

Peirce's Conception of Philosophy as a Science

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Introduction

The question about whether we should define philosophy as a science has a long history. Nevertheless, I will not adopt a historical point of view unless it is necessary to my present purpose. Rather than trying to ascribe Peirce's conception of philosophy a place in some general picture of history of philosophy, I shall confine myself to describing his position and its significance with regard to his philosophical project, and to evaluating the potential relevance it can have about contemporary problems.

It is well known that Peirce considered that philosophy should be a science: this claim provides us, I argue, with a key to the meaning of his whole philosophical program, which he tried to achieve or, at least, to initiate, with the hope that philosophers after him would work on the same task and follow the path he had explored. In order to make that first point clear, I should begin by examining the essential features that are, according to Peirce, essential to the concept of science. I will then be able to explain in what sense philosophy could and actually should be a science: in Peirce's view, philosophers should always aim at discovering the truth, which makes it necessary for them to conceive their activity as collective and based on some peculiar observation.

But to say that philosophy should be scientific also means that it should have a definite place in the architectonic of science, such as is exhibited in Peirce's classifications of the sciences. Peirce's point is, as a consequence of this interconnection of philosophy with the other sciences, that there must be some relationships between sciences. So I shall try to make it clear what these relationships consist in, and examine Peirce's concept of the unity of science and its consequences for philosophy. I'll study this concept by contrasting it with the analytic

concept of the unity of science: common features as well as sharp differences can be found.

This will finally lead me to some quick remarks about Peirce's activity as an experimental scientist, in order to evaluate whether taking it into consideration may shed some new and interesting light on his philosophical thought. I'll argue that some of Peirce's preoccupations can be more adequately understood if one refers them both to his activity as an experimental scientist and to his specifically philosophical concerns.

1. Peirce's concept of science and the project of a new philosophy

Peirce's reflexions about the nature and adequate definition of science are a leitmotiv in his writings, and it may be remarked that most part of what he wrote on these topics, as well as on classification of sciences, was intended to introduce his unachieved logic books. So that we can conclude that he considered these questions as logically important ones, questions indeed that would determine the task of one willing to devote to the study of logic and more generally of philosophy. I shall draw some more systematic conclusions from this fact later.

Peirce's concept of science opposes two definitions of science, the first one being as we shall see a mere refinement of the second one: these definitions can be summarized as D1. the kantian (or officially coleridgian) definition of science as systematized knowledge, i.e. the fact that science, contrary to ordinary knowledge or pre-scientific knowledge, is organized as a system; and D2. the cartesian definition of science as absolute knowledge, i.e. the fact that scientific method can lead us from absolutely undoubtable metaphysical premises to absolutely undoubtable conclusions. In Peirce's view, Kant's definition is but one version of the spirit of cartesianism. But both definitions miss the real essence of science. It must be remarked that Peirce always recommends himself from the scientific point of view, i.e. the internal perspective of the laboratory trained scientist, in order to show the inadequacy of classical definitions of science. In other words, philosophical reflexion about science has always been external and this could explain why it never ceased to consider science as a set of results rather than as an activity. This is the first critical argument of Peirce, meant to demonstrate the inadequacy of D1.

The claim to define science according to its systematicity can be traced back to Plato: see for example the distinction between science and true opinion:

When they [*true opinions*] are bound, in the first place, they have the nature of knowledge; and, in the second place, they are abiding. And this is why knowledge is more honourable and excellent than true opinion, because fastened by a chain. [*Meno*, 97e-98a].

But Kant offered a more elaborate version of this, in which the concept of systematicity is more explicitly exposed. In Kant's view science should ultimately take the form of a system, in which parts are rationally organized according to the idea of the whole. This description is supported by the consideration of the way knowledge gains some scientific status when it comes to find a unifying principle, rational and architectonic rather than empirical and rhapsodic. Peirce recognized the role of architectonic in science: he always tried to organize his own philosophy

according to such systematic criteria, and I think he broadly succeeded, though he was rather unsuccessful concerning the details. But he was more critical about the kind of systematicity and the origin of it that Kant's definition presupposes. In Kant's view, as well as for other philosophers from the German idealism trend, systematicity originates in the subject's reason, and may therefore be described purely *a priori*. To say it in few words, one could anticipate the end of science and so its final systematical design, by proceeding analytically from the concept of science to the idea of what it should be. According to Peirce, such a definition of science and its systematicity is "nothing but the last development of that sort of philosophy that strives to draw knowledge out of the depths of the *Ich-heit*", so that we find in it some essential features of the spirit of cartesianism, namely subjectivism and the recourse to it as a wide-range philosophical explanans.

