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Tolstoy's Argument: Realism and the History of Science*

Stathis Psillos†

I.

In his intervention in the "bankruptcy of science debate," which raged in Paris at the turn of the twentieth century, Leo Tolstoy was one of the first to use the historical record of science as a weapon against current science.[1] Among his various observations in his essay "The Non-Acting," published in French in August 1893, the following stands out:

> Lastly, does not each year produce its new scientific discoveries, which after astonishing the boobies of the whole world and bringing fame and fortune to the inventors, are eventually admitted to be ridiculous mistakes even by those who promulgated them? (...) Unless then our century forms an exception (which is a supposition we have no right to make), it needs no great boldness to conclude by analogy that among the kinds of knowledge occupying the attention of our learned men and called science, there must necessarily be some which will be regarded by our descendants much as we now regard the rhetoric of the ancients and the scholasticism of the Middle Ages. (1904, 105)

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[1] I tell the philosophical story of the bankruptcy debate in my "Revisiting 'the Bankruptcy of Science' Debate" talk delivered at the Rotman Institute of Philosophy, University of Western Ontario, 24 January 2014; available at https://www.youtube.com/watch?v=zwEYZNKeCpQ

Tolstoy couldn't put this history-fed pessimism more clearly or elegantly. Given the past record of failures of scientific theories, which emerged triumphantly only to be abandoned at a later stage, we are justified to expect "by analogy" that at least some of the presently accepted theories will have the same fate, unless we are entitled to suppose that currently accepted theories "form an exception," a supposition that "we have no right to make."

In his preface to the Russian translation of the English essayist Edward Carpenter's "Modern Science: A Criticism" (published in 1885), Tolstoy made clear that his main concern is with a conception of science that takes it to aim to explain "things near and important to us by things more remote and indifferent" (1904, 220).

II.

Note the sophistication of Tolstoy's argument. It is not inductive. It does not conclude that all current scientific theories will be abandoned; nor that most of them will be abandoned; not even that it is more likely than not that all or most of them will be abandoned. Its conclusion is modest: some of presently accepted theories will have the fate of those past theories that once dominated the scene but subsequently were abandoned. This conclusion is grounded on an *analogy*; and it is as strong as the basis for the analogy. Hence, it is subject to the proviso that the current ways to do science "form no exception." Because of its modesty, the argument is very pressing. Unless some kind of privilege is granted to current science or unless there are ways to identify the current culprits—those *current* theories which will have the fate of past abandoned ones—Tolstoy's point is compelling: we cannot simply *assume* that as science grows, we get to know more about the world.

III.

Tolstoy was very much among the "people of the world" whom Henri Poincaré had in mind when he said, in his keynote address to the 1900 International Congress of Physics:

> The people of world [*les gens du monde*] are struck to see how ephemeral scientific theories are. After some years of prosperity, they see them successively abandoned; they see ruins accumulated on ruins; they predict that the theories in vogue today will in a short time succumb in their turn, and they conclude that they are absolutely in vain. This is what they call *the bankruptcy of science*. (1900, 14)

But he added, "Their scepticism is superficial; they understand neither the aim nor the role of scientific theories; without this they would understand that ruins can still be good for something."

Poincaré's considered reply was that the history of science shows that there is continuity at the level of mathematical equations, which he took to express theoretical relations among things. Far from being bankrupt, science offers *some* knowledge of the world, which is certified by the historical record of diachronically invariant mathematical equations, and hence of theoretical relations. The history of the development of physics, Poincaré stressed, shows that "new relations are continually being discovered between objects which seemed destined to remain forever unconnected" (1900, 23).

This kind of response answers Tolstoy's pessimism by presenting a *strategy* that neutralises his argument. The answer is: look at what is retained when theories change; these invariant elements capture what we can legitimately describe as scientific knowledge of the world.

IV.

For various reasons that had to do with Poincaré's relationism, he thought that that the neutralising strategy was bound to vindicate only knowledge of the relational structure of the world and not of the intrinsic properties of things—the "things themselves" (*choses elles-memes*) as he put it (see Psillos 2014). Ludwig Boltzmann did not carry this philosophical baggage. He was committed to atomism and thought that the atomic theory of matter was, by and large, correct (a view that Poincaré came to accept shortly before he died based on the theoretical and experimental work of Jean Perrin on the molecular basis of Brownian motion—see Psillos 2011).

