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Mechanical materialism and modern physics[1]

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[5] We will soon celebrate the fifth anniversary of the discussion with the mechanists about dialectics. Cde. Stepanov's[2] book, *Dialectical Materialism and the Deborinite School*, is truly a milestone, if not in content, then in form. It would be impossible to carry on the debate in the spirit of Cde. Stepanov's book, in no small part because it exhausts every form of abuse acceptable to print. Whether or not a point of view is correct is not determined by the "strength" of the words employed in its defense, but by a methodological analysis of the content of a scientific problem within the historical development of science. Unfortunately, Cde. Stepanov's latest article contains even less concrete material than his previous ones and, as we try to show, is not at all up to the standards of the modern science that Cde. Stepanov so earnestly defends. We believe that the main drawback of the book is that it carefully avoids every hotly debated problem of modern natural science, zealously defending against whatever attack by whomever against the law of energy conservation, which has long been accepted as ironclad in natural science, and which no one disputes.

Cde. Stepanov replaces the discussion of new problems with curses and angry shouts. But swearing is a bad means of solving problems, and indirect proof that one is aware of the incorrectness of one's own position: "Jupiter, you are angry, therefore you are wrong" (Dostoevsky 1900, 910 [TN]).

We will not indulge Cde. Stepanov in his controversial methods, but will try to identify our point of view on the analysis of specific material in opposition to the mechanists' point of view.

Therefore, let us turn to an analysis of classical and modern natural science.

From the outset, we will limit our task to the analysis of problems in physics.

[6] §1. The mechanical and the "mechanistic" worldview

According to Cde. Stepanov's views, mechanistic materialism is a specific form of dialectical materialism in natural science. Dialecticians deliberately confuse pre-chemical, mechanical materialism with the "mechanistic" materialism of the end of the nineteenth and the beginning of the twentieth centuries, which is, essentially, dialectical materialism (Skvortsov-Stepanov 1928c, 80–121).

Is this actually the case? Is it possible to view modern natural science as "mechanistic"? What is the difference between the mechanical worldview of the eighteenth century and the mechanistic (in Stepanov's terminology) natural science of the end of the nineteenth? In order to resolve the question of whether there really is a significant difference between the mechanical and the

[1]The following is a translation of Gessen 1928, 5–47. Hessen's original references have been replaced by English translations where possible. References to texts not cited by Hessen have been provided by the translator. This translation was completed with the aid of translation software [Translator's Note; hereafter, "TN"].

[2]Ivan I. Skvortsov-Stepanov (1870–1892) was one of the earliest participants in the Russian revolutionary movement, and a member of the Bolshevik wing of the Russian Social-Democratic Labor Party starting in 1904. Working as an editor and writer, he went on to hold several important positions in the Communist Party and the Soviet government. In the "mechanist" and "Deborinite/dialectician" debate of the 1920s, Skvortsov-Stepanov was an ardent defender of the mechanist persuasion (Isaacs 1977, 509, n403; Sheehan 1985, 175, 177) [TN].

mechanistic view of nature and what the methodological essence of the mechanical worldview is, let us turn to the writings of those people whose works established the natural science of the nineteenth century (therefore, not the pre-chemical). What is the essence of the mechanical worldview for Helmholtz, Maxwell, Boltzmann and du Bois-Reymond? How did they determine the purpose and tasks of the mechanical worldview? Unfortunately, Cde. Stepanov, who defends modern natural science - at least, in his articles - does not appear to have sufficient familiarity with the actual views of the founders of the mechanical worldview in natural science. Let us try to understand the founders of nineteenth-century science's views. True, this is somewhat more difficult than reading an article in the encyclopedia. Cde. Stepanov must agree with us that although encyclopedia articles save a great amount of time and do not require much knowledge to understand them, they are insufficient for a scientific solution to a problem. Let us turn, therefore, to the original works of these natural scientists.

The mechanical worldview arises as a reaction against the Scholastic physics of hidden qualities.

Scholastic physics referred to those properties of things that cannot be perceived and studied, but which are causes of observable phenomena, as hidden qualities. A magnet attracts iron, because it possesses the magnetic force of attraction; this force is the hidden quality, *qualitas occulta*. To the question of why opium lulls, Molière's doctor answers: "because of its soporific virtues, the tendency of which is to lull the senses to sleep" (Molière 2003 [TN]).[3]

[7] This was the "scientific" explanation of phenomena according to Scholasticism. Clearly, such a methodology could not be an instrument of scientific research and Descartes strongly opposed it.

"I freely acknowledge," he states:

> that I recognize no matter in corporeal things apart from that which the geometers call quantity, and take as the object of their demonstrations, i.e. that to which every kind of division, shape and motion is applicable. Moreover, my consideration of such matter involves *absolutely nothing* apart from these divisions, shapes and motions; and even with regard to these, I will admit as true only what has been deduced from indubitable common notions so evidently that it is fit to be considered as a mathematical demonstration. And since all natural phenomena can be explained in this way, as will become clear in what follows, I do not think that any other principles are either admissible or desirable in physics. (Descartes 1985, 247)

The entire program of the mechanical worldview is formulated in these words: all natural phenomena must be explained and reduced to the motion and arrangement of elementary particles.

"The greatness of Descartes's plan," E. Whitaker[4] says in his famous work, *A History of the Theories of Æther and Electricity*, "and the courage with which he executed it stimulated scientific thought in an unrivaled way. From the wreckage of his system, later scientists created the most stable theories that have retained their significance to the present day" (Whittaker 1910, 3).

The mechanical view, as formulated by Descartes, becomes the guiding light for the natural science of the seventeenth and eighteenth centuries, the philosophical credo of natural scientists.

"True philosophy," Huygens says in his *Treatise on Light*, "conceives the causes of all natural effects in terms of mechanical motions. This, in my opinion, we must necessarily do, or else renounce all hopes of ever comprehending anything in Physics" (Huygens 1912, 3).

The primary task of mechanical physics was to eliminate the metaphysical view of nature as the specific set of manifestations of specific, hidden qualities. All natural phenomena must be explained from one principle: the motion and arrangement of elementary particles.

[3]The original Latin quote reads as follows: "*Quia est in eo Vertus dormitiva*, *Cujus eat natura Sensus assoupire*" [TN].

[4]Sir Edmund T. Whittaker (1873–1956) was a British mathematician, physicist and historian of science. A professor of astronomy at Trinity College, Dublin and professor of mathematics at the University of Edinburgh, Whittaker is perhaps best-known for his two-volume masterwork, *A History of the Theories of Aether and Electricity* (Temple 1956, 298–325) [TN].

While leading a merciless struggle against the Scholastic physics of hidden qualities, Descartes did not stop short of reducing mass to extension. For him, any other concept of mass would be a hidden quality.

Cartesians waged a bitter struggle against Newton precisely because to the formula of the mechanical worldview - an explanation of all phenomena from the motion and arrangement of particles - he also added forces acting between elementary particles.

[8] For Descartes, the concept of a force acting according to a certain law (the force of gravity) that was not reducible to the motion and arrangement of elementary particles was a hidden quality. In the second part of the "*Principia,*" he attempts to explain the forces of adhesion and attraction by the motion and arrangement of particles.

Despite the fact that gravitational forces did not lend themselves - and still do not to this day - to a mechanical explanation, the purely phenomenological concept of force introduced by Newton turned out to be so fruitful that it was quickly and decisively included in the inventory of exact, natural science. The formula of the mechanical worldview thus became supplemented by the concept of force, and the aim of physics became to explain all natural phenomena from the motion and arrangement of elementary particles and the forces acting between them.

This is how the eighteenth-century Newtonian, Laplace, who extended the law of gravity to the microcosm and provided a mechanical theory of capillary phenomena, formulates the objectives of the mechanical worldview. The greatest achievements of nineteenth-century physics are associated with the triumphant march of the mechanical worldview (i.e. Fresnel's mechanical, wave theory of light), culminating in the kinetic theory of gases.

But at the same time, its weaknesses were becoming more pronounced.

Above, we saw how Descartes defined the objective and the essence of the mechanical worldview. This definition is completely unrelated to the particular state of mechanics at the moment. It is not a matter of by what laws elementary particles move, of whether their motion can be expressed by Newton's equations or Lagrange's equations. Descartes's mechanical worldview is a general, methodological premise: all properties of matter should be explained *only* by the motion and arrangement of the elementary particles of matter. For him, the problem of explaining the world is a purely kinematic problem.

Forces acting between particles are already a compromise, as long as they are not explained by the same principle.

Thus, the development of chemistry and biology could not affect the basic formulation of the mechanical worldview. Of course, along with gravitational forces acting between particles, it was possible to introduce electrical forces, chemical affinities, etc., but until these new properties of the elementary particle (i.e. the molecule, atom, electron) were explained as the motion and arrangement of elementary particles, the criterion of the mechanical worldview was not yet fulfilled.

But what are these elementary particles? Which of their properties are really elementary and not subject to further reduction?

Descartes answered this question by saying that the only property of elementary particles was extension: volume.

> [9] The nature of bodies does not consist in hardness, a sensation that we sometimes experience of them. It also does not consist in their weight, warmth and other similar qualities; for if we analyze a body, we can always imagine that it does not have any of these qualities in it and at the same time, we clearly and distinctly recognize that it still has everything that makes it a body, if it has only length, width and depth. It follows that to be, it does not have any need for them, and that its nature consists only in the fact that it is a substance with extension. (Descartes 1985, 224 [TN])[5]

[5]This is Hessen's paraphrase of Descartes' text [TN].

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This is how Descartes formulates his views on the nature of elementary particles.

Of course, Cde. Stepanov would object that these are the views of pre-chemical, mechanical materialism, and that the situation is completely different with "mechanistic" materialism, the materialism of the second half of the nineteenth century.

Let us see. It is unlikely that anyone will deny that du Bois-Reymond, one of the most brilliant representatives of the mechanical view in natural science, is not a representative of "pre-chemical materialism."

Here is how he expounds his views on the essence of the mechanical worldview:

"For us, there is no other way of knowing than the mechanical [*mechanische*], and therefore, the only scientific form of thinking is physico-mathematical. Theoretical natural science only stops when it reduces [*zurückführt*] all phenomena to the movements of elementary particles, which occur according to the same laws as in raw, sensible matter [*gröberen sinnfälligen Materie*]" (du Bois-Reymond 1886b, 434).[6]

We see that there is no fundamental difference between du Bois-Reymond's formulation compared to Descartes's formulation; nor could there be, since Descartes provided a general, methodological foundation. And, at the end of the nineteenth century, the *fundamental* question of the mechanical worldview was the same as it was in the middle of the seventeenth century.

It is important to emphasize the following two features of du Bois-Reymond's definition:

1. He does not mention "chemical forces and processes" at all and only speaks of the motion of elementary particles.
2. He argues that the laws of the microcosmos and the macrocosmos are identical. This statement is one of the basic tenets of mechanical natural science, the guiding principle of the exact physics of the nineteenth century.

Of course, du Bois-Reymond does not accidentally omit chemical forces from his formulation of the mechanical worldview. He understands [10] that according to the mechanical view, elementary particles should not have any properties other than the ability to move in space. Therefore, when defining the properties of these elementary particles, du-Bois Raymond refers to them as *quality-less*.

"Dark and silent [in itself]," he says, "i.e. *quality-less*, looks the world, when perceived through objective observation according to a mechanical view, which, instead of sound and light, knows only the vibrations of a quality-less primordial substance" (du Bois-Reymond 1886a, 109–110).[7]

Indeed, it is rather strange that du Bois-Reymond speaks about quality-less particles in "Deborinite" language, but we will leave it to the Timiryazev Research Institute[8] to solve this problem.

So, there is nothing fundamentally new in du Bois-Reymond's formulation compared with the pre-chemical period. The basic principle is, as before, the scale of mechanics; the explanation of all phenomena from mechanical motion and the arrangement of elementary particles.

Let us turn to the views of leading physicists of the nineteenth century.

In his "Outline of Modern Molecular Science, and in Particular of the Molecular Theory of Gases", Maxwell, the founder of the mechanical theory of heat, the kinetic theory of gases and

[6]Note that the mechanical worldview did not prevent du Bois-Reymond from being an agnostic and from slipping into idealism sometimes.

[7]This is Hessen's paraphrase of du Bois-Reymond's text [TN].

[8]The Timiryazev Research Institute, also known as the State Timiryazev Scientific Research Institute for the Study and Propaganda of Natural Science from the Point of View of Dialectical Materialism, was a center for scientific research in the Soviet Union, named after the distinguished Russian biologist, Kliment A. Timiryazev (see n57). The Institute's members were committed to Marxism to only varying degrees, and emphasized the natural sciences over philosophy (Joravsky 2009, 125; Sheehan 1985, 156) [TN].

the theory of the electromagnetic field, begins with the following definition of the problems of molecular physics:

> We begin by assuming that bodies are made up of parts, each of which is capable of motion, and that these parts act on each other in a manner consistent with the principle of the conservation of energy . . . . We may also assume that these small parts are in motion. This is the most general assumption we can make, for it includes, as a particular case, the theory that the small parts are at rest . . . . We make no assumption with respect to the nature of the small parts . . . . We do not even assume them to have extension and figure. Each of them must be measured by its mass, and any two of them must, like visible bodies, have the power of acting on one another when they come near enough to do so. *The properties of the body, or medium, are determined by the configuration and motion of its small parts*. (Maxwell 2003, 451 [Hessen's italics; TN])

H. Helmholtz provides the exact same formulation in his study on the conservation of force. "To matter as such, we cannot ascribe qualitative differences, for when we speak of different kinds of matter we refer to differences of action, that is in forces. Matter in itself can therefore partake of one change only, a spatial change, that is, motion" (von Helmholtz 1853, 115–116 [TN]).

