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History of Science through Koyré's Lenses

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Alexandre Koyré was one of the most prominent historians of science of the twentieth century. The standard interpretation of Koyré is that he falls squarely within the internalist camp of historians of science—that he focuses on the history of the ideas themselves, eschewing cultural and sociological interpretations regarding the influence of ideologies and institutions on the development of science. When we read what Koyré has to say about his historical studies (and most of what others have said about them), we find him embracing and championing this Platonic view of his work. Ultimately I think this interpretation of Koyré's history of science is lopsided and in need of correction. I claim, rather, that a careful reading of Koyré's work suggests that a tension exists between internal and external methodological considerations. The external considerations stem from Koyré's commitment to the unity of human thought and the influence he admits that the 'transscientifiques' (philosophy, metaphysics, religion) have on the development of science. I suggest in conclusion then, that if we are to put a philosophical label on his work, rather than 'Platonist', as has been the custom, 'Hegelian' makes a better fit. © 2001 Elsevier Science Ltd. All rights reserved.

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Outside the discipline of the history and philosophy of science, few people read the primary sources themselves from scientists of past centuries. So most of what is believed today about the history of science is filtered through the lenses of a handful of historians of science. Given this state of affairs, it would behoove us to know something of the nature of those lenses. And while there has been in the last few decades a good deal of discussion in the literature concerning the methodology of writing history of science, there has been relatively little written about the historians themselves, and few interpretations of their work.[1] It is my aim in this paper to provide an interpretation of the work of one of the most influential historians of science of the twentieth century: Alexandre Koyré.

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[1]A notable exception is H. Floris Cohen (1994).

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While history of science as a distinct academic department is relatively new among most universities, writing histories of science is not. The eighteenth and nineteenth centuries saw the likes of Joseph Priestley and William Whewell; and into the first half of the twentieth century Duhem, Tannery, Sarton and Thorndike are recognized giants in the field. But in the middle decades of the 1900s and especially after World War Two, the discipline underwent a dramatic shift. This shift is largely attributed to the work of Alexandre Koyré. He challenged what Butterfield called 'the Whig interpretation of history'.[2] The positivist influence of the time (especially Comte's influence) manifested itself in fostering a conception of science's history as gradual growth and accumulation of knowledge. So it was natural for historians to look to the past and see the present theories in embryonic form, just waiting to be clarified and developed. What Koyré did, in distinction to this approach, was to work himself back into the frames of reference of those he was studying, and so assess their science from within the cluster of concepts with which the scientists of the period were working. In his view, the history of science should not merely focus on the achievements that were passed on to subsequent generations; indeed, he preached that studying the history of errors can often be more illuminating of the nature of the conceptual impediments that have to be overcome before a new theory can be posited and embraced—thus informing us of the conceptual bedrock in which successful theories are grounded.[3]

To understand the development of science according to Koyré's method, then, it is not a matter of working backward from our successful theories to find their antecedents, but rather one of working forward through the conceptual difficulties of the time to a transformation of mind such that one could, for example, 'see' motion as a state or 'see' orbiting satellites as bodies in constrained free-fall. The acknowledgment of such transformations in habits of thought is the hallmark of Koyré's history of science. He believed that the greatest of these transformations since the beginnings of civilization in ancient Greece occurred during what he called the scientific revolution of the seventeenth century in Europe.[4] How is that transformation best characterized? There are many correct

[2]Butterfield says: 'It is part and parcel of the Whig interpretation of history that it studies the past with reference to the present . . . The Whig historian stands on the summit of the twentieth century and organizes his scheme of history from the point of view of his own day. [He searches] for likenesses between past and present, instead of being vigilant for unlikenesses; so that he will find it easy to say that he has seen the present in the past, he will imagine that he has seen a "root" or an "anticipation" of the twentieth century, when in reality he is in a world of different connotations altogether' (*The Whig Interpretation of History*, 1931; quoted in Jones, 1989, p. 85).

[3]Koyré (1978), p. 66.

[4]Many contemporary writers credit Koyré with formulating the concept of 'the scientific revolution' as it is used today. In his survey of historiography on the scientific revolution H. F. Cohen says, 'As a conceptual tool for understanding the birth of early modern science, [the term "scientific Revolution"] was created by Alexandre Koyré in the thirties' (Cohen, 1994, p. 21). Shapin says in his book on the Scientific Revolution, 'The phrase "the Scientific Revolution" was probably coined by Alexandre Koyré in 1939' (Shapin, 1996, p. 2). Before these writers, however, Guerlac attributed the creation of the concept to Auguste Comte: 'So far as I can discover, Comte was the first to conceive of, and to baptize, the Scientific Revolution' (Guerlac, 1963, p. 805). An in-depth analysis of the usage of the

descriptions of the changes brought about by the scientific revolution, Koyré says, but,

> they seem to me to be reducible to two fundamental and closely connected actions that I characterised as the destruction of the cosmos and the geometrization of space.[5]

By these two actions Koyré meant that the idea that the world is a 'finite, closed and hierarchically ordered whole'[6] ceased to be a valid conception of the world from a scientific and philosophical point of view; this is the destruction of the cosmos. In its place arose the idea that space is identical to Euclidean space, infinite in all directions; this is the geometrization of space. Such is the formula given throughout Koyré's many works as capturing the essence of the Scientific Revolution.

Koyré's position as one of the greatest historians of science of the twentieth century rests on his four major works: *Galileo Studies* (1939), *From the Closed World to the Infinite Universe* (1957), *The Astronomical Revolution* (1961), and *Newtonian Studies* (1965).[7] While emphasizing different facets of his conception of the scientific revolution, all contribute toward forming that complete picture using the above-described methodology.

It is the general reputation of Koyré that he falls squarely within the internalist camp in the study of the history of science.[8] That is, it is maintained that he focuses on the history of the ideas themselves, eschewing cultural and sociological interpretations regarding the influence of ideologies and institutions on the development of science. When we read what Koyré has to say *about* his historical studies (and most of what others have said about them), we find him embracing and championing this Platonic view of his work. Ultimately I think this interpretation of Koyré's history of science is lopsided and in need of correction; I shall argue below that a careful reading of Koyré's work suggests that a tension exists between internal and external methodological considerations, and that he is better classified as a Hegelian. But first I will explore the evidence for the

term 'revolution' as applied to science as well as of 'the Scientific Revolution' is carried out in I. B. Cohen's *Revolution in Science*. Cohen notes that many science writers of the early nineteenth century referred to the sixteenth and seventeenth centuries as 'revolutionary' (Cohen, 1985). But the first explicit reference to this time period as *the* scientific revolution he found in the writings of Louis Figuier, a medical doctor and professor at l'Ecole de pharmacie in Paris who wrote a five-volume work, *Vie des savants illustres*, in the middle years of the nineteenth century.