Nevertheless, there are distinctive features of the Kantian definition, which require some specific critical approach. Peirce's main argument against it is, as I have just mentioned before, that it considers science only from its results, and not as an activity. What Peirce stresses is the philosophical tendency to draw from science only the results fitting the philosophical system:

The "philosopher" is a man with a system which he thinks embodies all that is best worth knowing [CP 1.44].

Whether Peirce was right or not with regards to history of philosophy is not my point. What is interesting in his criticism is rather that, as a consequence, he proposes to define science as a mode of life, i.e. according to the norms of conduct of those who occupy themselves with finding out truth. In other words, science is no abstract structure, but a living entity, the results of which are mere "exudations". According to this pragmatic definition, the main criterion for scientificity is not the systematicity of its results, but a sincere desire to find out truth, which all scientists have in common.

It makes no difference how imperfect a man's knowledge may be, how mixed with error and prejudice; from the moment that he engages in an inquiry in the spirit described, that which occupies him is science, as the word will here be used. [CP 7.54]

Science being defined as an inquiry with a specific goal, we may expect that it uses means more and more adapted in order to attain this goal. In this respect, some differences should be remarked, according not only to the theoretical results, but also to the methods used by scientists. Of course these are not elements that should be neglected in our description of science:

Science is to mean for us a mode of life whose single animating purpose is to find out the real truth, which pursues this purpose by a well-considered method, founded on thorough acquaintance with such scientific results already ascertained by others as may be available, and which seeks coöperation in the hope that the truth may be found, if not by any of the actual inquirers, yet ultimately by those who come after them and who shall make use of their results. [CP 7.54]

The quotation above tells us about the corollary requirements of a definition of science as a mode of life: adoption of an adapted method, acquaintance with previous scientific results, and long-run cooperation, so that to achieve the ideally determined inquiry. Looking at these requirements, it is easy to understand why Peirce was so critical about the Cartesian account of science. Though Peirce recognized Descartes' merit in insisting on the necessity of a method, he couldn't

accept other specific features of cartesian epistemology, namely subjectivism, the necessity of doubt as a first step in scientific inquiry, and foundationalism. I shall only summarize these criticisms.

Lots of commentators have put some emphasis on Peirce's well-known criticism of cartesian doubt: doubt cannot arise from a voluntary decision, but is always due to some external factors. Scientific inquiry aims at establishing stable belief, i.e. at appeasing doubt, which is a peculiarly uncomfortable state of mind. Again it's not my task here to examine in detail Peirce's arguments and evaluate their success. Whether they are convincing or not, I agree with Susan Haack that this is not Peirce's main point in the 1868 texts. At least, however, the insistence on the external factors seems to suggest something about Peirce's strategy, his scientific realism, and his view of thought as a public sign-process. This leads us to foundationalism: in Descartes' view, doubt is a necessary step insofar as we aim at building science upon undoubtable metaphysical foundations, of which knowledge of the subject by itself through the *cogito* is a good example. But according to Peirce, there is no such thing as an undoubtable foundation in science: every proposition could be rejected if the results of observation oppose it. It is not that, as the logical positivists, Peirce rejects metaphysical propositions as meaningless, but rather that he adopts a fallibilist position regarding science. As for doubt, intellectual intuition as a mode of knowledge has to be rejected, because it opposes the semiotic account of science defended by Peirce. But without intuition, it would be hard to find the kind of metaphysical foundations Descartes thought he had found. But the most important meaning of this rejection is, in my view, that intuition is a typically subjective operation. If we look at science as it is, in the way Peirce urges us to do, we clearly can see how Descartes' insistence on subjectivity is misleading. It is not, in that view, that Descartes was entirely wrong, and one could imagine that some scientific inquiry could be performed by a single person, but subjective inquiry cannot be considered as the most general case, and consequently it cannot play the paradigmatic role Descartes assumed it to play.