One of the main difficulties of the mechanical worldview - i.e. the problem of the properties of an elementary particle - is most clearly revealed [11] in Maxwell's definition. Effectively, no properties can be attributed to this elementary particle, as all properties of bodies must be explained by the motion and arrangements of these particles. Descartes identified the mass of the particle with extension, i.e. kinematics, as the basis for explaining all properties of bodies. His particles are characterized only by volume. Maxwell could not follow Descartes in this regard. For him, particles need to be measured by their mass, but he attributes to them *neither extension nor form*.

In his speech about Gustav Magnus, H. Helmholtz even more clearly articulates the difficulties surrounding the question of ultimate, elementary particles:

> Concerning atoms in theoretical physics, Sir W. Thomson very correctly notes that accepting them cannot explain any property of bodies if it was not previously attributed to the atoms themselves. By subscribing to this statement, I have no intention at all of speaking out against the existence of atoms - I only oppose the desire to deduce the foundation of theoretical physics from the purely hypothetical assumptions about the structure of the atoms of bodies. From experience, we directly obtain only extended bodies that are diverse in shape and composition. Their actions are themselves made up of actions, all of whose parts add to the entirety of the whole. Therefore, if we want to study the simplest and most general laws of the actions of the masses and substances on each other in nature . . . we must deal with the laws of action of the smallest extended parts. These particles are not discontinuous and dissimilar, like atoms, but continuous and homogeneous. (von Helmholtz 1871, 12 [TN])

Helmholtz clearly sees the difficulties of atomic theory. For him, atoms are identical, tiny particles, from which all properties of bodies must be explained by motion and arrangement. He opposed all hypotheses about the structure of elementary particles (atoms). For him, they are just parts of volume. He does not speak at all about reducing all phenomena to the physico-chemical, since attributing physico-chemical properties to the atom would mean assigning to it in advance those properties that are to be explained.

For him, as for Maxwell, W. Thomson and du Bois-Reymond, the only scale of explanation is the mechanical scale.

"The ultimate aim of physical science," he says in his speech "The Aim and Progress of Physical Science," "must be to determine the movements which are the real causes of all other changes and

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discover the motive powers on which they depend; in other words, reduction to mechanics" (von Helmholtz 1885, 375).

Helmholtz made this speech in 1869.

It is possible that Cde. Stepanov would call this the pre-chemical period as well.

Let us now turn to William Thomson's statements from 1898. William Thomson, one of the greatest physicists of the nineteenth Century, [12] the founder of the vortex-atom theory, devoted his whole life to trying to establish a mechanical theory of matter. When the most ambitious of all his endeavors, i.e. the vortex-atom theory, failed, he wrote:

"I am afraid that it is not possible to explain all the properties of matter by the vortex-atom theory alone, that is to say, merely by motion (i.e. mechanical) of an incompressible fluid; and I have not found it helpful in respect to crystalline configurations, or electrical, chemical or gravitational forces .... We may expect the time will come when we shall understand the nature of the atom. With great regret I abandon the idea that a mere configuration and motion suffices" (Thompson 1910, 1047n1).

Thus, even at the turn of the twentieth century, the guiding idea for natural scientists who adopted the mechanical worldview was to explain all the properties of bodies (i.e. chemical, electrical and gravitational properties) from the mechanical motion and arrangement of discrete particles or fluids.

The above quotations are enough to draw some conclusions.

Cde. Stepanov's distinction between mechanical and mechanistic views does not hold water. There was and still is a single, mechanical worldview, which, as we have seen, prevailed in the natural sciences of the nineteenth Century.[9]

When Cde. Stepanov accuses Cde. Stan of "stubbornly attributing to him the mechanical point of view, i.e., always descending from chemistry and physics to mechanics," then Cde. Stan has an excuse that his understanding of the essence of the mechanical worldview coincides with the views of Helmholtz, Maxwell, W. Thomson and du Bois-Reymond (Skvortsov-Stepanov 1928c, 97).

There is no difference between a mechanical and a "mechanistic" worldview, but there is a difference between a mechanical worldview and dialectical materialism. Thus, all of Engels's reproaches to mechanical materialism remain valid for all of the natural science of the nineteenth century.

We have seen what Engels reproaches the old materialists for: the application of the scale of mechanics to chemical and biological phenomena, which is characteristic of all of the natural scientists of the second half of the nineteenth century, who uphold the point of view of mechanical materialism.

Of course, this is not accidental. Du Bois-Reymond, Helmholtz, Maxwell, W. Thomson do not speak about physico-chemical phenomena in their formulas, not because they did not know chemistry, but because, [13] if one wants to be consistent, there was no other way of posing the question than *to reduce all phenomena to mechanics*.

If we recognize the principle of reduction as the primary, guiding principle of scientific research, then one cannot stop at physico-chemical phenomena; if we recognize that physico-chemical phenomena are elementary and that everything can be reduced to them, but that they themselves cannot be reduced to mechanics, then this identifies a certain realm of phenomena as irreducible, fundamentally dispensing with the principle of reduction. It is on this inconsistency that Cde. Stepanov's distinction between mechanical and "mechanistic" rests.

Therefore, all of the dialecticians' criticisms of mechanical science remain valid.

Of course, we criticize mechanical materialism not because it is materialistic, but because in setting the general, methodological task of reducing all phenomena to mechanics, mechanical materialism closes the possibility of studying those natural processes that do not fit into the

[9]Statements quoted above range from 1847 (the date of the appearance of Helmholtz's work on the conservation of forces) to 1898, William Thomson's quotes.

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mechanical framework. According to Lenin, it was impossible "*to develop the theory* [of materialism]" following this direction (Lenin 1977, 240 [TN]).[10] The development of exact natural sciences and the biological sciences urgently required the development of materialist theory. The problems that had arisen in science could not be solved within the framework of mechanical materialism. This led to the collapse of the mechanical worldview, which was already emerging in the 1880s. In W. Thomson's statement above, we saw the rejection of constructing a mechanical theory of matter (the vortex-atom). Attempts to construct mechanical theories of the electromagnetic field proved to be just as futile.

The last, most ambitious attempt to reduce all natural phenomena to mechanical motion was Hertz's mechanics. "The search of the mechanistic conception," M. Planck says, "for a unified world picture has been brought in it to a somewhat ideal completion. Hertz's mechanics does not really represent physics of today, but physics of the future, a sort of confession of faith for physics" (Planck 1960a, 31).

This "confession of faith" consists in the fact that G. Hertz considers it possible "to fully implement the mechanical point of view, on the basis of the assumption of moving, simple, uniform particles, the only true building blocks of the universe" (Ibid., 31–32).[11]

As is known, Hertz's attempt also ended in failure and had no influence on the further development of natural science.

Well, Cde. Stepanov will object, it is possible that all of these failures stem from the fact that they did not want to come to terms with the achievements [14] of chemistry and biology, they did not want to move beyond the mechanical picture to take on a mechanistic point of view. But then, he must explain why all natural scientists, starting with Helmholtz and du Bois-Reymond and ending with Hertz, speak of reducing all phenomena to the mechanical movements of simple, homogeneous points. After all, all of them were not only fully equipped with the knowledge of the second half and the end of the nineteenth century, but they themselves were pioneers of scientific progress.

The point, of course, is that there can be no other question: either a consistent mechanism - then everything should be reduced to the movements of homogeneous, material points - or an inconsistent mechanism, which, according to W. Thomson, attributes to the atom those material properties (i.e. chemical, electrical) that have to be explained.

The collapse of mechanical theories led to the revival of idealistic trends in physics. Instead of looking for a way out of the crisis through further developing materialist theory, many philosophers and philosophizing natural scientists began to look for a way out by "overcoming the materialism of natural science."

Cde. Stepanov wants to portray the matter in such a way that if we criticize mechanical materialism, we thereby identify with the trend of attributing the shortcomings of mechanical materialism to its materialism. We acknowledge the great achievements of the mechanical worldview of the nineteenth Century. We are well aware that at that time, being mechanical was equivalent to being materialistic for a natural scientist. However, it does not follow from this at all that it is currently impossible to be a materialist in natural science if one does not uphold the point of view of mechanical materialism. This is possible if we ascend to the point of view of dialectical materialism. "We can still look with a sense of … deep reverence upon the fundamental elements of the physical world," we say in R. Millikan's words, "as they have been partially revealed to us in this century. The childish mechanical conceptions of the nineteenth century are now grotesquely inadequate" (Millikan 1927, 27).

[10]While Hessen attributes the quote to Lenin, Lenin actually borrows this line from Engels. See also Engels 2010b, 371 [TN].

[11]The reader should pay attention to the fact that both M. Planck and Hertz speak here about simple, homogeneous material points, in the language of "Deborinites."

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The development of physics, chemistry and biology truly forced natural scientists to reconsider mechanical methodology, but in a completely different direction from the way it is depicted by Cde. Stepanov.

For Cde. Stepanov, the primary achievement of the "mechanistic" worldview was the extension of the law of energy conservation to all natural phenomena, including psychic ones. He even identifies the "mechanistic" worldview with energy conservation (Skvortsov-Stepanov 1928b, 96).[12]

Undoubtedly, the law of energy conservation was a powerful tool for expelling all mysterious forces from natural science. However, it is completely wrong to identify [15] it with the mechanistic view. Cde. Stepanov's train of thought is as follows: dialecticians reject the "mechanistic" worldview, which is nothing other than the law of energy conservation. Consequently, they reject the law of energy conservation and, thereby, open the door to all kinds of mysterious forces, including the vital force, which have forever been expelled from natural science.

First of all, a historical point of fact: the law of energy conservation was clearly formulated by Robert Mayer[13] in his study "Organic Motion in Its Connection with Metabolism" in 1845 and generalized to all phenomena by Helmholtz in 1847 (von Mayer 1845 [TN]).

At the time, the mechanical theory of heat did not yet exist. The first works of Krönig and Clausius date back to 1856. There were still no attempts to develop a mechanical theory of electromagnetic phenomena.

Historically, the law of energy conservation was scientifically grounded before the mechanical worldview became widespread in molecular physics. It is simply incorrect to consider the law of energy conservation as identical to the mechanical worldview, as Cde. Stepanov does. This contradicts the historical development of natural science. As we saw above, the mechanical worldview based on Newtonian mechanics was scientifically formulated by Laplace at the end of the eighteenth Century, at a time when the law of energy conservation was unknown.

Undoubtedly, the general premise of the mechanistic worldview played a large role in the history of the law of energy conservation, and it is not accidental that Descartes formulated the law of energy conservation simultaneously with his mechanical picture of the world. But this does not justify putting an equal sign between them, as Cde. Stepanov does.

The mechanical view has developed and cultivated the scientific formulation of the law of energy conservation, and the law of energy conservation remains the primary, guiding principle of the study of nature, albeit the mechanical (or mechanistic) view is not shared by the modern physicist.

The law of energy conservation (and transformation) played a tremendously revolutionary role in natural science, uniting all types of energy previously separated by an impassable chasm. It played a powerful role in the destruction of the metaphysical view of nature, establishing a connection between all phenomena of organic and inorganic natures.[14]

However, for all its value, the law of energy conservation is insufficient. Cde. Stepanov forgets that the foundations of modern natural science [16] include not only the law of energy conservation, but also the law of energy dissipation.

If the law of energy conservation emphasizes the unity of all phenomena, the law of energy dissipation establishes their specificity and introduces a fundamentally new point of view into science. It is no accident, we think, that Cde. Stepanov dismisses the law of energy dissipation. Establishing, absolutely correctly, the tremendous significance of the law of energy conservation for the dialectical view of nature, Cde. Stepanov forgets that the dialectical point of view is meant

[12]"Under the title of mechanistic understanding of nature, I am defending here the law of conservation (and transformation) of energy." (ibid, 96)

[13]Julius R. von Mayer (1814–1878) was a German chemist, physician and physicist. He is best known for being one of several co-discoverers of the law of energy conservation (Caneva 1993, xv) [TN].

[14]As a general, philosophical premise, the law of energy conservation was formulated by Descartes as the law of the conservation of motion.

to establish both *unity* and *specificity*. Therefore, he ignores the law of energy dissipation, which establishes the specific difference between purely mechanical systems and systems that, although obeying mechanical laws in their constituent parts, possess a specific distinction, i.e., specific laws.