[5]Koyré (1957), p. viii.

[6]Koyré (1957), p. 2.

[7]The articles 'Galileo and the Scientific Revolution of the Seventeenth Century', 'Galileo and Plato', 'Galileo's Treatise "De Motu Gravium": The Uses and Abuse of Imaginary Experiment' and 'An Experiment in Measurement' are also frequently cited and are collected in *Metaphysics and Measurement* (1968). Finally, the lengthy article 'A Documentary History of the Problem of Fall from Kepler to Newton' has been influential; it was published in 1955 in *Transactions of the American Philosophical Society* **45** (IV), 329–395.

[8]A notable exception to this is Yehuda Elkana. His interpretation of Koyré will receive attention below.

traditional interpretation (which is seemingly Koyré's own interpretation) of his work.

1. Koyré the Platonist

I cannot find a more characteristic self-avowal of Koyré's position than the following excerpt taken from the introduction he wrote for the 'Exact Sciences' section of Vol. II of Taton's *History of Science*:

> Our approach has the advantage of emphasizing the inherent logic of historical processes that would otherwise seem quite haphazard. In fact, many of the external influences which historians have called turning-points in the history of science are completely illusory. Thus the appearance of the cannon did not cause the emergence of the new dynamics—it was precisely the behaviour of cannonballs that Leonardo da Vinci, Tartaglia and Benedetti were unable to explain. The needs of navigators, of ecclesiastical calendar computers and of astrologers ought to have led to the correction of existing astronomical tables, but they did not; nor did they persuade Copernicus to change the traditional order of the celestial spheres and to place the sun at their centre. Commercial needs and the rise of banking certainly helped to spread elementary mathematical knowledge, but they cannot explain the spectacular advances made by early 16th-century Italian algebraists, nor their systematic attempts to 'symbolize' arithmetical and algebraic operations. However, though the series of events constituting the evolution of mathematics, astronomy and physics cannot be explained in isolation—it is always vain to 'explain' an invention or a discovery—they can at least be made intelligible. The history of scientific thought cannot content itself with less or demand more.[9]

To make the evolution of science intelligible—this is what Koyré saw his task to be. Consider also what I. B. Cohen, one of Koyré's closest confidants, said of him in a memorial piece: 'He was in his heart of hearts a Platonist.'[10] Koyré says he wants to make the evolution of science intelligible, and Cohen says Koyré is a Platonist. From these two clues we can see the basis for the traditional interpretation of Koyré's work. I suggest such an interpretation as follows.

The role of Platonism looms large in Koyré's reconstruction of the rise of modern science. The creators of modern science were influenced by Platonic ideas about the nature of reality and about the role of mathematics in relation to the natural world. In creating their theories, they were dealing with bodies belonging not to the observable world of qualities and friction, but to the abstracted world of precision. But these were not merely abstracted, unreal entities fashioned by the imagination for the purpose of tidier calculations; rather the abstract objects became the real entities of the world—inhabitants of the ideal realm of being. Koyré says:

> It is not the *phenomena*, but the *noumena* or the *noeta* that find themselves bound together by the causally unexplained or even unexplainable laws. Indeed, not bodies

[9]Koyré (1964), p. 11.

[10]I. B. Cohen (1966), p. 164. Cf. also Gillispie: 'For Koyré was ever a Platonist' (Gillispie, 1973, p. 483).

> of our common-sense world, but abstract, Archimedean bodies of the Galilean one, or the particles and atoms of the Newtonian world, are the *relata* or the *fundamenda* of the mathematical relations established by modern science.[11]

But now, if we take Koyré as himself being a Platonist in his own sphere of inquiry, a similar description of his work can be given. Just as Galileo dealt with his objects of inquiry—bodies in motion—in the abstract, free from the influences of friction or air resistance, so too Koyré's is a purely intellectual—or intelligible—account of the development of science abstracted from the messy details of economic systems, power structures, and the contributions of artisans.[12] His is an account of the way that science would go in idealized situations, and this can be arrived at in the same way that Galileo demonstrated his theories: through conceptual analysis.[13] Using this method Galileo and Koyré work through the conceptual puzzles step by step so as to reveal the inner structure of those concepts and therefore lead the reader to the knowledge of a conclusion by deducing it. Recall that knowledge, for the Platonist, is only of necessary things, of things which cannot be otherwise.[14] For Koyré, then, to recount the history of science through demonstration and conceptual analysis is to show that the concepts unfold according to their own inner logic, just as the laws of physics govern the path of a projectile—though in both cases the actual facts may deviate from their idealized descriptions. It is here in this idealized conceptual realm that the important truths about the history of science are found. It is as such that the history of science is made intelligible. Here, then, is Koyré's Platonism: we must look past the fleeting sense images to the ideal realm underneath; it is the ideal realm which is the reality with which we should be concerned, and it is in this realm alone that understanding can take place.

Koyré's self-avowed idealism is further seen in his comments at the 1961 symposium on the history of science at Oxford. He was one of the respondents to a paper delivered by Henri Guerlac in which the then current state of the study of

[11]Koyré (1955), p. 110. The connection between statements like this one and Koyré's involvement with phenomenology seems fruitful to explore. In a letter to Herbert Spiegelberg dated 1953, Koyré says: 'Now for your question, how far I am still a phenomenologist—I don't know myself. I have been deeply influenced by Husserl, probably learnt from him—who didn't know much about history—the positive approach to it . . . I inherited from him the Platonic realism that he discarded; the antipsychologism and the anitrelativism' (quoted in Jorland, 1981, pp. 27–28n.). Karl Schuhmann (1987) should also be consulted on this. Interestingly, he quotes Husserl as saying, in a letter to Roman Ingarden, 'Koyré est phénoménologue de bout en bout' (p. 160).

