

plementation of a well-known argument «With an iron hand we shall drive mankind to happiness» which will inevitably lead to consequences that are well-known, too.

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TWO DISPUTES OF METHODS, THREE CONSTRUCTIVISMS, AND THREE LIBERALISMS ¹

Part I

The paper proposes to reconsider the methodology and history of economics radically, whether present day mainstream or heterodox versions of it. The profession of economists must definitely abandon Cartesian dualism and adopt Vygotskian constructivism. In fact constructivist economics already existed in the past

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and was cognitively very successful and socially very useful. It was the economics of Gustav Schmoller's historico-ethical school and the institutionalist economics of John R. Commons, traditions of which are totally ignored by the contemporary community of economists. The former tradition was based on Dilthey's hermeneutics and the latter on Peirce's pragmatism. It is worth to underline that hermeneutics and pragmatism are both predecessors of Vygotskian constructivism. During the last two decades a lot was written by economists on pragmatist, constructivist and discursive approaches to the methodology and history of economics, but those who wrote on these topics viewed them from the dualistic point of view.

My paper is an appeal to economists to reconsider Methodenstreit. The dispute of methods between Schmoller and 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 started in 1660, when the Royal Society of London had been founded, the cradle of the institution of science. Schmoller was one of the creators of the Verein, and Boyle was one of the founders of the Royal Society. The activities of both societies were similar in several respects: they represented efforts to collect data, working out of detailed reports and collective evaluation of obtained results. For Hobbes, as for Menger, the model of 'science' was geometry. Boyle and Schmoller privileged collecting and analysing data. Boyle did win the dispute, Schmoller did loose. It happened because of different attitudes of powerful groups in societies towards natural scientific experimental research and experimental social research. They were interested in the former, and they saw much more danger than benefit for them in the latter. On the contrary, they were interested in abstract theoretical constructions justifying the market vision of society and 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, mathematics was considered as a summit of the scientific approach.

On the one hand, the adoption of constructivism by economists would turn their discipline into a science functionally close to natural sciences. On the other hand the Vygotskian constructivism, as a social and political philosophy, once accepted by economists, may lead them to become preachers of the communitarian liberalism with its emphasis on social responsibility, deliberative democracy, and discourse ethics.

Keywords: Methodenstreit; social constructivism; constructivist epistemology and ontology for economics; constructivist history of economics; economic policy and deliberative democracy; economic philosophy and discourse ethics, communitarian liberalism

1. Constructivism vs. dualism

I think that it could hardly be contested that the present day economics, whether mainstream or heterodox, is impregnated with Cartesian dualism. The dualist vision of scientific research may be presented in the following way:

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 dualist vision of scientific research. This vision is based on the sharp separation of (research) object and subject (researcher), as well as on an individualistic character of the process of scientific research. Elements of this vision 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 that allowed John S. Mill to announce po-

litical 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 dualist vision of scientific research. Finally, the last characteristic, but apparently the most important feature of the Cartesian dualist vision of scientific research, was its individualism: the researcher was

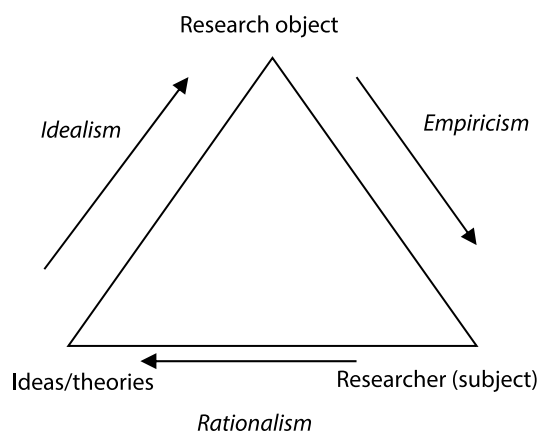


Fig. 1. The dualist vision of scientific research

alone in his search for truth as a copy of reality. It is the dualist vision of scientific research which is at the basis of the so-called “scientific method”. For mainstream economists, this method is based on Karl Popper’s philosophy of science [1] and for heterodox economists its foundations can be found in Roy Bhaskar’s critical realism [2]. The constructivism opposes to both of them.

