taught. I argue in §6 that such significance may attach to false theories when taught from within their historical context. In the intervening sections (§§3–5), I address other attempts to justify teaching false theories, arguing that they each face serious difficulties and complications.

2. Truth & Significance

A widespread view of science has it that we engage in science to discover the truths of nature. As practiced, scientific inquiry aims not only at building better bridges, but at revealing the structure and history of our world. Cosmologists do not ask about what the universe was like ten billion years ago in hopes of discovering some practical use of that knowledge in our society. They do so out of curiosity — out of the belief that knowing about the beginnings of our universe is of some intrinsic value.

We should bear in mind, however, that science does not aim to uncover just *any old truth*. Rather, science seeks "significant truths" (Kitcher, 2001, p. 65). Their significance helps justify both their pursuit and their teaching. While we might often locate *practical* significance in learning how the world is — knowledge that helps us get around and accomplish other things we value — realists often suppose that *epistemic significance* doesn't await these applications. Perhaps certain facts carry an irreducible epistemic significance that transcends even potential practical application. Of course, appreciation of this significance will likely depend on parochial considerations. As Kitcher notes: "If someone asked why we want to … know why the heavenly bodies move as they do, or why we are interested in the evolution of our hominid ancestors … it would be hard to say very much" (2001, p. 81). Sometimes all we can do is argue our case and try to get others to see the significance of these questions in the context of either shared norms or the admitted significance of connected questions. Fortunately, we often agree about matters of epistemic significance — e.g., that knowing about the beginning of the universe or why the planets move in ellipses *is* epistemically significant. Such agreement about significance — the belief that students may find their lives enriched by knowledge of their world, that the unexamined world is not worth living in — often underwrites our pedagogical efforts. So truth alone is not a sufficient condition for pedagogical justification. Is it necessary? Is *significance* alone necessary instead?

We might be tempted to try solving our pedagogical puzzle by answering this latter question in the affirmative, pointing to cases where truth plays little role in pedagogical significance. Clearly, we would be foolish to think that teaching simply involved conveying propositional knowledge (that the world is thus and so). We habitually teach what people (incorrectly) *thought* — we teach their mistakes, their prejudices, their fiction. We teach *Gulliver's Travels* with no illusions about the existence of Brobdingnag.[2]

However, this move neither diffuses our puzzle nor represents the last word on the necessity of truth in teaching. For one, a principle about truth as a necessary condition might be tacitly restricted to *truth-apt* objects of instruction. Friends of such a principle might only insist that *when* propositions are taught, they be *true* propositions, while admitting that all manner of non-propositional "knowledge" (manners, techniques, epistemic norms, and so on) gets appropriately communicated to students without being *true*. For two, we should take care

[2] Of course, these examples bear on the necessity of truth in teaching if our habits are justified. I assume they are; others might take a more radical approach.

to distinguish between the falsity of what is taught and what we might call "*embedded* falsity". Suppose Smith believes that the world is flat. Smith's belief is false — it's not the case that the world is flat. But when I tell students that Smith *thinks* it's flat, I say something true. I do not thereby recommend Smith's beliefs as true propositions; they are not to be incorporated with the rest of the students' beliefs.

Justifying the teaching of false science in this "embedded" way, as a certain selection of the history of ideas, — e.g., "this is what people believed about the atom" or "Newton supposed that space was flat" — seems a mistake. For one, this strategy places serious restrictions on how we should present of science. While I do not wish to make an empirical claim about how science is usually taught, it does seem likely that such restrictions (though as yet vague) are not generally met. No doubt some science teachers are sensitive to the fact that Newtonian mechanics is false and carefully embed its claims in "history of ideas" terms. But it's safe to say that many do not. For two, the strategy leaves unspecified a different justificatory question. Consider the example of Smith above: while I say something true about Smith's false beliefs, there remains a question about why his beliefs are worth teaching. This question is all the more poignant given their falsity. Yet I think it can, in principle, be answered: significance indeed may come apart from truth and only the former is *necessary* for pedagogical justification. Before making this case, it's worth seeing why other plausible responses to our puzzle do not clearly succeed.

3. Dissolving the Problem from an Instrumentalist Perspective

Anti-Realists of various stripes might argue that our pedagogical puzzle derives from adopting a realist stance which should be abandoned. They might claim that the realist teaching perspective in which "teachers conflate realist principles of theoretical terms which refer to hypothetical entities with existing entities" (Benson, 2001, p. 451) encourages a confused and misrepresentative picture of science. It represents an irredeemable naïveté regarding the social influences on science. If, on top of this, the realist perspective makes nonsense of teaching science so as to come to "understand the 'truths' of the universe" (443), then so much the worse for the realist perspective. The question about justification for teaching "false" theories then doesn't arise. Or if it does, can be answered simply on grounds of, say, personal or sociopolitical utility. If the idea of truth and falsity for scientific theories makes no sense, then there's no special problem about how to justify teaching *false* theories.

While I certainly cannot rebut such anti-realist argument here, it's worth pointing out that the realist has plenty of room in which to work. It seems to me that many critiques of realism attack a straw man. Monk and Dillon's construal of realism, for instance, as committed to "the notion that empirical enquiry enables scientists to describe and explain a reality that exists independently of the scientist's enquiry . . . [and enquiry which] uncovers or discloses reality" (2000, p. 82). Even influential anti-realists like van Fraassen (1980) acknowledge that no realist should accept this gloss, as it confuses the *aim* of science (to disclose facts about the world) with our entitlement to claim that our theories actually put us into contact with these facts.[3] A radical skeptic about the external world can yet be (and perhaps *must be*) a realist!