[12]It has been claimed that Koyré ignored more than these in his recounting of idealized history. Finocchiaro (1977) charges him with 'over-simplification, injudicious exaggerations, and questionable manipulation of the text by means of excessive quotations, of taking passages out of context and of not infrequent scholarly carelessness' (p. 27).

[13]See 'Galileo and Plato', reprinted in Koyré (1968), for his discussion of Galileo's debt to the methodology of Plato. Koyré says of his own work in the preface to his *Newtonian Studies* (Koyré, 1965) that '[My] central theme is the illustration by means of conceptual analysis of the way in which fundamental scientific ideas are at the same time related to the main currents of philosophical thought and yet determined by empirical controls' (p. vii).

[14]Koyré (1968) quotes Galileo saying as much (p. 42).

the history of science was criticized as being idealist, that is, for neglecting the link between the pure and applied sciences through excessive specialization, and thus distorting the picture of science.[15] Koyré suggests that such a criticism might be projecting the current state of the enterprise of science back onto that of the scientific revolution. In sciences like chemistry and electricity there might be more of a hard and fast link between the applications and the development of theory. But these are modern occurrences, he claims: 'the real joining together of *techne* and *episteme* is a modern, and in certain fields, even a contemporary phenomenon'.[16] And even today, he claims, it is the applications which result from the discoveries—they do not inspire discoveries.[17]

With regard to the charge of excessive specialization Koyré indeed finds it regrettable, but: 'What is to be done? Specialization is the price to be paid for progress.'[18] He claims that our thought is abstract and analytical and that the whole cannot be comprehended without distinguishing its various parts. But even though the idealist necessarily writes an abstraction of the actual course of the history of science, this abstraction does get at the essence of science. He summarizes his position:

> Thus, it seems to me—and if it is idealism, *tant pis*—that science, the science of our epoch, like that of the Greeks, is essentially *theoria*, a search for the truth, and that as a result of this fact it has, and has always had, value as an end in itself, and an inherent and autonomous—though not always regular and logical—development, such that it is only by the study of its own problems, its own history, that it can be understood by historians. I even believe that it is just in the fact of this autonomous development—and not in the increasing influence of science on the concrete conditions of life—that lies the great value of the history of science, of scientific thought.[19]

From Koyré's emphasis on the 'inherent and autonomous development' of science, his much discussed view on the role of experiment is an outgrowth. In Koyré's idealized realm, 'good physics is done a priori'.[20] In Koyré's account, 'Galileo did not learn *his* business from people who toiled in the arsenals and shipyards of Venice. Quite the contrary: he taught them *theirs*.'[21] It may seem that the development and use of controlled experiment, at the very least, helped to foster the scientific revolution; but for Koyré this is to allow 'air resistance' or 'friction' to interfere with the trajectory of his idealized account, and it would therefore misinform us of the true nature of the thing in itself. Hence his account of the relationship between experiment and theory:

> I am convinced that the rise and growth of experimental science is not the source but, on the contrary, the result of the new *theoretical*, that is, the new *metaphysical*

[15]Guerlac (1963), p. 812.
[16]Koyré (1963), p. 854.
[17]Koyré (1963), p. 856.
[18]Koyré (1963), p. 851.
[19]Koyré (1963), p. 856.
[20]Koyré (1978), p. 166.
[21]Koyré (1968), p. 17.

> approach to nature that forms the content of the scientific revolution of the seventeenth century, a content which we have to understand before we can attempt an explanation (whatever this may be) of its historical occurrence.[22]

It is the ideas themselves that are primarily responsible for the development of scientific theories rather than any contact theories might have with the business of the world. Not only is good physics done *a priori*, but Koyré under this interpretation is charged with holding that good history might also be done *a priori*.[23]

2. Some Problems with Koyré's Idealism

While there is no denying this idealist, internalist aspect of Koyré's work, I don't think it can be the whole story, as many seem to think. If it were, there would be little point in continuing to read Koyré's works as guides to history, because that completely internalist methodology can be shown to have serious difficulties—the lenses are too distorting. Consider two illustrations of this.

Koyré boldly announced in a 1952 lecture: 'It is obvious that the Galilean experiments are completely worthless: the very perfection of their results is a rigorous proof of their incorrection.'[24] But since that time a consensus has developed against Koyré's claims about the role of experiment.[25] In 1961 Thomas Settle, who was then a graduate student of Guerlac's, published a report about Galileo's experiment described in the third day of his *Two New Sciences* concerning balls rolling down inclined planes. Settle (1961) reconstructed the experiment using only materials and techniques available in Galileo's day, and he found that Galileo really could have obtained the kind of experimental results from this experiment to establish the science of naturally accelerated motion that he claimed he did.[26]

Interestingly, Koyré himself began to have doubts about the strong internalism which he attributed to himself. Gillispie recounts a conversation he had with Koyré during his last winter in America: 'Probably, he [Koyré] reflected, he had overstated the Platonism of the scientific revolution. He might (he felt) have examined more sympathetically the role of experiment there at the dawn of modern science.'[27]

Secondly, the above account of Koyré as Platonic idealist forcefully suggests a method of historical writing that Lakatos would champion a few years after Koyré's death: that of rational reconstruction. But this is not what Koyré was doing in his historical examinations. He was not writing an 'improved version' of the historical facts or relating the 'internal history in the text and indicat[ing] in the footnotes

[22]Koyré (1965), p. 6.

[23]Such is the suggestion of Gad Prudovsky (1997b), p. 74.

[24]Koyré (1968), p. 94.

[25]See, for example, Gillispie (1973), p. 487; I. B. Cohen (1987), p. 58 and n. 3; Crombie (1987), p. 86; A. Rupert Hall (1987), p. 488; and Marie Boas Hall (1987), p. 231.

[26]Settle's experiment was just the beginning of a movement to investigate the legitimacy of Galileo's experimental claims. Much of the results of this movement can be found in Stillman Drake's 1990 book, *Galileo: Pioneer Scientist*.