Below, we will try to show in detail that even within the *framework of physics*, the mechanical view is not sufficient. Even the mechanical theory of heat, i.e. the kinetic theory of gases, cannot be reduced to mechanics. But first, a few of words about specificity. Cde. Stepanov asserts that "to say 'specificity', to declare its irreducibility to physico-chemical processes and to stop there is equivalent to saying that we must recognize some part or region of the phenomena of life as unknowable" (Skvortsov-Stepanov 1928a, 12). First of all, speaking of specificity, it would be nice if Cde. Stepanov could point out where the dialecticians proposed stopping there. No one, anywhere, has ever said or written such a thing, we insist. However, we have always emphasized the problem of specificity along with that of the unity of phenomena, because after establishing the unity of phenomena, at minimum through the law of energy conservation, the question of specificity inevitably arose before science. It is not enough to say that light, heat and electricity are different types of energy; along with their unity, it was necessary to establish their specific differences. It is not enough to say that heat is the mechanical motion of molecules. It was not enough to show that the volume of a gas, composed of a large number of molecules, represents a mechanical system. It was also necessary to indicate the specific differences between this system and a simple set of individual molecules moving according to the laws of mechanics. And, these specific differences, which are irreducible to the laws of mechanics and are not exhausted by them, do exist, Cde. Stepanov, and they are precisely what Engels had in mind when he wrote: "Heat is a molecular motion ... [b]ut if I have nothing more to say of heat than that it is a certain displacement of molecules, I should best be silent" (Engels 2010a, 531).

Cde. Stepanov believes that recognizing specificity alongside unity amounts to being a reactionary in science (Skvortsov-Stepanov 1928c, 103). But Marx, when criticizing bourgeois economists, stresses that "determinations which apply [17] to production in general must rather be set apart in order not to allow the unity ... to obscure the essential difference. On failure to perceive this difference rests ... the entire wisdom of modern economists" (Marx 2010, 23).

We will learn Marxist methodology from Marx and not from articles in the *Soviet Encyclopedia*, even if they are written by eminent natural scientists.

§2. On the specificity and irreducibility of physical phenomena to mechanics

To analyze the question of the specificity and irreducibility of phenomena, let us consider more closely the law of energy dissipation and its kinetic interpretation. This will allow us the opportunity to clarify the meanings of our formulations with specific examples. Although we will always deal only with laws of physics, the principal content of our formulations will appear sufficiently clear already in the field of physics.

The volume of a gas consists of an enormous number of molecules in continuous motion. Therefore, a gas can be considered a system of molecules that move according to the laws of mechanics. Let us take some law related to a gas as a whole, i.e. Boyle's law, according to which the pressure of a gas on the walls of a vessel is inversely proportional to its volume. Clearly, this law only makes sense in relation to the volume of a gas as a whole, because in relation to one, isolated molecule, the concept of a change of pressure with a change of volume makes no sense, or at least has a completely different meaning, because in our assumption about the molecular structure of a gas, we assumed that the volume of an *individual* molecule does not depend on the pressure of the entire gas.

The motion of an *individual* molecule is a purely mechanical process, fully articulated by mechanical equations.

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What does it mean to say that the pressure of a gas and Boyle's Law can be reduced to the mechanical movements of molecules?

It means that having established the purely mechanical laws of motion of individual molecules, from these assumptions, and *from these alone*, we can derive the laws of a gas as a whole. The laws of a gas as a whole (in our example, Boyle's Law) are the simple, arithmetic sum of the mechanical laws by which individual molecules move. There are no specific patterns in a gas as a whole compared to the patterns of the elements of its components.

If all this takes place, we say that the pressure of a gas on the walls of a vessel is reduced to the mechanical collisions of molecules that continuously bombard [18] the walls of the vessel, and Boyle's law is reduced to the mechanical laws of the motion of molecules.

Is it possible to say that such a reduction of the laws of thermodynamics to mechanics is possible in the kinetic theory of gases? Cde. Stepanov argues that in principle, such a reduction does take place. From Cde. Stepanov's point of view, if it is possible to reduce all biological phenomena to the physico-chemical, then, reducing purely physical phenomena to mechanical is even more so. Whoever claims otherwise is a scientific reactionary and a "physical vitalist."

Let us see how things actually are.

I throw a stone from a certain height. A stone raised to a certain height has a certain amount of (potential) energy. Upon falling to the ground, the stone produces a certain amount of heat, i.e. thermal energy; it makes a sound, i.e. vibrations in the air; a certain amount of mechanical energy, i.e. a visible deformation on the soil, etc.

The law of energy conservation requires that the total amount of energy produced in the fall of the stone, i.e., energy converted from the potential energy stored in the stone when it was raised to a certain height, is equal to the potential energy of the stone.

But, we see that, in addition to the equal amount of energy, we are dealing with a process that takes place in a *certain direction*. *First*, we have potential energy, which is *then* converted into a number of different energies. The process proceeded in the direction, so to speak, of potential, mechanical energy to thermal and other types of energy. The stone was initially raised to a certain height, and by falling, produced other forms of energy. Is this the necessary direction of the process? Having fallen, the stone produced a certain amount of heat that warmed the earth and the surrounding air. Could the stone rise again to its previous height by using *this heat*[15] and the correlative cooling of the air and earth?

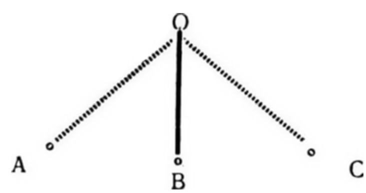
In other words, can the energy conversion process proceed in the opposite direction by itself?

By itself, we mean without outside interference, just as the transformation of potential energy took place by itself without the interference of outside forces.

The law of energy conservation does not provide us with any answer on this matter, but experience provides us with a negative answer. Never, nowhere in nature have we observed such a reverse transformation. The process proceeds in a strictly defined direction and cannot be reversed.

From the point of view of the *direction of processes*, we can divide all natural processes into *reversible* and *irreversible*.

[19] The most simple and common example of a reversible process is the swing of a pendulum.



[15]In view of their negligible magnitude compared with thermal energy, we do not take other types of energy into account.

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If we take the pendulum out of equilibrium OB to point A and then let it go, it will oscillate continuously between points A and C. Its potential energy will be converted into kinetic energy and vice versa. If we ignore the friction between the pendulum and the air and the thread upon which it is suspended, these oscillations, which correlate with the transformation from potential to kinetic energy, will occur indefinitely long, in one direction and then the other.

The process is completely reversible.

Generally, whatever mechanical process we consider, it will always be reversible.

If we take *thermal processes*, however, this is not quite the case. Place a heated, metal ball into a glass of cold water. Heat[16] from the ball will transfer to the water. The water will heat up, the ball will cool down. The amount of thermal energy lost by the ball will be exactly equal to the amount of energy acquired by the water.

However, the process of the transfer of heat from the ball to the water will come to an end. Heat from the warmer body will pass to a less heated one. At the end of the process, the temperature of the ball and of the water will be the same; thermal equilibrium will be established and the process will not proceed in the opposite direction.

The pendulum, after it began to oscillate and after having passed through the equilibrium position OB, continued on. It *descended* from point A to B and, having passed through the equilibrium point B, it began to *rise*. The thermal process did not move beyond the point of equilibrium. Its course was strictly one-sided. As in the stone example, the process can only proceed in one direction: always and everywhere, we observe the transition from a warmer body to a cooler one, but we never observe the reverse transition. *Thermal processes are irreversible*.

Thus, reversible processes are separated from irreversible[17] ones as if by an impassable chasm.

The direction of reversible processes is indeterminate. They can proceed in any direction. The direction of irreversible processes is strictly defined.

[20] This distinction between reversible and irreversible processes establishes the second law of thermodynamics, complementing its first law, i.e. the law of energy conservation, by indicating the direction of a transformation. The content of the law can be formulated in different ways. We will provide the two, most common, formulations. First: "heat can never pass on its own from a colder to a warmer body" (Clausius 1867, 117). Clausius's formulation clearly indicates a specific direction to thermal processes. We discussed the essence of this formulation in the ball heating water example. Another formulation, by W. Thomson, points to the difference between mechanical and thermal processes. "It is impossible to construct a periodically operating heat engine, all of whose activity comes down to lifting weights and cooling a certain reservoir with stored heat."[18]

We discussed the essence of this formulation with the falling stone example. No device can lift the fallen stone by cooling its surrounding environment.

The impossibility of building a machine, the so-called *perpetuum mobile* of the second kind, of which W. Thomson speaks in his formulation of the second law, does not follow from the law of energy conservation. Indeed, if it were possible to lift the stone by cooling the environment, then we would have the conversion of thermal energy into the energy of the position (potential energy) of the stone. These would be equal quantities of energy, meaning the law of energy conservation would not be violated.

The statement that such a direction of the process is impossible is a *new statement*, a *new principle*.

[16]Here, we have no need to delve into the question of the essence of heat.

[17]An obvious typo: "reversible [*obratimykh*]" is used twice in this phrase, but one of these words must be "irreversible [*neobratimykh*]" here [TN].

[18]This formulation of the second law of thermodynamics is actually closer to what is often referred to as the "Kelvin-Planck" formulation. W. Thomson's actual formulation of the law reads as: "It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects" (Thomson 1853, 265). Planck's formulation of the law reads as: "It is impossible to construct an engine which will work in a cycle and produce continuous work, or kinetic energy, from nothing" (Planck 1969, 40). See Rao 1997, 158 [TN].

The course of the conversion of energy processes is subordinate not only to the purely quantitative law of energy conservation, but also to the law determining the *direction* of processes, the specific pattern of their flow.

Establishing the one-sidedness of thermodynamic processes leads to very important, general consequences. The heat emitted by the sun is the source of all earthly work processes. In the end, all physical processes become thermal processes. For example, our pendulum, left to its own devices, will swing until its mechanical energy passes into the surrounding air in the form of thermal energy due to its friction against the air.

Electrical current will heat the wires and the filaments of light bulbs, and will activate motors, whose mechanical energy will be transmitted to, e.g., a drilling machine. During the drilling process, mechanical energy will be converted into heat, which will heat both the drill and the object being worked upon. In all of these processes, the mechanical energy of the pendulum, the energy of electrical current in wires and in light bulbs, in the motor and through it, into the machine, will eventually become thermal energy. But, right when such a conversion has taken place, we [21] cannot reverse the process; we cannot force the pendulum to rise to point B again by cooling the air, just as we cannot again produce electrical current from heat passed into the environment by wires, bulbs and motors.

However, if all processes are ultimately converted into thermal energy and if thermal processes proceed one-sidedly as we have seen, then the primary tendency of energy conversion is the "equalization" of energy.

In fact, if we place a heated body into a medium cooler than the body, heat will be transferred from the body to the medium until the temperature balances out, until thermal equilibrium sets in.

The thermal energy of a heated body is at a higher level, so to speak, when it has a higher temperature. Energy moves from a higher level to a lower one. The thermal levels of a body and an environment level off. The total amount of energy remains the same, but the energy, so to speak, has lost value, since it loses the possibility for any further transformation. It comes to a balanced equilibrium, levels off or, as they say, dissipates.

Therefore, the second law is sometimes called the law of energy dissipation.

If not *quantitatively*, then energy is lost *qualitatively*. All thermal processes tend to their inevitable end: thermal equilibrium. This thermal equilibrium is like the death of the world. In order for thermal energy to be converted into other forms, a differential is necessary, but the *one-sidedness* of the course of thermal processes inevitably results in a leveling off that will not lead to the formation of new levels of differentials. The flow of energy stops. Energy degenerates, degrades. The prediction of the law of energy dissipation is the quiet death, the inevitable end of the world.

We see that by its consequences, the second law of thermodynamics goes much further than the first. By determining the direction of the course of processes, it thereby determines their end. But, if processes inevitably proceed to an end, then it is a mystery as to how these processes began.

If the flow of the process that has come to an end cannot resume by itself, then the beginning of the process of the emergence of the initial differential, which determines the further course of the process, is only conceivable as a creative act of a special force that is external to matter, since no natural force known to us can produce a differential of thermal levels once equilibrium has set in. Of course, I can heat the cooled ball again and lower it into the glass again, having now produced a differential in thermal levels, but I can only do this because the alignment of the glass and the ball's thermal levels is not yet the overall general alignment in nature, [22] which still retains higher thermal levels. However, if in the end, complete thermal equilibrium were to set in, no physical force could take the world out of this state.

These are the conclusions that necessarily follow from Clausius's formulation of the law of energy dissipation. It separates physical processes by an impassable chasm. If the law of energy conservation connects, binds all natural processes, and shows their unity, the law of energy dissipation seems to limit the connection of processes, establishing an insurmountable distinction between reversible and irreversible processes.

Clausius's purely phenomenological formulation of the law of energy dissipation is unsatisfactory. Engels pointed this out with extreme clarity and insight.

L. Boltzmann's merit lies in the fact that he revealed the essence of the specific difference between reversible processes and irreversible processes and showed how the difficulties encountered by Clausius's formulation, which we discussed above, are overcome. Let us first examine the essence of the specificity of irreversible processes compared to reversible, mechanical processes.

When the kinetic theory of gases attempts to provide a mechanical interpretation of the law of energy dissipation, it stumbles upon fundamental difficulty from the outset.

The thermal motion of molecules is a mechanical process. After all, heat is nothing other than the kinetic energy of flying molecules.

We said above, however, that all mechanical movements are *reversible. Why, then, do thermal processes, which are nothing other than the mechanical movements of billions of molecules from the point of view of the kinetic theory of matter, behave as irreversible processes*?