That is not, of course, to say that realism is clearly correct or unproblematic. The realism/anti-realism debate persists. Its persistence (and intractability) makes difficult achieving any overlapping consensus about the justification of science education.[4] And even if anti-realism was the correct position to take about science, it seems unclear that all justificatory problems vanish. We may worry, for example, that instrumentalism and social-constructivism alone make nonsense of our current teaching practices. For we do teach the sciences of cosmology and physics often for the sake of "curiosity". Instrumentalists like Mach held that science pedagogy should ignore what he ridiculed as "odd and exceptional cases". As Fuller notes: "For Mach, science was a fit subject for general education as long as students could easily assimilate it into their normal lives." (1994, p. 204).[5] But in fact, we teach much more than what can be assimilated, even with difficulty, into the normal lives of most students. Monk and Osborne point out that despite the general aims of science education "science teachers find it quite difficult to think beyond the confines of the classroom and the laboratory", that schemes of work "are very inward looking, traditional and often lacking in anything other than the self-perpetuation of the science teacher's textbook knowledge" (2000, p. 209). Thus, the project of justifying current teaching practices (e.g., teaching Newtonian Mechanics) can quickly become simply an acceptance of (C).

Two alternatives arise here. First, we might attempt to identify an anti-realist-friendly surrogate for truth — van Fraassen's "empirical adequacy", for example — and reconstruct our justification in this mold. Indeed, we might not even need to accept anti-realism to adopt this move for what we regard as in our incautious moments as "false" theories. Let me put off this thought until §5. Second, we might be able to avoid the difficulty in identifying practical utility for particular students in false theories by seeking a wider *social* utility in their teaching. General education aims at producing informed and responsible citizens and voters. Thus one might argue that there is instrumental value in teaching, say, classical mechanics or genetics — even if only at the "good enough" level — so that voters can make responsible decisions in an increasingly technologically advanced society.

This line of argument thus supports my claim that (F) is false by maintaining that there is social value in teaching even false science. But it faces a number of problems. First, we might worry that the falsity of the theories in question might undermine whatever sense of "understanding" was relevant to the social issues at hand — indeed, perhaps this falsity would lead to *bad* decisions. So presumably *something* would need to be said to weaken the sense of falsity involved. For two, even if the falsity was irrelevant, we might doubt that the problematic theories in question (or indeed, *any* textbook science) would reach requisite level to be of any service.

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[3] I read Benson's claim above that the realist confuses "hypothetical" and "existing" entities as making essentially this mistake.
[4] I shall return to the question of the importance of such consensus in the last section.
[5] Mach's views on science education divided into claims about what should be taught to a general audience and what to those future scientists, a division I am purposely glossing in this paper.

Bauer argues along these latter lines that since most of the political issues for which scientific knowledge is purportedly useful concern cutting-edge (rather than textbook) science, this political justification fails. If we are unprepared as a society — either socially or practically — to require scientific education to extend to such depths, then we cannot expect current, textbook science education to lend itself to political decision-making of the envisaged kind — though he parenthetically admits that "one cannot understand the content of frontier science without already knowing a great deal of textbook science" (p. 11). Indeed, Bauer doubts that *any* level of science education should enjoy this social utility. The claim that scientific literacy "enables people — as voters and consumers — to make better decisions" in this scientific age may be discredited simply by taking note of famous disputes between members of the scientific elite, such as that of Oppenheimer and Teller about the production of nuclear weapons:

> Robert Oppenheimer and Edward Teller disagreed as violently as two people ever could about [whether to build nuclear weapons]; at the same time, they were both about as superbly qualified in the relevant science as anyone could be. . . . If the marvelously broad and deep knowledge of science possessed by Oppenheimer and Teller did not enable them to reach agreement on what should be done, what earthly use would be the scientific knowledge that most other people could hope to acquire? (1992, p. 13)

But Bauer's critique misses its target. It is compatible with making "better decisions" that people should still disagree. Clearly, other relevant factors figure into the decision about whether and why to attempt to construct nuclear weapons, just as they will most likely figure into other policy decisions. The point is not that knowledge of certain facts *compels a unique decision*, but that it allows an *educated* decision, sidestepping inane disputes about matters of fact. Surely *that* is valuable. A detailed examination of Oppenheimer and Teller's dispute would, I suspect, reveal that they did not suffer any significant *factual* disagreements. We might well wonder whether such is true about those who argue against stem-cell research.

What about Bauer's more moderate claim that political matters usually concern what he calls "frontier" science — a level of science that we would be unable and unwilling to require students to learn? Several other examples come to mind: questions about cloning policy or genetic modification of plants and animals, government expenditures on scientific research projects such as space exploration or the now defunct superconducting supercollider. Supposing that a general understanding of these projects (their aims, methods, chances for success, and so on) required a level of scientific understanding not achieved in secondary school, how could the current level of science education be supported on a political basis? If textbook science does little to inform political decisions, why include it in our curriculum?

One initial answer (which I explore in more detail below) might be that secondary school science lays a necessary groundwork for further process in science, just as primary school reading lays groundwork for further work in history. Bauer's critics might identify a role of science education for preparing students to be good judges of evidence and expertise, as a prelude for political decision-making. They might (plausibly, I think) deny that such decisions call upon "frontier science" out of reach of general education.