[27]Costabel and Gillispie (1964), p. 155.

how actual history "misbehaved" in the light of its rational reconstruction'.[28] Koyré always aimed to represent the development of historical ideas faithfully, frequently quoting extended passages from the original authors. Of course, any writing of history involves the selection of facts to recount, and Koyré had his lenses through which he viewed historical development. But the coherence he sought in historical developments was not purchased at the expense of fictionalizing actual details of history.[29] The idealized, completely internalist account which is usually attributed to him does not do justice to his historical work itself.

The exclusively internalist picture of Koyré will not do. If that is all Koyré had done, then his works would have value now only as pieces of history themselves. But they continue to hold value as historical examinations of the scientific revolution. And when Koyré's historical work itself—not just what he says about it—is studied, a different emphasis within that work emerges, one which is rarely acknowledged, and yet which better explains the enduring relevance of his work. Furthermore, it suggests a different philosophical label we might affix to Koyré rather than Platonist, namely, Hegelian.

3. The Unity of Human Thought

In reading the historical works of Koyré, I find that one theme stands out above the others: the unity of thought. I mean by this phrase that none of our beliefs stands alone, free from the influence of other beliefs. Rather, they are connected to and dependent on other beliefs for their justification and even for their original formulations. In this section I will explicate more fully this notion of the essential unity of thought and note its implications for Koyré's history of science.

Although he doesn't explicitly discuss it as such, I think we can say that Koyré's concern is not with assessing the truth of the beliefs of scientists he is studying, but rather with the rationality of their beliefs—he seeks to understand their beliefs on their own terms. The rationality of a given belief is largely dependent on its coherence with other beliefs in the system or web of beliefs. It would not have been rational for someone in the thirteenth century to believe that the distance to, say, the bright summer star Deneb is over ten billion million miles from earth. Theirs was a picture of the cosmos as a hierarchically structured system of concentric orbs with definite limits; such a distance would have been inconceivable to them. It was more rational for them to think of the universe as some 125 million miles in diameter, centered on the earth;[30] this belief was connected to many other beliefs which functioned as an explanatory network for their experience of reality. To accept the modern belief of the size of the universe would have meant rejecting their own belief and calling into question the larger network of beliefs to which

[28]Lakatos (1971), pp. 106–7.
[29]Cf. Prudovsky (1997a) for an example of this methodology in Koyré's work.
[30]Koyré (1957), p. 34.

that belief was connected. To do so without very strong reasons could only be seen as irrational.[31]

So, beliefs are not held in isolation from other beliefs. And the rationality and the intuitive plausibility of a belief are largely derived from the context in which that belief is held. In his *Galileo Studies*, Koyré sets out to show that the laws of motion which seem so evident and simple to us today were not at all evident and far from simple for those who toiled in developing them. He makes the point that such simplicity and self-evidence as a belief seems to possess comes from the complex of concepts and beliefs of which it is a part, rather than from its own nature:

> Could it be that we have stumbled here upon an indication that this simplicity of the law is only apparent? Or, to put it another way, does not this indicate that the law of falling bodies is only simple if it is located within a particular system of axioms, and if one starts with a particular set of concepts? In other words it presupposes and implies a certain number of specific concepts—concepts of space, of causality, of motion.[32]

For a scientific belief to be held, for example, that the orbits of the planets are elliptical, it must reasonably cohere with the beliefs that have already been accepted. Among these beliefs might be the metaphysical belief in the perfection of the heavenly bodies and the consequent belief that they only move in perfect circles. Such a metaphysical belief, then, constrains the available options for scientific beliefs. Only once this belief had been abandoned could ellipses be accepted—or, what is historically more accurate, the acceptance of the ellipses finalized the abandonment of the circle dogma.[33] Then the belief in ellipses entered into the noetic web, and any future beliefs which might be accepted—whether scientific or metaphysical—had to pass the test of cohering with that belief (or causing it to be abandoned).

The occurrence of the scientific revolution, with which Koyré is concerned, was not a case, then, of there finally arriving on the scene people who were smart enough to realize that the earth orbited the sun in ellipses and that bodies in motion continue unless some force stops them. Rather, those responsible for the scientific revolution:

[31]I have purposely left 'very strong reasons' vague. It is not my aim here to discuss just what constitutes rationally acceptable theory change, but rather to show that rationality is largely a function of a belief's relation to already accepted beliefs—regardless of their truth value. Nor am I committed to claiming that rationality is entirely a matter of internal coherence of one's belief system. Defending such a claim properly would take us too far afield in this study.

[32]Koyré (1978), p. 66.

[33]But note the constraining role that the circle dogma played for Kepler: according to the record of his inquiries into the matter it was only after nearly exhausting every other possible alternative that he allowed the force of the empirical evidence to alter his metaphysical assumptions. Moreover, other beliefs about the nature and motion of the heavenly bodies had already been questioned. This had the effect of weakening the complex of beliefs so that Kepler was able to break free from it.

> had not to 'discover' or to 'establish' these simple and evident laws, but to work out and to build up the very framework which made these discoveries possible. They had, to begin with, to reshape and to re-form our intellect itself; to give to it a series of new concepts, to evolve a new approach to being, a new concept of nature, a new concept of science, in other words, a new philosophy.[34]

The framework of thought had to be revised in order to accommodate the postulates of the new science. It was only then that the beliefs could be viewed as rational beliefs. This speaks to the fundamental role of the unity of thought.

As some beliefs in the interconnected web of beliefs change, revisions are forced (under pain of logical contradiction) in others. The more firmly entrenched a belief is, that is, the more connections it has to other firmly held beliefs (or metaphorically, the closer it is to the center of the web), the more difficult it will be to remove it, and the more drastic ramifications it will have throughout the web if it is altered. It is when this happens that we point to such instances as revolutions in thought: The replacement of the geocentric system by the heliocentric system; the acceptance of random mutation and natural selection as a means of generating new species of life; the rejection of the relativity of the speed of light in different frames of reference—all of these signify the abandonment of firmly held beliefs, and thus they effected dramatic changes throughout entire belief systems. Of course it takes time to ferret out all the implications throughout the web when a new set of core beliefs is accepted, but because of the different sets of core beliefs that are held, we can point to different worldviews that are held as a result of these 'revolutions' in thought.

