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1. Institution of university and economics

> Economists think of themselves as scientists, but <…> they are more like theologians. The closest predecessors for the current members of the economics profession are not scientists such as Albert Einstein or Isaac Newton; rather, we economists are more truly the heirs of Thomas Aquinas and Martin Luther.
>
> (Robert H. Nelson, 2001, p. XV)

Institutions of economics appeared in Germany, England and the United States in the second half of the 19th century. They were born inside national institutions of university. That is why I begin my analysis from the 13th century when the institution of university was created. It makes a difference with the "birth" of the institution of science in the 17th century which took place *outside* the university. This "birth" can be linked with foundation in 1660 of the Royal Society of London for the Improvement of Natural Knowledge. During several centuries university was a purely educational establishment subordinated to Church and state *having the task of forming clergymen and civil servants*. First universities were associations of teachers and/or students, having their own status which determined rules of teaching and learning. Main basic subjects taught were Latin language, rhetoric, logic, mathematics (arithmetic and geometry), astronomy, music and the central discipline was theology. Two applied subjects were also taught: law and medicine. The creation of these universities was linked not only with general progress of the West (economic renewal, urban growth and development of exchanges). It was linked with the need of Catholic Church, civil authorities and ruling classes in educated people for managing their affairs (Charle and Verger, 2007, p. 8-13). In spite of the fact that first universities were associations, they were strongly dependent upon civil authorities, i.e. the King, but the support of the Pope was really decisive and very soon, universities were practically subordinated to the Church (Ibid, p.15).

During the 14th and 15th centuries, while being officially clerical institutions, universities turned more and more under the control of cities and states, which expected from them forming knowledgeable people and competent lawyers that they needed for their developing administrations. These people were also expected to elaborate national and monarchic ideology which accompanied the creation of Modern State. The loss of initial autonomy of universities was compensated by various advantages, including wages for professors paid by the state and promises of good careers. In exchange of the favours of the

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Prince, universities were compelled to provide an orthodox education, to form future local elites and to contribute to the established social and political order (Ibid, p. 19). The disciplines taught were the same as before. The absence of experimentation by students in classes limited teaching of exact sciences to the old framework fixed by Aristotle and Ptolemy (Ibid, p. 33). Teaching of medicine was based on Roman sources. The emphasis in the teaching of law was made on canonical law. Teaching of Aristotle was not autonomous, but considered as a preparation to the learning of theology, which contained the study and interpretation of the Bible. At this time, theology was under the influence of Aristotle (Ibid, p. 26-27). Educational methods in all disciplines were scholastic. It was necessary to learn some limited numbers of texts which were considered to contain all knowledge or at least general principles on the basis of which all knowledge had to be based. Two kinds of methods were used: *lectio* and *disputatio*. The former consisted in reading aloud texts by professors or advanced students. The latter represented discussions between students under the leadership of a professor. These discussions had to be based on syllogisms making constant quotations of authoritative texts (Ibid, p.29). From the 16th century, the autonomy of universities disappeared, including elections of administrators. Rules of student admissions, syllabuses and exams were determined by the state. Religious loyalty of a student was checked by his solemn promise before admission (Ibid, p. 39). Up to the 18th century, universities continued to provide exclusively an obsolete education in all domains based on ancient Greeks and Romans, as well as on the Bible and theological texts. "The real education was obtained outside the university, either through family education, salon conversations, personal readings, or by practice itself at the beginning of a career. The university degree was first of all evaluated as a certificate of social belonging, a sign of allegiance to the established political order" (Ibid, p. 56). New knowledge was created outside the university.

The link between scientific research and university was first created in Germany at the beginning of the 19th century with the reform of Wilhelm Humboldt started in 1810 in the newly created University of Berlin. Johann G. Fichte participated actively in the creation of this university. He considered the vocation of the scholar in the following way: "To me, [to the Scholar], is entrusted the culture of my own and following ages; from my labours will proceed the course of future generations, the history of nations who are yet to be. To this am I called, to bear witness to the Truth: my life, my fortunes are of little moment; the results of my life are of infinite moment. I am a Priest of Truth; I am in her pay; I have bound myself to do all things, to venture all things, to suffer all things for her. If I should be persecuted and hated for her sake, if I should even meet death in her service, what wonderful thing is it I shall have done? what but that which I clearly ought to do?" (Fichte, 1851, pp. 59 - 60). German institution of university of Humboldt and Fichte has abandoned many features of medieval European university. University of Berlin became the first so called research university, i.e. research and educational establishment at the same time. University professors were allowed and even obliged to do their research inside the universities. The concept of Humboldt considered science not as something accomplished that teachers should transfer to students, but as "a problem which has not yet been solved" and for its solution the research should never be stopped. According to him "the university teacher is therefore no longer a teacher and the student no longer someone merely engaged in the learning process but a person who undertakes his own research, while the professor directs his research and supports him in it" (Humboldt, XIII, p. 261). Humboldt believed in the necessity of academic freedom but thought that appointments of university professors have to be made exclusively by the state because its interests are very strongly linked with them. Humboldt also admitted the usefulness of existence of frictions between professors on scientific questions. It is worthwhile to note that the vice-rector of the University of Berlin at the time of reform was

Friedrich Schleiermacher, the founder of hermeneutics as a general methodology of interpretation (Schleiermacher, 1998). This fact certainly helped later in German research universities to find a right version of functional analogy of natural scientific experiments for empirical research in social sciences in general and in particular in economics. I mean so called interpretive paradigm. Deep roots of Historism in German culture without doubt also helped finding this right version.

National professional societies/associations of economists began to appear at the end of the 19th century. In Germany, it took place in 1873 (Verein für Sozialpolitik), in the USA – in 1885 (American Economic Association) and in England – in 1890 (The British Economic Association, later renamed The Royal Economic Society). As the previously mentioned Royal Society of London for the Improvement of Natural Knowledge, these societies/associations of economists were voluntary associations, but unlike the former, their members were not just those for whom research was a passion without any link with their teaching activity, but university professors of economics. As a matter of fact, national communities of academic economists appeared as communities of teachers of this discipline in the universities of their countries. In this way, economics as an institution from the very beginning was attached to the institution of university. In the second half of the 19th century British and American universities were still very different from German ones. Curriculum of English and most of the American universities were dominated by classics and mathematics, and theology still occupied an important place. Unlike German universities, British and American universities of that time were very little influenced by the institution of science embodied in Royal Society of London for the Improvement of Natural Knowledge. To my mind it was one of the determinant factors of institutionalisation of economics in these countries as abstract discipline with its a priori method. On the contrary, German economics was institutionalised in new research universities, in which experimental approach was highly valued.

The English and German approaches were confronted in the United States. In the first half of the 19th century American universities were British-like: "By and large, the purpose of higher education in pre-Civil War America was to teach piety and discipline. The vast majority of faculty were involved in preaching and missionary work <…> The first American textbooks were written by clergymen, and a religious understanding of economic activity was pervasive. Capitalism and the laws of political economy were thought to be in harmony with the laws of god and consistent with the higher purpose of moral elevation" (Fourcade, 2009, p. 64). In Britain in the middle of the century John Stuart Mill, being as his father agnostic, transferred the legitimacy of political economy from religion to science. He took geometry as a model and characterised political economy as essentially an abstract science and its method as a priori method. According to him "it reasons and must necessary reason from assumptions, not from facts" (Mill, 1994, p. 56). On the basis of this methodology Marshall developed his economics as a "scientific" legitimacy of the established social order[1]. This type of economics was welcome by American businessmen who at that time increasingly replaced clergymen on college and university boards of trustees: "University leaders (presidents and boards alike) often favoured [economics and other social sciences] as 'secular substitutes for religion' and saw in them a continuation of the old courses in moral philosophy" (Fourcade, 2009, p. 66).

German academic economists of the second half of the 19th century followed natural sciences research tradition with their experimental method. They did affiliate neither Smithian nor Marxian economics which were based on Mill's methodology with its a priori method. Both the scope and method of German economics were different from economics of Smith and Marx. In the scope of their analysis there was not only quantitative variables such as

[1] I will come back to these topics in more detail in section 4 of this paper.

production, consumption, labour, values, prices and capital, but first of all qualitative entities: institutions and linked with them beliefs. They have found for social sciences, including economics, a functional analogue to experimental research in natural sciences what is called now qualitative research (Denzin and Lincoln, 2005) which includes monographic research. Monographic studies were in the centre of research of German economists. According to Bruno Latour, the specificity of scientific research does not consist in a special "scientific method", but in the design of experimental situation, in which the object has the possibility to resist, "to object" to the ideas of the researcher concerning it. Part of the experimental situation is a recording device assuring descriptions, the core of the experimental work. Latour contests conclusions of 'scientific methodologies' in social sciences which ignore the most important feature of scientific research: "Unfortunately, although it tastes and smells like hard science, those all-terrain 'scientific methodologies' are a sham and a cheap imitation for a reason that becomes clear if we go back to the definition of objectivity, as what allows one entity to object to what is said about it. If we lose the influence of the object in what is said about it, as quantitavists are so proud of saying, we also lose objectivity!" (Latour, 2000). It is this property of scientific research which is responsible for such a huge influence of science-based technologies on humanity. Monographic research fulfills in social science the role of recording device. It is the contents of monographic descriptions of objects of research which can resist to the ideas of the researcher concerning this object. The result of this resistance is an understanding of the object or phenomena under study.

The name of German school of economics "historical" is misleading. They understood the economic history in the same way as Schumpeter did later; it includes present-day facts (Schumpeter, 1954, p. 12). Its distinguished feature was monographic research whether it concerned the past or the present. Nevertheless, "nobody can hope understand the economic phenomena of any, including the present, epoch who has not an adequate command of historical facts and an adequate amount of historical sense or of what may be described as historical experience" (Ibid.., pp. 12-13). American institutionalism has appeared at the turn of the 19th and 20th centuries under the direct influence of German economics. Walton H. Hamilton, the author of the term "institutional economics", characterised the historical method of analysis in the following way: "It goes to the past only with the end in view and so far as is necessary to explain what a thing is in terms of how it came to be" (Hamilton, 1919). The absolute necessity to use this method in socio-economic research comes from *the sources of social regularities which are rules (institutions), and linked with them, beliefs*. These rules and beliefs often remain unchanged during long period of time. This historical (genetic) research can not be made on the basis of Mill's methodology ("reason from assumptions, not from facts"). Being a very gifted mathematician, Marshall, on the basis of this methodology, has started the process of graduate institutional transformation of economics into a mathematical science by the introduction of the Mathematical Tripos examinations in Cambridge University. These examinations served as a test for all who wished to study economics at Cambridge. This move was supported by such beliefs as "mathematics is the queen of sciences" and "real science needs quantifications". History of natural sciences shows that these beliefs are wrong. According to Latour "the imitation of the natural sciences by the social sciences has so far been a comedy of errors". He thinks that "believing what philosophers of science and some scientists were saying about 'the scientific method', social scientists have been paralysed by a 'physics envy'"(Latour, 2000). Economists were paralysed more than anybody else.

Anglo-Saxon institution of economics created in the framework of universities, very much touched by their medieval history and subordinated to the new capitalist establishment,

laid this discipline to the incarnation of the ideology favouring interests of this establishment[2]. Contemporary mainstream economics continues to fulfil the same function: "The social understanding we gain from modern economics is disappointing, even impoverishing <…> In the face of such limitations, why does economics enjoy such prestige? The awkward possibility arises that the reason is precisely because its modern form is ahistoric, apolitical, asocial. Olympian views have appeal to all social orders, and a view that eschews politics and sociology may have special appeal for a social order that celebrates its close relation to science. The nature of the appeal itself is a function of economics we have heretofore left unexamined. This is its service as ideology - not a narrow, consciously self-serving apologia, but a belief system of the kind that accompanies and supports all social orders. The purpose of such belief systems is to provide the moral certitude that is the precondition for political and social peace of mind, as much or more for the dominant elements in any social order as for its subordinate elements. No doubt this peace of mind is always tinged with doubt or tainted with hypocrisy, but in the end, social orders at all levels of hierarchy require some body of knowledge and set of beliefs to which to repair. Primitive societies have their myths and interpretations of nature, command societies their sacred texts. By no means exclusively, but also by no means in trivial fashion, economics serves that purpose for capitalism as a social order" (Heilbroner, 2004, pp. 629-630).

2. Institution of science and economics

> I want to propose that we see scientific method as a heavily textured phenomenon rather than as the mere execution of some philosophically intuited standard of reason <…> To pursue such a goal one must move inside the epistemic space within which scientists work and identify the tools and devices which they use in their "truth"-finding navigation. Not surprisingly, what we find there is not the clear and austere progress of verifications (or falsifications, or explanatory capacities) we might expect, but the "untidy" goings-on of various businesses of experimentation.
>
> (Karin Knorr Cetina, 1991, p. 107)

According to historians of science the first scientists were Galileo Galilei (1564 – 1642) and William Gilbert (1544 – 1603) (Gribbin, 2003, p. 68). Both of these scientists made their research outside of universities where they taught and their teaching activities had practically nothing in common with their research activities. Their beliefs and practices influenced substantially rules which members of scientific communities continue to follow up to the present. Both used actively experimental method, which includes specially organised observation, for example on the basis of a constructed telescope. Galileo was convinced that the universe is a book which is written in the mathematical language, and the symbols of this language are geometrical figures like triangles and circles. Without help of these symbols "it is impossible to comprehend a single word of it [the book of the universe]; without of which

[2] I will come back to this topic in the following sections of the paper.

one wanders in vain through a dark labyrinth" (Burtt, 2003, p. 75). He strongly believed in the force of mathematical models which once constructed can be used to understand the reality. This belief had a religious basis: "God has made the world an immutable mathematical system, permitting by the mathematical method an absolute certainty of scientific knowledge" (Ibid., p. 82). Gilbert, who apparently did not shared this belief, was the father of nonmathematical scientific current (Ibid., p. 163). Another scientist who tremendously contributed to the formation of rules of scientific research was Robert Boyle. Following Gilbert practice he championed the method of reasoned analysis of sensible facts, confirmed by exact experiment: "Experience is but an assistant to reason, since it doth indeed supply informations to the understanding but the understanding still remain the judge, and has the power or right to examine and make use of the testimonies that are presented to it" (Ibid., p. 170 – 171). Boyle was one of the organisers of the Royal Society of London for the Improvement of Natural Knowledge known as Royal Society[3]. It is his vision of scientific research which was institutionalised in the framework of associations of its practitioners, one of the first of which was the above mentioned Royal Society.

The Royal Society was founded in 1660 by members of one or two either secretive or informal societies already in existence. The origins of the Royal Society lie in an "invisible college" of natural philosophers who began meeting in the mid-1640s to discuss the ideas of Francis Bacon. The Royal Society enjoyed the confidence and official support of the monarchy. The "New" or "Experimental" form of philosophy was generally ill-regarded by the Aristotelian (and religious) academies. The motto of the Royal Society, "Nullius in Verba" ("Nothing in words") or in other words "Demonstration by facts and not by words", signifies the Society's commitment to establishing the truth of scientific matters through experiment rather than through citation of authority and logical reasoning. The philosophical basis of the Royal Society differed from previous philosophies such as Scholasticism, which established scientific truth based on deductive logic, concordance with divine providence and the citation of such ancient authorities as Aristotle. The Society was to meet weekly to witness experiments and discuss what we would now call scientific topics. The history of science since 1660 is closely intertwined with the story of the Royal Society. Approximately at the same time similar societies were created in Italy (Florentine Accademia de Cimento in 1657) and France (Parisian Académie Royale des Sciences in 1666). These societies "represented alternative organisational forms to the universities <…> The new societies aimed to provide a novel organisational form uniquely suited to the new practice; they made the production of the new knowledge, rather than the just guardianship of and commentary on the old, central to their identity; and they aimed, with varying success, to link the progress of science to civic concerns rather than wholly scholarly or religious ones" (Shapin, 1996, p. 133). The societies "placed high value on the necessity of disciplined collective labour in the making of proper natural knowledge" (Ibid.) and "manifested a pronounced concern for orderliness and the rules of proper behaviour in making and evaluating natural knowledge" (Ibid.). The Royal Society was dominated by scholars-gentlemen. They considered "veracity to be underwritten by *virtue*. Gentlemen insisted upon the truthfulness of their relations as a mark of their condition and their honour. The acknowledgement of gentlemanly truthfulness was the acknowledgment of gentlemanly identity" (Shapin, 1994, p. 410). Objectivity of truth seeking by gentlemen in the process of evaluating testimony of experiments and scientific debate concerning them was favoured by gentlemen' material independence: "Free action and

[3] Information on Royal Society was partially taken from http://royalsociety.org/page.asp?id=7483 .

integrity were seen as the condition for truth-telling, while constraint and need were recognised as the grounds of mendacity" (Ibid.).

Isaac Newton made all his research not as a university professor but as a member of the Royal Society. He was Its president from 1703 to 1727. Contrary to the opinion of Galileo, Newton estimated that there is a distinct difference between mathematical truths and physical truths. He thought that the world is what it is. It is well and good if it is possible to discover some exact mathematical laws, but "so far as not, we must seek to expand our mathematics or resign ourselves to some other less certain method" (Burtt, 2003, p. 212). During all his life he attacked the idea of "hypotheses". It is necessary to underline that Newton was convinced of the "superiority of the way of experiments to the method of deduction from a priori assumptions". His statement concerning it is well known: "I do not think it needed to explicate my doctrine by any hypothesis at all" (Ibid., p. 215 - 217). The famous Newton's experimental-mathematical method can be presented as a sequence of the three following steps: "First, the simplification of phenomena by experiments, so that those characteristics of them that vary quantitatively, together with the mode of their variation, may be seized and precisely defined <…> Second, the mathematical elaboration of such propositions, usually by the aid of the calculus, in such a way as will express mathematically the operation of these principles in whatever quantities or relations they might be found. Third, further exact experiments must be made (1) to verify the applicability of these deductions in any new field and to reduce them to their most general form; (2) in the case of more complex phenomena, to detect the presence and determine the value of any additional causes (in mechanics, forces) which can then themselves be subjected to quantitative treatment; and (3) to suggest, in cases where the nature of such additional causes remains obscure, an expansion of our present mathematical apparatus so as to handle them more objectively." (Ibid., pp. 221 – 222).

We can see here that Newton envisages using experiments at the beginning and at the end of the research process. Werner Heisenberg (2006), one of the authors of quantum mechanics, describes the research process in a similar way. This feature of the Newtonian method is often distorted by methodologists of science, by presenting so called scientific method as hypothetic-deductive. Newtonian simplification of the phenomena under study occurs not by simplified a priori modelling but a simplification of phenomena by experiments. In this way a contact with reality remains in spite of the simplifications and it is not the case in Anglo-Saxon economics based on Mill's methodology. Of course the Newtonian method as it is can be applied only to relatively simple phenomena. It is not the case with socio-economic-political phenomenon. German historical school (Schmoller) and American institutionalism (Commons) preserving the spirit of the Newtonian method in their research practices adapted it to complex realities which they investigated.

As it is well known, histories are always written by winners. The winner in "the struggle over the soul of economics, institutionalist and neoclassical economists in America between the wars" (Yonay, 1998), were the latter and during several decades they produced absolutely distorting discourses concerning the German historical school and American institutionalism. Among multiple negative labels are non-scientific, atheoretical, inductive, unproductive, useless, normative. I will discuss these labels in following sections of this paper, but here I want to say something about the last one. Economists produce texts. These texts can be of two types. The first type of texts concerns studies of "what is", that is of existing economic objects and phenomena, and the second represents reasoning about "what ought to be" in economic matters. If our understanding of science corresponds to the tradition created by the Royal Society of London for the Improvement of Natural Knowledge, then we cannot consider the latter type of texts as scientific. Nevertheless, the science practiced and

preached by this Royal Society has never been value-free. Without any doubt they studied "what is" but such its prominent members as Robert Boyle and Isaac Newton were devout Christians and enthusiastic students of the Bible. Both of them wrote not only scientific but also theological texts. Their faith influences their research activity in the sense that it created for it very high motivation: to discover the design of the Creator. I contrast to texts of political, social or moral philosophers like Thomas Hobbes[4], Boyle and Newton did not reflect in their scientific writings "what ought to be" but only "what is".

The German Gustav Schmoller and American John Commons had the same attitude to their research. They shared Christian values, and it motivated and directed their research work. They studied "what is", and on the basis of it, they expressed "what ought to be" as outcome of their studies and not its essence. The outcome finally was embodied in advanced social legislation of their countries. Their values did determine domains of studies but not their contents and understanding they got from them. Their merit as social researchers was connected with the fact that good legislation ("what ought to be") cannot be made without good understanding of social reality ("what is"). In other words, in order to change rules (introduce new rules), it is vital to understand the functioning of acting rules. Interpretation of investigated institutions (rules) as good or bad, just or unjust, could follow the research work, but their activity as researchers was concentrated on the understanding of how these institutions functioned. The results of investigations (understanding of functioning of institutions), moral and political evaluation of findings and proposal for institutional change can coexist in one text (book or article), but it just means that the author changes hats when she/he moves from studying "what is" to proposing "what ought to be". In Schmoller's and Commons' writings the former did influence the latter but not vice versa. On the contrary, Smith, Marx and Marshall, as well as their contemporary followers, incorporated their beliefs concerning the capitalist social order directly in their theories. In this way their theories are first of all scientifically looked holders of ideologies.