It seems to me that both Bauer and his critics misrepresent why we teach (or might teach) science and what we should expect out of science education. Phillip Frank makes Bauer's mistake in the opposite direction, arguing for an increased prominence of philosophy of science in physics curriculum. Frank cites, for example, the unfortunate symptoms of the popularization of Einsteinian relativity as giving credit "to the doctrine of the 'relativity of truth'", contributing to a disbelief "in the 'absolute values' of ethics" (2004, p. 100). Frank remarks on the dubious state of physics education that:

> The scientist will, as a matter of fact, often be more helpless than an intelligent reader of popular science magazines. We face the same situation if we ask our graduate in physics whether the theory of quanta has justified the belief in the freedom of the will or whether it has made a contribution toward the reconciliation between science and religion, as has been maintained frequently, even by scientists and philosophers of high reputation. . . . The great majority of these trained physicists . . . will be unable to give any but the most superficial answer. And even this superficial answer will not be the result of their professional training. . . . The result of conventional science teaching has not been a critically minded type of scientist, but just the opposite. (*ibid.*)

It is quite unclear that physics ought to have anything much to say about such philosophical matters or even that it should align itself with a particular school of thought about semantic or normative relativity, freedom of the will, or the reconciliation of science and religion. There is not yet (nor might there ever *be*) very significant agreement on such matters, nor is it clear what science might discover that would settle them either way. I certainly do not object to "critically minded" scientists — indeed, the world could use more critically minded *people*; but indoctrination by "popular science magazines" does not suffice. I seriously doubt, however, that Frank is correct that instruction in a sufficiently broad range of science will or *should* enable the scientist to conclusively resolve a broad range of ethical or philosophical disputes.

But again, while the fully informed scientist is in no *special* position to resolve those disputes, she may suffer no misunderstandings about the relevant facts. A reasonable physicist (even one trained only in classical physics) will likely look askance at quirky proposals purporting to secure a libertarian freedom of the will on the basis of "decision-making" particles in the brain (to invent a quirky proposal on the spot). It probably suffices for many purposes for students to merely understand that science appears to offer a compelling, *naturalistic* way of understanding the world. Explanations involving divine (or malevolent) supernatural forces are not needed, though it seems an open question whether employing strictly *false* theories for this purpose is a reasonable course of action. Laplace's demon helps us to exorcise more pernicious demons. We cannot expect science education to do too much, but we should not discount its instrumental value (as at least a precursor) to political and philosophical decision making. Such a value depends on there being at least *some* sense in which the theories in question are *true enough* (of which more in §5).

4. A Lack of Alternatives?

Bohr's wit furnishes an apt parable about trade-offs. Recall that Heisenberg's uncertainty principle states that both the position and momentum of a particle cannot be simultaneously determined: measuring one disturbs the other. Bohr referred to this property as "complementarity". When asked by an eager reporter what quality was complementary to truth, Bohr is said to have replied "clarity" (Weinberg 1992, p. 74).

"Simplicity" might have been another good answer. The structure of the world, according to our best theories, is *complicated*. Some of the more peculiar features of these theories — non-locality in quantum mechanics, time dilation and spatial curvature in relativity theory, and Planck-length, 11-dimensional strings as the fundamental stuff of the universe — require theorists to think weird and difficult thoughts and deal with weird and difficult mathematics.[6] Unquestionably, these thoughts and maths demand lots of training and struggle to understand; hence a simple thought: It is simply not *practical* to teach our best science. Not only is it constantly changing, thus presenting teachers with the formidable project of "keeping up" with research, but the experiments that have served to verify its predictions are well out of the range of secondary school capabilities. As a result, teachers would find it extraordinarily difficult to motivate the theories as better accounts of the data.

I take all of these facts very seriously. It is one thing to complain about science curriculum and quite another to produce a successful curriculum responding to these complaints. But simply claiming that the truth is too hard only provides an excuse for not teaching the true, *not a justification for teaching the false*. If correct science is too hard, and simple science is wrong, why teach science at all?

Indeed, it might be worth considering whether other radical restructurings of science education would be possible. Could students, for example, successfully begin their science education only when they had received sufficient mathematical education to make teaching our best theories possible? Could we just *skip* the simple and false until we could tackle the complicated reality? The are of course a number of problems with this idea. It's hard to motivate the relevant *mathematics* without *some* understanding of basic scientific concepts. What's the point of a derivative if you have no concept of *acceleration*? Why bother with linear algebra or complex geometry? But while these motivational difficulties are severe, it clearly seems *possible* to gain an understanding of the relevant mathematics without any practical application (indeed, I suspect that too often we fail to apply the mathematical concepts we teach).

Whether anyone would actually *receive* this rarified education is clearly another question. Would such a program foreclose on the possibility of securing the sorts of social benefits mentioned above? Would it be *pedagogically* or *psychologically* possible? I have fairly little idea. The important point, however, is that its *impossibility* (in whatever sense) does not compel teaching *false science* as a prelude to our best science unless it's

[6] I assume here (for simplicity) that these best theories — general relativity and quantum mechanics — are *true* theories, when they cannot both be. Maybe neither is. It does not matter much for my point. They are both at least *contenders* for truth.

clear that *other preludes* would not do just as well (or better). And it's not at all clear that there *aren't* such alternatives. Perhaps they're not ready to hand, but are we so sure that we could not dream them up? As long as we're teaching what's false, why not teach *other* false theories if they'd make for more compelling learning? The simple fact that certain theories are part of the history of science doesn't clearly suffice to privilege them other, perhaps more *pedagogically useful* accounts of scientific history. While I cannot hope to substantiate this claim here, it seems that the relevant *scientific history* is itself often highly truncated, misrepresented, or otherwise idealized in order to make for a pedagogically more useful, more *rational* account.