4. Some Implications of Unity of Thought in Koyré

The bibliography and lecture record of Koyré manifest the thematic centrality of the unity of thought in his work. His earliest research was concerned with the religious ideas of Descartes, Anselm, and more significantly of German mystics like Valentin Weigel, Caspar Schwenckfeld and Jacob Boehme.[35] There was a natural transition, then, from studying these mystics and the magical elements of their thought to that of Paracelsus and then to the thought of Copernicus which was itself imbued with Renaissance magic. It is often suggested that Koyré abandoned his interest in religious thought when he published the translation and commentary of Copernicus in 1934, and then went on to the landmark *Galileo Studies* in the later years of the decade. But Koyré would not have been comfortable with describing his work in separate categories: the history of the scientific revolution for which he is primarily known was buttressed by the wider context of the religious and philosophical climate of the times. This theme surfaces emphatically

[34]Koyré (1968), p. 3.

[35]In the 1920s Koyré gave courses or lectures entitled 'Le mysticisme spéculatif en Allemagne' in at least six different years. See Koyré (1986), pp. 20–4.

in his *From the Closed World to the Infinite Universe*.[36] Koyré himself testifies to the fundamental role of the unity of thought within his historical research. He wrote in a *curriculum vitae* of 1951:

> From the beginning of my researches, I have been inspired by the conviction of the unity of human thought, particularly in its highest forms. It seems to me impossible to separate, into tight compartments, the history of philosophical thought and that of religious thought . . . But it was necessary to go further. I had convinced myself quickly that it was equally impossible to neglect the study of the structure of scientific thought . . . The evolution of scientific thought, at least during the period that I studied then, did not form an independent series, but was, on the contrary, very closely related to that of the transscientific ideas, philosophical, metaphysical, religious.[37]

I consider below some implications that result from Koyré's commitment to *l'unité de la pensée humaine*.

It is no accident that people of the same community (variously defined) have generally the same sets of core beliefs. This is partially because of the experiential component that must be attributed to belief formation: these people have access to basically the same experiences. But the homogeneity also stems from the theoretical structures that are imbibed from one's culture or community. If it is accepted that the noetic web functions as a kind of filter through which we interpret experience, then we can say that people of different settings view reality through different filters. Some of what seems intelligible to us today would not have seemed intelligible to those of other times. This is the historical relativity—the ability to see outdated scientific theories as rational in their historical contexts—which became the hallmark of Koyré's scholarship. And this is a direct result of the unity of beliefs.[38]

For example, Koyré was always careful to assess the debates of *From the Closed World to the Infinite Universe* from within the perspectives of both opponents (rather than from only our bird's-eye point of view), so that the reader can feel the clash of positions with their historical force. His discussions are replete with qualifications like:

> Kepler's refutation of the infinitist conception of the world may appear to the modern reader unconvincing and even illogical. Yet, as a matter of fact, it is a perfectly consistent and very well-reasoned argument.[39]

[36]Jorland (1981) discusses the interpenetration of Koyré's religious and scientific work, claiming that the supposition that Koyré abandoned his religious concerns goes against all the evidence (p. 43). Olesen (1997) offers a corrective to Jorland: 'Dieses Urteil bedarf wohl einer gewissen Modifizierung, da die Bibliographie Koyrés nach 1934 kein dezidiert religionsphilosophisches Werk enthält' (p. 41). It cannot but be conceded that there is a shift in emphasis for Koyré in the 1930s—for there are no publications in the history of science prior to an article on Copernicus in 1933. But Jorland's point remains that Koyré's subsequent history of science is very well informed by his broader understanding of the philosophical and religious beliefs of the period—this is the point of the unity of thought.

[37]Koyré (1973), pp. 11–12.

[38]It may be interesting to compare my characterization of the historical relativity of Koyré's work with Olesen's characterization of his work as a 'diachronic representation' (Olesen, 1997).

[39]Koyré (1957), p. 61.

> Henry More is perfectly right. On the basis of traditional ontology—and no one in the seventeenth century (except, perhaps, Gassendi . . .) is so bold or so careless as to reject it or to replace it by a new one—his reasoning is utterly unobjectionable.[40]

> It is only from the point of view of the Cartesio-Leibnizian rigid dualism of mind and body, with its negation of all intermediate entities and consequent reduction of material nature to a pure, self-sustaining and self-perpetuating mechanism, that the intervention in nature of non-mechanical and therefore non-material agencies becomes a miracle. For Clarke . . . this dualism is, of course, unacceptable.[41]

In a quintessential bit of historical exposition, Koyré explains the rationality of Henry More's fantastical description of the attributes of space and his claim that the concept of spirit is as easy to understand as that of matter:

> The modern reader will be right, of course, in rejecting More's concept, patterned obviously upon that of a ghost. And yet he will be wrong in assuming it to be pure and sheer nonsense.[42]

Koyré carefully details the many examples accepted by people of the seventeenth-century of extended, though immaterial, substances: there were light, magnetism, gravity and the ether. Beliefs about the nature of these 'substances' provided a context of belief in which it was not 'sheer nonsense' for More to hold the belief—contra Descartes—that spirits are extended.

Thus, we can see that what I have called Koyré's historical relativism is a manifestation of his more fundamental commitment to the unity of thought. Gillispie says of Koyré that his most characteristic gift as a scholar was his 'ability to enter into the world of his subject and evoke for the reader the way in which things were then seen'.[43] Understanding scientific theories in their historical contexts, evoking the way in which things were then seen, is a matter of seeing those beliefs situated among the complex of other beliefs which were conditioned by their historical circumstances.[44]

Another of the consequences of Koyré's commitment to the unity of thought is

[40]Koyré (1957), p. 146.
[41]Koyré (1957), p. 258.
[42]Koyré (1957), p. 130.
[43]Gillispie (1973), p. 484.
[44]Consider one further quotation from a different work which nicely displays the relativity of rationality: 'The objection will perhaps be raised that one really cannot understand how Galileo could have believed in the possibility of so unreal a method as that which he had invented. And still less, certainly, in its reality. Surely that would appear very improbable. Let us not forget, however, that for the minds of the seventeenth century the frontier between the believable and the unbelievable did not lie exactly where it does for us. Did they not believe, the majority at least, in a finite world bounded by the celestial vault outside of which there was rigorously and absolutely nothing? Furthermore, did they not hold that the world had been created at a given moment, and not very long ago, in the past? Did not Newton himself believe that God had placed the heavenly bodies at their "proper" speeds which would be necessary for them to accomplish their revolutions? Why could Galileo not have believed that God had—or, at least, that he could have—used the mechanism of falling? Is this not a most elegant way, and the only natural one, to give to a body a particular speed? Did not Galileo himself use it in his own theory of projectiles when, as we have seen, in order to give his projectiles a *horizontal* speed, he has them fall from a given height instead of giving them this speed directly? The term "sublimity" itself which he uses—is it not revealing and significant?' (Koyré, 1965, p. 220).