Science can be considered as a social organism. The Royal Society at the beginning of its activity was a very small social organism. Now science is a huge global social organism with its formal and informal rules of recruitment, promotion, publication etc. These rules are an evolutionary result of ideological, political and financial influences from outside and inside of scientific communities and of beliefs shared by members of these communities. It is quite easy to understand, and we will expand this understanding passing from one section to another of this paper, why in the case of economics these rules lead the communities of economists to theological character of their discipline. It is important to understand why, in spite of all these influences, natural sciences continue to bring to the mankind means to dominate the nature and influence in a spectacular way its material environment. Already Charles Peirce saw the research as a collective action of investigators. He had remarked a seemingly magic capacity of scientific communities by observing and by analysing something separately, gradually to converge on the results of investigation: "Different minds may set out with the most antagonistic views, but the progress of investigation carries them by a force outside of themselves to one and the same conclusion. This activity of thought by which we are carried, not where we wish, but to a fore-ordained goal, is like the operation of destiny. No modification of the point of view taken, no selection of other facts for study, no natural bent of mind even, can enable a man to escape the predestinate opinion. This great hope is embodied in the conception of truth and reality. The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real." (Peirce, 1878, pp. 138 -139) The cause of this convergence is the same type of "resistance" of objects of study to different investigators. *"Nothing in words" as*

[4] I will come back to the methodological debate between Robert Boyle and Thomas Hobbes in the section 7 of this paper.

one of the main values and methodological principles of natural scientific communities reinforce the consideration of character of this "resistance" as *the main argument in the discourse inside of scientific communities. As long as communities of economists share neither this value nor this methodological principle, economics will remain primarily secular theologies.*

Charles Peirce also gives us the key for understanding the functioning of institution maintaining a theological type of thought: "Let an institution be created which shall have for its object to keep correct doctrines before the attention of the people, to reiterate them perpetually, and to teach them to the young; having at the same time power to prevent contrary doctrines from being taught, advocated, or expressed. Let all possible causes of a change of mind be removed from men's apprehensions. Let them be kept ignorant, lest they should learn of some reason to think otherwise than they do. Let their passions be enlisted, so that they may regard private and unusual opinions with hatred and horror. Then, let all men who reject the established belief be terrified into silence." (Peirce, 1877, p. 117) It is exactly what Alfred Marshall has initiated as the institution of economics in England at the end of the 19th century and what later expanded throughout the world. William Cunningham, an English advocate of German-type economics, provides the following testimony of the situation in the community of economists in England of that time: "Anyone who has refused to follow the economic fashion of recent years in England must have been greatly hampered in his efforts to pursue his own studies or guide those of others; boards of studies would exercise a galling control, and editors and publishers would view his writing with suspicion <…> There was no need for the English adherents of the realistic school of economists to complain when obstacles were placed in the way of their work, and avenues of publication were closed against them." (Cunningham, 1894, p. 327). In this paper, published in the USA, he discussed the attitude of Professor Marshall and disciples towards the German-type economics: "In Germany <…> a veritable revolution has taken place in economic studies during the last fifty years <…> a revolution in the whole conception and character of economic studies: it has come to be concerned with the observation and study of the actual economic conditions of society in the past and in the present; not merely with the formulating of hypothetical principles, which the sciolist was only too apt to convert into ready made receipts for removing any of the ills of social life. It is not a little remarkable that while this revolution has taken place in Germany and to some extent in America, England should have been almost untouched by it" (Ibid., pp. 317 – 318). He also criticised the Mill's methodology on which Marshallian economics was based: "The Germans began to devote themselves to the past, and thus opened up a field for discriminating observation and accumulation of facts. Mill took no pains about the past, and comparatively little with the details of contemporary experience. His eyes were fixed on the time to come <…> So far as its *matter* is concerned, the work did not stimulate to observation and research. Nor did the character of the science as treated by Mill undergo any decided change: he regards it as a hypothetical science" (Ibid., p. 319). At present communities of academic economists continue to follow the Millian methodology.

Upper quoted Bruno Latour has come to his characteristic of a scientific research, as an investigation dealt with resistance of the object under study to the researcher, on the basis of the research as an anthropologist using participant-observer methods in the study of the functioning of the Laboratory for Neuroendocrinology at the Jonas Salk Institute for Biological Studies in La Jolla, California. Begining in October 1975 for nearly two years he carried out a kind of ethnographic study of scientific research in this laboratory. In his study he closely followed the intimate processes of scientific work, i.e. every detail of what the scientists do and how and what they think (Latour and Woolgar, 1979, p. 12). In 1977 Roger Guillemin, the head of said laboratory, had received the Nobel Prize in Physiology or Medicine. It meant that what Latour studied was highly professional. The institution of Nobel

Prize has become an important part of the global institution of science. As it is well known, every year since 1901 the Nobel Prize is awarded for achievements in physics, chemistry, physiology or medicine, literature and for peace. I think that the three scientific disciplines were chosen by Alfred Nobel not by chance; they continued tradition lunched by the Royal Society. In his testament he expressed the will that prizes would be awarded "to those who, during the preceding year, shall have conferred the greatest benefit on mankind". Among the five indicated domains there was no mathematics nor philosophy or social sciences. In 1968, Sveriges Riksbank (Central Bank of Sweden) established The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, i.e. this prize is not really the Nobel Prize, but the prize of a bank. This prize is addressed to a community involved in absolutely incompatible with physics, chemistry, physiology practices, and whose "benefit on mankind" is rather questionable. Let us hope that one day this prize will deserve to be confused with the Nobel Prize.

Bruno Latour was not the only one to investigate activities of researchers in natural sciences. The world of high energy physics was studied by Sharon Traweek (1988). A comparative investigation of activities of researchers in the domains of high energy physics and molecular biology has been made by Karin Knorr Cetina (1991, 1999). The latter comes to the same conclusions as Latour concerning 'the resistance': "Molecular geneticists do interact with 'the world' – as it is featured in the laboratory of course, but this featuring does not preclude but rather enhances resistance. They constitute part of a behavioral system in which 'things' are not passive receivers but active reactants" (Knor Cetina, 1991, pp. 119 – 120). At present most of the economists consider economics as theory or theories. To do economics mean for them to develop or to apply theories. According to Knorr Cetina "much of laboratory science in molecular genetics neither directly draws upon, nor it seems terribly involved with establishing, theoretical representations. In molecular genetics, theoretical statements may indeed be post hoc 'representations' of materials <…>" (Ibid., p. 120). Apparently neither Latour nor Knor Cetina would agree with Milton Friedman's famous statement: "A theory is the way we perceive 'facts', and we cannot perceive 'facts' without a theory" (Friedman, 1953, p. 34). Some sincere mainstream economists do not agree with Friedman either: "By regularities I mean phenomena that appear repeatedly in similar environments at different points in time and at different locations. I have the impression that as economic theorists, we hope that regularities will miraculously emerge from the formulas we write leisurely at our desks. Applied economists often feel the need for a model before they mine data for a pattern or regularity. Do we really need economic theory to find these regularities? Would it not be better to go in the opposite direction by observing the real world, whether through empirical or experimental data, to find unexpected regularities? Personally I doubt that we need pre conceived theories to find regularities." (Rubinstein, 2006, p. 873) Finally what we learn from Knorr Cetina' investigation, it is the challenge to the accepted view of a unified science even in the framework of natural sciences. Research procedures can sharply differ in different disciplines, but if they represent interacting with the "resisting" entities under study, they certainly can be classified as scientific research.

Born as a political/moral philosophy, British political economy has been presented by J.S. Mill as a science on the basis of its wrong discourse about science (1836) later developed in his "System of Logic" (1843). The marginalist revolution of Walras, Jevons and Menger followed Mill by accepting a priori method as their method. With the start in Great Britain at the end of the 19th century of the profession of economics as a profession of university teacher of this discipline, Marshall guided the process of institutionalisation of economics on the basis of marginalism and Mill's methodology. He initialised in 1890 the creation of the British Economic Association (The Royal Economic Society), in order to strengthen the British institution of economics of which he was the designer. Formally it could look like the Royal

Society initiated by Boyle but in reality it resembled much more a guild, i. e. an association of craftsmen in a particular trade[5]. Institutionalisation of economics in Germany, also on the basis of profession of university teachers of economics, took nevertheless a totally different path. German *Verein für Sozialpolitik* can be rightly considered as a functional analogue for economics of the Royal Society of London for the Improvement of Natural Knowledge.

3. Institutionalisation of economics in Germany and historical institutionalism

> I felt that in the German universities there was room for growth and the development of individuality <…> It is that which makes studying in Germany such a pleasure to every real student. You learn here, and only here, how to do independent, real scientific work.
>
> (Richard T. Ely, 1938, p. 43)

Founders of the Royal Society were convinced that the universe works in accordance with the laws of nature which God established for its order and control. One of the most influential of them, Robert Boyle, in his book (1690) explained that the *study and dominion of nature is a duty given to man by God*[6]. The first scientists were very much motivated in their research activity because they thought that they were discovering the design of the Creator and in this way they approach the humanity to God. Community of researchers of the Royal Society did not earn their living by their investigating activity. All of them had independent sources of existence which had no connection with their research work. Thus the Royal Society cannot be considered as a guild. A century later, Johann Fichte, the second after Humboldt founder of the institution of research university, saw the motivation the researcher in different way: "The true vocation of the scholar is *the most widely extended survey of the actual advancement of the human race in general, and the steadfast promotion of that advancement."* (Fichte, 1851, p. 54). Institutionalisation of German economics happened in Humboldtian university and its architect, Gustav Schmoller, followed the ideas of Fichte. In this way Schmollerian *Verein* similar to the Royal Society had not been conceived as a guild. The foundation of the *Verein* took place quite quickly after the German unification. It is in 1871 in Versailles that Germany's princes elected Prussia's King Wilhelm IV. The German Empire (Deutsches Reich) was established as a federation of states. Schmoller has taken for the community of German economists a more modest position than Fichte for scholars in general: *most widely extended survey* of the actual advancement of the young German nation, and the *steadfast promotion of that advancement*. United Germany needed national unity and the political crisis of early capitalism created danger for this unity. This danger came from the existence of the so-called "social question".

Germany was behind Britain and France in industrialisation and urbanisation. Schmoller and his colleagues were very much impressed by the studies of Fridrich Engels over the British conditions (Engels, 2009) and of Lorenz von Stein over the French conditions. They considered these studies as indicators of what could happen in Germany in the future: "Engels's vivid descriptions of the commercial vibrancy and man-made hell produced as a consequence of industrial development, chaotic urbanisation, the litany of abuses and deprivations inflicted on the working class and their resultant moral and ethical

[5] The section 4 of the paper will deal with this matter.
[6] http://creation.com/the-man-who-turned-chemistry-into-a-science

degeneration, but also of the failure of charity, the ruthlessness of factory owners, and complacency of the British government in dealing with these problems caused a sensation in Germany" (Grimmer-Solem, 2003, p. 108 - 110). Lorenz von Stein came to the conclusion that "French society was driven with class-centred political conflict produced by increasing social inequality – a product of free market competition. Socialism and communism were thus simply an expression of justified proletarian aspirations to attain social equality"(Ibid., p.109). In his speech at the inaugural meeting of the *Verein für Sozialpolitik* Schmoller "warned of the threat from social revolution engendered by the division between employer and worker, propertied and propertyless classes, and suggested that popular economic beliefs concerning commercial freedom and economic individualism could well create even greater disorder, rather than the rosy future they imagined" (Tribe, 2002, p. 10) According to him "only the German state was in a position to reduce social tension and foster national unity, for it stood above selfish class interests, 'legislating, guiding administration with a just hand, protecting the weak, raising the lower classes'" (Ibid.). Representatives of propertied classes denied the existence of the social question, but with national unification and formation of a national government new light had been shed on economic conditions: factory laws, factory inspection, corporative organizations, and courts of arbitration were all dismissed (Grimmer-Solem, 2003, p. 178). "The new organisation [*Verein für Sozialpolitik*] was conceived as a body to exclusively research the social question to provide scientifically derived, general, and above all, practical information on reform to appeal to the parties of the political middle, the public, legislators, and government officials, it was hoped, would then use this 'scientific' information as a basis for policy decisions, and thereby not blinded by the fog of 'partisan economics'" (Ibid., p.179).

Founders of the *Verein* shared their general frustration with the mode of reasoning of classical economics "that seemed wholly at odds with positivist and materialist scientific climate of the time, when the natural sciences were celebrating success upon success by working empirically" (Ibid., p.123). Unlike most of the university professors of economics, founders of *Verein,* Gustav Schmoller and Georg Knapp, received a good training in natural sciences: Schmoller had studied at the university in Tübingen chemistry, physics, mechanical engineering and technology; Knapp studied physics and chemistry in Liebig's laboratory (Ibid., p. 133). The translation of Mill's *System of Logic* into German has been published in 1865, and like many others, Schmoller noticed the inconsistency of Mill's treatment of social sciences by excluding application to them of experimental approach. "Rejecting these inconsistencies, which he saw as serving not science but Mill's own opinion of what constituted human nature and natural law, Schmoller sought instead to approach economics as natural scientists did" (Ibid., p. 133 - 134). He believed that economic and social science had the same epistemology as natural sciences. According to Schmoller and his colleagues, major sources of social regularity were common morals, ethics, and institutions. Thus, to understand socio-economic phenomena it is necessary "to study all those institutions that had emerged over time to constrain and mould individual behaviour into purposive action and social interaction" (Ibid., p. 160). Their "search for moral commonality to construct new laws and institutions naturally led to historical investigations of those things that formed a common moral sphere and ethically constrained and moulded economic action: customs, norms, conventions, rules, regulations, laws, organisations, corporate bodies and other institutions, and not least, the state" (Ibid., p. 160 - 161). For Schmoller and his colleagues, "in economic analysis, morals and law could be viewed as causal factors". Social reform for them was a process of piecemeal institutional adaptation (Ibid., p. 161). On the basis of his historical investigations Schmoller has come to the conclusion that "the state and its bureaucracy could defend the general interest and be forces for social improvement; institutions in the economy provided greater certainty and order to market relations and injected into these a set of moral-

ethical norms" (Ibid., p. 168). In this way institutions were "the means to create for a modern industrial economy a new moral-ethical order" (Ibid).

Thanks to the *Verein* in the community of German economists, good professional practice became identified with empirical research. It guided and organised economic research by its agenda-setting standing committees of annual conferences. These conferences were not just meetings of members of the profession sharing with each other results of their research. These conferences were places of debate of commissioned studies. "In advance of conferences, *Verein's* standing committee held meetings to nominate and vote on the subjects to be discussed at the conferences. Sets of questions were then raised and parameters set for research and fieldwork (or in the case of surveys, detailed questionnaires were drafted and sent out) by a commissioned expert, and increasingly, groups of experts. The results of these investigations and surveys would then be compiled into summary studies which were circulated before conferences <…> Following the conferences, commissioned studies were published in the *Verein's* monograph series, the *Schriften des Verein für Sozialpolitik* <…> To get an idea of the scale of the *Verein's* research, by 1914 it had published some 140 volumes of its *Schriften* of an average length of about 350 pages."(Ibid., p. 69 -70). In many cases *Verein* received financial support from government departments for the collection of data (Tribe, 2002, p. 12). Results of investigations of German economists affiliated to *Verein* were published in several academic journals like Schmoller's *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft im Deutschen Reich* (Annal for Legislation, Administration, and Political Economy in the German Empire) and *Jahrbücher für Nationalökonomie und Statistik* (Annals for Political Economy and Statistics), chief editors of which were Bruno Hildebrand and Johannes Conrad.

A very good description of the university part of the German institution of economics has been made by Keith Tribe: "In Germany those who might be considered "economists" would by the later nineteenth century have been principally defined by their employment as a teacher of economics in one of the several state universities. Chairs of economics had existed in Germany since the eighteenth century, and although the teaching delivered from these posts was transformed over time along with the subject, the posts themselves were a constant feature of the university landscape. This teaching was primarily for the benefit of students of law, attendance at a course of lectures in economics being a compulsory part of legal education. Those who studied economics for its own sake were thus by definition reading for doctorates, since there was no other qualification; but a doctorate was a formal requirement for university teaching, and also important for entry into some parts of state administration, especially the statistical offices established from the 1830s. When linked to the system of formal and informal contacts through which students entered employment, this added to the power and influence of Professors like Schmoller, able to assign doctoral topics to a growing band of students and draw upon public funds for the prosecution of pieces of research." (Tribe, 2002, p. 4) Professors of economics in German universities were civil servants and decisions concerning their selection were made by the ministry of education. Nevertheless influential members of the *Verein* could influence this selection: "Gustav Schmoller, founder of the Younger School by virtue of the role he played in the formation of the *Verein für Sozialpolitik* in 1872-73, and who subsequently became an academic impresario mediating between academic and official institutions, as a consequence acquiring great influence over appointments and promotions in Prussian universities."(Ibid., p. 9). Since a qualification in law was routinely required for posts in public administration, as well as the legal system itself, the place of economics teaching in the late nineteenth century German university was secured by its place in the legal curriculum (Ibid., p. 2).

Recent historical findings concerning activities of Schmoller and his colleagues in the *Verein für Socialpolitik* [(Balabkins, 1988), (Priddat, 1995), (Schellschmidt, 1997), (Peukert,

2001a), (Peukert, 2001b), (Grimmer-Solem, 2003)] have showed its non-partisan scientific character. The result of these activities had national and even international value. Thank to their efforts Germany was one of the first countries to define a "social question" and develop a modern welfare state. Schmoller and his colleagues were successful in convincing the German government in the necessity of the reforms for solving the social question. A decade later of the *Verein*'s foundation, the head of German government, Otto von Bismarck, had the following discourse: "<...> the actual complaint of the worker is the insecurity of his existence; he is unsure if he will always have work, he is unsure if he will always be healthy and he can predict that he will reach old age and be unable to work. If he falls into poverty, and be that only through prolonged illness, he will find himself totally helpless being on his own, and society currently does not accept any responsibility towards him beyond the usual provisions for the poor, even if he has been working all the time ever so diligently and faithfully. The ordinary provision for the poor, however, leaves a lot to be desired <...>". The governmental reform program included Health Insurance, Accident Insurance (Workman's Compensation), Disability Insurance, and an Old-age Retirement Pension, none of which were then currently in existence to any great degree. Bismarck opened debate on the subject on 17 November 1881 in the Imperial Message to the Reichstag, using the term *practical Christianity* to describe his program. Based on Bismarck's message, the Reichstag filed three bills designed to deal with the concept of Accident insurance, and one for Health Insurance. The subjects of Retirement pensions and Disability Insurance were placed aside for the time being. The law concerning them was adopted several years later[7].

The epistemic culture of the younger German historical school totally corresponds to the vocation of the scholar as it was defined by Fichte: the most widely extended *survey* of the actual advancement of the human race in general, and the steadfast *promotion* of that advancement. Nevertheless this epistemic culture contradicts totally to the present professional activities of both orthodox and most of the heterodox streams of economics. We cannot call these activities of communities of economists an epistemic culture, because these activities are not oriented to knowledge acquisition about the real world but represent activities of the construction of some imaginary worlds. Usual accusation against German historical school and American institutionalism at the turn of 19th and 20th centuries lie in the absence of theories. Now economics is understood as the theory, or at best as theories. To get a professional training in economics means primarily to learn economic theories. To practice research means either construct/improve theories or apply them. As we could see earlier, it does not correspond at all to the understanding of research activities in natural sciences. It is this discrepancy that is the main cause of the failure of economics to bring to people knowledge/understanding on the socio-politico-economic world in which we live. Some honest members of the profession confess: "I believe that as an economic theorist, I have very little to say about the real world and that there are very few models in economic theory that can be used to provide serious advice" (Rubinstein, 2006, p. 881). Can you imagine that, for example, Werner Heisenberg could say something similar? I think no, you cannot imagine it. Nevertheless the author of this declaration is not a marginalised member of the community of academic economists, but its very honourable member: the text is part of his Presidential Address to the Econometric Society made in 2004. In his paper-confession, he continues: "As economic theorists, we organize our thoughts using what we call models. The word "model" sounds more scientific than "fable" or "fairy tale" although I do not see much difference between them" (Ibid.).; "What are we trying to accomplish as economic theorists? We essentially play with toys called models. We have the luxury of remaining children over the

[7] http://en.wikipedia.org/wiki/Otto_von_Bismarck#Bismarck.27s_social_legislation

course of our entire professional lives and we are even well paid for it. We get to call ourselves economists and the public naively thinks that we are improving the economy's performance, increasing the rate of growth, or preventing economic catastrophes. Of course, we can justify this image by repeating some of the same fancy sounding slogans we use in our grant proposals, but do we ourselves believe in those slogans?"(Ibid., p. 865). We can ask ourselves what is the cause of this confession? I suppose to find the answer in the following statements of the author: "I cannot ignore the fact that our work as teachers and researchers influences students' minds and does so in a way with which I am not comfortable."(Ibid., p. 881); "I am a teacher of microeconomics. I am a part of the "machine" that I suspect is influencing students to think in a way that I do not particularly like."(Ibid., p. 877). The answer is very simple: the priest and theologian is not very happy with the religion he deals with. Most of his colleagues do not have this problem. "Fairy tale" nature of the orthodox and of most of the heterodox economics, based on mathematical models or not, is evident. Nevertheless economists prefer not to see it.

In the middle of the 19th century August Comte proposed a three stage scheme of modes of making theories. According to it each of the leading conceptions, each branch of the knowledge, passes successively through three different theoretical conditions: the Theological, or fictitious; the Metaphysical, or abstract; and the Scientific, or positive (Comte, 2003, pp. 25 – 26). Schmoller indicated that the economics of his time was still at the metaphysical stage. I suppose that he could not suspect that a hundred years later it will come down to a lower stage, the theological one. The quotation which follows is taken from the section "Toward a Civic Religion" of the book of one the guru of contemporary economics: "This book is an expression of the hope that a new 'civic religion' is on the way to being born, a civic religion that will return, in part, to the skepticism of the eighteenth century concerning politics and government and that, quite naturally, will concentrate our attention on the *rules that constrain governments* rather than on innovations that justify ever expanding political intrusions into the lives of citizens. Our normative role, as social philosophers, is to shape this civic religion, surely a challenge sufficient to us all" (Brennan and Buchanan, 2000). Now, when the world is plunged into such a profound economic crisis provoked by the deregulation, this sentence sounds especially sinisterly.