"Peirce
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wrong"

Now if we apply these views to philosophical inquiry, it is possible to understand what Peirce has in mind when he argues for the necessity of a "scientific turn" in philosophy. Such a conception implies that we should adopt some new attitude regarding both the goal and methods of our activity, or perhaps that we should conform to the primary sense of "philosophy". For, as Peirce writes:

Science and philosophy seem to have been changed in their cradles. For it is not knowing, but the love of learning, that characterizes the scientific man; while the "philosopher" is a man with a system which he thinks embodies all that is best worth knowing. [CP 1.44]

In that sense, defining philosophy as a science is nothing but insisting on its primary etymological meaning. As for science in general, Peirce's criticism against non-scientific philosophy concentrates first on the question of goals: philosophy cannot be scientific as long as it aims at something else than truth, like solving practical problems. This is why Peirce strongly distinguishes, or to say it in his own words, "contradistinguishes" two kinds of philosophers: seminary-philosophers and laboratory-philosophers. These two categories are different in many respects, but the main point is the theological influence on the former, through which philosophy loses its character of truth-oriented inquiry, and seeks rather to the amendment of

souls. Though Peirce finds nothing reprehensible in such a religious ambition – he even came himself to attribute a great role to religion in science in his latest writings – it has nothing to do with scientific spirit as long as it deals with practical questions. Furthermore, this could explain why so many theology-inspired philosophical inquiries hold for an achievement the constitution of a system: in Peirce's view, as we have noticed before, philosophical systems are arbitrary constructions, in that their authors only take into account the very pieces of knowledge that fit their central *explanans*. If philosophy be a science, it shall on the contrary be open to the variety of knowledge in all of its aspects: it shall be an observational science, the conceptions of which should be abandoned, in case they don't pass the test of experience. One may say that, on the contrary, the main concern of seminary philosophers is to preserve consistency, no matter how high the price to pay with regards to the adequation to experience. This is in another words a purely internal account of truth.

In such a context, it is not surprising that the method used at least to establish the validity of the system's axioms – or to say it in peircean terms, to fix belief – is some kind of a mix of authority and a *a priori* methods. Peirce's criticism against Descartes combines a rejection of both methods: as the seminal source of subject philosophies, Descartes illustrates the *a priori* method in that he considers the cognitive subject as the epistemological author of knowledge ; but it is clear, if we remember Peirce's remarks about Descartes' strategy of "God made it so", that he considered this as a good example of appealing to external authority in philosophy, God being seen as the ontological source of knowledge. Now, if we leave aside Peirce's radical criticism of cartesian epistemology, it might well be that he agreed with Descartes' claim that establishing the right method should be a necessary task when one endeavors to attain truth. From this point of view, as Delaney justly remarks in *Science, Knowledge and Mind*, adopting the scientific method in philosophy is only a question of time and place, since the adoption of other methods (tenacity, authority or a *a priori* method) was clearly related to different socio-historical contexts.

Before we come to a closer scrutiny of the kind of truths to which philosophy should lead, it may be remarked that philosophical method such conceived is not less scientific than the method of other sciences. In its broadest sense, adopting the scientific method means at least three things. First, that we appeal systematically to experience, in order to form and test our conceptions. The question we should ask about this first point is: what kind of experience is philosophically relevant, once we have discarded inner and metaphysical experience? The second point is that we should make use of previous valuable results found by other members of the community, as a background for inquiry. This second claim should not be understood as incompatible with the first, given the peculiar concept of experience defended by Peirce: appeal to scientific authority is intended to avoid the risks of a cartesian doubt strategy, but this authority relies broadly on the fact that we are confident that previous results should be confirmed by our experience in the same way as they have been confirmed by previous experience. The third point of scientific method is that we should never consider any result as absolutely true, which is nothing else than fallibilism. It is easy to see how the first and second point entail this third one and how in turn fallibilism provides us with a clearer

understanding of their relationships: for appealing to scientific authority alone would commit us to some dogmatic account of science, like appealing to experience alone would make scientific inquiry rather implausible. Fallibilism is therefore the idea that results from Peirce's historical conception of science, and philosophy should be no exception. A scientific philosophy should not aim at establishing absolutely undubitable truths, but rather try to submit as systematically as in physics its conceptions to the test of experience, or in other words, accept the risk of refutation. This is why, instead of closed individual systems entailing a purely defensive attitude, philosophy should produce common theses on the basis of which inquiry could proceed. This is what Peirce tried to do.