Most economists totally ignore ideas of constructivism, but even those philosophers [3] and historians [4; 5] of economics, who studied literature on constructivism and tried to apply it to their publications, considered the constructivism from the dualist point of view. Bruno Latour and other specialists of Science Studies have shown that the dualist modernist vision of research never corresponded to the realities of scientific research: “We have never been modern” [6]. They developed a new vision of scientific research, the elaboration of which has been based on historical and field studies of scientific practices in natural sciences [7; 8; 9; 10].

The constructivism rejects the dualist vision with all its dichotomies and “isms”. In this vision of scientific research the research object is not separated from the researcher, but make together with the 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 into two categories: witnesses and judges. The 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 or not, 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 vision does not challenge the objectivity of scientific research and does not lead to relativism. 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” [11]. Application of this model to economics leads to the constructivist institutional economics [12; 13] resulting in the switch from primarily theoretical (a priori) type of research to experimental type

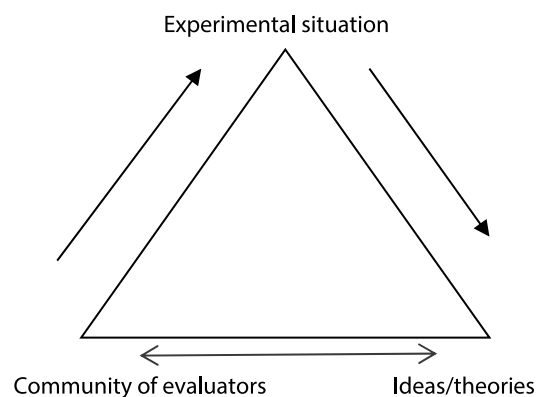


Fig. 2. *The constructivist vision of scientific research*

of research, and from primarily quantitative techniques to qualitative methods [14; 15]. 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 way that the research object can resist to the ideas issued by the researcher about it. It is this kind of resistance which has allowed to economists of the German Historico-ethical School headed by Gustav Schmoller and the Wisconsin Institutionalism of John Commons to come to the understanding of socio-economic-political processes in their countries, and elaborate on this basis propositions and legislative acts for the solution of the so-called “social question” at the turn of the 19th and 20th centuries.

The term “constructivist epistemologies” was initially introduced by Jean Piaget who was convinced that it is possible to know an object only by acting on it and by transforming it [16, p. 85]. These ‘acting’ and ‘transforming’ provoke ‘resistance’ of the ‘object’ which is the ‘source’ of any knowledge about it. The researcher performs these ‘acting’ and ‘transforming’ by mediation of certain instruments (see Fig. 3). In natural sciences, these instruments include not only material instruments but also all sorts of conventional signs. The inevitable mediation of socially created conventional signs in the interaction between subject and object was underlined by another founder of constructivism, an opponent of Piaget, Lev Vygotsky. According to him “instead of acting in a direct, unmediated way in the social and physical world, our contact with the world is indirect or mediated by signs” [17, p. 178]. These signs could take the form of “language; various systems for counting; mnemonic techniques; algebraic symbol systems; works of art; writing; schemes, diagrams, maps, and mechanical drawings” (Ibid.).