At present the German historical school is almost forgotten in the community of economists. In many manuals of history of economic thought it is not even mentioned. Its continuation in the USA, the so called old institutionalism, has practically the same fate. Nevertheless, beginning from 1980's, the approach used Schmoller's and Commons's schools has been resuscitated in American political science under the name of Historical Institutionalism. Sometimes it is considered even larger as a social science method: "Historical institutionalism is a social science method that uses institutions in order to find sequences of social, political, economic behavior and change across time. It is a comparative approach to the study of all aspects of human organizations and does so by relying heavily on case studies"[8]. Political scientists usually link the birth of historical institutionalism with the publication of the book of Theda Skocpol (1979). Sometimes representatives of this current are nominated as historical-interpretive institutionalists (Steinmo, Thelen and Longstreth 1992, p. 7)[9]. This school competes in political science with Rational Choice Institutionalism coming from new institutional economics. These two institutionalisms make together in political science New Institutionalism: "<…> new institutionalism in Canada tends to be historical institutionalism, whereas in the United States it is about evenly divided between the rational choice and historical streams" (Lecours 2005, p. 4). Historical Institutionalism in

[8] http://en.wikipedia.org/wiki/Historical_institutionalism

[9] Historical-interpretive approach used in historical institutionalism has not to be confused with Interpretive Institutinalism of Ludwig Lachmann (Foss and Garzarelli 2007) from Austrian school of economics.

political science continues traditions of the German historical school headed by Gustav Schmoller, thus it has to be classified as belonging to New Institutionalism, rather than to the Old Institutionalism, as it is practiced in the community of economists. In the English language economic literature of the end of 19th century, the German historical school was very often called the New School (Ely, 1884, p. 43).

Theda Skocpol has never felt any respect whatsoever for disciplinary boundaries (Skocpol, 1999, p. 16). Just like Schmoller and his colleagues she thinks that "social science should take up important, real-world problems and not simply engage in navel gazing discussions of purely internal theoretical or methodological issues" (Ibid.). According to her in the Schmollerian spirit, social science has to fulfill "'a civilizing mission' - social scientists should take up substantively important problems with broader significance to the communities with whom they are communicating, and then seek rigorous answers about outcomes of interest" (Ibid.). She expresses evaluations very similar to Schmoller: "Approaches ranging from the Marxist <…> to the rational choice often try to understand politics as a function of an immediate economically determined equilibrium. I simply do not believe we can understand political outcomes without highlighting historical path dependencies" (Ibid., p.17). The following sentence sounds as if it is Schmoller who speaks: "Institutions embody ideas. Actually they marry them to resources and patterns of power, social power, and institutions certainly offer definitions of the situation. At the level of political psychology, that explains why people simply are not short-term instrumentalists. They accept definitions of the situation that seem workable and are backed by powerful relationships. People have to deal with such institutionalized definitions of the situation on a day to day basis" (Ibid., p.18). At last Skocpol is willing to study "what is" and not "what ought to be": "I am a social scientist. I believe there is a difference between science and normative work, and good social science is not exactly the same thing as advocacy, though advocacy always benefits from sound scholarship" (Ibid., p.19).

Very often Schmoller's and Commons's institutionalism are accused by the economists to be politically not neutral. In fact the accusation concerns their embodiment in the analysis of asymmetries in power relations. Apparently political scientists do not have problems with it. "Historical institutionalists accepted the contention that conflict among rival groups for scarce resources lies at the heart of politics", they found explanations of inequalities that mark national political outcomes "in the way the institutional organization of the polity and economy structures conflict so as to privilege some interests while demobilizing others". German economists the hundred years ago saw the state in the same way as American political scientists now: many historical institutionalists see "the state no longer as a neutral broker among competing interests but as a complex of institutions capable of structuring the character and outcomes of group conflict". Analysts in this school explore how "social and political institutions associated with labour and capital, could structure interactions so as to generate distinctive national trajectories". "Historical institutionalists have been especially attentive to the way in which institutions distribute power unevenly across social groups. Rather than posit scenarios of freely-contracting individuals, for instance, they are more likely to assume a world in which institutions give some groups or interests disproportionate access to the decision-making process; and, rather than emphasize the degree to which an outcome makes everyone better off, they tend to stress how some groups lose while others win." (Hall and Taylor, 1996, pp. 941 - 947)

Why historical institutionalism at the turn of the 19th and 20th centuries in Germany and the United States had been abandoned by communities of academic economists? An understanding of this phenomenon is the central task of this paper. Comparative historical institutional analysis of economics in three countries gives us the possibility to get this understanding. Right now I will present here just a preliminary summary of this

understanding. The political economy of Adam Smith was a political/social/moral philosophy and not a science as it was practiced by first scientists Galileo and Gilbert and has been institutionalised in the framework of the Royal Society of London. This political economy can be classified according to August Comte's law of stages as metaphysical or abstract using a priori method. Both "homo oeconomicus" and "invisible hand" were the expressions of the Providence, of God's design and will[10]. This kind of discipline could not satisfy in the middle of the 19th century the scientifically oriented economists in German research university. Based on traditions of scientific research in natural experimental sciences and on the values of scholars formulated by Humboldt and Fichte, German professors of economics have developed an institution of economics with its central element *Verein für Socialpolitik.* This institution was oriented not for the self-protection of the community of economists but for the fulfilment of the mission: help to the state to solve the social question and to protect the new German nation from a national catastrophe. Two decades later the institutionalisation of economics in England took place. The process happened in an absolutely different institutional environment, not in a Humboldt's research university, but in an almost medieval type of universities where curriculum was dominated by mathematics, classics and theology. The architect of the British institution of economics, Alfred Marshall followed in its design the established university spirit. In addition, the creation of the institutions of economics like a secular theology was influenced by his lost of Christian faith, and his adoption of utilitarian ideology (Bentham). In this change of belief he followed the dominant cultural tendencies of his social environment. It has predetermined his professional choice not to be involved in hard and time-consuming empirical research of socio-economic realities but to start developing abstract a priori constructions. This choice has been supported by the distorted interpretation by John Stuart Mill of the process of knowledge acquisition. This interpretation gave to a priori constructions of political economists an aureole of science.

Both Schmoller's and Marshall's economics were responses to the existence of the social question; in England it was even sharper than in Germany, but the responses of different types. The former was oriented to helping the state to solve this problem by improving conditions of the working class by introducing new social legislation, and in this way, to prevent social unrest. The latter was oriented to create scientifically looking ideological construction legitimating the existing social order and conditions, and in this way to achieve the same goal, to prevent social unrest. Once established the institution of Marshallian economics attract much easier the support of those who consider the discipline of economics more like a craft than like a vocation. The work of Schmollerian economists as researchers and teachers is much more difficult than that of Marshallian economists. Frequent surveys/fieldwork and constant adaptation of courses to changing realities are much more time and labour consuming than the desk work of "a priori theorists". Very quickly the community of economists-craftsmen can become inaccessible fortress for those who would like to practice economics as a vocation with primarily socially-oriented altruistic aspirations. The problem with Schmollerian economics in comparison with Marshallian economics is not only social but also economic and political. Surveys and fieldwork request strong financial and political support on behalf of the governments (local and/or central). The political support of Schmollerian types of scholars is necessary because their research activity can discover undesirable for owners/managers details of the functioning of their private enterprises. The resistance to Schmollerian economists can take place in the domain of surveys and field studies' organisation and financing or in the domain of the recruitment and promotion of teachers/researchers controlled by university boards with businessmen as its members. All these factors probably played their role in the post-Schmoller's economics in Germany.

[10] See (Latour et Lépinay 2008, pp.113 - 114).

The interwar economic and political crisis could not be of no influence on the German community of economists. The *Verein für Sozialpolitik* has been dissolved in 1936 with the arrival to power of Nazis. After World War II, the military presence of the USSR in Eastern Germany and of the USA in Western Germany have predetermined the reign of the Marxist-Leninist political economy (Mittag, 1969) in one part of Germany and of neoclassical economics in another part. At least since the fall of the Berlin wall "neoclassical economics was and still is dominant in Germany <…> Until recently it was said that 'Schmoller is forever condemned and castigated' (Peukert, 2002a, p. 72). Nevertheless "it is surprising how many younger (German) scholars are interested in a reconstruction of institutionalism and historicism and how few survive the crowding out of the profession after their dissertation" (Ibid., p. 97). To my mind the attractiveness of the German school of thought comes from the fact that it produced such a rich set of ideas concerning the social world that they continue to reappear later. One of its central ideas has been entered in the social constructivism: "Institutions always have a history, of which they are the products. It is impossible to understand an institution adequately without an understanding of the historical process in which it was produced" (Berger and Luckmann, 1991, p. 72).

4. Institutionalisation of economics in England as a secular theology

> I am more interested in economics as a veil that obscures our social understanding than as a technique for discovering how our society works. What does the veil obscure? That the price system is also a system of power; <…> that the object over which the veil is spread is not a collection of individuals but a specific social order to which we give the name capitalism.
>
> (Robert L. Heilbroner, 1988, pp. 7 - 8)

Historical Institutionalism overlapping in some respects with Social Constructivism represents either a social sciences' approach or some kind social sciences' frame theory. It is this approach that I am using in this paper for analysis of economics. Two central questions which I would like to answer in this paper are why the Germany's born economics has disappeared and England's born economics expanded on a tremendous scale. The key to understand it gives the social constructivist connections between institutionalisation and social control: "Institutions, by the very fact of their existence, control human conduct by setting up predefined patterns of conduct, which channel it in one direction as against the many other directions that would theoretically be possible. It is important to stress that this controlling character is inherent in institutionalisation as such, prior to or apart from any mechanisms of sanctions specifically set up to support an institution" (Berger and Luckmann, 1991, p. 72). Historical institutionalism underlines connection between institutions and ideas/beliefs. Every institution cannot exist without the idea/belief connected with it. It was almost always *the belief which was linked with the birth of the institution*. We have seen that institutionalisation of economics in Germany in the second half of the 19th century has been very closely linked with the idea shared by natural scientists that *scientific knowledge is an experimentally based knowledge*. Social control in the community of natural scientists is finally a *control of the veracity of reports concerning conclusions based on data gathering and experiments*. The belief which was linked with *the birth of the institution of the British*

economics was absolutely different. It is the idea issued by John Stuart Mill that economics cannot use experimental method and should use *a priori method*. In this section of the paper I will first try to analyse, on the basis of autobiography of J.S. Mill, in what way and in what kind of social conditions this idea was expressed. Then, using as a source of information the biography of Alfred Marshall written by John Maynard Keynes, I will retrace the story of Marshall as the architect of British institution of economics, including social conditions of his adoption of Mill's ideas, and personal circumstances of his turn to a priori method. Afterwards, using some historical investigations, I will try to paint a picture of the process of the economics' institutionalisation guided by Marshall. I will describe in the next section of this paper what kinds of measures of social control were used to promote this institution in the United States. We will see later that when institutionalisation is over, social sanctions' "controlling efficacy, however, is of a secondary or supplementary kind. The primary social control is given in the existence of an institution as such. To say that a segment of human activity has been institutionalised is already to say that this segment of human activity has been subsumed under social control. Additional control mechanisms are required only insofar as the processes of institutionalisation are less than completely successful." (Ibid, p, 73) The expansion of Anglo-Saxon economics is due to its very successful institutionalisation. On the contrary the institution of German born economics was not adapted to changing politico-economic conditions and this in spite of the fact that its methodology and its contents were very efficient for studies of these politico-economic conditions.

In the beginning of professionalisation and institutionalisation of economics in Britain was the Verb: the idea of John Stuart Mill that Political Economy is and has to be an 'abstract science', which "must reason from assumptions, not from facts" (Mill, 1994, p. 56). For him the model for an abstract science is geometry: "Geometry presupposes an arbitrary definition of a line, 'that which has length but not breadth'. Just in the same way manner does Political Economy presuppose an arbitrary definition of man" (Ibid.). This Verb, formulated in 1836, determines until now the face of contemporary economics, either orthodox or most of its heterodox streams. In his famous statement in the preface to the 1st volume *Das Kapital* (1867) Marx expressed the same idea: "In the analysis of economic forms, more over**,** neither microscopes nor chemical reagents are of use. The force of abstraction must replace both." The idea formulated by J.S. Mill was just a confirmation of the status quo of the political economy. In this paper I will not go down to details of the origins of the a priori method in classical political economy (Prasch, 1996). It is sufficient to say here that it had its source in the medieval scholastic thought. J.S. Mill went never to school or university. His only teacher was his father James Mill. The latter did his studies at University of Edinburgh at the expense of a fund for educating young men for the Scottish Church. "He there went through the usual course of study, and was licensed as a Preacher, but never followed the profession; having satisfied himself that he could not believe the doctrines of that or any other Church." (Mill, 2008, p. 8) For a few years he was a private tutor in various families in Scotland, but ended by taking up his residence in London, and devoting himself to authorship (Ibid.). From 1806 to 1818 he was engaged in writing his *History of India.* Although he sharply criticized the East India Company and the British administration in India, in 1819 he was appointed to a position in the examiner's office of the India House in London[11]. James Mill solicited a post in this company for his son, position which was his only employment until his retirement.

J.S. Mill wrote in his autobiography: "I was brought up from the first without any religious belief, in the ordinary acceptation of the term. My father, educated in the creed of Scotch presbyterianism, had by his own studies and reflections been early led to reject not only the belief in revelation, but the foundations of what is commonly called Natural

[11] http://encarta.msn.com/encyclopedia_761552409/james_mill.html

Religion" (Ibid., p. 25). The beliefs of Mill-son have come from his father's friend, Jeremy Bentham, founder of Utilitarism: "When I laid down the last volume of the [Bentham's] Traité [de Législation], I had become a different being. The 'principle of utility' understood as Bentham understood it, and applied in the manner in which he applied it through these three volumes, fell exactly into its place as the keystone which held together the detached and fragmentary component parts of my knowledge and beliefs. It gave unity to my conceptions of things. I now had opinions; a creed, a doctrine, a philosophy; in one among the best senses of the word*, a religion; the inculcation and diffusion of which could be made the principal outward purpose of a life*[12]" (Ibid., p. 40). Another favouring reading of young Mill was the book of Bentham, published under a pseudonym, "Analysis of the Influence of Natural Religion on the Temporal Happiness of Mankind"[13]: "This was an examination not of the truth, but of the usefulness of religious belief, in the most general sense, apart from the peculiarities of any special Revelation <…> when those who reject revelation, very generally take refuge in an optimistic Deism, a worship of the order of Nature, and the supposed course of Providence <…> Next to the Traité de Législation, it was one of the books which by the searching character of its analysis produced the greatest effect upon me" (Ibid., p. 42). Graduated from the University of Edinburgh on the basis with a diploma of a Preacher, i.e. primarily theological education, James Mill certainly could not transfer to his son a spirit of experimental sciences. Both Mills were not Christians and became theologicians of Benthamian utilitarist "religion". Many other British political economists fulfilled the same function and J.S. Mill followed them.

Almost two hundred years after the founding of the Royal Society (1660) and the spread of the "Baconian" method, when the transformation of the meaning and application of empirical knowledge took place, British political economists tried to interpret their a priori method as an empirical one. Among them, may be the most advanced in this way of reasoning was Nassau Senior. "As late as 1836, Senior argued that the study of political economy should begin with a short list of broadly accepted and acknowledged premises understood as "general facts" <…> According to Senior, the premises of political economy were to be founded upon facts that "… are, however, so nearly self-evident, that we will venture in the mean time to assume their truth". <…> The "general facts" proposed as premises by Senior were as follows: (1) people are self-interested, (2) population is limited by available resources, (3) produced means of production enhance the productivity of labour, and (4) in the absence of technical change, diminishing marginal returns are evident in agriculture. Senior argued that the first could be derived through the method of introspection and that the others were trivially evident in our collective experience of the world" (Prasch, 1996). Senior was "convinced that his short list of "general facts" established a firm foundation for a genuinely empirical science of political economy". As such, he considered his method to be fully consistent with the empirical science pioneered by Bacon and Newton. "Satisfied with the soundness of his own approach, Senior criticised what he alleged to be the "hypothetical" approach advanced by John S. Mill: "But neither the reasoning of Mr. Mill, nor the example of Mr. Ricardo, induce me to treat Political Economy as a hypothetical science. I do not think it necessary, and, if unnecessary, I do not think it desirable"" (Ibid.). We can conclude that J.S. Mill was more intellectually honest than N. Senior. His "Principles of Economics" had served a methodological model for the creator of the British institution of economics which during the 20th century expanded all over the world.

Unlike J.S. Mill, Alfred Marshall went to school when he was nine years old. Ancient languages occupied an important part of the curricular. At the beginning, his father actively participated in the teaching of his son, as James Mill did with John Stuart: "He used to make

[12] Marking up is mine (VY).
[13] (Beauchamp, 1822) reprinted by Kessinger Publishing.

the boy work with him for school, often in Hebrew, until eleven at night" (Keynes, 1963, pp. 126 – 127). As a good pupil at school after its graduation "he became entitled in 1861 [at the age of 19 years old], under old statutes, to a scholarship at Oxford, which would have led in three years to a Fellowship <…> It was the first step to ordination in the Evangelical ministry for which his father designed him" (Ibid., p. 128). For Alfred it would mean a continued servitude to the Classics. Alfred did not like to learn ancient languages but was very capable in mathematics. Mathematics represented for him emancipation from the tyranny of his father who could not understand them. Alfred did not want "to be buried at Oxford under dead languages". His objective was to go to Cambridge to study mathematics. At Cambridge, Marshall fulfilled his ambitions. In 1865 he was elected to a Fellowship where he wanted to devote himself to the study of molecular physics. But this desire (in his own words) "cut short by the sudden rise of a deep interest in the philosophical foundation of knowledge, especially in relation to theology" (Ibid., pp. 128 - 131). "In Marshall's undergraduate days at Cambridge a preference for Mathematics over Classics had not interfered with the integrity of his early beliefs. He still looked forward to ordination, and his zeal directed itself at times towards the field of Foreign Missions. A missionary he remained all his life, but after a quick struggle religious beliefs dropped away and he became, for the rest of his life, what used to be called an agnostic." (Ibid., p. 133). Marshall's Cambridge career took place at the period when in England, or at least in Cambridge, Christian dogma disappeared in the serious philosophical teachings. The youthful mind of that time was directed by Metaphysical agnosticism (Herbert Spencer), Evolutionary progress (Darwin) and Utilitarian ethics (Bentham) (Ibid., pp. 134 -136).

At the end of his life Marshall said: "From Metaphysics I went to Ethics, and thought that *the justification of the existing condition of society was not easy*[14]. A friend, who had read a great deal of what are now called Moral Sciences, constantly said: 'Ah ! if you understand Political Economy you would not say that.' So I read Mill's *Political Economy* and got much excited about it. I had doubts as to the propriety of inequalities of *opportunity*, rather than of material comfort. Then, in my vacations I visited the poorest quarters of several cities and walked through one street after another, looking in the faces of the poorest people. Next, I resolved to make as thorough a study as I could of Political Economy." (Ibid., p. 137) As we can see young Marshall was touched in the same way as Schmoller by the existence of 'the social question'. "In 1868, when he was still in his metaphysical stage, a desire to read Kant in the original led him to Germany. 'Kant my guide', he once said, 'the only man I ever worship: but I could not get further: beyond seemed misty, and social problems came imperceptibly to the front. Are the opportunities of real life to be confined to a few?' <…> Hegel's *Philosophy of History* greatly influenced him. He came also in contact with the work of the German economists, particularly Roscher." (Ibid., p. 138 - 139) Finally Marshall settled down in Cambridge as a lecturer in Moral Science, giving courses on Political Economy, but also on Logic and Bentham. "He was attracted towards the new views of economics taken by Roscher and other German economists <…> He set himself to get into closer contact with practical business and with the life of the working classes. On the one side he aimed at learning the broad features of the technique of every chief industry; and on the other he sought the society of trade unionists, co-operators and other working-class leaders. Seeing, however, that direct studies of life and work would not yield much fruit for many years, he decided to fill the interval by writing a separate monograph or a special treatise on Foreign Trade; for the chief facts relating to it can be obtained from printed documents. He proposed that this should be the first of a group of monographs on special economic problems." (Ibid., p. 151). Bu he has never realised his plans.

[14] The marking is mine (VY).

As a biographer of Marshall, J.M. Keynes, wrote: "The fateful decision was the abandonment of the project to write 'a group of monographs on special economic problems' in favour of a comprehensive treatise which should be born complete and fully armed from the head of an economic Jove" (Ibid., p. 152). In the second half of the 19th century the social question existed in Britain even in a more sharp form than in Germany, but the professional reaction of British economists, including Alfred Marshall, to the social question was totally different from the reaction of German economists, including Gustav Schmoller. We could see just above that at the very beginning of his career as an economist, Marshall had intentions to follow the way of Germans. Keynes explained his refusal to fulfil his initial plans by some personal circumstances, his marriage and "an illness so serious that for some time Marshall appeared unlikely to be able to do any more hard work" (Ibid.). However I think that these personal circumstances are not the only causes of his decision. I suppose that his social environment has influenced this decision more than the circumstances. Keynes has discovered in the Marshall's archive a document (a manuscript) that, in my mind, can throw light on the deepest cause of the professional orientation of young Marshall. In this manuscript, which was designed for the Preface to the book *Money, Credit and Commerce*, Marshall wrote: "About the year 1867 (while mainly occupied with teaching Mathematics at Cambridge) [I have been in] touch with the question: how far do the conditions of life of the British (and other) working classes generally suffice for fullness of life? Older and wiser told me that the resources of production do not suffice for affording to the great body of the people the leisure and the opportunity for study; and they told me that I needed to study Political Economy. I followed their advice, and regarded myself as a wanderer in the land of dry facts; looking forward to a speedy return to the luxuriance of pure thought" (Ibid., p. 138). I suppose that "older and wiser men" advised him to study Political Economy to find there an answer to his question of a sort that the poverty of the great body of the people was inevitable and everybody in the British society got according to his merits, and it was not worthwhile to think about any social change/reform. I can guess that the same "older and wiser men" could hardly approve his desire to "get into closer contact with practical business and with the life of the working classes" and a "society of trade unionists, co-operators and other working-class leaders". I also assume that Marshall's decision not to do fieldwork investigations has been dictated by the fact that "direct studies of life and work would not yield much fruit for many years". At last but not least, scholastic traditions of Cambridge of that time shared by Marshall's superiors and colleagues certainly did not encourage this kind of activities by the faculty staff. On the contrary, Marshall was very strong in mathematics and the notion of applying mathematical methods in economics was in the air. His social environment probably should have supported his engagement in this domain. Thus, Marshall remained during a very short time "a wanderer in the land of dry facts" and his social environment has pushed him "to a speedy return to the luxuriance of pure thought". We probably can make the conclusion that in the values shared by Marshall, values of a "craftsman" working to earn his life dominated the values of an "altruist investigator" working to understand social reality in order to contribute to its improvement (the vision of J. Fichte). Certainly, Utilitarian ideas of J.S. Mill and his definition of economics as an abstract science have helped Marshall to legitimate his position.