Nevertheless, it is clear enough, though philosophy is conceived as a science of discovery, that philosophical truths should be of a somewhat different kind than mathematical, physical or psychological ones, otherwise it would simply dissolve in these sciences. And so philosophy requires a different kind of observation. According to Peirce, philosophy should be conceived as a *cenoscopic* science, that is to say, a science using common (*κοινος*) observation (*σκοπειν*). This doesn't commit us to the idea that philosophy is nothing but common sense, and the fact that the observation is common does not make it an easy task. "Cenoscopic" means only, for Peirce, that philosophy doesn't "depend upon any new special observation" (Letter to Lady Welby, December 1908 – CP 8.342). What is to be understood by the term "special"? If we take it as meaning "unordinary", it may well be an echo of Peirce's rejection of intellectual intuition. Then, philosophical observation is cenoscopic in that it doesn't appeal to such an intuition. But if special is intended to characterize the kind of observations made in the special sciences, this may mean that philosophy does not depend on any scientific observational result out of its own. And so my claim that philosophy is related not only to mathematics but also to empirical sciences would seem to be hardly tenable. I think both interpretations are valid, and it will make it necessary for me to go through a deeper analysis of the kind of "dependance" I have in mind. *

Now for the moment the very problem is to understand what Peirce meant by philosophical observation and in what sense philosophy should rely on observation in general. For this second claim seems clear enough in view of certain peircean results in phenomenology or semiotics, but it is much less clear when we turn to his metaphysics. Since phenomenology and semiotics aim at exposing the most general features of our experience and ways of rational thinking by signs, it should indeed do so without appealing to something out of our ordinary experience and ways of rational thinking. Peirce insists that the observations that lead to his results in phenomenology and semiotics are not purely passive, but imply some kind of abstraction, for the general features within experience and thinking to appear. In that respect, philosophical observation is certainly no ordinary one. But what is the most striking is, that he always urges us to do again, for ourselves, the same observations that he did. See for example, in CP 1.286:

There is nothing quite so directly open to observation as phanerons; and since I shall have no need of referring to any but those which (or the like of which) are perfectly familiar to everybody, every reader can control the accuracy of what I am going to say about them. Indeed, he must actually repeat my observations and experiments for himself, or else I shall more utterly fail to convey my meaning than if I were to discourse of effects of chromatic decoration to a man congenitally blind.

It is hard to imagine that one could spend even ten percent of the time Peirce really dedicated to phenomenological observations in order to repeat these, but the main point is, that we are given a method for controlling the accuracy of Peirce's conclusions: scientific observations in general should be repeated in order to be relied on. But it may be that someone finally decides to repeat them and finds the results to be different from those of Peirce. In that case, we would have to write a new list of categories. Phenomenological truths, as well as semiotic truths, could be falsified through contradictory observations.

There are of course some differences between phenomenological and semiotic truths. Phenomenology is a descriptive science, whereas semiotics, being the other name for logic, is part of the normative sciences. One problem appearing here is that we cannot see how any observation could ever lead to some normative statements. It is true that logic has some classificatory aspects, but logic as a project is normative in Peirce's view, since he defines it as "in its narrower sense, [...] the science of the necessary conditions of the attainment of truth" and, "in its broader sense, [...] the science of the necessary laws of thought, or, [...] general semeiotic, treating not merely of truth, but also of the general conditions of signs being signs [...], also of the laws of the evolution of thought" (CP 1.44). For now, I leave this question unanswered.