It's unlikely that Boltzmann knew about Tolstoy and the "bankruptcy of science debate" in Paris, but he certainly knew of the "bankruptcy of materialism" debate that had been initiated by Wilhelm Ostwald's paper, "Die Überwindung des Wissenschaftlichen Materialismus," which was delivered in 1895 at the annual conference of the Society of German Naturalists and Physicians in Lubeck. Boltzmann was the respondent. Ostwald's paper was quickly translated into French (Ostwald 1895) and English (Ostwald 1896).[2]

Part of Ostwald's argument against the "mechanics of atoms" was historical: all attempts so far to offer a mechanical account of optical phenomena had failed (1896, 593). But what bothered Boltzmann more was the view of the so-called "phenomenologists" (who, as he noted, included

[2] Ostwald's piece was linked to the "bankruptcy of science" debate by the French physicist Leon "Marcel" Brillouin (1854-1948), who published a pointed reply to Ostwald in the very same year in the journal *Revue Générale des Sciences Pures et Appliquées*, entitled "Pour la Matiere." His piece started with the observation, "After the bankruptcy of science, the bankruptcy of atomism!"

the early Max Planck). They, according to Boltzmann, were opposed to the explanation of the visible in terms of the invisible. According to the phenomenologists, the aim of science was to "write down for every group of phenomena the equations by means of which their behaviour could be quantitatively calculated" (Boltzmann 1901b, 249). The theoretical hypotheses from which the equations had been deduced were taken to be the scaffolding that was thrown away after the right equations had been arrived at. For phenomenologists, then, explanatory hypotheses of the visible in terms of the invisible were neither unnecessary nor useless. Rather, they had a heuristic value only: they lead to stable (differential) equations that describe the behaviour of the phenomena as they are experienced.

According to Boltzmann, a key motivation for this attitude toward explanatory scientific theories was the "historical principle," viz., that hypotheses are essentially insecure because they tend to be abandoned and replaced by other, "totally different" ones. As he put it:

> ...frequently opinions which are held in the highest esteem have been supplanted within a very short space of time by totally different theories; nay, even as St. Remigius the heathens, so now they [the phenomenologists] exhorted the theoretical physicists to consign to the flames the idols that but a moment previously they had worshipped. (1901b, 252-253)

Against this "historical principle," which was a bit more radical than Tolstoy's, Boltzmann argued that despite the presence of "revolutions" in science, there is enough continuity in theory change to warrant the claim that some "achievements may possibly remain the possession of science for all time, though in a modified and perfected form" (1901b, 253).

V.

So Boltzmann too, independently of Poincaré, argued that the answer to the historical challenge is to look for continuity in theory change. But unlike Poincaré, Boltzmann did not restrict the criterion of invariance to relations only. In fact, if the historical principle is correct at all, it cuts also against the equations of the phenomenologists. For unless these very equations remain invariant through theory change, there should be no warrant for taking *them* to be accurate descriptions of worldly relations.

Boltzmann went on to make a seemingly puzzling point:

> Indeed, by the very historical principle in question a definitive victory for the energeticians and phenomenologists would seem to be impossible, since their defeat would be immediately required by the fact of their success. (1901b, 253)

To see what he has in mind, let us stress his further point that the sole way that phenomenologists had to arrive at the desired differential equations was by relying on the basic assumptions of the atomic theory. These equations, Boltzmann noted, are "totally devoid of meaning without the assumption of a very large number of individual entities," that is, without assuming that matter is not continuous. And he added:

> An unthinking use of mathematical symbols only could ever have led us to separate differential equations from atomistic conceptions. As soon as it is clearly seen that the phenomenologists, under the veil of their differential equations, also proceed from atomistic entities, which they are obliged to conceive differently for every group of phenomena and as endowed now with these and now with those complicated properties, the need of a simplified and uniform atomistic doctrine will soon be felt again. (1901b, 252)

In other words, the very construction of the differential equations of the phenomenologists requires commitment to atomistic assumptions. Hence, the phenomenologists are not merely disingenuous when they jettison the atomistic assumptions after the relevant differential equations have been developed. As Boltzmann noted in his seemingly puzzling remark, their move is self-undermining in light of the historical principle that they themselves rely on to motivate abandoning the atomistic hypothesis and to defend the withdrawal to the level of mathematical equations. The reason is simple: their success (viz., that they have the right equations) would lead to their defeat since the very theory that led to this success would fall foul of the historical principle: it would have to be abandoned. That's exactly what Boltzmann means when he says that the phenomenologists' "defeat would be immediately required by the fact of their success."