Unlike in Germany, in England of the 19th century, one might be called "economist" without this automatically implying an employment as university teacher; to be an "economist" in Great Britain at this time meant the feature of a certain "social and political culture – hence the foundation and naming of *The Economist* in 1843 as a journal to support the cause of Free Trade" (Tribe, 2002, p. 4). Political economy was a hot topic in popular reviews and clubs, but was relatively neglected in universities (Fourcade, 2009, p. 132). Before the creation of the *Royal Economic Society,* "the Political Economy Club of London,

founded in 1821, provided an organisational centre for the discussion and propagation of 'sound' doctrines, particularly free trade. However, it was but one of many such private clubs at home and abroad, and the other prerequisites of professionalisation were lacking. " (Coats, 1993, p. 402). Practitioners of political economy, like members of this club, were leisured gentlemen, enlightened businessmen, intellectuals, journalists, statesmen, and civil servants (Fourcade, 2009, p. 132). The profession of university teachers of economics was underdeveloped: "In the mid-1890s there were only two full-time Professors of Political Economy – Marshall in Cambridge and Gonner in Liverpool – although some teachers, like Flux in Manchester, were fully occupied with a wide range of teaching" (Tribe, 2002, p. 4). Until the 1890 neither commercial professions nor the British civil service considered political economy as a useful discipline (Fourcade, 2009, pp. 131 – 132). Alfred Marshall was a real architect of the British institution of economics, which became a model for this institution in the United States and, after World War II, and especially after the fall of the Berlin Wall, with "Americanisation" of the discipline, for the whole world. However the architect acted in a determined social environment, in the framework of a specific elite culture which "was heavily vested in 'classics'" that included also mathematics, "regarded as part of a 'classical heritage' going to the Greeks" (Ibid., p. 149). "Natural sciences were incorporated quite late in British university education, sometimes not until 1880s" and "mathematics <…> throughout the nineteenth century reigned at Cambridge as a fundamental component of the prestigious tripos examination system" (Ibid.). In this intellectual atmosphere it is the connection of a discipline with mathematical method, and not with experimental one, which was the decisive sign of a truly "scientific" character of the discipline. In order 'to merit' this indication British economists were oriented to "the progressive elimination of most inductive and historical elements from the core of political economy, and concomitant ascendancy of the deductive method" (Ibid.).

"In 1885, when Marshall became professor of political economy at Cambridge, the condition of his subject was profoundly discouraging, and its immediate prospects seemed little better <…> Political economy still formed only a minor element in a traditional curriculum dominated by classics and theology; <…> and it was virtually ignored in the examination system of what has rightly been called an 'exam ridden' country" (Coats, 1993, p. 106). He improved social position of the discipline by several institutional measures. First, Marshall elaborated and introduced a unified examination system in economics (Ibid., p. 111), and in this way, all British students of economics passed through the purgatory of his neoclassical economics. This examination system "was a centripetal force tending to counteract the effects of the disintegration of classical dogma, a process which might otherwise have proved destructive at a time when provincial centres of academic economics were growing" (Ibid., p. 120). Secondly, he endeavoured quite successfully to eliminate the methodological disagreements in the community of academic economists and strengthen the sense of continuity and local loyalty, deference to authority and respect for tradition (Ibid., p. 107 - 109). Thirdly, he encouraged "specialised inductive studies only after and not before the B.A. degree" (Ibid., p. 111) and thus invited students to see the reality through the spectacles of his theory. At last, I suppose that Marshall realised that a successful start of an institution depends to a great extent on the initial membership of the community which follows newly introduced rules and shares ideas/values standing behind these rules. As one of his contemporary witnesses in this direction he was also successful: "half the economic chairs in the United Kingdom are [were] occupied by his [Marshall's] pupils, and the share taken by them in general economic instruction in England is [was] even larger than this" (Ibid., p. 107), "Marshall undoubtedly had a voice in most appointments in England (and possibly in Britain and the Commonwealth too) during his quarter-century as professor at Cambridge" (Ibid., pp. 121 – 122).

In comparison with *Verein für Sozialpolitik,* the *Royal Economic Society* created in Great Britain in 1890 was of an absolutely different nature. As we can judge by analysing the report of the inauguration meeting of this society (*The Economic Journal*, March, 1891, pp. 1 – 14), its main objective was the publication of a journal. At this meeting Alfred Marshall was opposed to the idea of holding meetings for discussions: "they might be attended chiefly by people whose time was not very valuable" (Ibid., p. 8). He was in favour of controlling of membership and opinions. A speaker at the meeting expressed ideas "which were not quite consistent with the catholicity which Professor Marshall has demanded" this control. Concerning membership "persons were required not only to desire to further the aims of the Association, but to be approved by the council before they could be admitted to membership. Probably some gentleman present would like to have these words omitted" (Ibid., p.9). The same speaker noted that "Professor Marshall would say that there must be authority somewhere, and some opinions must be excluded" (Ibid.). The Royal Economic Society was born *not as an association of researchers* similar to the Royal Society of London for the Improvement of Natural Knowledge but as a *guild of craftsmen-economists*, i.e. of persons belonging to the profession of university teachers of economics.

Schmoller's *Verein* included many members which were not academics but administrators. German economists worked with very close contacts with practical men. On the contrary, the British community of academic economists followed the tradition of distancing from the latter. Ricardians already had as the source of their influence "their claim for scientific authority which they strenuously asserted in books, pamphlets, magazines, newspapers, official enquiries and parliamentary debates" (Coats, 1993, p. 402). In their aspiration to look 'scientists' "the most distinctive feature was their effort to distance themselves, as experts, from the amateurs, especially despised 'practical' men" (Ibid.) James Mill, father of the J.S. Mill, has formulated it in the following way: "a reasoner must be hard pressed when he is driven to quote practical men in aid of his conclusions. There can not be a worse authority, in any branch of political science, than that of mere practical men" (Ibid.). British political economists of the 19th century tried to simulate the behaviour of natural scientists in a very superficial way by distancing from 'non-scientists', but ignoring the most important feature of their behaviour: experimental contact with the object under study. They did not realise that 'practical men' for many types of information are the only sources available. Practical men are elements of the objects of study and not being in contact with them for those who work as scientists in socio-politico-economic domains means to work with 'switched off recording devices'. In the domain of natural sciences it would mean the end of the profession of scientific researcher. Qualitative research methods like interviews, narrative inquiry, participant observation and participatory action research (Denzin and Lincoln, 2005), channel contacts of researchers with practical men, which are functional analogues of laboratory practices in natural sciences. German economists understood it, but British not. A few English economists, like William Cunningham, who were willing to follow the German example, were ostracized. Neither British state nor business community were interested in empirically based economic research, but both of them were interested in the existence of a theology legitimating their power. With the gradual disappearance of Christian belief supporting the power of king and landlords, new beliefs were absolutely necessary to legitimate the new social order: capitalism. These beliefs are provided by the neoclassical economics. This kind of functions totally corresponds to the British university traditions.

5. Institutionalisation of economics in the United States: from "craftsman" to "merchant"

> I asked myself the question, "Did [Columbia College, where I have had three years as an undergraduate,] stimulate and encourage that research which results in significant thought?" <…> The only honest answer I could give myself was "no". The centre of attention was occupied by classics and mathematics, and the question of academic freedom did not even arise.
>
> (Richard T. Ely, 1938, p. 124)

The German and English approaches were confronted in the United States at the end of the 19th century. At this time, this country had two types of universities. On the one hand, universities inherited from the colonial period, when territories on which the United States were created were just overseas territories of Great Britain; these universities were naturally created on the British model. On the other hand, after the Independence, new universities appeared in the United States, which were directly created as research universities, having as a model the German one. Harvard University belonged to the first type. First founded as Harvard College in 1639, it is the oldest university of the country. At the beginning, it was a Congregationalist educational establishment, which, during many years, trained Puritan ministers. It was named after a young British clergyman named John Harvard. The College followed the English university model. A brochure, published in 1643, justified the College's existence: "To advance Learning and perpetuate it to Posterity; dreading to leave an illiterate Ministry to the Churches". Its motto was *Veritas Christo et Ecclesiae* "Truth for Christ and the Church." Gradually, specialised faculties and schools appeared. At present Harvard University consists of nine faculties, among which Harvard Divinity School created in 1816[15]. John Hopkins University was directly created in 1876 according to the German model of research university. Its president, Daniel C. Gilman, stated that the goal of the university was "the encouragement of research…and the advancement of individual scholars, who by their excellence will advance the sciences they pursue, and the society where they dwell." Following Humboldt's concept, he merged teaching and research; he dismissed the idea that the two were mutually exclusive. John Hopkins University was the first American university to teach through seminars, instead of solely through lectures. The motto of this university was *Veritas vos liberabit* – "The truth will free you." Maybe the reader can already guess among these two universities which one will become a citadel of the neoclassical economics and which one will open the door for scientific (based on experimental approach) economic research in the United States.

Frank William Taussig (1859-1940) was not included by Mark Blaug in his list of 100 great economists before Keynes, but it does not correspond to his influence on the institutional evolution of economics in the United States. Now we can say that his influence is really worldwide because after World War II, American economics dominates in the Western World. He played in the U.S. a similar role than Alfred Marshall in Great Britain. He was the key person to spread in the U.S. neoclassical economics. He graduated from Harvard and occupied a highly influential position in American economics as a professor of this university

[15] http://en.wikipedia.org/wiki/Harvard_University

from 1885 to 1935. He influenced the profession of economist in the U.S. by his 1911 textbook and his control on the Quarterly Journal of Economics. He was the editor of this journal from 1889 to 1890 and from 1896 to 1935. In 1904-1905 Taussig was president of the American Economic Association[16] but he was not its founder. The founder of this association was Richard Theodore Ely (1854-1943). Unlike Taussig, Ely was included by Blaug in his list of 100 great economists. In his Ely's short professional biography, Blaug quite well depicts his professional life story as a sequence of events, but my interpretation of this story will be totally different.

In 1876 Ely graduated from Columbia College. A year after, he went for 3 years to Germany for graduate and post-graduate studies. In Halle, he met one of his American friends who introduced him to his professor Johannes Conrad (1863-1915), co-organiser of Schmollerian *Verein für Sozialpolitik* and chief editor of the influential journal, *Jahrbücher für Nationalökonomie und Statistik.* Young Ely hesitated in his choice in profession between philosophy and economics. Under the influence of lectures of Conrad, he made his choice in favour of economics. In 1878, he came to Heidelberg for PhD studies under the guidance of Karl Knies (1821-1898) and got his degree within a year. Knies' books and lectures became for him a kind of bible (Ely, 1938, p.111). He remembered: "From Knies and others I was learning a fundamentally scientific approach in which relativity and evolution played a large role." (Ibid., p.58) This approach denies that the same policies can be good for all times and all countries, and in its way, it totally rejects dogmatic English economics. Under the influence of Knies, he also understood that men rather than abstract mechanical laws of the classicists should stand at the centre of all economic studies (Rader, 1966, p.13). Knies was a representative of the old German historical school, but Conrad was a member of the new one, of which Schmoller was the leader. Certainly Ely was inevitably under the influence of Schmoller who affirmed that German economics had come to a historical and ethical conception of the state and society, totally different from that expressed by neoclassicists and Marxists. According to him, it was no longer a mere theory of market and exchange, a kind of political economy of business, which threatened to become a class weapon of the wealthy. He proclaimed that it has become again a great political and moral science, which studied production of goods and their distribution, the exchange phenomena and also economic institutions. This science has placed the men in the very centre of its studies, and no longer commodities and capital.[17]

From 1881 to 1892, Ely taught in John Hopkins University. This period was for him the most productive and at the same time the most difficult in his career. As a matter of fact, persons sharing the same views as Taussig were very numerous in American universities. Ely, as an advocate of ideas and approaches of German historical school, was often attacked by some of his colleagues, and not all students correctly appreciated his critics of a priori abstract approach and his account of German alternative approach to economic research (Ibid., p. 29). During the second year of his work in John Hopkins University, he already presented at the university seminar a polemical paper called "The Past and the Present of Political Economy". Later he published an extended version of this paper under the form of a brochure which is ended with the following sentences: "… the historical method of pursuing political economy

[16] http://en.wikipedia.org/wiki/Frank_William_Taussig

[17] „Die heutige Volkswirtschaftslehre ist zu einer historischen und ethischen Staats- und Gesellschaftsauffassung im Gegensatz zum Rationalismus und Materialismus gekommen. Sie ist aus einer blossen Markt- und Tauschlehre, einer Art Gesellschaftsökonomie, welche zur Klassenwaffe der Besitzenden zu werden drohte, wieder eine grosse moralisch-politische Wissenschaft geworden, welche neben der Production die Vertheilung der Güter, neben den Wertherscheinungen die volkswirtschaftlichen Institutionen untersucht, welche statt der Güter- und Capitalwelt wieder den Menschen in den Mittelpunkt der Wissenschaft stellt" (Schmoller, 1998, s. 202 – 203).

can lead to no *doctrinaire* extremes. Experience is the basis; and should an adherent of this school even believes in socialism as the ultimate form of society, he would advocate a slow approach to what he deemed the best organization of mankind. If experience showed him that the realization of his ideas was leading to harm, he would call for a halt. For he desires that advance should be made step by step, and opportunity given for careful observation of the effects of a given course of action. Again: this younger political economy no longer permits the science to be used as a tool in the hands of the greedy and the avaricious for keeping down and oppressing the laboring classes. It does not acknowledge *laissez-faire* as an excuse for doing nothing while people starve, nor allow the all-sufficiency of competition as a plea for grinding the poor. It denotes a return to the grand principle of common sense and Christian precept. Love, generosity, nobility of character, self-sacrifice, and all that is best and truest in our nature have their place in economic life." (Ely, 1884, p. 64)

The president of the university, Daniel Gilman, was upset by the appearance in the press of a sharp critical reaction on this publication. Fortunately for Ely a powerful regent of the university wrote to Gilman reassuringly: "The critique represents the school of laissez-faire, to which I incline myself very strongly, but Political Economy is not a completed science and the Historical School has something to say for itself" (Rader, 1966, p. 31). However, mathematician Simon Newcomb (1835 – 1909), soon sent a letter to the president Gilman concerning this brochure. In this letter he wrote: "It looks a little incongruous, to see so sweeping and wholesale attack upon the introduction of any rational or scientific method in economics come from a university whose other specialities have tended in the opposite direction <…> I have never been able to see any essential difference between the objections raised against political economy from the new school point of view and the general objections of the public against the value of theoretical science." (Rader, 1966, p. 32) It is worthwhile to mention here some biographical details of Newcomb because he was one of the first representatives of later very numerous specie of persons trained in mathematics and/or theoretical physics who tried their chances in economics. In spite of the peripheral character of economics in his professional interests and occupations, Newcomb was included by Blaug in his list of the 100 greatest economists before Keynes. "A follower of Stanley Jevons, Newcomb, wanted to introduce more mathematics and the concept of marginal utility into economics" (Ibid.)

Newcomb graduated from Lawrence Scientific School of Harvard University with BS degree in 1858. He supported himself with some school-teaching before becoming a human computer (a functionary in charge of calculations) at the Nautical Almanac Office in Cambridge, Massachusetts, in 1857, of which in 1877 he became the director[18]. In 1861 he became a professor of mathematics in the US Navy, and professor of mathematics and astronomy at Johns Hopkins University. In 1885 he published a book with the same title than Marshall's textbook: *Principles of Political Economy*. What attracted Newcomb, and later, especially during and after World War II, many of those who mastered and loved mathematics, in economics? I suppose it was, and continues to be, the easiness for them to get highly evaluated in the profession of economics "scientific results". Because of the tradition, coming from the University of Cambridge, to consider application of mathematics as a summit of "scientificity", it was very attractive to enter the scientific community via economics with much more chances and far fewer efforts to succeed than it would have been require via physics. We do not have memoirs or interviews of Newcomb, but I think we can better understand motivations of Newcomb by analysing those of Paul Samuelson who was actively interviewed. Once Samuelson told: "I became an economist quite by chance, primarily because the analysis was so interesting and easy" (Szenberg, Gottesman and

[18] http://fr.wikipedia.org/wiki/Simon_Newcomb

Rarattan, 2005, p. 33). It was so easy "as fishing in a virgin Canadian lake. You threw in your hook and out came theorem after theorem" (Samuelson and Barnett, 2007, p. 154). The working method of Newcomb and Samuelson was the same: to reproduce application of mathematical methods in physics to economics. Samuelson was not a physicist but he "had an acquaintanceship with scores of leading world mathematicians and physicists" and received "essential hints" for his work from a thermodynamicist[19] (Ibid., p. 155). This working method is a caricature of the Newtonian method: "Samuelson's signature method of economic theory, illustrated in his *Foundations* (1947), seems to follow two rules which can also be said to characterize much of Neoclassical economics since then: With every economic problem, (1) reduce the number of variables and keep only minimum set of simple economic relations; and (2) if possible, rewrite it as a constrained optimization problem" (Ibid., p. 144); "By modelling economic agents in this way, he hoped to be able to predict their behaviour in much the same way that physicists predicted the behaviour of physical objects" (Szenberg, Gottesman and Rarattan, 2005, p. 21).

In my opinion, it is the comparative *easiness* of Newcomb-Samuelson's version of working on papers for professional journals *and* teaching their type of economics which was one of the important causes of the final victory of their current of economic thought over interpretive/historical institutionalism in economics. As I already mentioned before, interpretive/historical institutionalist version of teaching economics, by the involvement of students in research concerning burning socio-economic-political problems, requires from professors much more efforts than just repeating without no very great changes quite simple mathematical constructions and supervising solution by students of numerical examples illustrating these constructions. Those university graduates with good mathematical background, who are looking for a job valorising their mathematical skills and having value system of craftsmen rather than altruist investigators, could be very much attracted by the profession of academic economists in its neoclassical version. As a result of the fact that representatives of the profession who tried to make research concerning burning socio-economic-political problems were persecuted (I will onto this topic below), by powerful forces from outside of the profession, the profession became unattractive for "altruist investigators". As a consequence, "craftsmen" became majority in the profession of academic economists and could then control its neoclassical purity without frequent punitive sanctions (coming, for example, from members of university boards of regents) towards dissidents. According to Berger and Luckmann in this case the institutionalisation is successful.

For Newcomb "rational or scientific method" was mathematics, and Ely's brochure was viewed by him as exclusively a "wholesale attack upon the introduction of any scientific method in economics". Apparently he did not pay attention to the following statements of the brochure: "Young German professors of the Historical School <…> studies the present in the light of the past. They adopted experience as a guide, and judged of what was to come by what has been. Their method may also be called experimental. It is in many respects the same which has borne such excellent fruit in physical science <…> These men claimed that the whole life of the world had necessarily been a series of grand economic experiments, which having been described with more or less accuracy and completeness, it was possible to examine <...> It is on this account that a knowledge of history is absolutely essential to the political economist <…> it is impossible to comprehend the economic life of to-day without regard to the past" (Ely, 1884, pp. 43 – 46) It is necessary to be autistic to evaluate the above

[19] The role of physical metaphors in the development of neoclassical economics was studied by Philip Mirowski (1989), "who argues that the formulation of neoclassical theory in the 1870s was a "wholesale" metaphorical appropriation of the analytical structure of mid-nineteenth century physics. Neoclassical economics is thus seen not as a "discovery," but as an arbitrary imposition onto social reality of a paradigm taken from an alien field of knowledge" (Carlson, 1997).

statements as “the general objections of the public against the value of theoretical science”. Newcomb made calculations in the field of astronomy which study quite simple system, and the mechanistic view of the world was sufficient for his work. However this view becomes totally inappropriate in the study of more complex natural systems. He did not know and did not understand it[20]. What was determining in Newcomb’s attacks on Ely: his world view as a scholar or his political beliefs? I suppose the latter was decisive. In his popular publications, he “took a stern line in favour of rugged individualism in opposition to the ‘fallacies’ of labour unionism” (Blaug, 1986, p. 177). Certainly Newcomb could not like Ely’s evaluation of classical political economy as a partisan ideological construction: “Privileged classes, especially captains of industry, found in classicism a rational for their exploitation and freedom from regulation. According to orthodox canons, assistance to economic unfortunates interfered with natural law. The labourer received exactly what he deserved” (Rader, 1966, p. 41 - 42). Apparently it is for this kind of discourse that Newcomb denounced Ely as unfit to hold a University Chair (Blaug, 1986, p. 177). John Hopkins University, based on the German model, could not very much protect its professor Ely from the attacks of those who did not wish the existence of experimentally-based economic research in the United States.