If we turn to metaphysics, we meet with a somewhat different kind of truths, neither the descriptive truths of phenomenology, nor the normative truths of logic. "Metaphysics", Peirce writes, "seeks to give an account of the universe of mind and matter" (CP 1.186). Sure, this is no classical metaphysics: Peirce's intention is to build a scientific metaphysics. This is why, though it should not depend on special observation like the one often claimed for in traditional metaphysics, it should nevertheless depend on some observation, namely of "kinds of phenomena with which every man's experience is so saturated that he usually pays no particular attention to them" (CP 6.2). We can conclude from this quotation that metaphysics is as cenoscopic as phenomenology and logic. What is more surprising is the kind of questions that this kind of observation is supposed to help solving. Peirce listed some examples of such questions:

Whether or not there is any definite indeterminacy? Whether there be any strictly individual existence? Whether there is any distinction, other than one of more and less, between fact and fancy? Or between the external and the internal worlds? What general explanation or account can be given of the different qualities of feeling and their apparent connection with determinations of mass, space, and time? Do all possible qualities of sensation, including, of course, a much vaster variety of which we have no experience than of those which we know, form one continuous system, as colors seem to do? What external reality do the qualities of sense represent, in general? Is Time a real thing, and if not, what is the nature of the reality that it represents? How about Space, in these regards? How far, and in what respects, is Time external or has immediate contents that are external? Are Time and Space continuous? What numerically are the Choris, Cyclosy, Periphraxy, and Apeiry of Space? [CP 6.6]

It seems hard to believe that such questions could be solved through common observation. Of course observation is not the whole in metaphysics, and furthermore it may be that metaphysical questions should be asked with regard to what we find in everyday experience: that this ordinary experience appeals to categories such as time, space, existence, conscience or matter seems obvious. But it may still be

asked how Peirce intends to answer these questions without appealing to something else than common observation. It gets even more disturbing when one considers the arguments in Peirce's metaphysical writings. In "Causation and Force" for instance, one could find references to the law of the conservation of energy, and an analysis of the concept of non-conservative forces. Should this be considered as part of everyday experience or common knowledge? It seems doubtful, and this could lead us to ask whether the "independence" of philosophy from special sciences is as complete as we have assumed it to be. In that view, it is necessary to examine the relationships between philosophy and the other sciences, as exhibited in Peirce's classifications of the sciences.

II. Philosophy in the architectonic of the sciences

Whether, as I have suggested before, the change of method in philosophy is only a matter of context and will come to happen sooner or later, is not evident. Even if we consider Peirce's theory as a prediction concerning the future evolution of philosophy, it is hard to see the history of philosophy after him as a full confirmation. It is only in some trends of analytic philosophy that something like a scientific turn in the sense of Peirce has occurred. In particular, logical positivism in its early Vienna Circle period seems to exemplify such a tendency, on several points. Like Peirce, members of the Vienna Circle conceived philosophy as a collective work. Like Peirce, they found it necessary to put the moral standards in scientific inquiry at a high level. Like Peirce, they were conscious of the necessity of a theory of meaning as one of the basis of philosophy. But on the other hand, Peirce surely wouldn't have felt very sympathetic with their radical rejection of metaphysics, their treating mathematics as a pure tautology, or the political background and social aim of their program.

Nevertheless, the main resemblance is about the idea of a close relationship between philosophy and sciences. It is reasonable enough to suppose that Peirce would have had the same kind of contempt as the logical positivists towards a philosophy as irrationalist as Heideggerianism, and it's finally no surprise that when they had to quit Europe in the 30's, members of the Vienna Circle found in the United States a new intellectually adequate home. Most Vienna Circle members were scientists, like Philip Frank, before they were philosophers.

However, this resemblance may be misleading, and it would be necessary to examine in some detail the differences between the positions of Peirce and the logical positivists, as well as the common points. Since I'm not a specialist of the second trend, I shall confine myself to a small number of remarks about the central theme of the unity of science. It seems to me that this idea can be a good point of departure for the comparison I'm intending to make, and it will also shed some light on Peirce's specific conception of it, and as a consequence, on his conception of the place of philosophy within science.