When L. Boltzmann formulated his kinetic interpretation of the law of energy dissipation, the so-called H-theorem,[19] he first of all met with the critique that from a gas, which is composed of molecules moving according to the laws of mechanics, representing a purely mechanical, and therefore reversible, model, we obtain irreversible processes, i.e. processes that are essentially different from mechanical laws. In a gas taken as a whole, fundamentally new laws arise, which differ radically from the laws of the motion of individual molecules.

Why, starting from a purely mechanical model of gas, do we come to irreversible processes, the exact opposite of the strictly reversible mechanical processes!

To answer this question, let us give the floor to A.K. Timiryazev:[20]

> Objections to Boltzmann's theorem boil down to the fact that the laws of mechanics, which depict strictly reversible processes, [23] cannot lead to the depiction of an irreversible, one-sided process, such as the transition from any distribution of velocities to the Maxwell distribution . . . . But, the fact of the matter is that in deriving the theorem, we use the theory of probability when we count the number of collisions of one type or another, therefore, the H-theorem should not be considered as a consequence of *mechanical equations alone*. (A. Timiryazev 1923, 88)

This is the crux of the issue.

In a gas as a whole, in the aggregate of molecules, new laws appear that are specifically different from the purely mechanical laws that govern the actions of a single molecule. These laws are not the result of the equations of mechanics alone.

Besides the laws of mechanics, what assumptions do we have to introduce in order to obtain a kinetic interpretation of the law of energy dissipation?

We imagine a gas as a combination of molecules. Molecules undergo a huge number of collisions every second. To derive the Boltzmann theorem, it is necessary to make a special assumption about the velocities of molecules *apart from* the assumption about the mechanical laws of molecular motion. It is clear that the velocities of all molecules are different. We assume, however, that molecules with different velocities are evenly distributed in space. In other words, if we set a certain velocity for a molecule, then molecules with that given velocity can be found in any point

[19]The "H-Theorem" refers to Boltzmann's postulate that "entropy must always increase or remain constant, and thus provides a microscopic interpretation of the second law of thermodynamics" (Boltzmann 2003, 263) [TN].

[20]Arkady K. Timiryazev (1880–1955) was the son of celebrated Russian biologist, Kliment A. Timiryazev (see n57) and a professor of physics at Moscow State University. Often mocked by his colleagues as "the monument's son," he adamantly opposed contemporary developments in the sciences, such as quantum mechanics and relativity theory, in favor of Newtonianism. In the "mechanist" and "Deborinite/dialectician" debate of the 1920s, he was a defender of the mechanist position. Timiryazev gained favor with Joseph Stalin due to his tendency to cite the latter in his writings on physics (Ings 2016, 246–247) [TN].

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in space with the same probability. Molecules possessing a given velocity do not accumulate in one place, but are evenly distributed throughout the entire volume of a gas.

This seemingly modest statement, known as the "hypothesis of molecular disorder," plays a fundamental role in the entire kinetic theory of gases. Without it, it is impossible to construct the kinetic theory of gases.

What is the nature of this hypothesis?

Why do we call it, and therefore also the Boltzmann theorem, non-mechanical?

First of all, it refers to the entire gas as a whole and expresses a specific pattern in the distribution of molecules within a given population. When applied to a single molecule, this pattern simply does not make sense. Furthermore, this pattern of the distribution of molecules within an aggregate is not contained in the mechanical law of the motion of a single molecule. It only arises from the *aggregate* of these mechanical movements. *The set of mechanical movements of individual molecules creates a specific regularity that relates to the entire set of molecules, a pattern of a non-mechanical nature.*

[24] This is what we mean by the specificity and irreducibility of phenomena and the laws observed in a gas as a whole versus the mechanical laws of its elements (molecules).

It is only due to the fact that in the aggregate of molecules, this specific law arises, which is fundamentally different from the elementary laws of motion of individual molecules, that we obtain a specific difference between reversible and irreversible processes. Later, we will see that the specificity of this law of the whole will enable us to establish not only the difference between reversible and irreversible processes, but also their unity.

Do mechanical laws that govern the motion of individual molecules continue to operate in a gas volume? Of course, they do. Do any *special*, *supernatural forces* arise in the aggregate of molecules that are extrinsic to a simple, mechanical system? *No, they do not*.

Is there a specific difference between the patterns observed in a gas as a whole and the patterns of its elements? *Undoubtedly*, there is. This specific pattern is molecular disorder.

Is this specificity of the law a consequence of some special forces, or is it based on the same mechanical laws of the motion of individual molecules?

The specificity of the laws of the whole is not the result of any special forces, but is the result of combining, into a single set (synthesis), a huge number of elementary laws that are *fundamentally different from it*.

The specificity of the laws of a gas as a whole, which, in the kinetic theory of gases, is designated as the "hypothesis of molecular disorder," is associated with the mechanical movements of individual molecules and is a consequence of these movements, but it represents something completely different from them and is not reducible to them.

"Scientific biology," Cde. Stepanov instructs us, "applies physico-chemical methods to the study of life processes, revealing in them the *same laws* that we observe in the field of inanimate nature" (Skvortsov-Stepanov 1928a, 61).

No, we object. This provision is inapplicable not only to biology, but even to physics. When applying mechanical methods in the study of the kinetic theory of gases, physics reveals patterns that are fundamentally different from a mechanical character.

Applying the laws of mechanics to the study of the kinetic theory of gases, we come to an explanation of irreversible phenomena through reversible (mechanical) ones because only in the mechanical model [25] of a gas that consists of a huge number of molecules do we discover certain laws that are not present in the mechanical equations of motion.

Well, the mechanist will object, we accept your specificity and even consider it possible to retract our reproach toward you for the introduction of special, supernatural forces. But, why do you refer to the *irreducibility* of these specific laws of the whole to the simple laws of its elementary parts? What is the meaning and justification of this statement?

After all, if I were given the exact equations for the motion of all the molecules of a given gas volume along with their arrangement at any given moment, this would be enough to deduce your

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hypothesis of molecular disorder with all its specificity. And, if I could find this specific pattern, proceeding from the complete knowledge of *only* the equations of the motion of molecules, it would mean that it *comes down* to these simple patterns.

True, I cannot do this presently; our knowledge is still inadequate. But, you raise the question in principle and emphasized many times that your statement is not connected with this particular state of science!

Let us open with you, the reader, a book written in 1795, entitled *A Philosophical Essay on Probabilities*. This book came from the pen of Laplace.

> An intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it - an intelligence sufficiently vast to submit these data to analysis - … would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes." (Marquis de Laplace 1902, 4)

The basic formulation of the problem of reduction for Laplace is exactly the same as that of the mechanists: if we knew exactly and completely all the laws of motion and the arrangement of elementary particles, then we could completely reduce all phenomena to the motion and arrangement of these particles and deduce all the laws of the universe and all its states at any given time.

The methodological content of the problem will not change at all even if we – following Cde. Stepanov - in addition to the motion and arrangement of particles, also introduce physico-chemical forces. In the previous chapter, we showed that, according to a genuinely "post-chemical," mechanical worldview, physico-chemical phenomena should be reduced to mechanical phenomena. But, even if you do not go to the end [26] and stop halfway, as Cde. Stepanov does, by recognizing the indivisibility and irreducibility of physico-chemical processes to mechanics, even then, we repeat, the mechanists' question is fundamentally indiscernible from the question posed by Laplace.

The only difference is that, being a consistent mechanist, Laplace understands forces and laws as mechanical forces and laws, while our mechanists mean physico-chemical forces.

So, let us even assume that physico-chemical phenomena cannot be reduced to mechanics but that all other phenomena are reduced to physico-chemical laws. Of course, this is inconsistent, since if, according to the mechanists, all phenomena are *fundamentally* reduced to the physico-chemical, then it is completely incomprehensible why physical phenomena, for example, do not reduce to simple mechanical ones.

What is the essence of such a formulation?

If the mechanists want to say with such a formulation that in nature as a whole, all the laws of nature are present in the foundation, that there are no other forces besides the forces that underlie the elementary processes of nature, that all the laws of living matter are fundamentally related only to the laws that prevail in inanimate nature, that there are no laws and forces that exist outside of nature and above it - in short, if by reduction we mean the genetic connection of higher forms with lowers ones in the process of development, then every materialist would agree. It would be nice if, instead of cursing us, the mechanists, including Cde. Stepanov, would have provided at least one line indicating that we recognize the existence of supernatural forces and laws that are not related to matter and that are not embedded in matter.

There is no doubt that this materialistic premise is contained in Laplace's formula. Laplace understood this well himself. It is by no accident that, to Napoleon's question as to why he did not leave room for God in his celestial mechanics, he replied: "I had no need of that hypothesis" (Rouse Ball 1908, 418 [TN]). But Laplace's formula contains more than a simple, materialistic premise. It contains a statement of a purely methodological nature, which consists in the fact that the direction of the progress of science consists in the decomposition of all phenomena into elementary phenomena and the reduction of all laws into the laws of parts. In this formulation of the question, the specificity of laws of the whole is a consequence of our ignorance.

Specificity is a subjective category and the task of science is to reduce the specific pattern of an aggregate to the simple laws of its elements. Specificity is a knot, and the task of science is, according to Cde. Stepanov, to untie it.

We do not object to the materialistic side of the reduction formula, but to its methodological approach.

[27] The correctness or incorrectness of a methodological approach is decided by scientific practice.

What does the development of science teach us, in particular the given example of the kinetic interpretation of the law of entropy? In what direction did science move: along the path of reduction or along the path of establishing, along with the patterns of the elements, the specific (i.e. irreducible) regularity of the whole?

Along the second path.

Of course, the molecular chaos hypothesis is embedded in the mechanical movements of individual molecules. It does not emerge as a *deus ex machina*. These mechanical processes are its condition. But, had science followed the path of deducing the laws of a gas as a whole from the movements of individual molecules, we certainly would not have had the kinetic theory of gases.

The image of the omniscient mind, drawn by Laplace, is nothing but an abstraction of the unending process of research.

This mind must possess exhaustive knowledge of all the laws of nature and complete knowledge of its structure (the arrangement of elementary parts). Both are inaccessible to real knowledge, since it is assumed that nature has become known to the last detail and is decomposed into its ultimate, indivisible, absolutely elementary particles and laws.

Perhaps the most obvious formulation of the abstraction of the metaphysical nature of the mechanical worldview and the "reduction" principle as the basis of scientific research came from L. Boltzmann:

> It was now attempted to prove *a priori* that every change, even if apparently qualitative, must be reducible to a motion of the smallest parts, motion being the only process in which the object moved remains always the same.[21] All such metaphysical reasons seem to me to be insufficient. Of course we cannot avoid forming the concept of motion. If, therefore, all apparently qualitative changes were representable by the picture of motions or changes of arrangement of smallest parts, this would lead to an especially simple explanation of nature. In that case nature would appear to us at its most comprehensible, but we cannot compel her to this, we must leave open a possibility that this will not do and that we need, in addition, other pictures of other changes; understandably, *it is precisely the more recent developments of physics that have made it prudent to allow for this possibility*. (Boltzmann 1974, 142–143)

Boltzmann rightly emphasizes the metaphysical nature of the mechanical conception. Indeed, in order to prioritize [28] the reduction of all natural phenomena to mechanical laws or to the motion of electrons, it is necessary once and for all to postulate as the first principle that the motion of matter is mechanical motion, by mechanical motion meaning not only the motion of discrete particles, but also motion in a continuous media. If we allow for the existence of electromagnetic phenomena that *cannot be reduced to any kind* of mechanical motion, then this is essentially a rejection of the mechanistic picture of the world; no matter what verbal trickery about the difference between mechanical and mechanistic is used to disguise this assumption about new,

[21]Boltzmann, of course, implies mechanical motion.

specific forms of the motion of matter that are qualitatively different from and not reducible to the mechanical.

Together with Boltzmann, we can now repeat that the need for such a postulate is not only unproven, but that the development of physics is taking a different path. We will return to this issue in the chapter on electron theory.[22]

"But still," the mechanist will object, "do you deny the need for an electronic interpretation of chemistry or not? Are attempts to use electron theory in chemistry not a step forward?"

Of course they are. Establishing the connection between different forms of the motion of matter always marks progress in science. Electron processes are one form of the motion of matter. Chemical laws are another form of the motion of matter. If by reduction we mean the establishment of a connection between various types of laws of motion of matter, then we do not object to such "reduction."