that there is a reciprocal relationship between scientific beliefs and philosophical beliefs. He says in an article to a lay audience:

> It is, indeed, my contention that the role of this 'philosophic background' has always been of utmost importance, and that, in history, the influence of philosophy upon science has been as important as the influence—which everybody admits—of science upon philosophy.[45]

Within a web of beliefs, the acceptance of a philosophical belief must not be in conflict with the other accepted beliefs—among which are scientific beliefs—and vice versa. This means that when the weight of evidence for a new scientific belief is great enough, then if its acceptance has ramifications throughout the web, philosophical beliefs will be influenced; and, again, vice versa.

In the preface or introduction to each of Koyré's book-length works there is a more precise avowal of his belief in the intertwined character of the scientific revolution in terms of philosophy and science (and sometimes religion or theology). Let one quotation suffice, from *Closed World*:

> This scientific and philosophical revolution—it is indeed impossible to separate the philosophical from the purely scientific aspects of this process: they are interdependent and closely linked together . . . [46]

We can develop the notion of the unity of human thought further. Koyré speaks of the revolution in European minds which 'changed the very framework and patterns of our thinking'.[47] Again, the victory of modern science 'was not a matter of battling against theories which were simply inadequate or erroneous, but of changing the very intellectual framework itself'.[48] This intellectual framework that had to undergo revision is the core network of beliefs against which other beliefs are held; it is the 'worldview' which is espoused.

5. Why Not Sociological Factors?

But we must now ask why for Koyré the context of ancillary beliefs which are admitted as influencing the development of science is limited to the '*transscientifiques*' that he mentions: philosophy, metaphysics and religion. Isn't it the case that other kinds of beliefs contribute to the context within which scientific beliefs are held? Shouldn't political ideologies and other social factors contribute to the worldview? Yehuda Elkana (1987) has given an interpretation of Koyré in which he argues that Koyré is implicitly committed to a such a view; in fact, 'He actually was among the creators of the historical sociology of scientific knowledge.'[49] Such a claim speaks directly against Koyré's reputation and against his own stated views

[45]Koyré (1955), p. 107.
[46]Koyré (1957), p. 2.
[47]Koyré (1957), p. vii.
[48]Koyré (1978), p. 3.
[49]Elkana (1987), p. 117.

of his work; yet it seems a natural development or extension of Koyré's commitment to the unity of thought, and so demands our attention.

Elkana's thesis depends on the distinction between what he calls the body of knowledge and images of knowledge. In the scientific sense, the former is the set of mathematically derivable and experimentally testable theories about the world; these are ideas people have about the world.[50] It is Koyré's lasting legacy that he has shown how this body of knowledge has been influenced by non-testable, general views of the world, that is, by metaphysics. What has not been acknowledged openly by Koyré and his commentators, according to Elkana, is that those metaphysical beliefs which influence the body of knowledge result from images of knowledge, that is, from ideas about knowledge as opposed to ideas about the world. These include things like the commitment to a mechanical or to a mathematical view of nature, Newton's quest for simplicity and unity in his theories, or Kepler's systemicity of knowledge as a criterion of truth.[51] It is Elkana's claim about the images of knowledge that:

> They are the kind of ideas that result from very complex processes of cultural change, involving the modification of man's position in the universe, his relation to God, etc. In this sense they are socially determined, and hence, only historical studies of socio-political-economic context can explain their genesis and spreading influence.[52]

So we have the thesis that socio-political-economic factors determine a specific culture's images of knowledge; then these images of knowledge give rise to general, metaphysical views of the world; and finally it is these worldviews that interact with and govern the development of science. Now it is not the case that Koyré objected to this thesis of the social determination of what Elkana calls images of knowledge—he did not specifically address the question of why, for example, there was a revival of Platonism. Rather he was concerned with how the images of knowledge of a particular cultural setting, say, the acceptance of a Platonic view of reality, affected the body of knowledge. So scientific knowledge, in Koyré's view, was not directly influenced by the social and political kinds of influences. But because of his emphasis on the importance of images of knowledge—which are socially determined and from which are derived the great metaphysical generalizations that guide the development of science—Koyré's contextual analysis comes very close to a kind of sociology of knowledge in Elkana's estimation.[53]

I am not inclined to go as far as Elkana does in this respect. The case he builds is far from convincing.[54] He can only offer quotes from Koyré that speak to the importance of spiritual changes—which Koyré already openly admits; and he

[50]Elkana (1987), p. 117.
[51]Elkana (1987), p. 119.
[52]Elkana (1987), p. 118.
[53]Elkana (1987), p. 118.
[54]But it is only fair to note Elkana's admission that a book-length work would be necessary to make his point convincingly; here he can only offer a few examples (Elkana, 1987, p. 118).

asserts that images of knowledge are socially determined without arguing for this claim, much less showing that Koyré is committed to it.[55] Still, it is not difficult to see how the sociological treatment of science is but a natural step on from Koyré's emphasis on the unity of thought and the importance of context in understanding the ideas on which he concentrated. Kuhn said of Koyré's work: 'Perhaps that strict concentration on ideas was prerequisite to the historiographical transformation Koyré's work induced.'[56]

So maybe we can transpose Elkana's assessment into the terminology I have been using, and thus capture the thrust of the contextual strain of Koyré's thought without committing him to more than his texts warrant. For Koyré, knowledge itself is not relativized to a framework of other beliefs, and thus he could say that concentration on the internal, autonomous aspects of science is the proper approach to the understanding of these ideas. But as we have seen, for Koyré the *rationality* of knowledge claims is relativized to the noetic context in which such claims take place. Because Koyré did not explicitly separate these two different aspects of his study—viz., the development of ideas and the rationality of the ideas espoused—an inherent tension arises between the two strains of thought. That is, as Elkana has said, there is a tension between 'the need to trace the inner logic of the development of ideas on the one hand, and the recognition that ideas are meaningless outside their context on the other hand'.[57]