Ultimately, this whole line of argument — that false theories are necessary stepping stones leading to candidates for *true* theories — faces the reality that most students will *never* encounter our best science. What purpose, then, could these "stepping stones" serve? Why *these stones* rather than others? I believe the answers must depend on features of the "prelude-sciences" themselves. Let us turn, then, to the question of approximate truth.

5. Is Newtonian Mechanics Really False?

So far I have focused on ways of justifying teaching certain theories independently of their truth or falsity. Time to turn to my claim that Newtonian Mechanics is false. Is that really so? One way out of our pedagogical puzzle simply denies premise (NF). But we should note right away is that this response doesn't generalize to other theories we may agree *are* false. Mendelian genetics, for example, assumes that genes are swapped around independently during meiosis and mitosis; but independent assortment is false. The optimistic can of course tackle these cases one at a time in something like the way I consider below.

In the case of Newtonian Mechanics, there seems to me a clear sense — taken as a collection of generalizations about the world — in which it is false. Ronald Giere agrees:

> …if [Newton's laws of motion and the various force laws one finds in the standard texts] are understood as statements making claims directly about the world, all the laws of motion and force laws one finds written down are known to be false — a discomforting fact to say the least. (1988, p. 84).

But perhaps, as Giere believes, this understanding of a scientific theory is too simplistic. On his view, theories are about populations of models — abstract, nonlinguistic objects — linked to the world by certain kinds of similarity relationships (pp. 82–86). So theories themselves are not truth-apt in the ordinary sense: their truth lacks "epistemological significance" (p. 79). As shall become clear, I find this a tremendously attractive view of scientific theories. But it's not for everyone. I tend to think it's not for secondary school students or younger.

In any case, many will wish to deny (NF) on more homely grounds. Perhaps it ignores the various ways we commonly *circumscribe* the theory's applicability or expresses an overly simple and dichotomous view of truth and falsity.[7] Perhaps we can make sense of its being "approximately true" and thus not simply false. The motivations

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[7] I thank an anonymous reviewer for pushing me to acknowledge these responses.

for denying (NF) are pretty easy to appreciate. Teaching, especially at the secondary level, involves providing a good deal of motivation. "Why do we have to know this?!" cry the students. Lots of reasons: but sometimes the teacher just wants to reply: "Because it's *true*!" Of course, as we've already seen, truth alone does not offer a sufficient pedagogical justification; but falsity credibly increases this burden. Denying (or downplaying) the falsity of Newtonian Mechanics can be forgiven on at least this motivational level. How exactly they should go about denying (NF) is another question.

5.1. Truth as Circumscribed Predictive Accuracy

We might attempt to cash out a sense in which Newtonian Mechanics is "not false" by pointing to its predictive accuracy for "ordinary applications". Though it is false (strictly-speaking, taken as a collection of unrestricted generalizations), it is not *outrageously* false. So, in a sense, Newtonian Mechanics is *true* — or perhaps, "true for its students" (so long as they do not butt up against the limits of its predictive accuracy). And for the vast majority of ordinary purposes — say, for building bridges and destroying them with canon balls — it is rather useful.

However, it's important to see that (so long as we deem it coherent to speak of truth of theories) claims of truth and utility are quite distinct. A theory's being approximately true needn't imply anything about its *general* "usefulness" or predictive utility — for some applications may require the sort of precision that "approximately true" theories lack. Likewise, predictive utility may be obtained by force (with enough deferents and epicycles) for even wildly false theories.

So this approach would likely need to be supplemented by claims about the circumstances in which the theory's predictions hold (within experimental error, to a degree of approximation, or whatever the proper measure is held to be). But it's worth noting that a theory like *Newtonian* Mechanics lacks these provisos (indeed, Newton famously trumped the universality of his account). Working physicists, of course, know better where one can safely employ "Classical" Mechanical laws. Whether these restrictions get explicitly worked into the theory or remain tacit, this line must still grapple with two dimensions of "fit" along which truth is approximated: the respects in which the theory works and the degree to which it works (see Giere 1988, p. 81). Let's address the last first.

Open a physics textbook and you will most likely find a table of contents of an eminently coherent and sensible sort, listing chapters about dynamics and force in nice progression. The progression is usually broken late in the book by awkward entries on Special and General Relativity, and perhaps by Quantum Mechanics (and much more rarely, String Theory). Sometimes qualifications are provided up front. In one popular text, the first chapter on 'Measurement of Space, Time, and Mass' lays out some typically cagey provisos to the ensuing several hundred pages of classical mechanics:

> That our space is three-dimensional is its most elementary and important property. Next in importance is the property that the geometry of space is Euclidean, at least to a very good approximation. . . . Equal in importance to the property that space is Euclidean ranks the property that time is absolute. (Ohanian, 1985, p. 3)

What it means for space to be approximately Euclidean to the novice is not at all obvious. Ohanian goes on to explain his qualification:

> The theory based on the assumption of an absolute time and a Euclidean space is called classical or Newtonian physics. . . . For more than 200 years Newton's theory stood unchallenged. But in the early part of this century, physicists found that the geometry of space is not exactly Euclidean and that time is not exactly absolute. (p. 4)

This way of setting up the classical/relativistic divide does quite a bit to lessen the impact or importance of the classical/non-classical distinction. Such vague provisos, if noticed at all (perhaps they will be reminded of them when and if they reach Chapter 23), give little indication that theories like general relativity and quantum mechanics are not merely machinations to tidy up exotic shortcomings of Newtonian mechanics, covering the non-classical misbehavior of a few quantum-sized particles or problems of perspective traveling at unthinkable speeds. If ours is the slow macroscopic world of rigid rods, pendulums, and canon balls, *who cares?*