Cde. Stepanov, however, poses the problem differently. This is how he formulates the task of science:

> From the fact that the known combination of physical and chemical processes occurring in inanimate matter once led to a nodal line, to a leap, to life, to a new quality, it does not at all follow that some processes unknown to physics and chemistry unfold in the organism and that science should cast aside as *vanitas vanitatum* the hope of reducing the phenomena of life, with all their specificity and complexity, to those relatively simple phenomena that it observes in inanimate nature. (Skvortsov-Stepanov 1928a, 35–36)

First of all, what does it mean to reduce more complex (biological) phenomena to the "relatively simple"? Indeed, "relatively simple" in this formulation of the question is in a certain sense conventional. It may seem to us, for example, that simple, mechanical motion is the most simple and clear. Boltzmann rightly emphasizes (from our point of view) that the mechanical picture of the world is the simplest [29] explanation of nature. But, nature does not care whether it is easy or difficult for us to learn this or that pattern, whether our differential equations are simple or complex, whether we can or cannot integrate them. Therefore, objectively, we cannot say that the mechanical motion of a neutral, material point is simpler than the motion of an electron. Oliver Lodge[23] expressed this perfectly in his Kelvin speech on 19 April 1928, at the Institution of Electrical Engineers:[24]

> Modern physics aims at simplifying the complex of problems with the help of quanta and the theory of relativity, but it has raised difficulties where previously we detected none, and has made simple things complex.... In the nineteenth century everything was reduced to mechanics; now the very concept of mechanical motion of matter itself is in need of explanation (Lodge 1928a, 430; Lodge 1928b, 1005).

The main task of science is not simply to reduce the complex to the relatively simple, but to establish a connection between various forms of the motion of matter, to study the *essential* laws of the

[22]Hessen never published this section of the article [TN].

[23]Oliver Lodge (1851–1940) was a British physicist and longstanding principal of the University of Birmingham. He is best known for his contributions to the study of electromagnetism and radio waves, as well as his belief that psychic research could one day open the possibility of communicating with the spirits of the dead (Chant 1940, 435–436) [TN].

[24]The Institution of Electrical Engineers was a British professional society for engineers that existed from 1871–2006, at which time it was absorbed into the Institution of Engineering and Technology. The Institution's 1921 charter states that its mission was "to promote the general advancement of Electrical Science and Engineering and their applications to facilitate the exchange of information and ideas on those subjects among members" (The Institution of Engineering and Technology website: n.d.) [TN].

motion of matter (of course, we understand motion in Engels's sense as change in general) (Engels 2010a, 362 [TN]).

"To *comprehend* natural phenomena," Helmholtz says in his speech, "The Aim and Progress of Physical Science," "[means] to ascertain their *laws*" (von Helmholtz 1885, 372).

And, this is absolutely true. Helmholtz, who was a consistent mechanist and who believed that everything could be reduced to mechanics, at the same time, did not formulate the task of science to be the task of reduction.

But the objection will be posed: is there no objective distinction between simple and complex? Are you really saying that one cannot call the objective biological laws more complex than physical or chemical ones?

The distinction between simple and complex can only be established if we take the standpoint of the *development* of the complex from the simple, of the aggregate of elements. But it is precisely this point of view of development that is obscured by the reduction formula.

The world is matter in motion. In the process of development, the motion of matter generates new varieties of forms. To study nature means to establish the essential laws and connections between various forms of motion, to establish the *law of their development*. If we can talk about simple and complex forms, then it is only from the point of view of development, but with Cde. Stepanov, the moment of development fades into the background.

[30] A chemical element is composed of electrons. Living matter is composed of chemical elements. The process of natural development passes from inorganic to organic matter.

In this sense, can we say that the chemical element is more complex than the electron, and a living substance is more complex than its chemical elements?

Only in the sense that in comparison with the chemical element, living matter - a later product in the development of inorganic matter - produces new, specific patterns that are characteristic of this formation and are not contained in its elements. Therefore, the task of science consists in studying the specific laws that characterize a new formation (i.e. an atom from electrons), and in establishing the connection between this new, specific law and the elementary laws that underlie it.

It is important and necessary to study the relationship between physico-chemical laws and phenomena in organic matter.

It is important and necessary to study the relationship between electrons and chemical elements.

However, it is also important to study the specific patterns of living phenomena, the specific properties of the atom. It is important precisely because a new entity, representing a later link in the chain of development, is always richer in content and always presents new specific patterns that are different from those of its elementary parts. With the example of the kinetic theory of gases, we have seen that the regularity of a gas as a whole (molecular disorder) represents a unique regularity compared with the mechanical motion of molecules.

Only by establishing the connection between the specific regularity of the whole (volume of a gas) and the mechanical motion of elements (molecules) can we get the kinetic theory of gases with its new, specific regularities: statistical regularities. Mechanics will never supplant the kinetic theory of gases and physics, just as biology will never replace sociology.

* * *

Let us summarize.

In nature, there are two types of relationships between the whole (aggregate) and its parts, between simple and composite. A whole can represent the simple, arithmetic sum of its parts. The properties and laws of the whole are no different from the properties and laws of the parts. A kilogram (by weight) is nothing but the arithmetic sum of 1,000 grams. No matter how we add 1,000 grams (by weight), we will not get any new properties.

A kilogram is completely decomposed into a sum of grams, "reduced" to 1,000 grams.

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[31] We refer to these properties of aggregates as additive properties.

Besides additive properties, an aggregate may also possess certain properties that are not contained in its elements. When we bring a pair of different metals into contact, we get contact potential difference. This potential difference is not specific to any metal in itself, but represents a new property of the aggregate; a new, specific formation not embedded in its elements. These properties of an aggregate, which originate in the laws of its elementary parts, but which only arise in an aggregate as a whole as a result of the synthesis of elements, and which disappear if the aggregate is decomposed into its component parts, we call the non-additive properties of an aggregate.

Had only additive properties existed in nature, then we could have rightfully talked about reduction. A kilogram really does come down to the sum of grams. A kilowatt is reduced to 1,000 watts.[25]

But the additive properties of an aggregate cannot account for the *developmental process*, because the process of development is characterized by the *emergence of new properties*. Since dialectics is the theory of development, our main task is to study the new properties of higher forms and to establish a connection between these new forms and elementary forms; in other words, the study of the forms of the motion of matter in their development.

Thus, the task is not to reduce the specific regularities of the whole to the elementary regularities of the parts, but to study each of them in their mutual interconnection and development. We cannot reduce or dissolve a higher form of motion into the sum of the lower forms of motion, since the specificity of the higher form of motion lies precisely in the fact that it represents not the sum of the lower forms, but their synthesis.

C. Stepanov demands that we try to explain what sort of a mysterious synthesis this is. It is not difficult to do, since Engels provided the definition of synthesis: "*Simple and compound*. Categories which even in organic nature likewise lose their meaning and become inapplicable. An animal is expressed neither by its mechanical composition from bones, blood, gristle, muscles, tissues, etc., nor by its chemical composition from the elements" (Engels 2010a, 494–495).

An organism is a synthesis, not a sum, of tissues, bones, etc.; living tissue is a synthesis of chemical elements, not their sum; a chemical element is a synthesis of electrons and not a simple collection of them. We refer to all this as synthesis because, in a higher form (from the point of view of development), [32] new, specific properties and regularities arise, which are not embedded in its elements. The emergence of these new, specific properties and patterns is the characteristic and essential property of the synthetic combination of elements, in contrast to a simple summation.

To decompose a synthesis into basic elements means to destroy these specific properties and regularities. That is why analysis alone is not enough. Chemistry analytically comprehends perfectly well the composition of living matter (protein), but on the basis of this analytical knowledge alone, it still cannot create a protein. Therefore, along with analytical chemistry, there is also synthetic chemistry, which studies the specific laws of synthesized substances from the point of view of the methods and regularities of their occurrence.

Cde. Stepanov is absolutely wrong when he states: "When chemistry studies (*by analytical method - there are no others*) the structure of proteins, it will be able to synthesize them. And, how else could it (natural science - B.H.)[26] study metabolism in a living organism and arrive at scientific agronomy?" (Skvortsov-Stepanov 1928a, 47n1).

Ignoring synthetic methods of studying phenomena is one of our most significant disagreements.

As a matter of fact, along with analytical methods, there are also methods of synthetic study, methods of synthesis.

[25]A typo: "100 vattam [watts]" in the Russian original, instead of 1000 [TN].

[26]Hessen's note [TN].

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"Empty phraseology," Cde. Stepanov says, "there is no and can be no other methods besides the methods of analysis" (Skvortsov-Stepanov 1928a, 47n1).

Let us turn to Kliment Arkadyevich Timiryazev[27] to resolve our dispute.

Here is what he writes about analysis and synthesis in his wonderful article about Marcellin Berthelot[28]:

> Even the most prominent chemists up until the middle of the nineteenth century were infected with the vitalist point of view borrowed from physicians, because they recognized their powerlessness in one of the two main tasks of their science as soon as they entered the much more complex field of studying living bodies. These two tasks were analysis and synthesis. In the area of dead [non-living, inanimate] bodies, there was no fundamental difference between these two processes. Perhaps we can say that the synthesis of bodies had been known previously from analysis and the main task was to analyze. When Lavoisier, in particular, insisted that the chemist "divides and subdivides, and further subdivides," this was because for inanimate nature, in most cases, knowledge of synthesis often preceded analysis . . . . The picture completely changes when we move to the world of living beings; to organisms and their constituent bodies, called organic, because on the basis of countless experiments, they were convincingly found only in organisms, that only organisms [33] possess the secret of their formation. Already Lavoisier, by "dividing and subdividing" organic compounds, disassembled them down into the same elements that were also parts of inorganic bodies, but neither he nor anyone else before Berthelot set himself the idea of creating organic matter from the elements. In the field of organic chemistry, analysis reigned supreme and synthesis was viewed as the secret of life, the result of the activities of a mysterious life force; ordinary physical forces were not enough for this. This was the slogan of the triumphant vitalism . . . . The first attempt was to resolve the question of the immediate composition of natural fats, to create them synthetically from their nearest constituent parts. (K. Timiryazev 1920, 706–709).

We see that Cde. Stepanov's formulation does not at all embrace the entire complexity of the tasks facing natural science and, in particular, chemistry.

No one objects to the fact that analysis is a powerful method of research, but it does not follow from this that analysis is the *only* "method, because there are no others."

Analysis is the decomposition of an aggregate into its component parts and the isolated study of these parts is one side of the knowledge of nature. Synthesis, the necessary, second side of knowledge, is the method of studying the development of the whole from the parts and the specific laws of the whole as a newly formed entity.

And, to paraphrase Cde. Stepanov, we say:

Whoever aims at analysis as the only task of the scientific knowledge of nature, only dissection into component parts and the study of the laws of these isolated parts (isolated, because studying them in their mutual connection would be synthetic knowledge), abandons the task of the complete investigation of nature, limiting himself to only studying additive properties that are preserved during the decomposition of an aggregate.

The true task of knowing nature is to study the essential regularities of the forms of the motion of matter in their mutual connection, interaction and development.

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[27] Kliment A. Timiryazev (1843–1920) was a Russian professor of biology, specializing in plant physiology, who worked at Moscow State University. Known for his ardent defense of Darwin's theory of evolution, he was equally noteworthy for his political radicalism against the Tsar, which made him a celebrated figure in the Soviet Union (Graham 1993, 67) [TN].

[28] Pierre E.M. Berthelot (1827–1907) was a French professor at the Collége de France and member of the French Academy of Sciences, who specialized in organic chemistry. Berthelot is best known for his contention that organic compounds could be synthesized as well as his contributions to thermodynamics. He also held several political positions throughout his life (Doremus 1907, 592–595) [TN].

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§3. Statistical method in physics and the problem of chance and necessity

Let us return again to the mechanical interpretation of the law of energy dissipation and consider it from a new point of view. Above, we emphasized the specific difference between reversible (mechanical) and irreversible processes; now we will consider where their unity lies. Establishing the relationship between the specificity and unity of reversible and irreversible processes affords us the opportunity to approach a new type of regularity: statistical regularity. The problem of statistical regularity will allow us to clarify [34] the importance of interpreting the concept of chance as an objective category.

Therefore, before proceeding to a statistical interpretation of the law of energy dissipation, we will have to make a rather lengthy introduction about the character of the two main types of physical regularities, i.e. statistical and dynamic, and in so doing, also clarify the relationship between probability and chance.

As soon as the kinetic theory of gases began to develop, Maxwell, with his usual insight, raised the question of the new type of regularity put forward by this theory.[29]

"I think," he says in one of his little-known, philosophical essays, "the most important effect of molecular science on our way of thinking will be that it forces on our attention the distinction between two kinds of knowledge, which we may call for convenience the Dynamical and Statistical" (Maxwell 1882, 210).

One of the characteristic and main distinctions between statistical and dynamic regularity is that dynamic regularity refers *to singular, individual phenomena*, while statistical regularity refers to an aggregate of individuals or phenomena.

Dynamic regularity is not adapted to the study of the laws of a collective. The tool for studying a collective is a different type of regularity: statistical regularity.

As with the question of reversible and irreversible processes, the contradiction between statistical and dynamic regularities is the contradiction between the macroscopic and the microscopic worlds, the contradiction between the individual and the collective.

Let us dwell on the most essential features of statistical regularity.

The contradiction between the individual[30] and the collective is basically the contradiction between the whole and the part. The collective differs from the simple sum of individuals (parts) precisely by the fact that it is a new entity, with new qualities that are not inherent in singular individuals, but which only appear in the collective. Therefore, the collective *consists* of a certain number of individuals, *but cannot be reduced* [35] to singular individuals. For our purposes, it is especially important to emphasize that the properties of a collective do not disassemble into a simple sum of the properties of its constituent parts.

Therefore, dynamic regularity is not applicable to the study of the collective: in dynamic regularity, we study the properties of a singular individual. In the study of the collective, we are not interested in a singular individual in itself and its properties, but in the properties and regularities of the collective.