The Figure 3 is a slightly modified Vygotsky’s basic triangular representation of mediation [18,

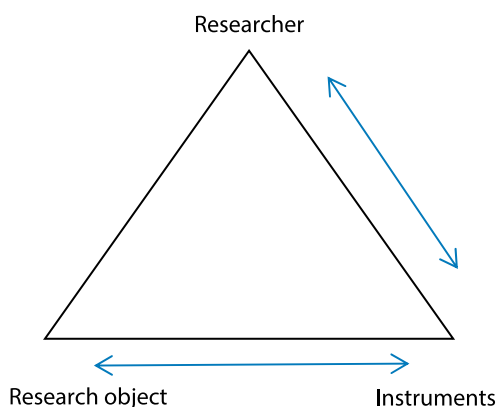


Fig. 3. *The subject-object interaction in experimental situation*

p. 5]. It could seem that this figure express the belief of Milton Friedman that “we cannot perceive ‘facts’ without a theory’, but it is not so. Many economists 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”¹ [19, p. 24]. In his book he criticised Friedman not for his dualist 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 Hodgson’s mistake is to consider “conceptual framework, language and theoretical system of the observer” almost as synonyms. Constructivist institutionalist investigator before engaging in a field study has his conceptual framework: she/he studies institutions (formal and informal rules) and beliefs linked to these rules. Nevertheless the statement “all facts are expressed in some form of language, and an a conceptual or a theoretical 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 knowl-

¹ According to Hodgson, empiricism “boils down to the view that sense data, or ‘experience’, are the ultimate source of all knowledge” [19, p. 24]. It is a typically dualist statement.

edge 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 to their conversations, and watching their life as it flows along” [20, p. 37].

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 (high-level 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 institutionalist 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 but a vision². 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 the construction of new or the correction of existing categories/concepts/theories on the basis of 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” [19, p. 36].

Probably the Hodgson’s confusion between ‘theory’ and ‘language’ comes from the practice

² Vision can be called also ‘frame theory’.

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” [21, 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 beings, 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, which were relevant to its empirical support ... Hypothetico-deductive method allowed scientific theories to be ‘based on’ empirical observations (deductively) without being ‘built up from’ those observations (inductively).” [22, p. 376]. In this way, the realism of research depends on a priori guessing of a realistic theory. In the case of simple systems, which were studied in classical physics, such guessing was probably 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” [23, p. 41].

It is already more than ten years that I am writing on pragmatist, constructivist and discursive approaches to the methodology and history of economics. It is true that last two decades a lot was written by economists on these subjects, but I think that those who wrote on these topics missed central characteristics of them. Economists did not take into consideration that the pragmatism, taking its origins from Charles Peirce, is a philosophy that underlines experimental character of scientific investigations and of human life in general, and that any change in habits and beliefs is the result of ongoing experiments, which make together our lives. Economists did not understand that the successor of pragmatism, the social constructivism, launched by the book of Berger and Luckmann [24], is about construction of social reality, which is reality of rules (as typification of habitualized actions) and beliefs legitimizing these rules. These rules and beliefs are emerging and changing through social interactions. If we take off experimental, interactive and communal treatment of research and life in pragmatism and constructivism, then we lose the core of these teachings. Many economists, following J. S. Mill, continue to think that economics should inevitably be an abstract science because ‘controlled’ experiments in social sphere are impossible. But the notion of ‘controlled’ experiments is closely linked with the notion of causality which is inappropriate in this sphere¹. The search for causality should be substituted by the search of rules and beliefs which justify them. It can be made only on the basis of discursive approach. The discursive approach is wrongly reduced in the community of economists to the rhetoric used in economic theories. On the contrary in the framework of constructivism, it is the discourses of actors which are considered. Such constructivist as Rom Harré claims that “the primary human reality is persons in conversations” [26, p. 58] and “there are only two human realities: physiology and discourse (conversation) — the former an individual phenomenon, the latter collective” (Ibid., p. 345). He insists on

¹ «Using the language of causality to describe research methods and results can only distort and obscure what has been discovered. At best it is a rhetorical device to make the work look more like physics. At worst it is barrier to the development of a truly scientific psychology» [25, p. 80].

Table 1

Two ontologies [27, p. 29]

| Ontologies | Locative systems | Entities | Relations |
|------------|------------------|-------------------|-----------------------|
| Newtonian | Space and time | Things and events | Causality |
| Discursive | Arrays of people | Speech acts | Rules and story lines |

“the centrality of conversations in both social reality and the study of that reality [25, p. 65].