But what does it mean to claim that space is Euclidian "to a very good approximation"? A tempting technical answer: *Euclidean space is a special case of the Minkowskian space of general relativity: that in which the Gaussian curvature equals zero*. Perhaps Ohanian means that the average curvature of space is close to zero: that our Minkowskian space is very close to the special limit case of Euclidean space — a near miss. Compare the claim that the universe is a near vacuum. True enough, but putting matters this way (as with any facts about averages) is dangerously ambiguous, especially in the context of secondary education. For without saying more, one may take this as the claim that space contains very little matter, whereas, more specifically, it means that space contains little matter on average per cubic meter. Some familiar, local regions of space contain a comparatively large amount of matter. Likewise, some regions of space are dramatically curved — that is, dramatically non-Euclidean. Or imagine an immensely large, flat landscape dotted with a small handful of dramatic, tall mountains (think Mount Fuji). Visitors to this landscape who describe it as 'flat' would presumably be scolded during the slideshow. Described as 'flat to a very good approximation', curiously, seems even less appropriate, insofar as it calls to mind a land of gentle slope. What of time being "not-exactly absolute"? Special relativity teaches that locutions like 'Events A and B were simultaneous' are semantically incomplete in rather the same way as 'A is to the left of B' is incomplete, without reference to a particular perspective. It just happens that we can treat the former locutions as complete and grammatical since we differ in our perspectives only very slightly.

One might insist in reply that there is a simple sense in which the above sorts of provisos are *precisely* the sorts of things that science teachers should say if they do not wish to get into the details of general or special relativity. The landscape analogy, if it establishes how such provisos are misleading, can itself be *employed* in explaining how our space is non-Euclidean. Moreover, these provisos provide the very rationale for teaching the simpler, though strictly false, classical mechanics: the truth requires only numerical correction in cases very remote from our experience. We denizens of the "Earthly reference frame" have no need for such corrections.

Implicit in this claim is the idea that the adjustments required for relativistic effects are only in minor respects: say, as only minor numerical transformations to accommodate "perspectival" changes. As Sorensen notes, this approach is part of a long tradition that ironically stems from Einstein himself (more precisely, from his early Machian leanings): "From the earliest popular expositions to the present time, students get the idea that rapidly moving bicycles and streetcars 'look' shorter. The Lorentz *contraction* is usually discussed in terms of the Lorentz *transformation* and not in terms of atoms pulling and pushing on each other" (1995, p. 413). But such approaches, while tempting, incorrectly minimize the respects in which classical mechanics diverge from relativistic mechanics. In a well-known paper, "How to Teach Special Relativity", J. S. Bell offers a nice thought experiment:

> Three small spaceships, A, B, and C, drift freely in a region of space remote from other matter, without rotation and without relative motion, with B and C equidistant from A. On reception of a signal from A the motors of B and C are ignited and they accelerate gently. Let ships B and C be identical, and have identical acceleration programmes. Then (as reckoned by an observer in A) they will have at every moment the same velocity, and so remain displaced one from the other by a fixed distance. Suppose that a fragile thread is tied initially between projections from B and C. If it is just long enough to span the required distance initially, then as the rockets speed up, it will become too short, because of its need to Fitzgerald contract, and must finally break. It must break when, at a sufficiently high velocity, the artificial prevention of the natural contraction imposes intolerable stress. (Bell, 1987, p. 68)

If the contraction were a merely "perspectival" matter, we are to suppose, the string would stay taught.[8] Whatever you think of Bell's conclusion, it's clear that relativity involves more than simple numerical corrections to our "perspective" — otherwise, how could general relativity explain the precession of Mercury's perihelion?

5.2. The Interest-Relativity of Approximate Truth

The denier of (NF) will probably reiterate at this point that such failures of fit involve respects that do not obtain near the surface of the earth or at speeds we ordinarily travel at. But this line seems rather difficult to pull off in any detail. Scientists must often make what seem like one-off, ad hoc adjustments to bring theory in line with reality, given their particular interests. These adjustments are not easily or mechanically achieved these interests.

Consider an analogy. We can regard it as true that everyone came to the town meeting, though over six billion people didn't show up: quantification is regularly tacitly restricted. Moreover, there is an obvious account

[8] It should be noted that it's not *necessarily* the case that classical mechanics gets the wrong answer: it issues no predictions until the relevant forces are specified. The thought experiment does reveal something interesting about how special relativity is widely construed. As Bell notes, his informal survey of physicists in the CERN canteen resulted in the initial conclusion that the string would not break, though most recanted on reflection. Perhaps this error was due to a popular misunderstanding of special relativity that the effects of Lorenz/Fitzgerald contraction and time dilation are somehow just a matter of perspective.

of how such restriction proceeds, one that makes little reference to individual purposes. In this context, by 'everyone' we mean everyone in the town. Does the proposition stay true when Herbert the hermit and comatose Cletus don't show? Here the restriction is less automatic: perhaps we meant 'everyone' (in the context of local politics) as everyone in the town who is capable and interested in or affected by local politics. In these cases, context is our guide. 'Everyone in town is conscious' ought to take into account poor Cletus.[9]

The project of giving an account of approximate truth resembles accounting for the truth-value of the latter sentences. As Richard Boyd writes, "Our conception of relevant approximation reflects considerations specific to the particular theory or theories, historical settings, and contexts of applications under consideration" (Boyd 1990, p. 218). In the case of Newtonian Mechanics approximating relativistic mechanics, the approximation concerns low speeds and densities. But of course there are many other respects in which the classical picture fails to be an approximation of what we now take to the reality of things. As Giere rightly points out, "classical laws" cannot be universal statements about even slowly-swinging pendulums. Actually applying these laws involves making simplifying assumptions (ignoring the material and geometrical heterogeneity of the earth, ignoring complex friction effects, other sources of gravity, and so on) (1998, p. 77; see also Cartwright 1980 and 1999). Making these assumptions and approximations need not result in claims that will be confirmed within experimental accuracy. Adjusting them to fit, on the other hand, will tend to depend on the particular situation we are modeling and the purposes to which we put our theory. Circumscribing classical mechanics to independently-specified "ordinary circumstances" — to some particular region of spacetime or to certain types of motion — so that it remains true to relevant respects to good approximations.