While the study of an individual phenomenon cannot be approached form the point of view of statistical regularity - since it simply does not have a meaning when applied to an individual - the question of the applicability of dynamic regularity to the study of an aggregate, of a collective, is not as simple. Every aggregate consists of individuals. Therefore, at first glance, it seems that

[29]Maxwell elaborates upon the question of necessity and chance as applied to physics in a short article published by Campbell and Garnett, presenting Maxwell's report to the philosophical circle in Cambridge (the Seniors Club). The article is entitled "Does the Progress of Physical Science Tend to Give Any Advantage to the Opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of Will?" in Maxwell 1882, 209–213.

[30]Here, and in the following exposition, by the word "individual," we mean the objects of research in physics, i.e. a molecule, atom, electron. In relation to these objects, macroscopic bodies will be collectives. In general, a collective can be considered an individual if it is part of a broader collective; a molecule will be an individual with respect to the volume of a gas, but at the same time, it will be a collective with respect to the electrons that compose it.

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dynamic regularity is quite applicable to the study of an aggregate. The situation, however, is more complicated.

Two points of view are possible with respect to statistical regularity: subjective and objective.

An aggregate consists of a huge number of individuals. The behavior of each individual is uniquely determined by a dynamic law.

We can study an aggregate as the sum of a large number of dynamic regularities. Such a study is possible, but extremely difficult. Therefore, we turn to statistical regularity, which, although it is second-class knowledge compared to dynamic regularity, nevertheless still successfully fills the lack in our knowledge and partly overcomes the extraordinary difficulties arising from the study of a huge number of individuals in an aggregate.

It is not difficult to recognize in this argument the characteristic reasoning of the *subjective* interpretation of statistical regularity. In this view, statistical regularity is a consequence of the limitations of our cognitive abilities.

The *objective* interpretation of statistical regularity is that the *raison d'être* of this regularity lies not in the limitations of our knowledge, but in the special, characteristic structure of the aggregate objects to which it is applied.

By approaching the study of an aggregate by statistical method, we consider the aggregate *as a whole*.

In the process of our study, we do not break down an aggregate into individual elements, even though it consists of individual elements. We study it synthetically, as a whole. Therefore, our statistical laws are also applicable to an aggregate only as a whole and do not make sense when applied to individual elements.

But what does it mean to say that statistical regularity is valid for an aggregate as a whole, but is not applicable [36] to its individual elements? In other words, what is the relationship between the dynamic laws governing the individual elements that make up an aggregate and the statistical regularities that only apply to an aggregate as a whole?

Non-additive properties are characteristic of an aggregate as a whole and only as a whole. They are not virtually embedded in its constituent individuals. They only manifest in a whole and are *qualitatively* different from the properties of individuals.

Statistical regularity captures and investigates precisely these non-additive properties. Therefore, it is clear that in its very essence, statistical regularity cannot be related to the singular individuals that make up an aggregate. And this is not its *flaw*, but its characteristic *feature*, since it applies to the study of precisely those properties that only manifest in a whole and which *do not exist* in individual particles.

By viewing the presence of non-additive properties in an aggregate as a feature of the objective structure of the aggregate, we thereby ascribe an objective character to statistical regularity.

In this respect, the relationship between statistical and dynamic regularities is the relationship between the regularities of whole and parts. Dynamic regularity remains the regularity of the individual. But it is insufficient for the study of regularities of a whole. It is not sufficient, because besides additive properties, a whole also has non-additive properties.

Statistical regularity does not eliminate dynamic regularity and does not contradict it. It is necessary and valid in its domain, as dynamic regularity is in its own.

M. Planck[31] rightly notes that dynamic regularity is the condition for the emergence of statistical regularity. But it does not follow from this that statistical regularity is fully reducible to dynamic regularity. This is true only in cases when the whole turns out to be identical to the sum of its parts.

When such identity does not exist - which is revealed by the presence of non-additive properties in the aggregate - then statistical regularity is generated from dynamic regularities just as the whole arises from a part, but it is not composed of and does not decompose into dynamic

[31]See Planck 1960b, 56–68.

regularities. It represents a qualitatively new entity, belonging only to the whole and not to its parts. Thus, statistical regularity is not a second-class knowledge compared to dynamic regularity, but a completely equal method of investigation, conditioned by the uniqueness of the objective structure of the objects of study.

[37] In order to formulate statistical regularity, the concept of probability is introduced.

The concept of probability is inextricably linked with the concept of chance, and therefore, depending on our interpretation of chance as a subjective or an objective category, the concept of probability acquires a subjective or an objective quality, and with it also the concept of statistical regularity. We see, therefore, that our debate about the objectivity of chance is of paramount importance to physics.

Before proceeding to an analysis of the problem of chance, let us clarify the relationship between the concept of probability and the concept of chance.

Laplace already pointed out the fact that the concept of probability is connected with the concept of chance:

> All events, even those which on account of their insignificance do not seem to follow the great laws of nature, are a result of it just as necessarily as the revolutions of the sun. In ignorance of the ties which unite such events to the entire system of the universe, they have been made to depend upon final causes or upon hazard, according as they occur and are repeated with regularity, or appear without regard to order; but these imaginary causes have gradually receded with the widening bounds of knowledge and disappear entirely before sound philosophy, which sees in them only the expression of our ignorance of the true causes.... The curve described by a simple molecule of air or vapor is regulated in a manner just as certain as the planetary orbits; the only difference between them is that which comes from our ignorance. Probability is relative, in part to this ignorance, in part to our knowledge [this last sentence may be translated more precisely]. (Marquis de Laplace 1902, 3, 6).

For Laplace's "all-knowing Mind," chance does not exist. For him, all knowledge is certain. For our limited mind, there are chance phenomena, and therefore, probable knowledge appears alongside certain knowledge. I flip a coin. The process of flipping a coin is an extremely complex set of phenomena. We are not able to accurately calculate how a coin falls, i.e. heads or tails, and therefore, we say that landing heads or tails is random. We say this because it is equally unknown to us whether heads or tails will fall. We know with certainty that either heads or tails should fall, but the fact that with this coin toss, it will fall heads and not tails, is not certain, but only probable.

The theory of probability determines the value of the probability of an event. If our coin is well-formed (completely symmetrical), we say that the probability of heads falling is half. However, this does not mean that if tails falls in any given throw that [38] heads will fall in the next throw. A probability equaling one half means that if we make *a large number* of throws and count the number of heads and tails that fall, they will be distributed almost equally, with the number of heads (or tails) being closer to half the number of throws the more times the coin is thrown.

Thus, probability expresses the regularity of the distribution of heads and tails *within the entire aggregate of throws* and does not tell us anything about a single throw.

Knowing the probability of heads falling, we still cannot say anything about what will happen in this single throw.

Probability expresses a statistical law, i.e., regularity, related to the entire aggregate of throws as a whole. That is why we say that probability is a way of expressing statistical regularity.

The concept of probability is closely related to the concept of chance.

We saw above how Laplace defined this relationship. His interpretation of chance is purely subjective. Chance is a measure of our ignorance. Therefore, in this respect, probability is also a subjective concept. Statistical regularity is also a subjective category according to this concept. Knowledge of statistical regularities is second-class knowledge compared to dynamic regularities.

We see how important a solution to the question of chance is for physics: it determines our interpretation of probability and with it, statistical regularity.

It is well known how fiercely the dialecticians were attacked for defending the point of view that chance is an objective category.

It came even to the point that dialecticians were accused of defending a-causality.

It is easy to understand what this absurd accusation is based on. Above, we saw that Laplace (of course, he was not the first and he was not alone) considered chance to be that for which the reasons are unknown to us. The following conclusion can be drawn from this: chance is that for which reasons are unknown. In a subjective sense, this does not contradict the general basis of determinism. "*Chance is only the measure of our ignorance*" (Poincaré 1914, 65). If the concept of chance is objectivized, then the subjective absence of a cause (ignorance) turns into an objective absence of a cause without reason – into acausality.

Such reasoning is by itself not accidental, but is only representing a measure of ignorance.

The dialectical view on chance allows us to assign an objective value to chance, while at the same time retaining the point [39] of view of determinism. Of course, however, the mechanical concept of causality is insufficient.

Let us study a series of 10,000 coin flips. From the subjective view of probability, the position of heads and tails, the manner in which they alternate, is random, since we cannot predict, *due to our ignorance*, in which sequence heads and tails will proceed. From a dialectical point of view, the sequence of heads and tails is random, because it does not affect the basic, statistical law of the number of heads approaching the number of tails with an increase in the number of elements in the aggregate.

A single throw is random with regard to the regularity that prevails in the aggregate as a whole. Of course, it does not at all follow from this that there is no pattern for each individual element. But even though the regularity of an element (throwing a coin) is the basis of the regularity of a whole, the individual outcome of this regularity does not affect the behavior of the collective as a whole.

In order to further clarify our argument, let us use an image of statistical regularity suggested by Quetelet.[32]

Draw a circle on a blackboard with chalk. The circle is formed by a large number of tiny particles of chalk adhering to the board. If we examine a part of the circle under a microscope, we see a chaos of particles that have nothing to do with the circle. If we consider the circle as a whole - for example, by moving some distance away from the board - the chaos of individual particles will appear as a definite, geometric line. The aggregate of regularities of arranged chalk particles produces a completely new regularity: a circle. With respect to it, the arrangement of individual particles is random. If we change the position of any particle, this will not affect the regularities of the entire set of particles on the line of the circle.[33]

The laws governing the motion of an individual molecule remain and continue to operate in the volume of a gas, which we consider an aggregate of molecules. But the laws of this collective as a whole encompass the entire set of molecules. A change in the motion of an individual molecule does not affect the law of the whole. With respect to the law of this whole, the motion of an individual molecule is a random process.

This is the sense in which we speak of the objectivity of chance. If all the chains of causal dependencies were equal, if, in the words of Engels, that a particular pea-pod contains five peas and not four or six, that a particular dog's tail is five inches long and not a whit longer or shorter, that this year a particular clover flower was fertilized by a bee and another not, that last night I was bitten by

[32]Lambert A.J. Quetelet (1796–1874) was a Belgian astronomer, mathematician and sociologist. Founder of the Royal Observatory in Brussels and a longstanding secretary for the Belgian Royal Academy, Quetelet is best known for his application of statistics to social phenomena through the study of crime, mortality, etc. (Gregersen 2011, 282) [TN].

[33]Unknown source [TN].

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a flea at four o'clock in the morning, and not at three or five o'clock, and on the right shoulder and not on the left calf - these are all facts which [40] have been produced by irrevocable concatenation of cause and effect, by an unshatterable necessity of such a nature indeed that the gaseous sphere, from which the solar system was derived, was already so constituted that these events had to happen thus and not otherwise," then knowledge of nature would be impossible (Engels 2010a, 499 [TN]).

The fact that we can study nature and, in practice, distinguish essential from non-essential, random from necessary, regularities, is proof of the inequality of the chains of causal relationships, and, consequently, of the objectivity of chance.

One can only be surprised that, in his last book, L.I. Axelrod[34] again upholds an objective point of view on chance. The subtitle of L.I. Axelrod's book is "Against Neo-Scholastics" (Akselrod 1928 [TN]). In his book, Cde. Stepanov cautiously avoids the problem of chance and necessity in its entirety.

Let us now see what point of view is justified by the development of modern natural science.

If we take the era of Laplace, then L.I. Axelrod is absolutely right. Laplace, Poisson, Bertrand and other classics of the probability theory of the eighteenth and nineteenth centiries did indeed consider chance and probability a subjective category.

But already starting with Cournot,[35] the view on chance changes radically.

"Mathematical probability," Cournot says, "becomes the measure of *physical possibility*, so that these expressions are interchangeable. The advantage of the term … is that it clearly denotes the experience of an [objective] ratio which exists between the things themselves and does not depend on our manner of judging or sensing varying from one individual to another according to their circumstances and the degree of their knowledge. [This relationship is in the things themselves]" (Cournot 2013, 47 [TN]).

The objective point of view on probability theory leads Cournot to a definition of chance that is very close to that of Plekhanov.[36]

Accidents are "*events causally produced by combinations or encounters* [rencontres] *of phenomena belonging to independent series*" (Ibid., 43 [TN]).

Poincaré criticizes Laplace's subjective concept of chance in *Science and Method*, where his definition of chance approaches Cournot and Plekhanov.[37]

Note that Hegel's formulation of the concept of chance, later adopted by Engels, is deeper and methodologically more fruitful, since it establishes the distinction between essential or necessary from non-essential or random regularity.[38] It is not difficult to show that it embraces Plekhanov's formulation.

[34]Lyubov I. Axelrod (1868–1946), pen name "Orthodox," was a Russian revolutionary and philosopher of Marxist theory. A member of the Menshevik of the Russian Social-Democratic Labor Party, she would go on to be a member of the Institute of Red Professors and the Sverdlov Communist Academy. She was considered one of the most respected philosophers of the early Soviet Union, and the primary spokesperson of the mechanist persuasion in the "mechanist" and "Deborinite/dialectician" debate of the 1920s (Sheehan 1985, 179; Wetter 1964, 149–150) [TN].