6. Koyré the Hegelian

So what are we to make of these two strains in Koyré—the internal and the contextual? I do not think that there is a tidy, linear resolution of Koyré's thought

[55]Consider this characteristic passage in which Elkana compares Koyré to Merton and Kuhn: 'Koyré, like Kuhn and Merton knows that important influences of problem-choice and other aspects of scientific processes are exercised by what is thought about sources, aims, kinds of legitimization, etc., of knowledge. However, Koyré studies these images of knowledge the way a comparative epistemologist would do and connects them with the historical background in some detail. He does not abdicate from finding the historical explanation for the emergence of socially determined ideas about knowledge, as Kuhn does, but he rather limits himself to research of the images of knowledge themselves, and unlike Merton chooses to leave aside the various economic, institutional and ideological factors involved in their creation' (Elkana, 1987, p. 116). No examples are given of how Koyré gives such 'historical explanations', nor is it argued that Koyré accepted (or even would accept) the blithe assertion that ideas of knowledge are socially determined. Indeed, Elkana admits that Koyré chooses to 'leave aside the various economic, institutional and ideological factors'. What else remains by which Koyré can be interpreted to be engaged in sociology of knowledge?!

Also, I would like to see Elkana place more of an emphasis on the dialectical process by describing the reciprocal influence that science can have on culture. His seems to be more of a one-way, linear line of influence from social factors, to images of knowledge, to metaphysical views, to scientific beliefs.

[56]Kuhn (1970), p. 69.

[57]Elkana (1987), p. 122. Elkana is not alone in recognizing this tension in Koyré's work. Amsterdamski notes: 'D'un côté . . . il traite la science, y compris sa méthode, comme un produit de l'histoire, il la relativise donc aux conditions dans lesquelles elle est produite; de l'autre côté, il se prononce contre toutes les explications sociologiques et parle de l'évolution du savoir comme du cheminement de la pensée vers la vérité, cheminement qui s'accomplit grâce à la géométrisation et mathématisation de la structure ontologique du monde, comme si les conditions dans lesquelles cette pensée s'était formée ne comptaient guère' (Amsterdamski, 1987, p. 109).

in this respect—the opposition runs deeper than that. There is an irreconcilable tension in Koyré's work between the unfolding of the inner logic of scientific ideas and the importance of their extra-scientific context. So, the charge of idealism or Platonism applies only to one strain of Koyré's thought; there is another opposing, contextual strain that must be accounted for in an accurate interpretation of the totality of his thought. This fundamental or inherent opposition suggests a different philosophical label which may be more appropriate to apply to Koyré: Hegelianism.

That Koyré was interested in the philosophy of Hegel is evident from a perusal of a bibliography of his works. Among them we find: 'Note sur la langue et la terminologie hégéliennes' (1931), 'Rapport sur l'état des études hégéliennes en France' (1931), 'Hegel à Iéna' (1934), 'Hegel en Russie' (1936).[58] Besides these, also testifying to Koyré's interest in Hegel is a paper written about Koyré after his death, detailing the impact he had on the course of Hegel studies in France during this century.[59]

Of course, writing about Hegel at one point in your career does not make you a Hegelian.[60] But the company Koyré kept gives further reason to suppose that Hegelian influences were operating in the development of his thought. During the early 1930s he belonged to a group of intellectuals in Paris which included the self-avowed Hegelians Emile Meyerson and Alexandre Kojève. The group's discussion was supported by Koyré's teaching on the Hermeticism of the Renaissance, and according to Redondi, Koyré's work on Paracelsus caught the attention of the historians Lucien Febvre and Hélène Metzger—the latter having proposed that Koyré become a member of the history of science section at the Centre de Synthèse.[61] Koyré was then asked to give a conference or series of lectures on 'Les débuts de Galilée'. It is plausible to suppose that the Hegelian tendencies of the discussion group to which he belonged during these formative years influenced the way he interpreted the history of science.[62]

Still, Koyré does not, so far as I can find, explicitly connect Hegelian philosophy to his interpretation of the history of science.[63] But in considering several of the important themes that emerge throughout his works, the groundwork can be laid for characterizing Koyré as a Hegelian.

I think it is usual to interpret Koyré as claiming that there is a discontinuity between successive theories of physical science or, if we may import Kuhn's ter-

[58]The first two articles are included in Koyré (1971); the third was published in Koyré's *Etudes d'histoire de la pensée philosophique, en Russie*, Paris: Vrin. (1950).

[59]Wahl (1965).

[60]Koyré had also written about Plato: his *Discovering Plato* was first published in 1945.

[61]Koyré (1986), p. 33.

[62]It is not without consequence for this point that *Galileo Studies*—the book which emerged from this conference (and Koyré's first major study in the history of science)—was dedicated 'To the Memory of Emile Meyerson'.

[63]In 'Newton and Descartes', Koyré says: 'Human thought is polemic; it thrives on negation. New truths are foes of the ancient ones which they must turn into falsehoods' (Koyré, 1965, p. 65). This has obvious affinities with Hegel's thought, but Koyré does not mention it by name.

minology, between paradigms.[64] Koyré himself says in the introduction to his *Galileo Studies* that the necessity of changing the intellectual framework explains why,

> [i]n spite of appearances to the contrary—appearances of historical continuity to which Caverni and Duhem give such emphasis—classical physics, issuing from the thought of Bruno, Galileo and Descartes, was not in fact continuous with the mediaeval physics of 'the Parisian precursors of Galileo': it was from the very beginning located on a different terrain, a terrain that we would like to define as Archimedean.[65]

But this testimony and the theme of the Galileo book must be contrasted with that of *From the Closed World to the Infinite Universe*. For in the latter the emphasis is on the gradual steps that had to be made in overcoming the conceptual obstacles—several times Koyré says: 'the world-bubble has to swell before bursting'.[66] The idea here is that obstacles could only be overcome a little bit at a time, and rarely more than one would be overcome by any individual thinker: Copernicus gave us heliocentrism, but left crystalline spheres and perfect circles; Brahe got rid of the spheres but left circles and geocentrism; Kepler gave us elliptical orbits; Galileo and Descartes developed inertia; Newton discovered the laws of universal gravitation. Each of these new steps in modern science built on the previous ones. Here Koyré's story sounds more like an account of continuity, in which later developments carry over the successes of the previous thinkers rather than making a complete break with what went before.