Or consider the Bohr model of the atom, still a common part of secondary school chemistry courses. It encourages a sort of Copernican conception of the atomic structure, with the nucleus as the sun around which the electrons orbit like planets. Taken literally, it is very difficult to see how this counts as an approximation of the quantum picture of atomic orbitals. Still, Boyd is sanguine about the realist's prospects:

> Classical conceptions of the atomic world were, let us agree, poor approximations to the truth in metaphysics. Does this preclude their having been good enough approximations in other respects to sustain the realist's account of the development of quantum theory? Plainly not. (230)

This seems especially plausible when the particular failures of a model prompt the questions that direct us to the better model. Bohr's 1913 model of the hydrogen atom (and other one-electron ions) conceives of the electron as whirling around the nucleus in a circular orbit of a certain radius, the length of which determines its potential energy. As the electron decreases this orbital radius, it emits light of different wavelengths. Given this picture, though, one should expect the atom to spiral into the nucleus, giving off a continuous spectrum of light. But the atomic spectrum of hydrogen is not continuous: it consists of several distinct lines. So Bohr simply added a

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[9] Often context does not suffice to produce precise restriction as in 'By 10AM everyone in town is awake'.

feature to his account: that the angular momentum of electrons is "quantized", taking the value only of integer multiples of Planck's constant over 2π. Remarkably, when this feature is added and electrons are allowed to "jump" from different energy levels, it predicts the observed atomic spectra.

Despite its lack of metaphysical illumination, then, the literal Bohr picture can seem amply confirmed. But the realist account of the development of quantum mechanics Boyd mentions requires treating Bohr's theory in a very instrumentalist way — in Mach's sense of theory conveniently "recording observation". The crucial question with which Heisenberg, de Broglie, and others grappled, was *why* is the electron's momentum quantized? The planetary model of atomic structure cannot answer this question: only by treating the electron as a wave could sense be made of this behavior. This insight eventually led to our current conception of atomic structure.

We should distinguish here between giving a realist version of approximate truth of a classical theory that undergirds that theory's epistemic significance as a picture of the world and a realist understanding of scientific progress — of how a theory, while literally wrong, can be regarded as laying the instrumental groundwork for later literally true theories. Another schematic analogy: suppose Theory A now recognized to be wildly false, through utter happenstance came to inspire interest in concepts that proved crucial for the new (presumed) correct Theory B. Though I hesitate to submit that Theory A achieves some degree of epistemic significance in virtue of this fact (just *how* coincidental or weak is the inter-theory link?), one could certainly make the case. It might be worth sketching Theory A *just as a historical prelude* to Theory B and that this counts in favor of Theory A's epistemic significance. (Whether it counts *enough* in favor to warrant teaching it is another question.) So epistemic significance need not covary with approximate truth. The epistemic significance of the Bohr model inheres in its historical context as leading instrumentally to other theories we regard as epistemically significant (in part because of *their* literal truth).

5.3. Truth, The Whole Truth, Nothing but the Truth?

Other distinctions suggest themselves. A claim or theory can be true in a full-blooded sense without being complete — it can be true without being the whole truth. In the classical world, Kepler's laws of planetary motion were not false — they just were not the whole story. His explanations of planetary motion gave way to Newton's because Newton offered a more complete and unified account of motion (of the motion not only of planets, but of rocks and people). But Newton, like Kepler, regarded his theory as incomplete, as it reserved a central theoretical place for action at a distance in the law of gravitational attraction — it was incomplete in the sense that it failed to give some more basic local, mechanical account of the inverse-square law.

Approximate truth may subsume incomplete truth. Must it exclude outright falsity? The old problem of excluding exceptions to scientific generalizations crops up in how we interpret theoretical claims. Treating the Bohr model as just that — a model — demurs from a realistic interpretation of some claims (that the electrons literally travel in a circular orbits round the nucleus), while holding onto a realistic interpretation of others (that electrons find themselves in discrete energy levels). Our interests and subsequent theories (not to mention the historical course that lead to those theories) determine how we make these choices and how much emphasis we place on different aspects of theories and whether we treat those aspects as incomplete realistic claims or instrumental models.

The complexity of scientific theories may be a boon in this context. For nothing forces us to treat all of the claims of the Bohr model as on a par. Thus, while certain features, taken literally, look as false as false can be, we may take them as instrumental components, as models, while hanging onto other literal claims that can be held as epistemically significant in virtue of their approximate truth. The approximate truth of the theory itself inherits the approximate truth of these claims. As Kitcher writes:

> The philosophical problems of understanding progress are resolved by appreciating the multidimensionality of scientific practice, and thus focusing on truth for individual significant statements. Once this is done, the artificial problems that have been at the focus of much logically ingenious work on verisimilitude can be bypassed. (Kitcher 1993, p. 122)

But perhaps a worry about realism remains. It seems that an account of approximate truth for a given theory requires a story about the particular aims and (perhaps) parochial interests of those engaged in that inquiry. As such, it may give pause to those realists committed to a picture of science as achieving something like an observer-neutral truth (even if approximate). A sensible realism need not eschew the role of scientists in producing scientific theories. It figures prominently — and necessarily — into the account of approximate truth sketched above as that which our old theories strive to approximate. No surprise that the full account turns on individual (perhaps cultural) aims and goals, for individual aims and goals ought to figure prominently into any account of epistemic significance.