The antagonism between Ely and Newcomb lasted at least two years (1885 and 1886). It was accompanied by attacking each other in papers at seminars and by publication of mutually critical articles in journals. It was a time when in American research universities a hostile coexistence between theology-like and research oriented currents of economists’ profession was yet possible. The institutionalisation of American economics has just started and punitive sanctions from outside of the profession were necessary to orient it in favour of the ruling class. Ely became subjected to this kind of sanctions in 1894 when he already worked in Wisconsin University. Concerned by the social question, as his German teachers, Ely could not be indifferent to its aggravation. He remembered: “In 1894 the nation was in the throes of a depression; unemployment and misery reached new heights; radical sentiment was rising” (Ely, 1938, p.218). Engaged in field studies Ely expressed his concern in his publications. It was used as a pretext by the Wisconsin’s State Superintendent of Education and a member of the University of Wisconsin’s Board of Regents, Oliver E. Wells, to provoke a real trial with participation of attorneys on behalf of the accusation (Wells) and the defence (Ely) in a special commission organised for this occasion [(Ely 1938, p. 218 – 233), (Rader 1966, p. 130 – 158), (Schlabach, 1963 - 1964)]. Finally Ely has been acquitted but he became increasingly conservative in his opinions after surviving the accusation [Rutherford 2005, p. 4]. As A.W. Coats indicates, “it is easy to understand why the shift of emphasis from teaching of established truths to the advancement of knowledge and the investigation of current problems was liable to generate frictions between the social scientists and certain segments of their audience” (1993 p. 439). He explained these frictions in the following way: “The late nineteenth century was a time of disturbing economic, social, and political tensions, and the fact that the business community was generally getting a bad press when the economists were undertaking more thorough studies of their activities increased the likelihood that even the most objective and impartial enquiries would furnish ammunition for the innumerable critics

[20] A quite an interesting detail of Newcomb’s biography perhaps may also throw a light on his attacks of Ely: “Newcomb studied mathematics under Benjamin Peirce and the impecunious Newcomb was often a welcome guest at the Peirce home. However, he later became envious of Peirce's talented son, Charles Sanders Peirce and has been accused of a "successful destruction" of C. S. Peirce's career. In particular, Daniel Coit Gilman, president of Johns Hopkins University, is alleged to have been on the point of awarding tenure to C. S. Peirce, before Newcomb intervened behind the scenes to dissuade him. About 20 years later, Newcomb allegedly influenced the Carnegie Institution Trustees, to prevent C. S. Peirce's last chance to publish his life's work, through a denial of a Carnegie grant to Peirce, even though Andrew Carnegie himself, Theodore Roosevelt, William James and others, wrote to support it.” http://en.wikipedia.org/wiki/Simon_Newcomb

of contemporary capitalism <…> Laissez-faire and conservative social Darwinism were still the ruling beliefs among members of the social and business elites, whereas many of the younger social scientists were reformers who regarded uninhibited individualism and unfettered competition as the cause of many , if not most, current economic and social evils <…> As might be expected, in some quarters such views were regarded as dangerously radical, and tensions mounted within the academic community as businessmen increasingly replaced clergymen on college and university boards of trustees" (Ibid., pp. 439 – 440).

The personality of Richard Ely can serve as an illustration of the connection which inevitably exists between values shared by an economist and methodology she/he is using. Experimental studies are very difficult and it is necessary to be very motivated in order to do them properly. For Newton and Boyle this motivation was linked with the idea that by their investigations they discover God's design and in this way they approach themselves and human race to God. For such researchers as William Cunningham, Richard Ely and John Commons, Christian ideas also played a very important role in their professional orientations. All three of them besides their works reflecting their scientific investigations left us books in which they considered socio-politico-economic problems from the point of view the Christian teaching [(Cunningham 1902, 1909), (Ely, 1889), (Commons, 1967)]. Ely was one of the leaders of the Social Gospel movement, a Protestant Christian intellectual movement which applied Christian ethics to social problems, especially towards poverty, inequality, liquor, crime, racial tensions, slums, bad hygiene, child labour, weak labour unions, poor schools, and the danger of war. Above all they opposed rampant individualism and called for a socially aware religion. Important concerns of the Social Gospel movement were labour reforms, such as abolishing child labour and regulating working hours for mothers. By 1920 they were crusading against the 12-hour day for men at U.S.[21]. Experimental socio-economic studies are perhaps even more difficult in the domain of natural sciences and it is necessary to be very much motivated *socially* in order to do them properly. To my mind, some kind of altruism is absolutely necessary to someone engaged in social experimental research. Apparently not all Americans who got their PhD degrees in economics in Germany in the second half of the 19th century had the needed level of altruism for experimental work. But the decisive factor forcing them to deviate from application of German research methodology was the hostile climate towards economic experimental research in general, and experimental research oriented to socio-economic reforms in particular, in most of the American universities due to their explicit or implicit control by business community.

The story[22] of John Bates Clark (1847 –1938) represents maybe the most spectacular turnabout from institutionalism to neoclassicism (Fourcade, 2009, p. 81). From 1872 to 1875 he attended the University of Zurich and the University of Heidelberg where he studied under Karl Knies. Upon his return to the United States, Clark taught economics, history and a whole series of other subjects at Carleton (where he taught Thorstein Veblen). After his return from 1877 onward Clark published several articles most of them edited later in *The Philosophy of Wealth* (1886). Early in his career Clark's writings reflected the view of his German teachers that competition is no universal remedy – especially not for fixing wages. His view on fair wages changed in 1886. By the time he wrote *The Distribution of Wealth* (1899) he was convinced that pure competition was the natural and normal law by which the economic order obtained justice. One cause that prompted this reorientation could be the Haymarket Riot 1886 in Chicago when some strikers were shot and others hanged – event still remembered the 1st of May worldwide outside the US. In the US it resulted in a cleansing of higher education from *socialist* reformers. In 1988, he wrote: "*The science adapted ... is economic*

[21] http://en.wikipedia.org/wiki/Social_Gospel
[22] The story is taken from http://en.wikipedia.org/wiki/John_Bates_Clark

Darwinism. … Though the process was savage, the outlook which it afforded was not wholly evil. The survival of crude strength was, in the long run, desirable". This was the fundament to develop the theory which made him famous: Given competition and homogeneous factors of production *labor* and *capital*, the repartition of the social product will be according to the productivity of the last physical input of units of *labor* and *capital*. In 1895, Clark finally won a position at Columbia University where he spent most of his career. In the history of American economics he is considered as neo-classical economist, one of the pioneers of the marginalist revolution and opponent to the Institutionalist school of economics. The John Bates Clark Medal, one of the most prestigious US-awards in the field of economics, was awarded in 1947 to Paul A. Samuelson whose textbook *Economics* divulged Clark's capital concept worldwide.

Stories of other American economists graduated from German universities are less grotesque than that of Clark but can help to understand why historical institutionalism had not very large ground in the USA. Among them Nelson Patten (1852-1922) who graduated from University of Halle in 1878 with a PhD in economics. It was Patten who connected Ely with Professor Conrad, and in this way involved Ely in the study of economics. Patten found only disappointment upon his return to America. He worked on a farm for a year. He tried to study law in Chicago, but developed terrible eye troubles that prevented him from reading. After receiving a successful treatment for his eyes, he worked as a teacher in a district school and subsequently became superintendent of schools in Rhodes, Iowa. In the meantime, after his vision was restored, Patten began working on his first book "The Premises of Political Economy" which was published in 1886. The book was an adaptation of John Stuart Mill's thinking to fit the economic situation in America, and led to Patten's appointment to a position at Wharton School of Economics in University of Pennsylvania. He would remain at Wharton School until 1917 and made it the most influential centre of economic theory in the United States[23]. Another American receiving his doctorate in political economy from University of Halle (1877) was Edmund J. James (1855 — 1925). Upon his return to the United States he received an appointment as a high school principal. In 1883, James was appointed at the University of Pennsylvania as professor of public finance and administration. It was there that he became the director of Wharton School of Finance and Economy[24]. Patten and James in 1884 tried to organise in the United States something similar to *Verein für Sozialpolitik,* but failed (Ely, 1938, pp. 132 – 135). Neither Patten nor James contributed to the introduction of German historical school experimental tradition in the United States.

Beginning from the mid-nineteenth-century Christianity inspired both camps of American economists, advocates of laissez-faire and its opponents. I already spoke of Social Gospel movement, but the opposite movement existed: "Members of so-called clerical school of academic economists <…> worked closely with a group of wealthy and prominent men of affairs. Their common goal was the installation of laissez-faire as an American system of economics" (Furner, 1975, p. 37). After the Civil War they continued to develop the mid-century economic synthesis which "was the joint creation of academics who domesticated English classical economics as a scientific substitute for moral philosophy and American businessmen who needed just such a rationale for the developing industrial economy" (Ibid., p. 36). Perhaps the most prominent academic economist issued from the clergy was William Graham Summer (1840-1910), influential Yale professor and Social Darwinist. He defended radical *laissez-faire* as being justified by laws of evolution[25]. The influence of Summer was far more significant than that of Newcomb. As Ely, he studied in Germany, but theology and not economics. Leaning under the guidance of gifted theologians he grasped the meaning and

[23] http://www.encyclopedia.com/doc/1G2-3468300189.html
[24] http://en.wikipedia.org/wiki/Edmund_J._James
[25] http://homepage.newschool.edu/het//profiles/sumner.htm

spirit of scholarship (Ibid., p. 43). After his return to America "Summer was ordained an Episcopal minister and actually preached for a few years in a stylish New York church" (Ibid.). "Like many of his educated contemporaries, Summer wanted to study the economic and social problems of his day scientifically" and "in "Herbert Spencer's *Study of Sociology* (1870), he found the model for an organic study of society which he had been seeking. "Resigning his clerical post, Summer became the first occupant of a chair in political economy and social science at Yale" (Ibid.).

The controversy with Newcomb has inspired Ely to organise the *American Economic Association* in 1885 in order to combat "the Summer-Newcomb crowd". Simon Newcomb was already president of the Political Economy Club located in New York where conservative economists dominated. The Ely's proposal for this association included the following statements: "While we recognise the necessity of individual initiative in industrial life, we hold that the doctrine *laissez-faire* is unsafe in politics and unsound in morals; and it suggests an inadequate explanation of the relation between the State and citizens <…> we believe that political economy is still in the first stages of its development, and we look not so much to speculation as to an impartial study of actual condition of economic life for the satisfactory accomplishment of that development <…> We hold that the conflict of labour and capital has brought to the front a vast number of social problems whose solution is impossible without the united efforts of Church, State, and Science" (Rader, 1966, p. 35). He wanted an organisation not just for a mutual interchange of economic ideas but for publicising and implementing a social reform program. Many participants of the organisational meeting did not support a platform aimed directly at reform (Ibid., pp. 35 – 37). In spite of Ely's efforts during many years as secretary of the AEA, it was finally transformed in a British-like association. The continuation and very successful development in the United States of the scientific economic tradition born in Germany under the form of Wisconsin Institutionalism was due to an extraordinary combination of circumstances: move of R. Ely, and later of one of his students, John Commons, to Wisconsin where political will of the governor/senator Robert La Follette, supported by the President Theodore Roosevelt, created very favourable institutional conditions for experimentally-based economic research in the University of Wisconsin. These institutional conditions received the name of the "Wisconsin Idea". In spite of the great national socio-economic-political impact of the Wisconsin Institutionalism, its institutional model (rules of producing knowledge in the discipline of economics and teaching students specialised in this discipline) remained an isolated island where German approach to economics was adopted in the sea of traditional British-like university model. That is why I will jump here Wisconsin Institutionalism; I will come back to it in the following section of this paper, and will try to mention very shortly some decisive factors which oriented the itinerary of institutional evolution of the discipline of economics in the United States.

As Mary O. Furner (1975) has shown, the most decisive factor in the evolution of American economics at the turn of the 19th and 20th centuries was a political one. She indicated that academic economists were subject to an increasing "external control (such as boards of trustees and university administrators, or state legislatures in the case of public universities)" dissuading them from working in favour of social reforms. I already mentioned below in this paper political attacks on Richard Ely, but he was not alone: "During the wave of academic freedom cases that spanned from the 1890s to the 1910s, many economists came under sharp public attack for promoting views that offended powerful constituencies in matters as varied as the labour movement, free silver coinage, public utility franchises, or fiscal policy" (Fourcade, 2009, p. 79). This type of political attacks "drove them to confine their scholarship to 'safe' intellectual grounds". Neoclassical economics and especially in its mathematical form was ideal for serving such ground. That is the reason why it became an "attractive research strategy by American economists, especially by the younger generations

who had to create a position for themselves" (Ibid., pp. 79 – 80). The purely political and economic interests behind these attacks were camouflaged by the falsifying discourse of the necessity of the switch from "advocacy" to "objectivity". This switch meant in reality the *refuse from studying objectively reality* in order to solve some burning socio-politico-economic problems in favour of *studying constructed abstract imaginary worlds* legitimating the status quo and avoiding any touch with disturbing matters for the establishment.

Other factors, linked with the first one, influencing the institutional evolution of the discipline of economics in the United States were the demise of the Social Gospel movement and the use of the image of science to legitimate conservative opinions (Bateman, 1998). The discourse of Social Gospel movement concerning religion, social justice, and welfare was substituted by the discourse of conservative economists about science, efficiency, and free enterprise. "Not all American economists in 1920 would have happily identified with the whole range of Ely's and Commons's work <…> but in the glow of the Social Gospel's golden years, historical and institutional approaches were accepted and respected" (Ibid. p. 41). These acceptance and respect disappeared, or at least decreased, with the switch from the image of science as experimental activity to purely theoretical one, in which just the fact of using mathematics already signifies its scientific character: "Whereas institutional economics seemed perfectly 'scientific' in 1922, by 1947, it was no longer unquestionably regarded as such" (Ibid., p. 48). Academic freedom in the case of the discipline of economics is very relative: "During the twentieth century, there have been primarily four patrons of economics: higher education, the government, the business community, and charitable foundations" (Goodwin, 1998, p. 54). Practically all of them contributed, including by the selective financing, to the gradual diminution of the weight of any current of economic thought different from neoclassical and considered as troublemaking (Ibid., pp. 78 – 79). Finally, because very close interaction between community of economists and community of mathematicians, evolution of economics was more influenced by the internal tendencies in the development of mathematics than by the changing economic reality with its burning problems (Weintraub, 2002). Thus *the reign of neoclassical economics and its transformation into the mainstream cannot be considered to be due to its scientific superiority.* The result of this evolution is disappointing. Surveys of PhD students in USA universities (Colander and Klamer, 1990), (Colander, 2007) show "a picture of a profession lost in pure theory and technicalities with little focus on ideas. There was a sense that economics dealt with mind games, not real economics problems" (Ibid., p. 9). It is just a confirmation of the evaluation of Ariel Rubinstein given in the third section of this paper: the contemporary economics has a character of a set of "fables" which has "very little to say about the real world and that there are very few models in economic theory that can be used to provide serious advice".

Contemporary economists are either *Philosophers/Mathematicians* with their a priori method or *Econometricians/Statisticians* trying to make explanation and/or forecasting on the basis of purely quantitative models and data. German and American interpretative/historical institutionalists worked more like *Anthropologists/Historians* who used interviews, participant observation, action research and different types of analysis of qualitative and quantitative historical data. The latter mode to work requires particular professional values. My long participant observations in departments of economics of Russian and French universities, as well as my experience of communication with members of communities of academic economists throughout the world, allow me to propose the following typology of academic economists from the point of view of their professional values:

1. Economist - university teacher/researcher has to study the reality in order to understand it, and in this way to be useful to the society by transferring to it, in particular to his students, this understanding. This cognitive activity itself represents for him a passion, and material

reward for this activity has a secondary importance. I call this type of economist an *Altruist Investigator*. This type corresponds to the Fichte's notion of the vocation of the scholar.
2. The profession of academic economist is just a profession as any others. In a profession, to do your work properly means to respond to expectations of your employers, clients and colleagues. The academic economist produces theoretical constructions, beauty of which brings him satisfaction only in case of a good rewarding of his productive activities. I call this type of economist a *Craftsman.*
3. Academic economist has to survey carefully which directions of scientific production are in fashion, allow getting grants, welcomed in professional journals, conferences and seminars. He has to orient and organise his research activity (writing and presentation of papers) in such a way that it allows him to better "sell" results of his scientific production. By adopting the term used by Marion Fourcade (2009), I call this type of economist a *Merchant.*

The table below shows compatibility of certain methodologies with certain values. It gives an additional explanation why interpretive/institutional current of American economics has disappeared. When the community of economists is created as a community of "craftsmen" and "merchants", an "altruist investigator" will never survive in this community. The rules instituted for "craftsmen" and "merchants" will never fit to him.

	Type of academic economist from the point of view of her/his **professional values**		
Type of academic economist from the point of view of applied in her/his work **methodology**	*Altruist investigator*	*Craftsman*	*Merchant*
Philosopher/Mathematician		X	X
Econometrician/Statistician		X	X
Anthropologist/Historian	X	X	

Table 1. *Compatibility of shared professional values and used methodology*

At present the American institution of economics is based not only on the false idea of science inherited from modernity, but also on the neoliberal vision of any human activity. The contemporary American institution of economics is constructed on the ideas that the neoclassical economics proclaims itself transforming the community of economists into a market place. Each member of the community is "selling" her/his products (papers intended for publication in professional economic journals) to its powerful members present in different committees and boards and receiving in exchange evaluations necessary for obtaining different resources like permanent and temporary university positions and grants. The institutionalisation of this type of economics was very successful, it means that it does not require frequent sanctions of deviators for its stable functioning. It became possible because several generations of economists were "processed" by the institutionalisation to maintain economics in the prescribed framework and after that, the probability of dissidence is very low.

6. Institution of economics and institutional economics

> The history of economics suggests that survival has often depended upon the ability of doctrine to fit in with the habits of thought of the times. If the next decade demands formal value theory that avoids a discussion of what the economic order is like, institutional economics will fail. If it demands an understanding of our relationship to the world in which we live, it will survive.
>
> (Walton H. Hamilton, 1919, p. 318)

British-type institution of economics took its legitimatisation in the ideas produced by modernity[26]. Now it became clear that the origins of modernity were theological (Gillespie, 2008). One of the main ideas of modernity was universally applied dualism. It is ironical that they often connect the beginning of modernity with Copernican revolution; however Copernicus destroyed the false duality Earth-Havens, which was very important for the Church, but modernity, taking its inspiration in theology, created many others dichotomies such as subject-object, fact-value, theory-practice, appearance-essence. Modernity is in many respects reflected by Cartesianism[27].

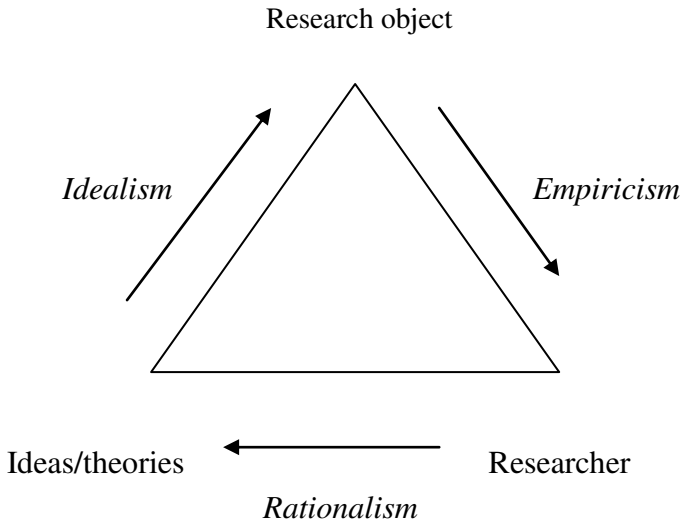

Fig. 1. *The old (modernist) model of scientific research*

[26] "Modernity is a term that refers to the modern era <…> in different contexts, refers to cultural and intellectual movements of the period 1630-1940." http://en.wikipedia.org/wiki/Modernity

[27] Descartes was a real personification of modernity: "Cartesianism destroyed the balance which underlies true science: the balance between thinking and observing, deduction and induction, imagination and common sense, reflection and action, reason and passion, abstract thinking and realism, the world within and the world without the mind. Under the impact of Cartesianism the second element of the equation was sacrificed to the first <...> Descartes' epistemological reflections opened an era of axiomatic, unhistorical, deductive thinking broadly called the Enlightenment." (Mini, 1994, p. 39) The Cartesian dualism with its separation of knowing from doing, object from subject, fact from value, theory from practice serves an epistemological foundation for neoclassical economics (Bush, 1993, p. 65).

According to the dualistic tradition, mutually excluding doctrines were developed such as empiricism-rationalism and materialism-idealism. For a century and a half, economics claims to be a science having natural sciences as a model. It used in this claim the modernist model of scientific research. Modernist model is based on the sharp separation of (research) object and subject (researcher), as well as on an individualistic vision of the process of scientific research. Elements of this model are: Research object, Researcher and Ideas/Theories. Empiricism considers the links between these elements in following way: Research object → Researcher → Ideas/Theories. Rationalism linked them differently: Researcher → Ideas/Theories → Research object. Comte's positivism hesitated between these two visions of scientific research which allowed John S. Mill to announce political economy an abstract and only abstract science. Idealism saw the sequence of elements in a third way: Ideas/Theories → Research object → Researcher. Materialism turned the direction of arrows in the opposite way. In spite of all these differences, all these "isms" appealed to the same modernist model of scientific research. The use of such metaphors as "law", "mechanism" and "organism" is also very characteristic of Modernity. The former has religious and political origins: Ancient Testament and Absolutism. It is this metaphor which oriented Positivism in a wrong direction. Classical physics appealed to mechanism. Finally, the last characteristic, but apparently the most important feature of modernist model of scientific research, was its individualism: the researcher was alone in his search of truth as a copy of reality. It is the modernist model of scientific research which is at the basis of the so called "scientific method", which is often presented as hypothetic-deductive method.

Charles S. Peirce was the first to make a flaw in the modernist model of scientific research. He saw the research as a collective action of investigators who, by observing and by analysing something separately, gradually converge on the results of investigation. The pragmatist philosophy of Peirce, according to his own words, corresponds to the experimentalist's mind (Peirce, 1905, pp. 331, 332)[28]. His understanding of science was very close to that of Latour and Knorr Cetina: "What is Science? We cannot define the word with precision and concision with which we define *Circle* or *Equation,* any more than we can so define *Money, Government, Stone, Life.* The idea, like these, and more than some of them, is too vastly complex and diversified; It embodies the epitome of man's intellectual development … a particular branch of science, such as Physical Chemistry or Mediterranean Archeology, is no mere word, manufactured by the authority of some academic pedant, but a real object, being the very concrete life of a social group constituted by real facts of inter-relation". (Quoted by Mirowski (1987, p. 1009). The next step in the destruction of modernist model of scientific research was made by Thomas Kuhn with his understanding of scientific revolutions (1970). Paul Feyerabend also contributed to this destruction by his attack against the identification of scientific character with a particular 'scientific method' (1978). At last, the social constructivism (Berger and Luckmann, 1991) provided a building material with the help of which it became possible to create a new model of scientific research, much more realist than modernist,.