VI.

The point here is not to classify Boltzmann as a realist—though I think he was. The point is that Boltzmann saw clearly that the historical challenge—Tolstoy's argument, the phenomenologists' principle—is not compelling, though it is illuminating, provided that parts of past and abandoned theories have been shown to be "stable and established" (1901b, 250). But it is significant that he also made clear that though showing continuity in theory change is important and indispensable in rebutting the historical principle, it is folly to think that this continuity is, or can *only* be, at the level of mathematical equations. More about nature can be known than mathematically-expressed relations among magnitudes.

Poincaré was, at least before Perrin's work on Brownian motion, reluctant to accept that more of nature can be known. In the 1900 address we referred to above, he illustrated his relationist account of invariance in theory change by referring to the accommodation of Fresnel's laws within Maxwell's theory. As is well-known, Fresnel's laws relating the amplitudes of reflected rays vis-à-vis the amplitudes of incident rays in the interface of two media were retained within Maxwell's theory of electromagnetism, although, in this transition, the interpretation of the amplitudes changed dramatically. The fact of retention was for Poincaré evidence that Fresnel's theory was not a mere "practical recipe" for prediction. More specifically, it was evidence that Fresnel got some relational facts about light right, where these facts were expressed in the relevant mathematical equations. Poincaré noted:

> These equations express relations, and if the equations remain true it is because these relations retain their reality. They teach us, now as then, that there is such and such a relation between some thing and some other thing; only this something formerly we called motion; we now call it electric current. But these appellations were only images substituted for the real objects which nature will eternally hide from us. (1900, 15)

It is significant to note that for Poincaré the order of dependence between the worldly relations and mathematical equations is *from the former to the latter*. It is because real relations remain invariant that they are represented by invariant mathematical equations in different theories. The retention of mathematical equations in theory change is certainly a *sign* for an underlying invariant natural relation. But this should not obscure the fact that the invariance of the equation is *explained* by the invariance of the natural relation.

VII.

But is it right to say that only relational facts about light got retained in the transition from Fresnel to Maxwell? The Fresnel case was a matter of national pride for the French. Already in 1895, in a reply to Ostwald's "irresponsibly" titled essay, Marie Alfred Cornu, vice president of *l'Académie des Sciences*, emphatically noted, "Thus, according to Mr. Ostwald, nothing remains of the work of Fresnel, of this admirable theory of the light waves, the influence of which was widespread and fecund for three quarters of a century" (1895, 1031).

The readers of the *Revue* (which was partly a popular journal), Cornu noted, will think that Fresnel's theory was "mediocre" since "it was buried without a noise" by the electromagnetic theory.

Cornu went on to point out that Fresnel's law was still alive and was retained in Maxwell's theory because it got right two important facts about light, viz., that optical vibrations are wave propagations and that this propagation is transversal. It is exactly these two features that were captured by Maxwell's "electric waves" (1895, 1031).

This is something that I too argued for (independently of Cornu, I must say) in Psillos (1995). The key point, I think, is that what counts is invariance in theory change, and that invariance in theory change is not *merely* relational or structural. Differently put, what's important is that Fresnel correctly identified some properties of light vis-à-vis Maxwell's theory, and not whether these properties were relational or not.

VIII.

Why, one might wonder, should we take seriously the invariant elements in theory change? What do they tell us about the world and why? Poincaré was fully alive to this problem and I think he had the right answer: there is no God's eye point of view; we work from within the scientific image of the world but we still want to find out to what extent it has latched onto the world. Tolstoy's argument suggests a historical reason to doubt that we have reason to believe that scientific theories have latched onto the world. Invariance in theory change neutralizes this reason. But one might still wonder: why should we think that *some parts* of the theory have latched onto the world *in the first place*?