So perhaps there is *something* to verisimilitude, after all. Yet I think it is the wrong gambit for defending teaching what we might often regard as false or overly-simplified science. First, as I hope is clear by now, defending the approximate truth of a particular theory may be no easy task, and even if accomplished, not applicable to another theory. Predictive utility is not all there is to a theory. Sometimes, theories that are plausibly regarded as approximately true carry with them philosophical or methodological biases that are irredeemably false. Wesley Salmon points out that despite the acknowledged falsity of classical physics, "certain philosophical views concerning the nature of science that arise directly out of a [Newtonian] conception of the world continue to exert an enormous influence on current thought about scientific explanation" (Salmon, 1998, p. 50). Second, the details of such a defense — be they in the wildly different styles of Giere, Kitcher, Cartwright, Boyd, van Fraassen — call upon philosophical resources and motivations that usually far outstrip the context in which the theories in question are taught. Third, while I do not claim that there is *no way* of couching these claims in salient terms to fourteen year olds (though I cannot see how it would go), I do not believe that such justificatory strategies will enjoy much overlapping consensus.

There seems to me a better way upon which various parties can agree.

6. A Role for History and Philosophy

I've claimed above that the strategies of justifying teaching false science by reference to its instrumental value or by abandoning talk of truth or falsity altogether face a number of difficulties. Might there be others I haven't considered? Of course. Do the arguments concerning Newtonian Mechanics generalize to other theories? Perhaps not straightforwardly, but I think they do — indeed, I suspect that the challenges facing making sense of the simplified chemistry and biology students receive in secondary school pose even *greater* difficulties as typically more qualitative sciences. But that case will have to wait. Might there be ways of fixing up the problems I have mentioned? Yes.

I am much less confident about whether whatever shape these attempts take would be communicable to students or would garner any consensus from teachers and scholars of science deeply divided by different philosophical and methodological approaches. It does not seem enough for a pedagogical approach to be justified by several different, incommensurable means of justification — though we often settle for this. Justification ideally should be transparent: different agents should be able to see, as it were, its "inner workings" — able to appreciate the justification of these components in turn. Achieving that kind of overlapping consensus seems important enough to put aside minor scholarly debates about realism versus anti-realism or about the instrumental value (or lack thereof) of general science education and focus on what is available as common ground. If I'm right about what this common ground is — that pedagogical significance accrues to theories within from the history of science notwithstanding their truth, falsity, or otherwise —, then it has the added benefit of being easily communicable to the students right along with the science they are already learning.

So let me turn at last to my favored strategy of justification and its justification. I argue that rejecting (F) above — the claim that we should not teach any false scientific theory — solves our particular puzzle about teaching Newtonian Mechanics (and *mutatis mutandis* for many other false theories, though all of these cases should ideally be made in detail). We can appreciate the epistemic significance of even false (or merely approximately true) theories from within their historical context: as exemplars of how science was or is done, as waypoints on the way to our current best understanding, or as illustrations of the fact that confidence in a theory (even given its elegance or impressive track-record) needn't betoken its truth. Really appreciating this significance requires, I think, not settling for half-remembered slogans about what previous, apparently careless or benighted, investigators thought and why they went wrong. We ought to see how they were able to come as far as they did. Einstein wrote in his "Autobiographical Notes", as if to Newton: "you found the only way which, in your age, was just about possible for a man of highest thought and creative power. The concepts, which you created, are even today still guiding our thinking physics, although we now know that they will have to be replaced by others farther removed from the sphere of immediate experience, if we aim at a profounder understanding" (quoted in Ferris, 2003, p. 121).

I will be the first to admit that teaching science on something like this model would require a great deal of time, care, and ingenuity — I don't know whether we have enough. Obviously I cannot say all there is to be said here to fully vindicate this approach. Considerable challenges face those faced with the task of developing a curriculum incorporating both history and our best science at the secondary school level (to say nothing of more mathematically peculiar and speculative theories such as the variety of string/brane theories). As they say, the proof is in the pudding. Science teachers often face incredible (and perhaps more pressing) challenges in trying to make science seem *relevant* to their students. How could increased focus on the history and philosophy of science possibly bear on the lives of students who do not (initially) place much value on the pristine truth, far away from their lives and concerns?

I have no sure answer for such concerns. No doubt part of the solution will involve considerations of what *is* of interest to the students in question and how to bridge the gap. But I do not think that my urging for a greater place in science curriculum for historical and philosophical issues will *necessarily* make their jobs harder in the long run (to use a famously slippery qualifier). Indeed, my insistence upon the need for an overlapping consensus about the justification of science education reflects commitment to the need for teachers to be able to readily relate the value of what they teach in the face of yawning or sarcastic resistance. My personal experience is that students are often *excited* by the recognition that science is still unfolding. Indeed, history seems to illustrate the reverse psychology: Along with the widespread fin-de-siècle optimism that gripped 19^{th} century physicists that most of science had been *done*, came a general disinterest in pursuing science. It seemed to many that all that remained was tidying up some of the details and correcting numerical discrepancies occurring after the fifth decimal place — a project to be carried out by technicians and other scientific janitors.