These two different emphases can be reconciled with a Hegelian notion, viz., that when enough *quantitative* changes mount up, a *qualitative* change can be effected. That is, when we look at the two theories at either end of the processes Koyré discussed—impetus dynamics and Galilean dynamics, or a closed cosmos and an infinite universe—the two are quite clearly 'located on different terrains'; the concepts employed, such as 'motion' and 'space', have undergone dramatic and revolutionary changes. And yet to get from the beginning theory to the end theory, human thought did not take one sudden step. Rather, there were many cautious and tentative steps along the way that together amounted to a revolutionary transformation of thought. This is the sense of Koyré's comment: 'Revolutions, too, need time for their accomplishment; revolutions, too, have a history.'[67] Now I do not think we can say just how much of a difference in degree it takes to effect a difference in type; what is clear, however, is that Koyré is committed to this kind of a Hegelian mechanism.

Also, consider Koyré's emphasis on error. Among Koyré's commentators, the vast majority of whom flatly label him an internalist and a historian of disembodied

[64]Cf., for example, Jones (1989), Lindberg (1990), Shapin (1992) and Ariew and Barker (1992).
[65]Koyré (1978), p. 3.
[66]Koyré (1957), pp. viii, 35.
[67]Koyré (1957), p. viii. Consider also a parallel passage: 'The Galilean and Cartesian revolution—which remains, nevertheless, a revolution—had been prepared by a strenuous effort of thought' (Koyré, 1968, p. 1).

ideas, there is very little mention of the Hegelian framework of Koyré's thought.[68] One exception is found in a paper by Mario Biagioli (1987). The paper mainly concerns the work of Emile Meyerson and his concept of the irrational, but a comparison with the work of Koyré is drawn. Koyré was not content to recount the history of science as some of the positivists had: a chronicle of past successes. Rather, he suggested the errors and dead ends into which science has run can often be more instructive for us in understanding the thought processes that were present then. Koyré explicitly acknowledges the importance of error in history in many of his works, and the tracing of error takes an especially prominent role in his *Galileo Studies*, where in the search for the law of falling bodies the errors of Descartes and Beeckman mirror those of Galileo fifteen years earlier.[69]

Biagioli sees in Koyré's emphasis on error a development of the Meyersonian dialectic of science with the irrational. According to Biagioli, Meyerson looks to Hegel's dialectic for a suggestion of how to integrate the polarities of his own theory of scientific knowledge—these polarities being, on the one hand, the irreducibly dynamic (and hence irrational) character of nature as evinced by Carnot's principle, and on the other, his assumption of identity in time as the fundamental, unchanging category of thought.[70] Koyré transfers Meyerson's concept of the 'other'—the irrational—from the realm of nature into the arena of history, and so to his own idea of error. Biagioli's comparison of the two culminates in this paragraph:

> To Koyré, error was as necessary in science as 'the irrational' was in Meyerson's theory of knowledge, but, unlike 'the irrational', error did not derive from the presence of irreducible patterns of change in nature as indicated by the principle of Carnot. Error, instead, was produced during the historical development of a given discipline and through the work of specific individuals. It is a history-dependent and man-made error that, according to Koyré, is dialectically synthesized during the *cheminement* of science.[71]

So, in Biagioli's interpretation we have a further corroboration of my attributing Hegelianism to Koyré.

Still, I think the best evidence for calling Koyré a Hegelian comes from the deep-seated opposition that he is content to allow in his thought: that between the internal logic of ideas and the relevance of their context. That we cannot merely look at what Koyré himself has to say in assessing his work is apparent here. He would not have been pleased to hear his work called a stepping stone to the sociology of knowledge. But consider Emile Meyerson's remark in this context: '[Man]

[68]Another underexploited facet of the study of Koyré is the relation of his early writings on mysticism to his studies in the history of science. Marie Boas Hall and Elkana are among the few who have even begun to look at this connection.

[69]See the second part of *Galileo Studies* (Koyré, 1978): 'The Law of Falling Bodies: Descartes and Galileo'.

[70]Biagioli (1987), p. 177.

[71]Biagioli (1987), pp. 179—180.

does not see himself in the act of reasoning. He therefore does not know directly the way by which he has come to such or such a conclusion; the motives which have influenced him in adopting it may be very different from those which he himself supposes.'[72]

It is clear, I think, that besides the internalist strain of conceptual analysis that Koyré acknowledges, there is also present in his work the contextual strain of thought which comes very close to (or rather, is easily developed into) a sociological treatment of science. By the very fact that Koyré admits the '*transscientifiques*', he has opened the door for influences external to science itself. I think he would have to admit that beliefs other than those falling easily into just these categories had a role to play in the transformation of the intellect—even if they relate primarily to the context of beliefs against which the scientific beliefs are held.

It is hardly surprising to find that Koyré has read the history of science through lenses which were colored by the research he had conducted prior to (or in preparation for) his work on the scientific revolution. The discussion group, the study of Hegel himself, his early work on the German mystics[73]—all of these fostered a Hegelian framework of thought. But again, the claim is not that Koyré intentionally sought to force his historical work into such a mold; he charged that attempts to do so are artificial and without value.[74] His work is effective, rather, because the opposing strains of thought are seen in retrospect to coexist in tension with one another. And we see that these two opposing tendencies—the tracing of inner logic through conceptual analysis, and the importance of context due to the unity of thought—are synthesized in the body of Koyré's historical work itself, where they have produced one of the most important pictures of scientific development that has been offered.

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[72]Meyerson (1930), p. 7.
[73]Koyré's work on Jacob Boehme seems especially important in this respect. According to Nicolescu (1990), Boehme was another who explicitly sought 'to reconcile opposing principles, while preserving their specificity: the rational and the irrational, matter and spirit, finality and endlessness, good and evil, freedom and law, determinism and indeterminacy, the imaginary and the real' (p. 101).
[74]Koyré (1973), p. 148, n. 2.

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