[35]Antoine A. Cournot (1801–1877) was a French philosopher and professor of mathematics, who held positions at the University of Grenoble and Dijon Academy. He is best known for his work on probability theory along with his contributions to "wealth theory" in economics, exemplified in his work *Researches into the Mathematical Principles of the Theory of Wealth* (Martin & Touffut 2007, xiv–xv, 1–7) [TN].

[36]Georgi V. Plekhanov (1856–1918) was a Russian revolutionary and philosopher of Marxist theory. One of the founders of the social-democratic movement in Russia, he is best known as the "father of Russian Marxism" and for having coined, or at least popularized, the term "dialectical materialism" in his *The Materialist Conception of History*. He was a member of the Menshevik wing of the Russian Social-Democratic Labor Party and an outspoken opponent of Lenin and the Bolsheviks (Graham 1987, 25; Sheehan 1985, 114, 115n; Wetter 1964, 100–109) [TN].

[37]Of course, they will object that Poincaré was a Machist. Without delving now into the question of whether the criticism of the subjective interpretation by Poincaré's is related to his Machism, we will turn below to a presentation of the views of Smoluchowski, who was not a Machist.

[38]Here, Hessen is referring to a passage from Engels' *Dialectics of Nature*, in which Engels elaborates Hegel's conception of chance and necessity. According to Engels, "in contrast to both conceptions, Hegel came forward with the hitherto quite

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[41] So, one or another solution to the question of chance defines our attitude to probability theory and to statistical regularity. And, since statistical regularity is becoming dominant in modern physics, clearly the solution to the question of chance and necessity is of great methodological significance to it.

M. Smoluchowski emphasizes with particular clarity the growing importance of statistical method and the need to clarify the methodological foundations of the concepts of probability and chance:

> After a temporary period of stagnation, as a result of the ultimate victory of the atomistic viewpoint, [probability theory] has become a fundamental factor for physics and today constitutes the most important research tool in the field of modern theories of matter, electricity, radioactivity and the theory of radiation .... Despite the enormous extension of the scope of probability theory's applications, a precise analysis of its underlying concepts has made little progress. It is probably still true today that no other mathematical discipline is based on such unclear and shaky foundations. Thus, the basic questions about the subjectivity or objectivity of the concept of probability, about the definition of chance, etc., are answered in a diametrically opposite way by different authors.... [T]his study means to... highlight and correctly explain the main guiding idea which, up until now, have been almost neglected, namely on the objective side of the concept of probability. (Smoluchowski 1918, 253)

As we see, M. Smoluchowski emphasizes, in particular, the significance of his article in putting forward the question of the objectivity of probability, and consequently, of chance.

"I am fully aware of the fact," Smoluchowski continues,

> that this concept of chance contradicts the usual definition, which considers the partial ignorance of causes to be its most salient point; therefore, I will mention the following in support of my view: the application of probability theory in the kinetic theory of gases would retain its significance and would be fully justified even if we knew the exact structure of the molecules and their initial positions and if we were able to accurately, mathematically describe each of their movements in time.... It seems to me that for a philosopher, it is quite important that at least in a narrow field of physics, the concept of probability [42], understood in the usual sense as a regular sequence of random phenomena, has a strictly objective meaning, and that the concept and origin of chance can be precisely defined while all the while clinging to determinism. (Ibid., 254, 262)

An accurate and scientific definition of probability can only be provided if we interpret the concept of chance, of probability, in an objective sense. The extensive literature devoted to criticizing the foundations of probability theory is largely directed against Laplace's subjective interpretation of probability and chance, which clearly relies on *petitio principii*. Recently, Mises's work provides a criticism of the subjective interpretation of the concept of probability and attempts to establish probability theory on the basis of an objective definition of probability. These works are recognized by physicists, and in the recently published volume *Handbuch der Physik*, the latest

unheard-of propositions that the accidental has a cause because it is accidental, and just as much also has no cause because it is accidental; that the accidental is necessary, that necessity determines itself as chance, and, on the other hand, this chance is rather absolute necessity (*Logik*, II, Book III, 2: 'Die Wirklichkeit'). Natural science has simply ignored these propositions as paradoxical trifling, as self-contradictory nonsense, and, as regards theory, has persisted on the one hand in the barrenness of thought of Wolffian metaphysics, according to which a thing is *either* accidental *or* necessary, but not both at once; or, on the other hand, in the hardly less thoughtless mechanical determinism which in words denies chance in general only to recognize it in practice in each particular case" (Engels 2010a, 500–501. See Hegel 2010, 482–485; Hessen 2019, 97–98) [TN].

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and most authoritative physics encyclopedia, probability theory is presented on the basis of an objective definition of the concept of probability.

And now, after the works by Cournot, Smoluchowski, Mises, etc., and after the philosophical question has been exhaustively explained by Hegel and Engels, we are, once again, being pushed back to Laplace's concepts and to identifying chance as a subjective category. And this is called the fight against "Neo-Scholastics." Our harsh critics can only have one excuse, namely that the entire latest development of physics and mathematics is, for them, a book with seven seals.

Once the question of the relationship between chance, probability and statistical regularity has been clarified, it is not difficult to establish the relationship between reversible (mechanical) and irreversible (thermal) processes and the essence of the statistical interpretation of the law of energy dissipation.

The thermal state of a body can be described by its temperature. Temperature expresses the degree of *bodily heat*. The concept of temperature is not associated with any hypotheses regarding the structure of a body; it is an empirical, directly observable quantity (i.e. by the position of the level of mercury in a thermometer) that determines the thermal state of a body. The law of energy dissipation in the aforementioned formulation (i.e. the Clausius formulation) is based on the definition of temperature as a directly observable quantity. In this form, the formulation of the law is *macroscopic* and the law itself is expressed by the Clausius formulation in the form of a dynamic regularity.

If we turn to the microscopic, atomic structure of matter, we need to approach the task of determining the thermal state of a body differently. According to the mechanical theory of heat, the degree of bodily heat is determined by the energy of the motion of molecules. The molecules do not all have the same velocity and energy [43], but the velocity and energy are distributed between the molecules according to a certain law.

Each particular distribution of the velocity between molecules corresponds to a certain thermal state of the body. In other words, each microstate determined by the distribution of molecules corresponds to a macrostate determined by temperature.

But the same macrostate, determined by temperature, can correspond *not to one*, *but to several microstates*.

The same thermal state of a body can be actualized by *different distributions* of velocities between molecules, provided that the average amount of energy remains the same. Indeed, which particular molecules possess this or that velocity does not matter for determining a thermal state.

The velocity of a particular molecule is random. It is random precisely in the sense of objective chance, since the general distribution of velocity, which characterizes the entire aggregate of molecules as a whole, and not the distribution of velocities among particular individuals, is important for determining the thermal state.

In the same way as in the aforementioned example, in a series of coin tosses, it is not the individual tosses of heads or tails that matter, but the general distribution of the number of heads and tails that characterizes the studied series as a whole.

So, the same thermal state can be actualized by a certain set of microstates.

This means that if we consider all possible microstates of, for example, a gas body, when we pay attention to the velocities of *each individual molecule*, we will get an extremely large number of microstates. But we saw above that the whole series of microstates can actualize the same thermal state.

The larger the number of microstates that result in the same thermal microstate, the more probable it is, just as it is *more probable* to remove a white ball than a black ball from an urn containing 1,000 white balls and 1 black.

From a *microcosmic point of view*, each thermal state has a certain probability.

When observing the course of thermal processes, we see that the difference between the equations of macroscopic, thermal states tends to equalize, i.e. to come to equilibrium. The process will not proceed in the opposite direction.

This is the essence of Clausius's formulation: a transition always takes place only from a warmer body (at a higher thermal level) to a less heated body (at a lower thermal level).

[44] Since we constantly observe only this course of the process, we conclude from this that *microprocesses that actualize a state of equilibrium are the most probable*.

If we have one gas volume heated more than another gas volume, then the thermal process will proceed so that the temperature equalizes, i.e. *so that the most probable microstate is obtained, and this will be the state of thermal equilibrium*.

Artificially, by external intervention, we can disturb the thermal equilibrium, for example we can heat one of the bodies, and likewise, also artificially, we can *choose* a black ball each time from the urn. But when left to its own devices, the thermal process will again reach an equilibrium, just as when we remove a ball randomly, without choosing, we will obtain a distribution that corresponds to the probability of the occurrence of one ball or another.

It is now clear why we consider thermal processes to be irreversible. They are irreversible because the thermal process is a transition from a less probable state to a more probable one. The probability of a reverse heat transfer from a cold body to a warm one is very small, but not equal to zero!

We do not observe such a transition in nature, because it is extremely unlikely, but not because it is entirely impossible.

If we put a pot of water on a stove, the water will boil. We constantly observe this. But it is not necessary that the water boils. It may freeze, i.e. heat from the water may go to the flame of the burner. This is not completely impossible, but so unlikely that even a single occurrence of such a situation could require a period of time, compared to which, the entire lifetime of our solar system is vanishingly short. Just as well, in order to remove a black ball from an urn containing 1,000,000 white balls and one black ball, it would be necessary to make an inordinately long series of attempts. Of course, a black ball does not have to come out only at the very end. It can appear at any moment, even at the very beginning, but the number of its appearances compared to the number of the appearances of white balls will be a million times smaller.

Generally speaking, the occurrence of a highly unlikely microstate is not impossible and, like the appearance of a black ball, can happen at any moment. But, even if it happened and if we observed it, this would not at all disrupt the basic tendency of thermal processes to pass from a less probable state to a more probable state.

This is the concept of thermal processes as expressed according to *statistical regularity*.

In the case of dynamic regularity, we have a certain, unambiguous course of the process. A stone lifted above the ground *must* [45] fall down by the force of gravity. This statement necessarily applies to every single case.

If I put a pot on a stove, *the water can boil or it can turn into ice*.

The general regularity of the course of thermal processes, consisting in the transition from less probable to more probable states, says nothing about the course of *individual processes*. Statistical regularity is a statement regarding the entire aggregate of processes.

The law of energy dissipation is a *statistical law*, which resolves the contradiction between reversible and irreversible processes: every process is reversible and irreversible. It is *irreversible* as a macrocosmic process, as an object of human practice, because the probability of its reversal compared with the time of human and earthly practice is vanishingly small. Therefore, we will not try to freeze water by placing it on a stove and we will not heat a stove with ice, not because it is absolutely impossible, but because the occurrence of such processes in practical terms is vanishingly small.

But every process is also reversible as a microcosmic process, because for the cosmic time intervals, any, even an arbitrarily unlikely microstate, will be actualized.

The statistical interpretation of the law of energy dissipation eliminates the difficulty over the question of the temporal origin of the world. Any equilibrium state is only death from a limited, "earthly" point of view. From a cosmic point of view, any unlikely formation, any deviation from

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thermal equilibrium, is possible. Thermal death is the beginning of new life. *The world has no beginning and no end, neither in time nor in space*. This is the ultimate conclusion from the statistical interpretation of the law of energy dissipation.

The unity of reversible and irreversible processes is that irreversible (thermal) processes are *based* on the mechanical movements of billions of molecules, and therefore, *in principle*, any thermal state is reversible.

The *specificity* of irreversible processes consists in their regularity being statistical, not dynamic; therefore, it is possible to talk about irreversible processes, even if, in rare cases, heat can transfer from a less heated body to a more heated one.

If we accept that chance is a subjective category, we must thereby declare that the specificity of irreversible processes is subjective. But everyday experience teaches us that thermal processes are *practically* irreversible. And their irreversibility is not based on the fact that statistical regularity is incomplete [46] knowledge, but on the specific difference between macroscopic thermal processes and the mechanical motion of molecules. This qualitative difference, confirmed by our practice, does not disappear even if we study the motion of each molecule in detail and "eliminate chance as being a result of our ignorance" (Poincaré 1914, 66).[39]

Chance, and hence probability, and statistical regularity, are also objective, as is *quality*.

Only by properly understanding both the unity and the specificity of reversible and irreversible processes can we come to understand the essence of the law of energy dissipation.

We summarize our discussion in the conclusion:

From direct observation of the visible (the macroscopic) world, we establish the presence of reversible (mechanical) and irreversible (thermal) *processes*. These processes are separated by a seemingly impassable chasm.

Formulating the law of energy dissipation macroscopically (Clausius), we do not make any assumptions about the elementary structure of bodies. The body acts as an individual.

When we move to considering a gas from an atomistic point of view, we establish that gas is a collection of molecules, each of which moves according to the laws of mechanics.

Mechanical movements are reversible. Thermal phenomena are irreversible. Thermal phenomena are based on mechanical phenomena. How can the latter lead to irreversible processes?

The solution to the problem consists in adopting the hypothesis of molecular chaos, according to which we ascribe irreversible processes not with a dynamic (purely mechanical) interpretation, but a *statistical* one.

Thermal processes turn out to be both reversible and irreversible. Along with their unity, this establishes their specificity.

A statistical interpretation becomes possible, because the same thermal (macroscopic) state of a body is actualized by a large number of microstates.