It seems to me that the "business-as-usual", textbook-science model of science education faces two closely related worries. On the one hand, teaching as true what we or our students may otherwise reasonably regard as strictly false theories may feed a similar misconception amongst present-day science students (and adults alike) that science is *done*, that we know pretty much everything about the world, and that what's left is stamp-collecting and number-crunching. It is not usually until upper-level undergraduate classes that one discovers that the situation is emphatically not that. Sometimes, this optimism bloats into the belief (apparently shared by Bauer and Frank) that scientific instruction puts scientists in a special position to decide matters quite orthogonal to science. In the limit, such unrealistically neat, rational, complete, and pristine views of science — "scientism" or what Kitcher has called "Legend" — serve as irresistible targets for equally unwarranted *irrationalist* views of science.

Optimism is also vulnerable to corruption from within. Perhaps the world looks neat and Newtonian for a while, but then one is told that it just isn't (quite). When even the "*replacement theories*", taught as true, are replaced by yet different theories later in the curriculum, optimism may turn to pessimism. After receiving enough of these jolts ("You were taught last year that X, but forget about that: instead, Y"), a student may abandon hope that (in Mulder's words) "the truth is out there", that science is worth doing. At the college level theories are presented as cutting-edge replacements of those false theories taught earlier. But the process iterates. One learns of the orbital theory of the atom and of orbital hybridization in an organic chemistry class, discovering later that the hybridization theory is at best a model in the same sense as Bohr's atom. Glossing a false theory as true (when in the teacher's mind perhaps she means "true enough in certain respects"), without spending adequate time discussing the origin, status, and problems of the theory thus plausibly runs a grave psychological risk.

The resolution of this dilemma further supports my solution to our pedagogical puzzle. Moderate realists suppose that our understanding of the world has gradually improved, through fits and starts, and detours through blind alleys, notwithstanding the workings of parochial and social influences. The history of science is a complicated, messy, at times salacious story; but it is the story of how we've come to our present state of understanding — both of how the world is (what are its laws, constants, fundamental furniture, and so forth) and *what concepts and methods inform this understanding*. Making the falsity of the Newtonian worldview clear rather than trumping up its approximate truth can lead to a deeper understanding of what are plausibly more important scientific notions: confirmation, explanation, causation, and so forth. It is with these that science begins, at the earliest level.

In other words, I believe that the best way of teaching false science is by teaching it *as false*, but *illustratively* — incorporating a critical historical perspective into the science curriculum. We ought to teach as much about science as a practice (of how, realistically, it is done) as what a particular theory's claims and methods are. Part of the significance of a bit of scientific knowledge depends on how that knowledge fits in with other valuable pieces of knowledge. We can make room for approximately true theories in our curriculum only if we are willing and prepared to spend time on *how* they are approximations (and in what ways they fail to be). By refocusing on conceptual aspects of science and scientific method — including topics usually broached only in the university philosophy or history classroom — the approximately true theories of the likes of Newton, Bohr, and Mendel may retain their significance.

REFERENCES

Bell, John (1987) *Speakable and Unspeakable in Quantum Mechanics*. Cambridge: Cambridge University Press.

Benson, Garth D. (2001) "Science Education from a Social Constructivist Position: A Worldview", *Studies in Philosophy and Education* 20: 443–452.

Boyd, Richard. (1990), "Realism, Approximate Truth, and Philosophical Method" in Papinaeu (ed.), *The Philosophy of Science*. Oxford: Oxford University Press.

Cartwright, Nancy (1980) *How the Laws of Physics Lie*. Cambridge: Cambridge University Press.

Cartwright, Nancy (1999) *The Dappled World*. Cambridge: Cambridge University Press.

Ferris, Timothy (2003) *Coming of Age in the Milky Way*. New York: HarperCollins Publishers.

Frank, Phillip (2004) "The Place of the Philosophy of Science in the Curriculum of the Physics Student", *Science and Education* 19: 99–120.

Fuller, Steve (1994) "Retrieving the Point of the Realism-Instrumentalism Debate: Mach vs. Planck on Science Education Policy", *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1994*: 200–208.

Giere, Ronald (1988) *Explaining Science*. Chicago: University of Chicago Press.

Kitcher, Philip (1993) *The Advancement of Science: Science without Legend, Objectivity without Illusions*. New York: Oxford University Press.

———— (2001) *Science, Truth, and Democracy*. New York: Oxford University Press.

National Research Council (2007) *Taking Science to School*. New York: National Academy of Sciences.

Ohanian, Hans (1985) *Physics*. New York: W.W. Norton & Company.

Rawls, John (1993) *Political Liberalism*. New York: Columbia University Press.

Salmon, Wesley (1998) *Causality and Explanation*. New York: Oxford University Press.

Sokal, Alan, and Bricmont, Jean (1998) *Fashionable Nonsense* (Vol. New York). New York: Picador.

Sorensen, Raymond A. (1995) "Lorentz contraction: A real change of shape", *American Journal of Physics* 63(5): 413–15.

Van Inwagen, Peter (1990) *Material Beings*. Ithaca: Cornell University Press.

Monk, Martin and Dillon, Justin (2000) "The Nature of Scientific Knowledge" in Monk and Osborne (2000).

Monk, Martin and Osborne, Jonathan (Eds.) (2000) *Good Practice in Science Teaching*. Philadelphia: Open University Press.

Van Fraassen, Bas (1980) *The Scientific Image*. Oxford: Clarendon Press.

Weinberg, Steven (1992) *Dreams of a Final Theory*. New York: Basic Books.