Bruno Latour and other specialists of *Science Studies* have shown that the modernist type of discourse never corresponded to the realities of scientific research: "We have never been modern" (Latour, 1997). They switched from the discourse around a modernist model of scientific research to a new model of scientific research, the elaboration of which has been

[28] An experimentalist "has had his mind molded in the laboratory <…> With intellects of widely different training from his own, whose education has largely been a thing learned out of books, he will never become inwardly intimate, be he on ever so familiar terms with them; for he and they are as oil and water, and though they be shaken up together, it is remarkable how quickly they will go their several mental ways, without having gained more than a faint flavor from the association." (Peirce, 1905, p.331)

based on historical and field studies of scientific practices in natural sciences (Latour and Woolgar, 1979).

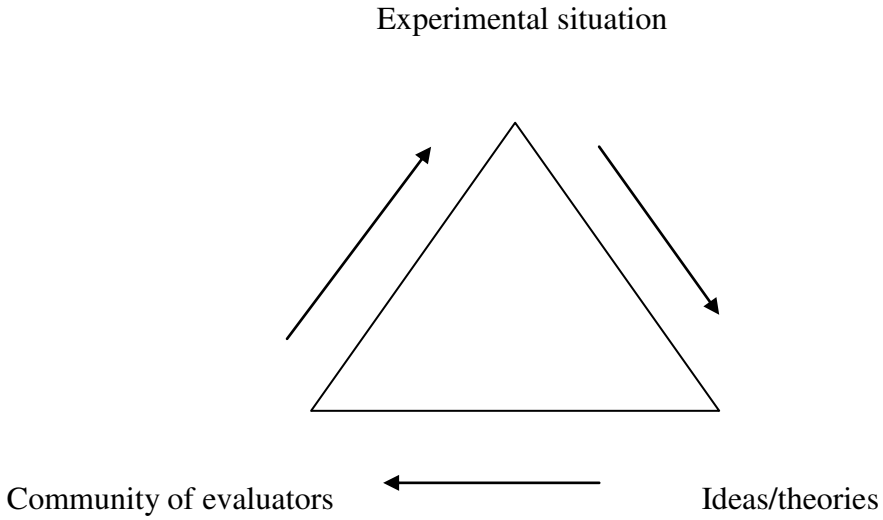

Fig. 2. *The new (Bruno Latour's) model of scientific research*

Bruno Latour rejects the modernist model with all its dichotomies and "isms". In his model of scientific research the Research object is not separated from the Researcher, but make together with researcher and her/his "instruments" an *experimental situation*, and ideas and theories coming from observations/experiments are evaluated not by the individual researcher but by a *community of evaluators* consisting not only of members of the corresponding scientific community but of a larger community which includes administrators, politicians and concerned segments of the public. Evaluators can be divided in two categories: *witnesses* and *judges*. Difference between them consists in their respective roles in the procedure of evaluation: the former only express their opinion about evaluated ideas/theories, but the latter, taking into consideration these opinions, make decisions concerning destiny of ideas/theories and future of experimental situation itself. Most of the members of scientific communities, except powerful members, belong to the category of witnesses. This model does not challenge the objectivity of scientific research and does not lead to relativism. It allows to change totally the discourse about science and to turn it from absolutely sterile "what ought to be" to the direction of "what is".

I give here my own interpretation of the model of scientific research proposed by Latour[29]. As I already mentioned earlier, according to Latour, the specificity of scientific research does not consist in a special "scientific method", but in the design of experimental situation in which the object has the possibility to resist, "to object" to the ideas of the researcher concerning it, "when things strike back" (Latour, 2000). Application of this model to economics leads to the interpretive/pragmatic paradigm (interpretive/pragmatic institutional economics) resulting in the switch from primarily theoretical (a priori) type of research to experimental type of research, and from primarily quantitative techniques to qualitative methods. Analysis of historical data (basically texts: laws, political discourses, etc.), interviews, focus groups and action research should become the core of economics research. This mode of research can bring valuable results only if the research is organized in such a

[29] To my mind, the Figure 2 and my comments to it, correctly reflect Latour's ideas; however I am the only one responsible for this interpretation.

way that the research object can resist to the ideas issued by the researcher about it. It is this kind of resistance which has permitted to economists of the New German Historical School and Wisconsin Institutionalism to come to the understanding of socio-economic-political processes in their countries, and elaborate on this bases propositions and legislative acts for the solution of the so-called "social question" at the turn of the 19th and 20th centuries.

In section 3 of this paper I gave a description of the German institution of economics, which was created under the leadership of Schmoller collaborating very closely with the government of Bismarck. Institution of economics in the form of Wisconsin Institutionalism was in many respects similar to the German one. The role of Schmoller was played by John R. Commons working very closely with the administration of the governor/senator of the state of Wisconsin, Robert La Follette. In Wisconsin, the German model has even been improved: in addition to historical and monographic research, researchers began to use a technique which was later called Action Research. It was made under the form of the Wisconsin Industrial Commission, which served as a laboratory for the Commons's group where they made their investigations. Story of Wisconsin Institutionalism, which I will tell shortly here, shows the practical possibility and very high efficiency of the Commons's type institutional research.

The favourable conditions for the start of the Wisconsin Institutionalism began to be created with the move in 1892 of Richard Ely from John Hopkins to Wisconsin. In spite of his many concessions, "Ely's position at John Hopkins had never been fully secure" (Rader, 1966, p. 107). Ely has received an appointment in the University of Wisconsin where he worked as professor and director of the School of Economics, Political Science and History until 1925. When Ely came to Wisconsin, La Follette greeted him with the remark: "You have been my teacher!" Ely wrote in his memoirs: "He has never been my student, but what he meant was that he had got a great deal out of my writings. I was never his close adviser, but I saw him frequently; often he was a guest at my house as I was at his <…> When I was attacked bitterly, in an attempt to oust me from Wisconsin, he said he would take the stump in my behalf before he would allow my enemies to prevail" (Ely, 1938, p. 216). Ely succeeded to introduce in his school the spirit of German Humboldtian universities where teaching was realised by the involvement of students in the research process. In the case of economic domain, it meant the involvement in the understanding of the real economic practice: "My experience in Germany had first brought to my attention the importance of linking book knowledge and practical experience" (Ibid., p. 187).

The start of the Wisconsin Institutionalism can be marked by the arrival in 1904 in Wisconsin of the former Ely's student, John R. Commons. The latter decided to become Ely's student in John Hopkins after reading publications in the press attacking him (Commons, 1964, p. 40), and later (around 1888) he was fascinated by the "new" (originated in Germany) political economy taught by Professor Ely (Ibid., p. 44). He joined the University of Wisconsin's economic department at a particularly favourable time, when the state government and the administration of the university were controlled by the advocates of social reforms: "In Wisconsin Commons found himself an insider whose talents were useful to those in power, where elsewhere he had been considered a dangerous radical. He could continue to engage in reform activities in Wisconsin, but he did so for the authorities instead of in spite of them" (Harter, 1962, p. 45). It was Commons and his students, rather than Ely, who really made Wisconsin institutionalism[30]: "Commons was a leader in the Wisconsin Idea, that is, the movement that brought experts from the university faculty into partnership with state government policymakers" (Lampman, 1993, p. 27). The economic research oriented to reform culminates in proposal for institutional change. Finally these proposals take the form of legislative acts. Unlike German economists who advised civil servants in drafting

[30] The above described analysis of Wisconsin Institutionalism has to a great extent been borrowed from (Rutherford, 2006).

legislative acts, Wisconsin institutionalists (Commons and his students) were directly involved in drafting laws. Commons himself participated in drafting the Civil Service Law of 1906, and the Public Utility Act of 1907. Later his students, such as Edwin E. Witte and Paul A. Raushenbush, took the relay. Witte's PhD dissertation was concerned with the role of the government in labour disputes (Witte, 1932). He became the real father of the Social Security in the United States (Schlabach, 1969). Raushenbush had played a crucial role in the development of the US unemployment compensation legislation (Raushenbush & Raushenbush, 1979). As Philip Mirowski wrote: "Many of the economic functions of the US government that we take for granted today were the handiwork of Commons and his students in the first half of the twentieth century" (Mirowski, 1987, p. 1027)[31].

Ely transmitted to Commons the Humboldtian method of university teaching; by the involvement of students in the research process. Ely has witnessed that no man has been more successful than professor Commons in carrying out this idea: "He kept in touch, on one hand, with labour, and on the other, with the management of industry. He mingled with all classes of people. He introduced to his classes people <…>, who were regarded as very dangerous radicals. To him, these people were simply human representatives, whom his students should known face to face. On the other hand, he was just as eager to have his classes know capitalists and leaders of industry. He could admire a labour leader; he could understand the slugger; and he had a great admiration for the big industrial leaders. In order to understand their point of view, he became a member of the Wisconsin Industrial Commission, while on a leave of absence from his university duties" (Ely, 1938, pp. 187 – 188). It is worthwhile to mention here that, as we saw below in section 4 of this paper, Alfred Marshall, at the beginning of his career, had intentions to contact closely similar persons in England but he did not implement it following advise of "older and wiser" people from his social environment and realising that such type of research would take too much time and efforts. In this way Marshall had not become "Anthropologist/Historian" but had stayed as his predecessors, English political economists, "Philosopher", adding to it a mathematical style: a "Philosopher/Mathematician". It became inevitable because his value system was much closer to a "Craftsman" than to an "Altruist investigator".

Commons was certainly an "Altruist investigator". He preferred individual and small group research sessions to lectures: "He did lectures and he often gave talks to large groups, but his success came from his marshalling of facts and from his enthusiasm for what he was advocating" (Harter, 1962, p. 77). This is the testimony of his student Edwin Witte: "He [Professor Commons] inspired his students to devote their lives to the improvement of our democratic way of life and our economy of free enterprise, for which he developed in them

[31] The President Theodore Roosevelt characterised the achievements in Wisconsin in the following way: "The Thanks to the movement for genuinely democratic popular government which Senator La Follette led to overwhelming victory in Wisconsin, that state has become literally a laboratory for wise experimental legislation aiming to secure the social and political betterment of the people as a whole <…> All through the Union we need to learn the Wisconsin lesson of scientific popular self-help, and of patient care in radical legislation. The American people have made up their minds that there is to be a change for the better in their political, their social, and their economic conditions; and the prime need of the present day is practically to develop the new machinery necessary for this new task. It is no easy matter actually to insure, instead of merely talking about, a measurable equality of opportunity for all men. It is no easy matter to make this Republic genuinely an industrial as well as a political democracy. It is no easy matter to secure justice for those who in the past have not received it, and at the same time to see that no injustice is meted out to others in the process. It is no easy matter to keep the balance level and make it evident that we have set our faces like flint against seeing this government turned into either government by a plutocracy, or government by a mob. It is no easy matter to give the public their proper control over corporations and big business, and yet to prevent abuse of that control. Wisconsin has achieved a really remarkable success along each and every one of those lines of difficult endeavor." (McCarthy, 1912, pp. 2-3)

not only profound admiration, but also an appreciation that American idea is one of continuous progress. As is common with young people, many of Commons' students were dissatisfied with things as they are. But they emerged from his classes, indeed, as men who wanted to improve what they thought was wrong, but without destroying our political, economic, and social structure. Commons taught them to see that they must thoroughly know the facts and offer workable proposals for improvements. He told them not only to study all that was written about a given subject, and to reason logically about it, but to make their own observations, and to think in terms of remedies, rather than criticisms, and to learn from the people directly interested" (Harter, 1962). This testimony shows that Commons totally corresponded to the vocation of the scholar as it had been determined by Fichte. It also shows how an alternative to the present mode of teaching could be organised in university departments of economics. The involvement of graduate students by Commons in projects of reform was a very efficient teaching method: "Instead of working on the usual projects, which are often destined to dusty shelves, they worked on reforms that in many cases became laws. It was easy for them to feel the importance of what they were doing. Consequently, their work took on an urgency that drew from them considerably more effort than they otherwise might have given" (Harter, 1962, p. 78).

John Commons called interviewing "the prime method of investigation" (1934a, p.106). He practised extensively case studies of the past, for example the Slaughter House Cases (1924, pp. 47 - 54), and of the present. The latter was investigated by him as a member of the Wisconsin State Industrial Commission (1934b, pp. 142, 143). He wrote: "Academic teaching is merely brains without experience. The 'practical' extreme is experience without brains. One is half-baked philosophy – the other is rule-of-thumb" (1934c, p.160). German economists understood it very well, but Commons has enriched the set of techniques of the economist "Anthropologist/Historian" by adding to interviewing and archive work, participant observation and action research. His student, Elizabeth Brandeis Raushenbush, remembered: "Commons was certainly an all-purpose labour economist, because he did the most important fundamental research, the documentary history of labour <…> although he deserved his academic reputation, the reason his students distinguished themselves and were so prominent in various areas was because he soon involved them in current labour economic problems. He wanted them to have research tools and to know what they were doing. It was not research for the sake of research. Perhaps there were some people who continued to work in the history of labour and felt there plenty more to do there; but most of his students did research for the purpose of using it in the various battles to improve things, in what we now call 'action research'" (Raushenbush & Raushenbush, 1979, p. 9). Commons established himself as one of the leading American labour economists, recognition coming with his election as President of the American Economics Association in 1917.

Commons supervised or co-supervised to completion forty-six PhD students during his Wisconsin career. It is Commons and his students that did most of the teaching in the economics department of the University of Wisconsin. Commons offered a course based mainly on the study of reported legal cases, involving a correlation of law, economics and psychology. In the 1928/29 year, Paul Raushenbush taught an undergraduate course Economic Institutions. Raushebush himself described this course as dealing with the social control of business (Ibid., p. 4). The course Labour Legislation (Witte and Brandeis) focused on government action in relation to labour, protective legislation and its constitutionality, collective bargaining and its legal status, and Wisconsin legislation and the work of the Wisconsin Industrial Commission. Brandeis used to get her students to attend hearings and legislative sessions, and to follow the progress of bills dealing with labour issues (Ibid., 15). A special course on Technique of Field Investigation was also available to students. "There cannot have been very many, if any, other professors of economics in the US over the period

Commons was at Wisconsin who produced as many graduate students, or whose students had such a marked impact on government legislation and policy. In this sense *Commons and his students were extremely successful*[32]. What, then, happened to Wisconsin institutionalism, and why did its particular type of economics and economic education suffer a decline in the years that followed?" (Rutherford, 2006). I suppose that the answer to this question is already given in the previous section of this paper:

- a generally negative attitude of businessmen when researchers try by *field studies* to understand what is going on within their enterprises;
- the encouragement and support by businessmen and politicians depending on them to those economists who *advocate laissez-faire* in their theoretical constructions and teaching[33];
- much *harder work* of "Anthropologists/Historians" in comparison with "Philosophers/ Mathematicians";
- *communication difficulties* of the functioning of communities of academic economists "Anthropologists/Historians" (their theoretical constructions are not universal, but contextual);
- and last but not the least, *deeply enrooted views of Modernity* in the Western culture.

Wisconsin Institutionalism gives us a concept on what kind of change is necessary for the institution of economics in order to transform economics in a real science which could bring as tangible effects as natural science does.

The research and reforming activity of Commons was financially supported by the big business. In 1906, he participated in the study of some thirty-five municipally and privately owned gas, electric light and power, and street railroad companies in the United States and England. As part of the study, he made a five-month trip to the British Isles. (Harter, 1962, p. 72) This study was made for the National Civic Federation financed by the big business. This Federation was involved in elaborating and lobbing, at state and at federal levels, laws concerning public insurance of unemployment, federal regulation of trade and enterprises of public utilities[34]. Later Commons, with several of his students, undertook the supervision of the labour portion of one of the surveys financed by the Russell Sage Foundation (Ibid.). In 1907, Commons has been elected as the secretary of the American Association for Labour Legislation, which was financed by Rockefeller and Morgan. This organisation was involved in elaborating legislation concerning enterprises of public utilities, minimum wages, maximum working hours, and laws in favour of trade unions. In my opinion, the big business supported Commons in these activities because it was interested to resolve the social question and to prevent worsening of class conflicts. When the task of social reform had been fulfilled, the big business has lost interest to institutional research. And that is the reason why the extension of this type of research to other areas such as banking and financial regulation did not happen. Institutional research in the United States by "Anthropologists/Historians" became a local and temporary episode, the existence of which was due to extraordinary circumstances. The forecast of Walton H. Hamilton, author of the term "institutional economics", which I put as epigraph to this section of this paper, has been totally realised.

The received view that institutional economics of John R. Commons is something absolutely inacceptable for contemporary academic economists is dominant. This view unites very different representatives of the community of economists. The school of thought launched by Schmoller and Commons is often accused to create no theories as neo-classical economists do[35]. Mainstream economists try in vain to find in texts of Commons deductive

[32] The marking is mine (VY).
[33] See (Furner, 1975).
[34] See the article of Murray N. Rothbard (2006) at http://mises.org/story/2225 .
[35] See for example section on American institutionalism of Mark Blaug's book (1985).

theories based on a priori axioms: "The institutionalists seem to have suffered from a methodological confusion regarding the nature of theory. They thought a description was a theory." (Ward, 1966, p. 187); "Theory was never Commons's metier. When he calls his 'theories' are exclusively poorly wrought and somewhat lackadaisical classifications and sub-classifications of phenomena as they appear to him from the dimly held and mainly intuitive conception impossible to define." (Seckler, 1975, p 124). Geoffrey Hodgson who has the modernist vision of institutional economics has been trapped in the same way: "Commons did not have the statute of a major theorist such as Alfred Marshall or Karl Marx. Furthermore, he did not have the aptitude for careful definitions or logical chains of reasoning" (Hodgson, 2003, p. 548). The following evaluation of Douglass North could be due to a misunderstanding or ignorance: "The problem with institutional economics, and the reason it faded from sight, was that it did not explicitly address the issues we had to solve. What we have to do is understand what makes economies work the way they do – that is a necessary precondition to being able to say something about how we can make them work better." (North, 2003, p.1)The following statement looks invalid in the light of the exposed above Latour's model of scientific research and enumerated earlier five factors influencing the decline of the Commonsian institutionalism : "In the interwar period institutionalism was actually the dominant school of economic thought in the US. It lost ground to neoclassical formalism partly because it neglected its own task of underlying theoretical development. It is not difficult to see how institutionalism became bogged down. After establishing the importance of institutions, routines and habits, it underlined the value of largely descriptive work on the nature and function of politico-economic institutions. Whilst this was of value it became the predominant and almost exclusive practice of institutionalist writers. The institutionalists became data-gatherers *par excellence*. The error here was largely methodological and epistemological, and committed by many institutionalists with the exception of Veblen himself and few others. It was a crucial mistake simply to clamour for descriptive 'realism', by gathering more and more data, or by painting a more and more detailed picture of particular economic institutions" (Hodgson, 1988, pp. 21 – 22). I will come to some of Hodgson's statements in the next section of the paper. Now, it is sufficient to say that he unfortunately is caught in a net of modernist model of scientific research.

The conclusion, that we can make on the basis of what has been said above in this section of the paper, is following: the American institution of economics of British origin had killed the American institutional economics coming from the German tradition[36]. It happened because the British-type institution of economics, for the reasons exposed above, became dominant. Nevertheless after this death, which happened before Wold War II, the label "institutional economics" is actively used in the community of academic economists throughout the world.. Among them it is easy to distinguish advocates of Thorstein Veblen and advocates of Ronald H. Coase. Both types of "institutionalists" work in the modernist framework and that is why they can professionally survive under the existing institution of economics. The advocates of Veblen are to a great extent marginalised, and can exist primarily in such professional niches as methodology of economics and/or history of economic thought. On the contrary advocates of Coase prosper, becoming an influential part of the mainstream.

I indicated earlier that Commons, as a student of Ely, was fascinated by the new German economics and has learned a lot of his professor about how to make research and teaching. Veblen attended Ely's course only for one term and left John Hopkins for Yale. There is a testimony that "Veblen had 'undoubtedly' chosen a better place to study economics" (Rader, 1966, p. 20). He obtained his B.A. in economics at Carleton College

[36] Very rare examples of publications reflecting research in the continuation of this tradition are (Degnbol-Martinussen, 2001) and (Yefimov, 2003).

under John Bates Clark in 1880, when Clark has not yet abandoned his German-born ideas. I suppose that Veblen's institutional background, received from earlier Clark, got a philosophical support when in 1881 he undertook a graduate work at Johns Hopkins University under Charles Sanders Peirce ; Veblen's notion "habit of thought" was certainly adopted from Peirce. Veblen's PhD adviser at Yale University was William Graham Sumner, from whom apparently comes his attachments to Darwinist ideas, but certainly not his political views. Of course, Veblen was a keen observer of America of the end of the 19th, beginning of the 20th centuries, but he never made any systematic field studies. It was probably Veblen who started the widely accepted discourse concerning the absence of theory in the German Historical School[37]: "The whole broad range of erudition and research that engaged the energies of that school commonly falls short of being science, in that, when consistent, they have contented themselves with an enumeration of data and a narrative account of industrial development, and have not presumed to offer a theory of anything or to elaborate their results into a consistent body of knowledge" (Veblen, 1990, p. 58). This evaluation comes from his "naïve conception of science" (Mirowski, 1987, p. 1023), which is purely modernist: "Science creates nothing but theories. It knows nothing of policy or utility, or better or worse. None of all that is comprised in what is to-day accounted scientific knowledge. Wisdom and proficiency of the pragmatic sort does not contribute to the advance of a knowledge of fact. It has only an incidental bearing on scientific research, and its bearing is chiefly that of inhibition and misdirection" (Veblen, 1990, p. 19).