The classical idea of the unity of science, defended by Carnap and Neurath and summarized by Oppenheim and Putnam among others, can be expressed, in its standard physicalist acception, using the following claims: science should be unified at two levels, the level of scientific language, and the level of scientific explanation

through unification of laws. It is clear moreover that, in that program, unifying means nothing else than reducing, ultimately reducing every scientific proposition, whether within biology or social sciences, to the more fundamental propositions of physics taken as the most basic science. Oppenheim and Putnam express these claims as a peculiar conception of the order of science, in which different levels of description, e.g. social group, biological organism, cells, molecules, atoms and elementary particles, are organized according to a hierarchic structure. This is a strong reductionist version of the unity of science program. Philosophy, in that context, has two functions: it is first supposed to show the consistency of scientific theories, through a logical reconstruction using a broadly russellian logic apparatus, and an observational basis. Second, it shall give unitary researches the impulse, not only on a purely philosophical level, but also on the level of scientific research itself. Different versions of this program exist, some of which have different consequences for philosophy. Quine's naturalization program, for example, is physicalist too, but makes philosophy a mere part of behaviorist psychology applied to scientific propositions. In both cases however, there is some insistence on the idea that philosophical results should be evaluated with regard to their truth or success, though of course not in exactly the same sense.

It is well known that one of the strategies in order to attack this program is related to our insisting on the methodological differences between particular sciences, a strategy of which Jerry Fodor was one of the first advocates. But it is unclear that denying a reductionist version of the unity of science as the one I've exposed above forbids us to defend another, non-reductionist version of unity. It seems to me that Peirce defends such a version. In Peirce's view, unity of science can be understood as continuity within science, i.e. that we can consider in some way different particular sciences as related, though not in a reductionist way. It may be remarked that, for Peirce, as soon as we come to the study of particular sciences, their procedures and results, we lose the unity we had found at the level of science in general. Though mathematicians, philosophers, physicists and psychologists all try to find out truth, it is clear that they don't try to find out the same truth according to the same methods, as I still have noticed with regards to the peculiar kind of observation required by philosophy. According to Peirce, this scientific particularism is so important that scientists from different fields consider each other as strangers:

I have already remarked that a definition of science in general which shall express a really intelligent conception of it as a living historic entity must regard it as the occupation of that peculiar class of men, the scientific men. The same remark may be extended to definitions of the different branches of science. The men who pursue a given branch herd together. They understand one another; they live in the same world, while those who pursue another branch are for them foreigners. [CP 1.99]

Such a statement may seem hard to conciliate with the idea of a unity of science. If unity is only the kind of similarity of aim that we have studied before, or even if it can be extended with reference to a general scientific method, this may well appear as trivial and useless with regards to an epistemological project. Peirce's way to solve this problem is to show that simpler sciences in the hierarchy of sciences provide more specialized sciences with principles or methodological models. This allows him to establish links between the sciences, without having to reduce sciences one to another. Peirce's classifications of sciences are intended to exhibit such relationships. Mathematics provide philosophy with some of its central

concepts (such as continuity) and methods (e.g., imaginary experiment); philosophy in turn provides the empirical sciences with metaphysical principles; physics can serve as a methodological model for experimental psychology, and so on.

If we accept such a schema, it is easy to see that it gives philosophy a peculiar place in the architectonic of science. Its role cannot be limited to rationally reconstructing empirical sciences and in a way, in Peirce's view, it is unclear that such a reconstruction is useful. Philosophy shall rather be considered as a critical science, in a kantian sense of it, i.e. it shall examine what is presupposed in the sciences and show in what sense that renders knowledge possible. This is no foundationalism, since in a sense no science has to be founded on anything else than itself: that one science may need to take some elements from another does not mean that it should be founded on it, and in some way, reductionism is a sophisticated form of foundationalism. Sure, from this point of view, there are degrees of independancy, and the only truly independent science is mathematics. 11

Now what about philosophy? Saying that it has its place in the architectonic of science suggests that it should have some distinctive relationships with the other sciences. As is well known, one of the most typically peircean claims is the insistence about the necessity of appealing to mathematics in philosophy, though not in the cartesian sense of applying geometrical method to systematical attempts. As I have said before, this appeal is twofold. First, there are philosophical concepts that cannot be adequately understood unless we study their mathematical meaning. Continuity can be considered as a good example, and in that view Peirce's mathematical work cannot be disconnected from his philosophical concerns. As a metaphysical concept, synechism is rooted in mathematics. Second, cenoscopic observation shall take as a model mathematical observation, as it was explained by Peirce in his theory of theorematic reasoning and, more generally, philosophy shall try to attain the same kind of exact thinking than mathematics. I won't say more about the relationships between philosophy and mathematics. Due to the pattern exhibited by his classifications of sciences, Peirce's views on these relationships have been studied extensively and it makes no doubt that part of the scientificity of philosophy derives from its mathematical roots.