In his reply to the extreme anti-realism of Eduard LeRoy, Poincaré rightly thought that the correct attitude toward science should be such that theories are taken to be neither dreams nor fictions (1902, 290). The fact that they are not dreams guards against idealism; the fact that they are not fictions guards against fictionalism. The invariance in theory change separates theories from dreams, since the transmissiblity of theoretical elements that survive theory change guarantees their inter-subjectivity. Still, the retained parts of theories could fail to represent anything real; they could be fictions! That's precisely why Poincaré went beyond invariance and added another condition to his defence of science. I have called it *correspondence* (though I do not want it to be confused with the correspondence theory of truth). The idea is that the elements of theories that survive theory change should be such that there is reason to think that they have latched onto reality.[3]

Though I will not argue for this here in any detail, this condition of correspondence captures an early version of the so-called "no miracles argument," which Poincaré put in terms of how unlikely it is that a theory is radically false and yet yields successful predictions. Science makes better

[3] For a detailed analysis of this, see Psillos (2014, 129-131).

than chance predictions ("the scientist is less mistaken than a prophet who should predict at random") and this suggests that science "is not without value as a means for knowledge" (1902, 265).

I am not claiming that Poincaré was a scientific realist, though, as I noted earlier, he progressively became more convinced of the reality of unobservable entities such as atoms (see Psillos 2011; also Poincaré 1913, 90). But I am claiming that he rightly took it that a successful defence of science as an enterprise that delivers at least some knowledge of the unobservable world requires two kinds of argument: an historical argument that blocks Tolstoy's argument, and a conceptual one linking the success of theories especially in yielding novel predictions to their having in some sense or other latched onto the world.

IX.

As noted already, Poincaré, at least initially, thought that this kind of conceptual argument warrants only the conclusion that empirically successful theories have latched onto the relational structure of the world. We saw Cornu making the point (that I too defended extensively in Psillos 1999, chapter 7) that more is actually warranted. Boltzmann too made this point in a rather elegant way.

Unlike Poincaré, Boltzmann didn't live long enough to see the triumph of atomism after Perrin. When he was defending atomism, he was fully aware of the anomalies that the atomic hypothesis faced (e.g., the specific heats anomaly) and he was also aware that molecules must have had an internal structure. So he knew very well that the atomic hypothesis was not "the final word." Yet, as he stressed in "The Relations of Applied Mathematics" (1906, 599), the atomic conception of matter has given "a better explanation of the previously known facts, it inspired new experiments and permitted the prediction of unknown phenomena." Besides, it has provided a unified framework for the study of all forms of matter (cf. 1901a, 73, 76).

These might not be conclusive arguments for the reality of atoms. But they are certainly good and cogent arguments. Moreover, they suggest the right kind of defence of a realist view of science: in defending realism, we can and should go beyond the claim of invariance in theory change and offer some positive explanatory reasons for taking at least some current theories as not being radically false.

X.

Let's go back to where we started: Tolstoy's argument. Can it be turned into a positive argument for realism? Assume that there is no privilege, that is (though this certainly can and should be contested), that current science is no different than past science when it comes to methods and reliability. Assume, with Poincaré, that "science has already lived long enough for us to be able to find out by asking its history whether the edifices it builds stand the test of time, or whether they are only ephemeral constructions" (1902, 292). It is certainly the case, as both Poincaré *and* Boltzmann noted, that there is a non-trivial pattern of retention in theory change: elements of past theories have been retained in subsequent theories and are part of the current scientific image. We may conclude then "by analogy," using Tolstoy's words, that "among the kinds of knowledge occupying the attention of our learned men [and women] and called science, there must necessarily be some which will be regarded by our descendants much as we now regard" atomism, Newton's law of gravity, Maxwell's equations, Dalton's laws, and many other components of past theories that are still with us as part and parcel of the scientific image. This is an optimistic lesson coming from the history of theory change in science. It might be modest, but it is robust enough to be realist.

XI.

I will close with two questions that Boltzmann raised:

> Is it possible that the conviction will ever arise that certain representations are per se exempt from displacement by simpler and more comprehensive ones, that they are "true"? Or is that perhaps the best conception of the future, to imagine something of which one has absolutely no conception? (1902, 256)

His reply was typically modest: "These are, indeed, interesting questions. One regrets almost that one must pass away before their decision. O arrogant mortal! Thy destiny is to exult in the contemplation of the surging conflict!"

Boltzmann took his own life on September 6, 1906. A few years later the "surging conflict" in which he took sides in favour of atomism led to the vindication of atomism. He did not live to see it. Still, his questions remain very interesting.

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