Each one from this series of microstates that actualizes a given thermal state is random with respect to that state, since one or another distribution of molecules, from which a given thermal state is formed, within certain limits, does not affect the latter. The fact that we can consider known microstates as random allows us to form the concept of the probability of a given thermal state and thus arrive at a statistical interpretation of the law of energy dissipation.

If we treat chance as a subjective category, then the law of energy dissipation, as a statistical law, will take on a subjective hue. Then it would turn out that, in contrast to the law of energy conservation, the law of energy dissipation is not an expression of an objective regularity, but only an expression of the insufficiency of our knowledge of nature.

[47] If, however, we take the point of view of dialectical materialism and treat chance as an objective category, we will conclude that two, equal laws form the foundation of natural science: the law of energy conservation and transformation, which is essentially a quantitative law, and the

[39]This is Hessen's paraphrase of Poincaré's text [TN].

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law of energy dissipation, which essentially reflects the specific regularities of the motion of energy, and which emphasizes, in addition to the law of energy transformation, the qualitative aspect of the motion of energy processes.

(End in next issue).[40]

§. Bibliography

English

Boltzmann, Ludwig. 1974. "On the Principles of Mechanics." In *Theoretical Physics and Philosophical Problems: Selected Writings*. Edited by Brian McGuinness and translated by Paul Foulkes, 129–152. Dordrecht/Boston: D. Reidel Publishing Company.

Boltzmann, Ludwig. 2003. "Further Studies on the Thermal Equilibrium of Gas Molecules." In *The Kinetic Theory of Gases: An Anthology of Classic Papers with Historical Commentary*. Edited by Stephen G. Brush and Nancy S. Hall, 262–349. London: Imperial College Press.

Caneva, Kenneth L. 1993. *Robert Mayer and the Conservation of Energy*. Princeton: Princeton University Press.

Chant, Clarence A. 1940. "Death of Sir Oliver Lodge." *Journal of the Royal Astronomical Society of Canada* **34**: 435–436.

Clausius, Rudolf. 1867. *The Mechanical Theory of Heat, with Its Applications to the Steam-Engine and to the Physical Properties of Bodies*. Edited and translated by **Thomas Archer Hirst**. London: John van Voorst.

Cournot, Antoine A. 2013. *Exposition of the Theory of Chances and Probabilities*. Translated by Oscar Sheynin. Berlin: NG Verlag.

Descartes, René. 1985. *Principles of Philosophy*. In *The Philosophical Writings of Descartes*, *vol.* **1**. Edited and translated by John Cottingham, Robert Stoothoff & Dugald Murdoch, 177–291. Cambridge: Cambridge University Press.

Doremus, Charles. 1907. "Pierre Eugène Marcellin Berthelot." *Science* **25**: 592–595.

Dostoevsky, Fyodor. 1900. *The Brothers Karamazov*. Translated by Constance Garnett. New York: The Modern Library.

Engels, Friedrich. 2010a. *Dialectics of Nature*. In *Marx & Engels: Collected Works, vol.* **25** *– Engels*. Edited by Natalia Karmanova, et al. and translated by Clemens Dutt, 313–629. London: Lawrence & Wishart.

Engels, Friedrich. 2010b. *Ludwig Feuerbach and the End of Classical German Philosophy*. In *Marx & Engels: Collected Works, vol. 26 – Engels 1882–89*. Edited by B. Tartakovsky, et al. and translated by Nicholas Jacbos, et al., 353–398. London: Lawrence & Wishart.

Graham, Loren R. 1987. *Science, Philosophy, and Human Behavior in the Soviet Union*. New York: Columbia University Press.

Graham, Loren R. 1993. *Science in Russia and the Soviet Union*. Cambridge: Cambridge University Press.

Gregersen, Erik, ed. 2011. *The Britannica Guide to Statistics and Probability*. New York: Britannica Educational Publishing.

Hegel, Georg Wilhelm Friedrich. 2010. *The Science of Logic*. Translated by George Di Giovanni. Cambridge: Cambridge University Press.

Hessen, Boris. 2019. "Preface to Articles by A. Einstein and J.J. Thomson." Translated by Sean Winkler. *Society and Politics* **13**: 87–102.

Huygens, Christiaan. 1912. *Treatise on Light*. Translated by Silvanus P. Thompson. London: Macmillan and Co., Limited.

Ings, Simon. 2016. *Stalin and the Scientists: A History of Triumph and Tragedy, 1905–1953*. New York: Atlantic Monthly Press.

The Institution of Engineering and Technology. "A History of the Institution of Engineering and Technology." Accessed 17 February 2021. https://www.theiet.org/membership/library-archives/the-iet-archives/iet-history/a-history-of-the-institution-of-engineering-and-technology/

Isaacs, Bernard, ed. 1977. *V.I. Lenin: Collected Works, vol. 34 – Letters, November 1895–November 1911*. Translated by C. Dutt. Moscow: Progress Publishers.

Joravsky, David. 2009. *Soviet Marxism and Natural Science, 1917–1932*. London/New York: Routledge.

Marquis de Laplace, Pierre-Simon. 1902. *A Philosophical Essay on Probabilities*. Translated by Frederick W. Truscott & Frederick L. Emory. New York: John Wiley & Sons; London: Chapman & Hall, Ltd.

Martin, Thierry & Jean-Pierre Touffut. 2007. Introduction to *Augustin Cournot: Modeling Economics*. Edited by Jean-Pierre Touffut, 1–7. Cheltenham/Northampton: Edward Elgar.

Lenin, Vladimir. 1977. *Materialism and Empirio-Criticism: Critical Comments on a Reactionary Philosophy*. In *V.I. Lenin – Collected Works, Vol.* **14;** *1908*. Edited by Clemens Dutt and translated by Abraham Fineberg. Moscow: Progress Publishers.

Lodge, Oliver. 1928a. "The Revolution in Physics." *Journal of the Institution of Electrical Engineers* **66**: 1005–1020.

Lodge, Oliver. 1928b. "The Revolution in Physics." *Nature* **3073**: 429–431.

[40]Hessen never published the final portion of this article [TN].

Marx, Karl. 2010. *Economic Manuscripts of 1857–1858 (First Version of* Capital*)*. In *Marx & Engels: Collected Works, vol.* **28** *– Marx 1857–61*. Edited by Natalia Karmanova, et al. and translated by Ernst Wangermann. London: Lawrence & Wishart.

Maxwell, James C. 1882. "Does the Progress of Physical Science Tend to Give Any Advantage to the Opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of Will?" In *The Life of James Clerk Maxwell with a Selection from his Correspondence and Occasional Writings and a Sketch of His Contributions to Science*. Edited by Lewis Campbell & William Garnett, 209–213. London: Macmillan and Co.

Maxwell, James C. 2003. "Atom." In *The Scientific Papers of James Clerk Maxwell, vol. 2*. Edited by William D. Niven, 445–484. Mineola: Dover Publications, Inc.

Millikan, Robert A. 1927. *Evolution in Science and Religion*. New Haven: Yale University Press.

Molière. 2003. *The Imaginary Invalid*. Translated by Charles H. Wall. http://www.gutenberg.org/files/9070/9070-h/9070-h.htm (last accessed 23 February 2021), online version of Molière. 1673. *The Imaginary Invalid*.

Planck, Max. 1960a. "The Place of Modern Physics in the Mechanical View of Nature." In *A Survey of Physical Theory [Formerly Titled: A Survey of Physics]*. Translated by R. Jones and D.H. Williams, 27–44. New York: Dover Publications, Inc.

Planck, Max. 1960b. "Dynamical Laws and Statistical Laws." In *A Survey of Physical Theory [Formerly Titled: A Survey of Physics]*. Translated by R. Jones & D.H. Williams, 56–68. New York: Dover Publications, Inc.

Planck, Max. 1969. *Treatise on Thermodynamics*, 3*rd* *Ed.* Translated by Alexander Ogg. Mineola: Dover Publications, Inc.

Poincaré, Henri. 1914. *Science and Method*. Translated by Francis Maitland. London: Thomas Nelson and Sons.

Rao, Y.V.C. 1997. *Chemical Engineering Thermodynamics*. Hyderguda: Universities Press [India] Private Limited.

Rouse Ball, Walter W. 1908. *A Short Account of the History of Mathematics*, 4*th* *Ed.* London: Macmillan and Co., Limited.

Sheehan, Helena. 1985. *Marxism and the Philosophy of Science: A Critical History*. Atlantic Highlands: Humanities Press International, Inc.

Temple, George F.J. 1956. "Obituary: Edmund Taylor Whittaker, 1873–1956." *Biographical Memoirs of the Royal Society* **2**: 298–325.

Thompson, Silvanus P. 1910. *The Life of William Thomson, Baron Kelvin of Largs; in Two Volumes, Vol.* **2**. London: Macmillan and Co., Limited.

Thomson, William. 1853. "On the Dynamical Theory of Heat, with Numerical Results Deduced from Mr. Joule's Equivalent of a Thermal Unit, and M. Regnault's Observations on Steam." *Transactions of the Royal Society in Edinburgh* **20** (2): 261–288.

von Helmholtz, Hermann. 1853. "On the Conservation of Force; a Physical Memoir [Read before the Physical Society of Berlin on the 23rd of July, 1847. Berlin. G. Reimer]." In *Scientific Memoirs, Selected from The Transactions of Foreign Academies of Science and from Foreign Journals, vol.* **1**. Edited by John Tyndall & William Francis and translated by John Tyndall, 114–162. London: Taylor and Francis.

von Helmholtz, Hermann. 1885. "On the Aim and Progress of Physical Science: An Opening Address Delivered at the Naturforscher versammlung, in Innsbrück, 1869." In *Popular Lectures on Scientific Subjects*. Translated by Edmund Atkinson, 363–397. New York: D. Appleton and Company.

Wetter, Gustav A. 1964. *Dialectical Materialism: A Historical and Systematic Survey of Philosophy in the Soviet Union*. Translated by Peter Heath. London: Routledge & Kegan Paul.

Whittaker, Edmund T. 1910. *A History of the Theories of Æther and Electricity: From the Age of Descartes to the Close of the Nineteenth Century*. New York/Bombay/Calcutta: Longmans, Green, and Co.

German

du Bois-Reymond, Emil. 1886a. „Ueber die Grenzen des Naturekennens." In *Reden von Emil du Bois-Reymond, Erste Folge*, 105–140. Leipzig: Verlag von Veit & Comp.

du Bois-Reymond, Emil. 1886b. „Goethe und kein Ende." In *Reden von Emil du Bois-Reymond, Erste Folge*, 418–447. Leipzig: Verlag von Veit & Comp.

Smoluchowski, Marian. 1918. „Über den Begriff des Zufalls und den Ursprung der Wahrscheinlichkeitsgesetze in der Physik." *Die Naturwissenschaften* **6**: 253–263.

von Helmholtz, Hermann. 1871. *„Rede zum Gedächtnis an Gustav Magnus."* Berlin.

von Mayer, Julius R. 1845. *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel. Ein Beitrag zur Naturkunde*. Heilbronn: C. Drechsler.

Russian

Akselrod, Lyubov I. 1928. *V zashchitu dialekticheskogo materializma. Protiv skholastiki* [In Defense of Dialectical Materialism: Against Scholasticism]. Moskva [Moscow]/Leningrad: Gosudarstvennoye izdatel'stvo [State Publishing House].

Gessen, Boris M. 1928. «Mekhanicheskiy materializm i sovremennaya fizika [Mechanistic Materialism and Modern Physics]». *Pod znamenem marksizma* [Under the Banner of Marxism] **7** (8): 5–47.

Skvortsov-Stepanov, Ivan I. 1928a. «Osnovnyye itogi diskussii [The Main Results of the Discussion]». In *Dialekticheskiy materializm i deborinskaya shkola* [Dialectical Materialism and the Deborinite School], 7–68. Moskva/Leningrad: Gosudarstvennoye izdatel'stvo.

Skvortsov-Stepanov, Ivan I. 1928b. «Dialekticheskoye O moikh oshibkakh, „Otkrytykh i ispravlennykh" t. Stenom [On My Mistakes, 'Open and Corrected' by C. Stan]». In *Dialekticheskiy materializm i deborinskaya shkola*, 69–79. Moskva/Leningrad: Gosudarstvennoye izdatel'stvo.

Skvortsov-Stepanov, Ivan I. 1928c. «Dialekticheskoye ponimaniye prirody. mekhanisticheskoye ponimaniye [The Dialectical Conception of Nature: The Mechanistic Conception]». In *Dialekticheskiy materializm i deborinskaya shkola*, 80–121. Moskva/Leningrad: Gosudarstvennoye izdatel'stvo.

Timiryazev, Arkady K. 1923. *Kineticheskaya teoriya materii [The Kinetic Theory of Matter]*. Moskva/Petrograd: Gosudarstvennoye izdatel'stvo.

Timiryazev, Kliment A. 1920. «Lavuaz'ye XIX stoletiya (Marselen Bertlo. 1827–1907) [The Lavoisier of the 19th Century (Marcellin Berthelot, 1827–1907)]». In *Nauka i demokratiya. Sbornik statey 1904–1919 gg. Chast' 1* [Science and Democracy: Collected Articles, 1904–1919, vol. 1], 680–747. Moskva/Leningrad: Gosudarstvennoye izdatel'stvo.

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