On the contrary, since they've not been at scrutiny in any central and systematic way, relationships between philosophy and empirical sciences promise to be far more interesting and puzzling. Of course, there's one side of these relationships that is not problematic, and that is the critical aspect I've mentioned before. In that respect, metaphysics is supposed to make explicit the essential features of that reality which empirical sciences aim at knowing. But it may be added that philosophy shall sometimes correct particular sciences and so have an influence on their course. In case a particular science has not fixed precisely enough the kind of phenomena it intends to study, it may be the task of philosophy to tell this science that there's something misleading in the way it proceeds, or in the conception it has of its scope. See for example how Peirce suggests that psychology misses its point as long as it defines itself as a study of conscious phenomena:

What the psychologists study is mind, not consciousness exclusively. Their mistake upon this point has had a singularly disastrous result, because consciousness is a very simple thing. Only take care not to make the blunder of supposing that Self-consciousness is meant, and it will be seen that consciousness is nothing but Feeling, in general, -- not

feeling in the German sense, but more generally, the immediate element of experience generalized to its utmost. Mind, on the contrary, when you once grasp the truth that it is not consciousness nor proportionate in any way to consciousness, is a very difficult thing to analyze. I am not speaking of Soul, the metaphysical substratum of Mind (if it has any), but of Mind phenomenally understood. To get such a conception of Mind, or mental phenomena, as the science of Dynamics affords of Matter, or material events, is a business which can only be accomplished by resolute scientific investigation. But the psychologists have been prevented from making that investigation by their delusion that Mind is just Consciousness, a simple affair, as far as the mere phenomenon goes, about which there is no room for error or doubt. [CP 7.365]

However, the central intuition in my research project is that such an account of the relationships between philosophy considered as a science and the other sciences does not provide us with a sufficiently dynamic vision of Peirce's philosophical program and of his pragmatic conception of epistemology. I'd like to focus on this point to finish my talk.

III. Pragmatic conception of epistemology: Peirce as a scientist-philosopher

By describing Peirce as a scientist-philosopher, I just want to insist that the relationship between his activities as a philosopher and an experimentalist in different fields of physical and psychical sciences, though it's widely recognized, has been underestimated as a way to understand Peirce's project as a whole. It is true that several studies have been made, but insofar as the claim I'm doing seems to conflict with Peirce's official point of view about this matter, scholarship has often been more historical than systematic. In a way, my claim is inspired by some common sense remark: when one thinks of Peirce's life as a life devoted to science in almost every field, one should be surprised to discover that he conceived his work in one field as radically separate from his work in the others. Unless we discover that he was suffering of an extreme form of schizophrenia, I think it is reasonable to make the hypothesis that Peirce viewed his scientific life as a whole. And so, if science is a living entity, we may try to find out whether there is some kind of adequation between the different phases of its development: rationality, if it is understood as control, would require that we aim at finding, more generally, some harmony between the distinct evolutions of particular sciences.

Such a claim should ideally be tested through a historical and comparative study of Peirce's works. This is a very vast project, and I shall confine myself today to the example of his work as a stellar photometrist, which may be convincing, not necessarily with regards to the absolute truth of my hypothesis, but at least to its being worth working on. From 1859, when he came to be a regular auxiliary at the United States Coast Survey, until 1878, when he published his only book, entitled *Photometric Researches*, Peirce devoted a large amount of his time to stellar photometry, in both observational and theoretical work. His book opens on some methodological remarks concerning the sensation of light, and the way this sensation could be represented using a "Newton diagram", i.e. a diagrammatic representation of any sensation of color on the basis of three conventionally chosen primary colors. This point is essential because photometric observations should be as public as possible, so that one can use observational results from the others. For Peirce, as a scientist, it must have been an important problem, since he had

endeavored to verify previous catalogues of stars, and intended his own observations to serve as a basis for further investigations.