Ronald Coase wrote in his autobiography that the decisive in his career of economist was his attendance of the course of Arnold Plant at the London School of Economics in 1930: "<…> that was to change my view of the working of the economic system, or perhaps more accurately was to give me one. What Plant did was to introduce me to Adam Smith's "invisible hand". He made me aware of how a competitive economic system could be coordinated by the pricing system". He planned to continue his studies in the university in order to become a lawyer, but, as a result of Plant's influence, the University of London awarded him a travelling scholarship and he spent the academic year 1931-32 in the United States "studying the structure of American industries, with the aim of discovering why industries were organized in different ways". He "carried out this project mainly by visiting factories and businesses". As we can judge he saw these factories and businesses through the spectacles of the "invisible hand". At the end of his visit to the United States he formulated ideas that became the basis for his article "The Nature of the Firm", published in 1937, cited by the Royal Swedish Academy of Sciences in awarding him the 1991 Alfred Nobel Memorial Prize in Economic Sciences. Coase was awarded this prize for his discovery of "the significance of transaction costs and property rights for the institutional structure and functioning of the economy". As I know, during more than thirty years nobody paid attention to this article until it has been "discovered" by Oliver Williamson. The notion of transaction costs suit quite well with the phenomena of vertical integration, but according to the modernist tradition and institution of economics adapted to it, it became a universal concept applied to everything. In this way an extraordinary rise of the Rational Choice Institutionalism has been lunched, and the domain has received the name of "New Institutional Economics". The manual (Furubotn and Richter, 1998) and the handbook (Ménard and Shirley, 2005) of New Institutional Economics underline its close link and mutual complementarities with neoclassical economics. Following the methodology of J.S. Mill, New Institutional Economics is an "abstract science". Douglas North noticed: "There is a lack of empirical work on the subject (…) When Lee Alston, Thrain Eggertsson and I were working on a book of reading on empirical studies in new institutional economics, we had difficulty finding a

[37] (Chavance, 2008)

sufficient number of case studies to use. The reason is that few have been done." (North, 2000, pp. 8, 9) Erik Furubotn and Rudolph Richter consider New Institutional Economics as an amalgamation of the theory of transaction costs, economic theory of property rights, and theory of contracts. These authors quoted the opinion of Ronald Coase concerning the distinguishing feature of economists working in this domain. According to him, it is the application of standard economics for analysing institutions and discovering what role they play in economic performance (Furubotn and Richter, 1998, p. 436). However Furubotn and Richter notice that not all members of the new institutional economics community share this opinion, and debates on whether it is necessary to go to a different paradigm, or not, are not finished (Ibid., 1998, pp. 437 - 438). Among those who disagree with Coase' definition is Douglass C. North.

Professional story of North can be divided in two parts: before 1993 and after. After awarding him the 1993 Alfred Nobel Memorial Prize in Economic Sciences, he radically changed the discourse in his writings. He has made a new turn in New Institutional Economics which separated him from the rest of the community. His new writings are in sharp contradiction with his previous ones. Before 1993, as economic historian, he tried to explain economic history using neoclassical economics supplemented by three theories mentioned above making together the New Institutional Economics[38]. But in 1994 he published together with Arthur Debzau the article "Shared Mental Models: Ideologies and Institutions" where the vision of social reality has nothing in common with neoclassical economics. This vision was further developed nine years later in his book *Understanding the Process of Economic Change* (North, 2005). The title of the book is very revealing; his *understanding* is very close to Weberian *Verstehen*. In this book, North, without making any references to Schmoller and Commons, has returned to their theoretical views. At the very beginning of the book he declares: "The economic paradigm – neo-classical theory – was not created to explain the process of economic change. We live in an uncertain and ever changing world that is continually evolving in new and novel ways. Standard theories are of little help in this context. Attempting to understand economic, political, and social change (and one cannot grasp change in only one without the others) requires a fundamental recasting of the way we think. Can we develop a dynamic theory of change comparable in elegance to general equilibrium theory? The answer is probably not. But we can achieve an understanding of the underlying process of change then we can develop somewhat more limited hypotheses about change that can enormously improve the usefulness of social science theory in confronting human problems" (Ibid., p. vii). Thus, instead of elegant theories he proposes to achieve an understanding, which helps to develop hypotheses useful for solution of human problems. In this way the elegance of theory is sacrificed in favour of the usefulness. What was a stumbling-block in the dispute between Menger and Schmoller, namely laws[39] and determinism, even in its probabilistic/stochastic form (ergodic world), are rejected by North with the same resoluteness as by Schmoller. He criticises Samuelson for whom "the ergodic hypotheses was essential for a scientific economics". He disagrees with Solow, who wrote that "the best and the brightest in the profession proceed as if economics is the physics of society" and that "there is a single universally valid model of the world", "it only needs to be applied". North concludes that "to an economic historian surveying the ten millennia of human history from onset of the Neolithic revolution, however, the ergotic hypothesis is a-historical". (Ibid., p. 19). As German historical economists more than hundred years earlier, North comes to the conclusion that our theories have to be inevitably contextual in time and space (Ibid., p. 20 – 21).

[38] A comparison of Schmoller's and North's methodological and theoretical positions is given in (Peukert, 2001).
[39] See (Menger, 1883), especially the Appendix V.

What happened? How to explain such a radical change in North's methodological and theoretical views? Let me tell you a story which perhaps could help to answer this question. Several years ago I participated in a seminar on New Institutional Economics. Most of the participants were young economists from different European countries. One day I assisted a session where a former very close North's collaborator was present. He shared with young colleagues the advice he received from North on how to succeed his professional career. According to him, North advised to start the career of academic economist with articles treating small topics in the framework of the accepted approach. "Gradually, he continued, during your career you can increase the scale of your topics and slightly deviate from the accepted approach. After achieving some authority you can write not only articles but also books". The former North's assistant concluded that according to North, you can begin to write what ever you want only after being awarded an Alfred Nobel Memorial Prize in Economic Sciences. The former North's collaborator very often used in his speech the word "sell". After he finished his advice, I addressed to young economists and said that in my opinion we, economists, are not sellers, and that our profession is closer to that of a priest than that of a merchant. After the end of the session, he told me: "Vladimir, you are Russian, you probably follow Dostoevsky and Tolstoy, but I have to feed my children". I said nothing but thought: "In this case it would be better for you to choose another profession".

That makes now many decades that economics of Schmoller and Commons are not practiced at any significant way. It is ironic that these currents of economics are often unfairly accused now as partisan/normative when practically all contemporary currents of economic thought, either orthodox or heterodox, are becoming more and more normative. It is their dominant a priori method, that orients them in this direction. There exists among economists a great confusion, whether they study what is, or they construct what ought to be. In most cases the economists are dealing with "what ought to be" without very much care to know in detail and understand "what is". It is really a very important characteristic of the contemporary institution of economics. The absolute majority of economists and many social scientists, thinking that they are practitioners of science, which was started on an institutional basis by the Royal Society of London, are at the most prescience political/social/moral philosophers. They do not investigate in detail the reality but elaborate some social projects. To confuse these two activities and call philosophy science is very dangerous, because it makes impossible for economics to achieve similar practical results significant for the humanity as natural sciences did. An illustration of this confusion can be the manifesto: "Towards an institutionalist political economy" recently issued by three French scholars[40]. A French version of this manifesto was published in the issue number 30 (2007) of the half-yearly edition of *La Revue du MAUSS*, with the title "Towards another economic science (and thus another world)?". The normative disease of economists is just felt in the title of this issue of *La Revue.* Let's make an experiment; let's change the word "economic" in this title by the words "physical", "chemical", or "biological". As a result of this experiment we get a very strange statement: another type of physics (chemistry, biology) will give us another type of world in which we live, that is physical properties of materials will become different,

[40] At the beginning of the manifest the relation economics vs. political economy is characterised in the following way: "Historically, economic analysis has been practiced in two definitive traditions: within its political context, as *political economy* and without, as *economics* or *economic science*. We believe that the first must be recognized as having principled superiority over the second. That is, we consider economic science, not as a mechanical or mathematical science (even though mathematics certainly has a critical role to play), but as linked tightly to general social science and to political and moral philosophy. In this sense, political economy forms a branch of political philosophy, and political philosophy, the context for each of the social sciences. Economics (or economic science) is thus correctly interpreted only as the analytical moment of political economy."
Revue du MAUSS permanente, 9 September 2008, http://www.journaldumauss.net/spip.php?article232

chemical reactions will proceed differently, and biological qualities of organisms will be transformed. You agree that is nonsense. However, the author of the manifesto, and articles published in this issue of *La Revue,* meant something different. In his introductory article for this issue, the author of the manifesto, Alain Caillé, indicates that economics "became the true revealed religion, the authentic substitute and heir of Christianism (with which however it undertakes narrow historical, cultural, metaphysical and psychological links)" (Caillé 2007, p. 6). It is absolutely true that "the modern world is to a great extent a realisation of the dream ("the dream comes true"), prophecy and preaching of the economic science" (Ibid., p. 7)[41]. The above pages of my paper are in total accordance with this judgement, but I insist to say that *this* economics is not a science but the theology of a secular religion which indeed has conquered the world. The world looks more and more like the realisation of this prophecy because a larger and larger number of people became believers of this religion, and thus perceive their human environment and act according to these beliefs. It is of course necessary to struggle against this religion and one of the ways to do it is to elaborate and propagate alternative beliefs. However, it is not the task of economic scientists/ researchers/investigators of socio-economic-political reality, but the task of political/social/moral philosophers. New generation of scholars from the French regulation school understand it very well: "political economy is not a moral science"[42] (Amable et Palombarini, 2005). The task of researchers is to investigate and try to understand "what is". The task of politicians, taking into consideration what they learned from scientists on "what is", and what they learned from philosophers on "what ought to be", is to make their choice. It is their responsibility, and it is mutually harmful if politicians delegate this responsibility to scholars-researchers. It devaluates politicians and move efforts of researchers away from what they really could and should do. The researchers can assist politicians in generating of alternatives, "what ought to be", but should not substitute them in the operation of choice (Ibid., pp. 270 – 271).

[41] This is the continuation of the sentence with a passionate accusation of this religion: « Jusqu'au cauchemar parfois. Et cela devient chaque jour plus vrai, à l'échelle de la planète, où plus rien d'autre ne semble doté de la réalité que l'enrichissement personnel et matériel. Face à elles, tout – toute valeur, toute croyance, toute action menée pour elle-même, pour le plaisir, toute existence qui n'est pas voué à la recherche de l'utilité – tout semble illusoire, inopérant, n'en valant pas la peine, superflu, irréel » (Ibid.). It could not be said better and I do not dare to translate it into English.

[42] It would be better to say: political economy should not be a political/social/moral philosophy.

7. *Methodenstreit* reconsidered

> If social scientists wanted to become objective, they would have to find the very rare, costly, local, miraculous, situation where they can render their subject of study as much as possible able to object to what is said about them, to be as disobedient as possible to the protocol, and to be as capable to raise their own questions in their own terms and not in those of the scientists whose interests they do not have to share! Then, humans would start to behave in the hands of social scientists as interestingly as natural objects in the hands of natural scientists.
>
> (Bruno Latour, 2000, p. 115)

Dispute of methods, or *Methodenstreit*[43], between Gustave Schmoller and Carl Menger can be considered as a repetition of a similar dispute taking place more than two hundred years earlier between Robert Boyle and Thomas Hobbes. Schmoller-Menger dispute started soon after the beginning of the institutionalisation of experimentally oriented economics which happened with the creation in 1873 of the *Verein für Sozialpolitik*. Boyle-Hobbes dispute[44] started in 1660, when the *Royal Society of London* had been founded, the cradle of the institution of science. The activities of both societies were similar in several respects: they represented efforts to collect data in the framework of experimental situations, working out of detailed reports and collective evaluation of obtained results. The reports of the Royal Society served to enlarge the number of witnesses of experiments and in this way "to make virtual witnessing a practical option for the validation of experimental performances" "(Shapin and Schaffer, 1985, p. 69). Boyle insisted on his "lack of preconceived expectations, and, especially, of theoretical investments in the outcome of experiments (Ibid., p. 68). Now, it is almost forgotten that Hobbes was not only a political philosopher but also a physicist (natural philosopher). He criticised the experimental way of producing physical knowledge and he insisted on rationalist methods, as Menger also did, of obtaining knowledge. Both of them proceeded from definitions through deduction to consequences. For Hobbes, as for Menger, the model of "science" was geometry, which "yielded irrefutable and incontestable knowledge" (Ibid., p. 100). On the contrary, the "the Royal Society advertises itself as a 'union of eyes and hands'" (Ibid., p. 78). Hobbes thought that "the factual knowledge, it was true, had a valuable role to play in constituting our overall knowledge, but it was not of the sort to secure certainty and universal assent" (Ibid., p. 102). Boyle did win the dispute, Schmoller did loose. In my opinion it happened for the following main reasons. The motto *Nullius in Verba* (demonstration by facts and not by words) has become the rule at the basis of the institution of natural sciences, the most important feature of the scientific culture. From the very beginning activity of researchers according to this rule did not contradict interests of powerful groups of the society, and later such groups were even very much interested in the existence of this rule because of profitable practical results. It did not occur in economics just because of the strong resistance of such groups to experimental social research. They saw much more danger than benefit for them in this type of research. On

[43] The dispute began with the publication by Schmoller of a negative evaluation in his article « Zur Methodologie der Staats- und Sozial-Wissenschaften » of the Menger's book (1883).
[44] The book (Shapin and Schaffer, 1985) is devoted to the analysis of this dispute.

the contrary they were interested in abstract theoretical constructions justifying laissez-faire. This kind of constructions corresponded to deeply enrooted scholastic traditions of European universities to teach theology and linked with it philosophy. In the framework of these traditions the mathematics was considered as a summit of scientific approach. Powerful groups of society supported these traditions in the economics profession and made all possible to suppress newly born experimental current of economics. Modernist type of discourse concerning science allowed to legitimate scholastic approach used by economists as scientific one. Can you imagine, in what world, from the point of view of technology, we would live now if Hobbes' concept of science had overcome? The humanity remained at the technological level of the 17th century. Now imagine the world's history of the 20th century if the Schmoller's concept of economics would have overcome. I think that, in this case many human catastrophes could have been avoided.

Today, the dispute between Menger and Schmoller can be seen as a dispute between modernist and pragmatist/constructivist paradigms (see the Table 2 below)..

	Modernist paradigm	*Pragmatist/constructivist paradigm*
Basic assumptions	The world is external and objective The observer is independent Science is value free	The world is socially constructed and subjective The observer is an integral part of what is observed Science is moved by human interests
Basic characteristics of research	Data presents objective facts Context is given a priori Sublimation of complexity Logic of verification	Data presents subjective meanings Context is apprehended a posteriori Taking into account of complexity Logic of discovery
The researcher must:	Elaborate concepts in such a way that they could be measured Find causality links and fundamental laws Reduce the phenomena to their simplest elements Formulate hypotheses and test them	Elaborate concepts rooted in qualitative and quantitative data Try to understand the phenomenon under study Observe every situation in its totality Develop ideas by abduction from data

Source : (Usunier, Easterby-Smith and Thore, 2000, p. 37) and (Mucchielli, 1996, p. 197) adapted by the author.

Table 2. Basic characteristics of modernist and pragmatist/constructivist paradigms

Let me comment just one line in this table which is dealing with complexity. One of the modernist thinkers, the founder of positivism, Auguste Comte, professed to simplify observed phenomena before explaining them. Social sciences and in particular economics followed this positivist tradition. The scholars reduced phenomena under study to a certain number of quantitative variables or made a synthetic description before starting an analysis. In this way the positivist approach in general and the quantitative approach in particular sublime or deny the complexity of the phenomena under study. On the contrary the pragmatist/constructivist approach in social sciences, associated with qualitative research, does not reject the complexity but tries to manage it in the research process. This approach foresees rich and thick descriptions in order not to lose some crucial information. These descriptions often take the form of quotations from interviews. Descriptions incorporated in an article, or a book, do

not represent raw data but are chosen as significant. They are ranged according to constructed categories and accompanied by comments. In this way they represent an important part of the reasoning and allow the readers to make their own judgements concerning results of the research.

Experimentalist type of economics was dead[45] but, some schools in sociology and anthropology resisted to the modernist methodology and followed in some way methodological tradition of German economics. Among them the Chicago sociological school[46] of Symbolic Interactionism. A prominent representative of this school wrote: "No theorizing, however ingenious, and no observance of scientific protocol, however meticulous, are substitutes for developing a familiarity with what is actually going on in the sphere of life under study" (Blumer, 1969, p.39); "We must say in all honesty that the research scholar in the social sciences who undertakes to study a given sphere of social life that he does not know at first hand will fashion a picture of that sphere in terms of pre-established images <…> [In the framework of usual research practices] in place of being tested and modified by firsthand acquaintance with the sphere of life they [pre-established images] become a *substitute* for such acquaintance. <…> There is no demand on the research scholar to do a lot of free exploration in the area, getting close to the people involved in it, seeing it in a variety of situations they meet, noting their problems and observing how they handle them, being party of their conversations, and watching their life as it flows along. In place of such exploration and flexible pursuit of intimate contact with what is going on, reliance is put on starting with a theory or model, posing a problem in terms of the model, setting a hypothesis with regard to the problem, outlining a mode of inquiry to test that hypotheses, using standardized instruments to get precise data, and so forth" (Blumer, 1969, pp. 36 - 37) The last sentence of

[45]This statement can sound strange taking into consideration the existence of Experimental Economics of Vernon Smith. This branch of mainstream economics is in my opinion a parody of experimental approach. Most of so called experimental economics laboratory experiments are carried out with students (undergraduate and MBA) on the basis of *economic equilibrium, game theory, and utility theory schemes*. In addition constructed experimental situation contains *a priori rules*, which neoclassical authors misleadingly called institutions. These experiments follow the logic of theory/hypothesis verification in the framework of modernist paradigm. Logic of verification of this kind of experiments requires that "structure of the experimental design should be planned with consideration for the subsequent statistical analysis of the hypotheses of interest" (Davis and Holt, 1993, pp. 30, 31). Advocates of neoclassical experimental economics see that advantages offered by laboratory methods are generation of data and control: "Distinguishing natural data may sometimes exist in principle, but the data are either not collected or collected too imprecisely to distinguish among alternative theories. In other instances, relevant data *cannot* be collected, because it is simply impossible to find economic situations that match the assumptions of the theory (sic!). An absence of control in natural contexts presents critical data problems in many areas of economic research" (Ibid., p.15). If it is "impossible to find situations that match the assumptions of the theory" why should these data be collected? An absence of control in natural contexts does not present critical data problems if the objective of the research is not to predict or to verify but to understand with the help of people acting in these contexts. The Experimental Economics of Vernon Smith deals with a different kind of people. In order to guarantee an "unbiaseness" of participants' behaviour, the experimenters make a lot of efforts to prevent participants from discovering objectives of the experiment.

The methodology of pragmatist/constructivist laboratory gaming simulation experiments is the opposite. Most of the data generated by these experiments are not quantitative but qualitative. The experimental situation as the whole experimental design is based on this kind of experiments not exclusively on abstractions, but primarily on results of preliminary field studies. The players-participants execute roles and make research. In an experiment so designed, the players-participants are directly interested in studying the functioning of the analysed institutions. The emphasis on the aim to win may disturb the necessary level of dual behaviour and thus devaluate the experiment (Yefimov, 1979, pp. 404 – 409). The method of gaming-simulation gives possibility to organise laboratory economic experiments as a research process which follows the pragmatist/constructivist logic of theory/hypothesis discovery/construction. The research process is organised in this kind of laboratory experiments on the basis of a dialogue between scholars (experimenters) and players-participants (experienced actors in the area of real life under study), as a special form of "brainstorming session" accompanied by a computer simulation model managed by the players-participants (Yefimov, 1981, p. 198).

[46] One of the famous works of this school (Thomas and Znaniecki, 1926) was based on the life history method.

this quotation describes exactly in what way most of the economists do empirical research. It was expressed in an explicit form by Milton Friedman (1953): "A theory is the way we perceive 'facts', and we cannot perceive 'facts' without a theory" (p. 34). This idea is shared by an absolute majority of economists and they are not even ready to discuss this idea because it seems to them so obvious. The idea legitimates the work of the army of academics involved in creating and improving economic abstract a priori theoretical constructions because following this idea no economic analysis is possible without them.

To my mind this idea is totally erroneous and looks obvious because it identifies any preanalytic cognitive act with theory. Josef Schumpeter proposed the term Vision to signify this act: " <…> in order to be able to posit to our selves any problems at all, we should first have to visualise a distinct set of coherent phenomena as a worth-while object of our analytic efforts" (1954, p. 41). He illustrates his notion of vision by the example taken from Keynes's *General theory of Employment, Interest and Money*. He indicated that "in order to give convenient expression to certain facts of 'the world in which we live' – although, as Keynes himself emphasised, these facts are attributed to his fundamental schedules (propensity to consume, attitude to liquidity, and marginal efficiency of capital) <…> there can be no question of their having been established by antecedent factual research" (Ibid., pp. 41 – 42). Keyes underlined that they are "plausibly ascribed to our [the English] world, to our general knowledge of contemporary human nature" (Keynes, 1936, p. 250). The example shows that by Vision, Schumpeter means some set of a priori concepts on the bases of which an analysis is made. Robert Heilbroner understood very well the "theological" nature of mainstream economics (1988, 2004) and transformed the Schumpeter's notion of vision in the following way: "By vision we mean the political hopes and fears, social stereotypes, and value judgements <…> that infuse all social thought" (Heilbroner and Milberg, 1995, p. 4). The authors appeal mainstream economists to change their present laissez-faire vision in favour of "a recognition of the necessity for the widening degree and deepening penetration of public guidance into the working of capitalism itself" (Ibid., p. 119). I do not believe that economics should remain "theological" and that is why I cannot accept Heilbroner's deviation of Schumpeter's notion of vision. I propose to extend the Schumpeter's notion of vision to include in it not only a set of a priori concepts, but also types of techniques of study on the basis of these concepts, and also forms of presentation of the research results. In other words, the Vision determines first, what to study, secondly, how to study, and the thirdly, under what form the results of study should be presented. For the interpretive institutional economics which follows Schmoller-Commons's tradition the vision can be formulated as follows: it studies institutions (formal and informal rules) and beliefs linked with these institutions; it uses in the study qualitative research techniques (Denzen and Lincoln, 2005), which include among others, Action Research (Reason and Bradbury, 2001) and Grounded Theory (Bryant and Charmaz, 2007); and, it presents results of the study in the form of thick descriptions (Geertz, 1973). I will discuss below in this section of the paper in more detail the first and the second elements of the vision of interpretive institutional economics, but before I will characterise briefly the third one.