Now if we turn to his philosophical work during the same period, we can clearly see how he is concerned by some similar problems. The 1868 texts had established that thought is a public and external phenomenon, rather than a private and internal one. Given this program, Peirce's attempt to elaborate his pragmatic theory of meaning, in the general frame of his theory of inquiry, could be understood as a way to solve the problem of scientific communication, using external criterions for meaning and clearness. In that context, intuition should be defined as an inferential and complex phenomenon, rather than as a simple one. Exploring the semiotic structure of sensations is nothing but discovering the conditions in which sensations can be signs, i.e. can be taken as means in scientific communication. Is this not what Peirce intends to show with the theory of Newton diagrams?

In my view, Peirce's epistemology cannot therefore be separated from his scientific work. Epistemological problems emerge on the basis of our scientific practice: this is the first way in which we can describe Peirce's theory as a pragmatic epistemology. In other words, epistemology is not only, and even not really some specifically philosophical work: it is mainly a task for scientists themselves. In that sense, saying that philosophy is a science means that it should not be considered as external to the other sciences, but rather as the internal and reflexive side of sciences. This, of course, tends to a limitation of its autonomy, but it may be asked if this autonomy, as it is claimed for usually, can have any justification. With regards to truth, it does certainly not. For if we take epistemology to be pragmatic in a second sense, we cannot evaluate the meaning and truth of our epistemological theories unless we consider their practical effects, and their adequacy to scientific theories. So as for synechism, I think there is a metatheoric use of the pragmatic maxim. See for example how, in *A Guess at the Riddle*, Peirce tries to retrieve his metaphysical triad in physics, in psychology, in physiology, and so on. I think it is not only to show its extension, but mostly to give us with a clearest account of what these metaphysical principles really mean.

Now, there can be two interpretations of this pragmatic relationship between epistemology and sciences. The strongest one would be to say that the existence and content of scientific theories is some necessary condition for the emergence of our epistemological theories: if you allow me this metaphor, I would call this interpretation "epistemological supervenience", to indicate that though there is a necessary link, it does not imply that epistemology should be reduced to science. The second interpretation would be to say that reference to scientific theories allows us only to evaluate our epistemological theories, and to choose the more adapted one. I would call this interpretation "epistemological hermeneutics", since in that case, it is only when we compare our epistemological theories to scientific theories that we can get their full meaning. Both interpretations can be criticized as naïve, insofar as we don't take scientific theories to be something completely determined, the interpretation of which cannot be equivocal. But I think such a criticism misses Peirce's point, since his theory precisely aims at showing that no theoretical conception can be perfectly determined. Considering Peirce's theory of semiotical and ontological vagueness, we might understand the bilateral relationship between philosophy and science that I have tried to expose as a way to deal with vagueness

in a context of increasing semiotic control: though we may never reach a perfect state of clearness and determinacy, we shall nevertheless try to reduce such an indeterminacy as much as we can. I believe that this is the role of epistemology, and that Peirce conceived it like that.

Concluding remarks.

I am aware that what I have exposed in that talk is pretty vague and cannot be considered as a definite contribution to peircean scholarship or to philosophy in general. But I may add that what I said should be understood rather as a program. I have ever mentioned one reason why my hypothesis should be adopted, even carefully and provisionally, and it is that it seems reasonable enough with regards to both Peirce's life and writings. I have just noticed at the beginning of my talk that the questions I have dealt with concerning the nature of science often appear, in Peirce's unachieved book projects, as introductory material. In my view, this very fact suggests that the rest of the book, whether it deals with logic or another topic, should be read on this peculiar background. It may be remarked that I seem to reduce Peirce's philosophical program to epistemology: but it seems clear enough that this is no reduction, since his conception of science implies that we adopt a different view of science, including for instance moral aspects.

A second reason is that it could lead to further cooperation between science, history of science and philosophy. If Peirce is right and if we adopt his views on this topic, then a fallibilist and evolutionary epistemology might well appear as an impossible task for any individual. On the contrary, Peirce's view of science as an affair of community makes it a plausible if not an easy task. So this is finally a call for cooperation. It seems relevant to make such a call in the context of this Peirce Studies Center.

In any case, if I am wrong, I will be happy, consistently with Peirce's ethics of science, if any of yours can show me that I am, and help me to get rid of an error.