Clifford Geertz is the founder of interpretive social anthropology. According to him anthropologist should not to discover "laws" but try to understand meanings of actors. In this idea he follows Max Weber, who indicated "that man is an animal suspended in webs of significance he himself has spun" (Ibid., p. 5). As Latour and Knorr Cetina, Geertz thinks that "if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it ; you should look at what the practitioners of it do" (Ibid.). Finally what social anthropologists do it is "thick descriptions" (Ibid., p. 5, 6). Their activity can be compared with reading a "manuscript – foreign, faded, full of ellipses, incoherencies, suspicious emendations, and tendentious

commentaries, but written not in conventionalized graphs of sound but in transient examples of shaped behaviour". (Ibid., p. 10). "Thick description" is a translation of such a manuscript into understandable to us language.

Let us discuss the first element of the vision; what to study. Unfortunately not only neoclassical economists share Friedman's opinion that "we cannot perceive 'facts' without a theory". It happens because they often use the term "theory" instead of the notion "vision". Geoffrey Hodgson who is one of the very active writers with 'institutionalist label' confessed: "Contrary to many institutionalist writers, the epistemological position here is strongly anti-empiricist"[47] (Hodgson, 1988, p. 24). He criticised Friedman not for his modernist positivism but on the basis of this 'strongly anti-empiricist' position (Ibid., pp. 28 - 35). Let us discuss his 'anti-empiricists' arguments, which are indeed pro-Cartesian arguments: "The key criticism of empiricist epistemology (…) is that no observation can be independent of the conceptual framework, language and theoretical system of the observer. Consequently, no 'objective' facts can be known untainted by the preconceptions of the investigator" (Ibid., p. 35). I believe that the Hodgson's mistake is to consider "conceptual framework, language and theoretical system of the observer" almost as synonyms. Interpretive institutionalist investigator before engaging in a field study has his conceptual framework: she/he studies institutions (formal and informal rules) and beliefs linked with these institutions. Nevertheless the statement "all facts are expressed in some form of language, and an aconceptual or atheoretical language is impossible" is a sophism because of the use of undefined here notions of 'theory' and 'language'. 'Theories' and 'languages' in social studies can be of different levels. The statement is true if the notions of 'language' and 'theory' are used in the sense of categories shared in a certain socio-cultural linguistic environment (low level categories) which can be very large. For example, if the area under study was the Russian countryside, then knowledge of Russian language including technical agricultural terms would be sufficient *to begin* 'observation', i.e. "getting close to the people involved in it, seeing it in a variety of situations they meet, noting their problems and observing how they handle them, being party of their conversations, and watching their life as it flows along".

At the same time the statement of Hodgson is false if the notions of 'language' and 'theory' are used in the sense of categories and models/theories/hypotheses shared by a certain community of scholars. For example, gathering data concerning preconceived quantitative variables used in a theory and *escaping* any other information, which could be collected in the field if it does not enter in this set of preconceived variables, will make investigator 'blind' to many possible *insights*. In the case of using low level categories shared by actors of the area under study the scholar has a possibility to make a discovery and to reconsider his pre-established image of the area under study by developing *his own new* categories. In the case of using high level categories shared by members of a certain scientific community, a scholar, member of this community, is certainly dependant of the conceptual framework of the community. The potential validity of the institutinalist conceptual framework is based on the belief that social regularities come from socially shared rules and beliefs. But this conceptual framework is not a model or theory as it is understood in economics. On the contrary, in case of the use of an a priori model or theory, the scholar is unable to discover in the field something outside of her/his a priori model/theory/hypothesis and what she/he can do only is to 'test' this a priori model/theory/hypothesis. Geoffrey Hodgson is right by saying that "we cannot ever gain a more accurate or adequate understanding of economic reality exclusively by observation and the gathering of data" because understanding of economic reality can progress only through 'representational redescription', i.e. the correction or even total change of categories/concepts/theories

[47] According to Hodgson, empiricism "boils down to the view that sense data, or 'experience', are the ultimate source of all knowledge" (Ibid., p. 35). It is a typically modernist statement.

constructed on the basis or emerged from observation and gathering of data. I agree with professor Hodgson when, following the sociology of science, he said that "science is a social activity and its development involves the social generation, scrutinization and acceptance or rejection of theories, procedures and norms. Consequently, science can never be 'neutral' in the sense that it is entirely free of the biases and preoccupations of society and the scientific community" (Ibid., p.36).

Probably the confusion between "theory" and "language" comes from the practice of experimental physics. According to Prigogine and Stengers natural scientists have some kind of experimental dialogue with Nature. This dialogue "corresponds to a highly specific procedure. Nature is cross-examined through experimentation, as if in a court of law, in the name of a priori principles, Nature's answers are recorded with utmost accuracy, but relevance of those answers is assessed in terms of the very idealizations that guided the experiments. All the rest does not count as information, but is idle chatter, negligible secondary effects" (Prigogine and Stengers, 1985, p. 42). It means that this dialogue is going on using the language of a theory which "guides the experiment" (Ibid., p. 49). However unlike the Nature which can communicate with human being researcher only in language of researchers' theories, the actors, active objects of socio-politico-economic research, are also human being, who can communicate with researchers not in language of researchers' theories but in English, French or Russian which can be understood by both sides. It means that economists who transfer the practice of dialogue with Nature in natural sciences to practice of dialogue with Human beings commit a mistake. What Prigogine and Stengers are saying that quite often investigators of nature are forced to design their experiments as testing of some theories. Some methodologists of economics understand potential dangers of this kind of testing: **"**The first step in testing a scientific theory was to deduce certain empirical predictions from the theory and its initial conditions. The second step was to check these predictions against the observational evidence; if the empirical predictions turned out to be true, the theory was confirmed, and if these predictions turned out to be false, the theory was disconfirmed. In either case, it was not induction, but rather the deductive consequences of a scientific theory, that were relevant to its empirical support <…> Hypothetico-deductive method allowed scientific theories to be 'based on' empirical observations (deductively) without actually being 'built up from' those observations (inductively)." (Davis, Hands and Maki, 1998 p. 376) In this way the realism of research depends of a priori guessing of realistic theory. In case of simple systems, which were studied in classical physics, such guessing was possible. For more complex system such guessing becomes very improbable. Systems studied by economic science are never simple and that is why a priori theories do not have any chance to serve a basis for understanding of economic phenomena. No testing of this kind of theories would help: "Cut off from observation as a *source* of truth, the Cartesian mind puts great on 'testing' to reaffirm its realism. But testing is not a guarantee of correct ideas because, having lost its mooring in reality, the economic mind has created so many conundrums, puzzles and purely mental constructs that testing proves everything and nothing." (Mini, 1994, p. 41)

The described above elements (the first and the third) of the vision of interpretive institutional economics are in many respects shared by late Douglass North. If the New German Historical School and Wisconsin Institutionalism were inspired by philosophical constructions of hermeneutics and pragmatism, North turned to cognitive science. At the end of the 20th century the latter conquered some positions of epistemology[48]. The following quotation from the North's book (2005) contains its main idea: "Economic change is for the most part a deliberate process shaped by the perceptions of the actors about the consequences

[48] Gerald M. Edelman, the winner of the Nobel Prize, develops a so called brain-based epistemology: "This term refers to efforts to ground the theory of knowledge in an understanding of how the brain works" (Edelman, 2006).

of their actions. The perceptions come from the beliefs of the players – beliefs that typically blended with their preferences < …> Understanding is a necessary prerequisite missing in the economists's rush to model economic growth and change. We are a long way from completely understanding the process" (North, 2005, p. viii, ix). North even approached to the Geertz'notion of thick descriptions: "It is one thing to be able to provide a summary description of the process of economic change; it is something else to provide sufficient content to this description to give us an understanding of this process" (Ibid., p. 4).

Let us come now to the second element of the institutionalist vision: how to study. The interpretive institutional economics, the heir of Schmoller-Commons's economics, study socio-politico-economic reality on the basis of qualitative research. Qualitative research can analyse data at several levels: "At the most basic level, data are organized chronologically or sometimes topically and presented in a narrative that is largely, if not wholly, descriptive[49]. Moving from concrete description of observable data to a somewhat more abstract level involves using concepts to describe phenomena <…> This is the process of systematically classifying data into some sort of schema consisting of categories, themes or types. The categories describe the data, but to some extent they also interpret the data. A third level of analysis involves making inferences, developing models, or generating theory." (Merriam, 2001, p. 187) Some categories can be of similarity-based type when the others are of explanation-based type. The process of qualitative research can be characterised as a progressive move from actors' meaning to researcher's meaning (sense).

Points of comparison	*Quantitative Research*	*Qualitative Research*
Philosophical roots	Cartesianism, positivism	Pragmatism, hermeneutics
Goal of investigation	Prediction, confirmation, hypothesis testing	Understanding, description, discovery, meaning, hypothesis generation
Design	Predetermined, structured	Flexible, evolving, emergent
Sample	Large, random, representative	Small, non-random, purposeful
Data collection	Inanimate instruments (statistical data, surveys, questionnaires)	Researcher as primary instrument, semi-directive interviews, observations, documents
Mode of analyses	Deductive and inductive (by statistical method)	Abductive (by researcher)
Findings	Precise, numerical	Comprehensive, holistic, expansive, richly descriptive

Source: (Merriam, 2001, p.9) adapted by the author.

Table 3. Characteristics of quantitative and qualitative research

The Table 3 above presents a comparison of characteristics of qualitative research with the quantitative approach. The dichotomy indicated in this table does not exclude the use of quantitative methods in the framework of a qualitative research. The most important characteristics of qualitative research are the following (Ibid., pp. 6-8, 61):

[49] "Data are compressed and linked together in a narrative that conveys the meaning the researcher has derived from studying the phenomenon. While description is an important component of all forms of qualitative research, few studies are limited to this level of analysis." (Ibid., pp. 178-179).

1) Qualitative researchers are interested in understanding the meanings people have constructed, that is, how they make sense of their world and the experiences they have in the world. It is assumed that meaning is embedded in people's experiences and that the meaning is mediated through the investigator's own perceptions. The key concern is understanding the phenomenon of interest from the participants' perspectives, not the researcher's.
2) Qualitative researcher uses his data not to answer questions like how much or how often but to discover what occurs, the implications of what occurs, and the relationships linking occurrences. In this case his sample has not to be large and random but has to correspond to this purpose. Purposeful sampling serves to the investigator to discover, understand, and gain insight and therefore he must select a sample from which the most can be learned. The size of the sample can be determined gradually: sampling continues until a point of saturation is reached, i.e. no new information is forthcoming from new sampled units.
3) The researcher is the primary instrument for data collection and analysis. It determines by a creative character of communication with actors and abductive nature of data analysis (insights).
4) Qualitative research usually involves fieldwork. An occasional qualitative study could be undertaken using documents alone, but these are exceptions.
5) The product of a qualitative research is richly descriptive.

Among the qualitative research techniques, the most advanced ones are Action Research and Grounded Theory. As I mentioned above, Commons was may be the first to use Action Research. Now, it quite a common experimental practice in social sciences (Greenwood and Levin, 1998; Stringer, 1999; Reason and Bradbury, 2001). The main pioneer of action research – Kurt Lewin – associated the idea of action research with the idea of doing experiments in the field (Reason and Bradbury, 2001, p.17). Now action research is usually understood as "the whole family of approaches to inquiry which are participative, grounded in experience and action-oriented" (Ibid., xxiv). In action research, new knowledge is created through active experimentation. The results are always tested in real life because action research aims to find solutions to real problems. The results of its application are justified through their workability (Levin and Greenwood, 2001, p. 107). "Action research focuses on solving context-bound real-life problems. Knowledge production cannot be done without taking into account the wholeness of a situation. Inquiry is based on questions emerging from real-life situations as opposed to the conventional academic way of working where questions arise from within the academic community which is subdivided into professional fiefdoms. Reading other researchers' work as a way of identifying new research questions, as a the standard practice, is partly supplanted in action research by a more direct process of researching what social stakeholders understand to be pressing problems <…> Action researchers do not believe in the idea of scientific, cosmopolitan knowledge that is valid everywhere, and we reject the notion that valid knowledge can be produced only by 'objective' outsiders using formal methods that supposedly eliminate bias and error." (Ibid., p. 105, 110) Nevertheless action research carried out for different contexts allows making generalisations and creating theories. At the same time, knowledge of different contexts by action researchers could be very useful in a particular situation: "By setting the local situation in the context of these broader comparisons, the professional action researcher can assist the local group in opening up its sense of the situation and some options for the future." (Ibid., p. 99) In action research, actors (insiders) and investigators (outsiders) closely collaborate. Action research can be called cogenerative research. "Local participants [insiders] are enabled to take charge of the meaning construction process. At the same time, trained researchers cannot make sense of local social life without secure communication links to these

participants. The dynamic tension between insider and outsider knowledge is the basis for this cogenerative process <…> The interaction between local knowledge and expert knowledge through a cogenerative process is a core feature of action research. One consequence of this is that most accounts of action research, trying to be true to the process that constructed them, are rendered in narrative form." (Greenwood and Levin, 1998, pp. 113, 123)

Theorising in the interpretive institutional economics can be nothing else than a creation of sets of concepts with their interrelations coming from "a close and reasonably full familiarity with the area of life under study". These concepts must be deeply rooted (grounded) in the data gathered about rules, beliefs, and shared meanings. Most of this data can be collected exclusively through direct contacts with actors. Knowledge concerning these realities for different countries, and even for different regions and different economic sectors inside the same countries, in different periods of time can hardly be presented on the basis of the same categories/concepts. They will inevitably be different because of the complexity, cultural diversity and dynamics of economic realities. Special guidelines for producing this kind of theories (sets of concepts with their interrelations) were called Grounded Theory methodology [(Glaser and Straus, 1967; Strauss, 1987; Strauss and Corbin, 1998; Dey, 1999; Locke, 2001)]. A grounded theory represents an interpretation of an observed social phenomenon in the sense of interpretative sociology of Max Weber. John Commons spoke about theorizing in the Max Weber's (grounded theory) sense: "[The Weber's contribution] converts the whole process of theorizing from a 'theory', in the older sense of the logical consistency of reality, to the mere methodology of constructing intellectual tools to be used in investigation. There is no longer a question of antagonism between theory and practice, for a theory is a tool for investigating practice." (1934a, p. 722). Commons underlines that the "search for the *meaning* of human activities can never be expected to yield an 'exact' science, or even an approximation to the quantitative requirements of other sciences. Yet that is not wanted, anyhow. What the economist wants is *understanding*, and he wants *measurement* only as an aid to understanding" (1934a, p.723).

Grounded theory was defined by its elaborators as a discovery of theory from data (Glaser & Strauss, 1967, p.1). Grounded theory methodology is a continuation of the methodology of symbolic interactionism and is the most sophisticated version of qualitative research. According to this methodology, a theory must be generated not in an armchair but in the field. The process of data collection and the generation of a theory are not totally separated. The investigator collects documents and contacts actors to get data for analysis. She/he tries to set aside theoretical ideas during data collection and initial stages of their analysis. She/he does it "in order to assure that the emergence of categories will not be contaminated by concepts more suited to different areas" (Dey, 1999, p. 4). Categories have to emerge from (created on the basis of) texts of documents and transcripts of interviews. The categories discovered by the investigator are not just names of some pieces of data but reflect meanings of these pieces of data. The categories formulated by the researcher are the result of an abductive process of a detailed creative analysis of texts of documents and transcripts of interviews. This process is oriented to capturing insights. The interviews led by the investigator are active: interviews are based on approximate questionnaires and the interviewer lets speak the actor; interviewed actors are not considered by the analyst as passive "subjects" and the interviews can even take the form of brainstorming sessions.

"Beyond the decisions concerning initial collection of data, further collection can not be planned in advance of the emerging theory" (Glaser & Strauss, 1967, p. 47). These decisions are taken gradually following the process of emergence (creation) of categories. The founders of the grounded theory called this type of data collection Theoretical Sampling. "Theoretical sampling is the process of data collection for generating theory whereby the analyst jointly collects, codes, and analyses his data and decides what data to collect next and

where to find them, in order to develop his theory as it emerges. This process of data collection is controlled by emerging theory" (Ibid., p. 45). Under theoretical sampling, data collection stops when new data does not influence the established categories, their properties and the links between categories. "As a study proceeded, however, ideas would become more focused, and the methods could correspondingly become more structured; interviews, for example, might resemble long conversations at the start of the study but become highly selective and focused on particular topics (and therefore much shorter) by its close" (Dey, 1999, p.6). It is important to choose one or several core categories among the generated categories. "Through the process of integrating categories, a central theoretical framework could crystallize around a 'core' category <…> A framework would 'solidify' out the analysis and delimit the research by differentiating between core and peripheral categories and identify the scope and boundaries of the theory. This framework could in turn direct further data collection and analysis – but with a more circumscribed and focused agenda" (Ibid., p. 9). During the analytic process the number of categories may be reduced and the theory can be formulated with smaller set of higher level concepts (Glaser & Strauss, 1967, p.110) The generated categories are hypotheses which help to understand the phenomena under study. According to Commons "it is a synthesis, which helps to formulate a hypothesis, for it sets up the following problem: What is the *meaning* of the activities in their relations to each others? And thus suggests the kind of hypothesis needed to select the facts and weigh their relative importance. It is a synthesis of all the factors out of which we formulate a hypothesis. It differs from the theory of Menger as synthesis differs from analysis" (1934a, p.723).

Direct application of the grounded theory methodology to economic realities produces context embedded theories. It is clear that, for this type of theories, the continuity of institutional research can be more methodological than substantive. But it does not mean that every grounded theory research is an isolated investigation from the scratch. Some important theoretical substantive continuity is possible in the framework of the same or close contexts. In the latter case the comparative analysis of several grounded theories could produce categories/concepts of a higher level which would form new more abstract theory embracing several contexts at the same time. Grounded Theory methodology is nothing else but an explicit application of human cognition scheme to social scientific research. Any real research would follow it implicitly. Its other advantage is its orientation to discovery which diminishes the probability not to notice an important information concerning the area under study. This probability diminishes also by the requirement to set aside theoretical ideas during data collection and initial stages of their analysis. At later stages of analysis the influence on it of previous theoretical ideas shared by the researcher became inevitable but these ideas are confronted with data not filtered by these ideas, as in normal standard practice, that allows to the researcher to evaluate their appropriateness. The fact that the most important sources of information for social research are texts depicting rules (habits) and ideologies (beliefs) determines the Grounded Theory methodology as the methodology of generation of categories on the basis of texts.

Vision of the interpretive institutional economics presented above corresponds to the requirement expressed in the epigraph to this section. Advocates of pragmatist/constructivist paradigm in social sciences see their professional practices in a similar way. President of the International Sociological Association, Michel Wieviorka, formulated how to fulfil the requirements expressed in this epigraph in the very precise way. I his address to participants of the Third Russian Sociological Congress[50] in 2008, he formulated and answered two

[50] http://www.polit.ru/science/2008/10/22/wieviorka.html

questions: 1) what is usefulness of sociology? 2) what allows to say that sociology is a science? According to Wieviorka, these two questions are closely related. He said that a sociologist can be useful if he is involved in politics. Many prominent sociologists serve as advisers to different governments. He noted that in France a good sociologist plays the role of intellectual, i.e. he takes an interest in social life, participates in politics, publish articles in newspapers, and gives talk on the TV and radio. On the contrary a good American sociologist does not go on the air, does not broadcast on the TV, and does not write for newspapers. He is professional, activity of which is evaluated exclusively by his colleagues and who lives exclusively in academic world. Wieviorka thinks that the division between sociologist-intellectual and sociologist-professional should be overcome. By his opinion, the sociologist should be politically engaged. He has to study the social life in order to influence positively its change. Wieviorka thinks that the fact that sociologists very often play the role of experts, i.e. they provide purely technical information to those who needs it, is negative. If sociologists want really to be considered as producers of scientific knowledge they have to separate themselves very clearly from experts and expertise. What can be considered as a proof that sociologists have really grasped what they studied? A biologist can say that she/he repeated 10 times an experiment and got the same result, but this option is unavailable to sociologists. Usually it is colleagues who recognise that a sociological result is right. Wieviorka said that it is not sufficient. Validation of knowledge has to be made also outside of scientific community. It allows checking whether the knowledge produced by sociologists is really scientific. Several options of this validation are possible. Among them are presentation of the research results by radio/TV broadcasting or in newspapers which permits to get comments and judgements of a large public; "return" for evaluation of the results of investigation to those who were studied; interaction with such audience which asks questions, discuss, doubts and in this way participates in the improvement of the results. Michel Wieviorka thinks that these types of validation can be good additions to action research practices and so called sociological intervention when actors participate in the research process. He proposes to connect in this way the sociology and society. Please note that Wieviorka proposes all these contacts of researcher with different types of public to share with them results of research findings, that is her/his understanding "what is", and not some kind of social projects, that is "what ought to be". In the latter case she/he would cease to be a researcher and would become a social philosopher. Unlike experts that provide only technical information requested by those in power, politically engaged researchers communicate their findings to the public whether those in power like the results of these findings or not. I think it would be possible in a similar way to *link economists and economics with society.*

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