At present the absolute majority of economists and many social scientists do not pay attention to the fact that “people alone among animals can speak [and] people can give accounts of what they are doing, disambiguating their actions and justifying them by reference to rules, conventions and customs” (Ibid., p. 25). Because language is a major instrument in many human activities, then studying the uses of this instrument would be a way of studying these activities: “Through the mediation of language there is an unbroken continuum between thought and action” (Ibid., p. 12). Following the discursive approach, the researcher “is concerned predominantly with language in use as the accomplishment of acts or as attempts of their accomplishments” (Ibid., p. 58). The discursive approach is based on ontology totally different from that inherited from the Newtonian mechanics. What we have to investigate in social sciences — economics is (or has to be) a social science — are not things and events, but discourses consisting of speech acts. Instead to look for causal relations, social scientists (including economists) have to try to reveal rules and supporting story lines (see Fig. 1). In order to do it “the experimenter or the observer has to enter into a discourse with the people being studied and to try to appreciate the shape of the subject’s cognitive world” [27, p. 21].

The researcher has “to know what a situation means to a person and not just what the situation is (say, according to a description in terms of its physical characteristics as these are seen by an observer) if we are to understand what that person is doing” (Ibid., p. 21). For this kind of research, it does not matter where and even when something was said but what really matters, it is who said that. Institutional or social knowledge is not universal; it relates to concerned communities. That is why the people to be contacted have to have the social knowledge linked with phenomena under study. In this sense ‘array of people’ means people from a certain appropriate community. For example, in order to study financial markets, it is necessary to contact financial professionals like traders and not graduate students of economics as it takes place in the so-called ‘experimental economics’. At the same time, ‘array of people’ means a sample from a target community. The choice of the peo-

ple in the sample and its size made in the framework of the discursive approach are done in a totally different way in comparison with the mechanistic approach. The researcher contacts people who are willing to share their social knowledge. The size of the sample (number of people contacted) is determined by the so-called ‘theoretical saturation’, when the researcher learns nothing new by additional people contacted from the target community.

Most of the content of this paper is based on Figure 2. Initially, it was presented to Russian economists in a largely disseminated text [28], to participants of a EAEPE conference [29], and to French heterodox economists [30]. No one of these communities paid any attention to this figure. I am afraid that the same fate could be reserved for the present paper. Without understanding and accepting the Figure 2, the assimilation of the text of this paper becomes impossible. Figure 2 in this text is not just a decoration but represents its central concept. The upper vertex of the triangle expresses the idea of Jean Piaget that it is possible to know an object only by acting on it and by transforming it. Figure 3 adds the Piagetian and Vygotskian dimension of language in these ‘acting’ and ‘transforming’. The left lower vertex of the triangle in Figure 2 shows, on the one hand, the Peircean social character of scientific research, and, on the other hand, it underlines the role of power relations in the functioning of academic communities with a special emphasis on influences coming from outside of these communities.

2. On constructivist history of economics

Mark Blaug, the author of the popular textbooks on the history [1] and methodology [31] of economics declared in his article entitled “Why I am not a constructivist: confession of an unrepentant Popperian” that there is “the tension between the methodology of science and the history of science” and that “it is impossible to study the history of science without some notion, however, crude, of the difference between science and non-science” [32, p. 109]. He developed his history of economics from the point of view of the rhetoric of popperian post-positivism. Those, who would like to try to study the history of economics from the constructivist point of view, should understand the main feature of its methodology, the

role of resistance, which was indicated in the first section of this article, and the discursive nature of it[33]. Unfortunately, at present economists do not understand them. D. Wade Hands in his book [3] quoted one of the prominent constructivist scholars, Karin Knorr Cetina¹, who underlined the crucial role of resistance of the research object to researcher inside the experimental situation, but did not notice that inside it the object of study and researcher with her/his instruments cannot be separated. He relates constructivism to relativism asking the question to what degree the constructivism “endorse the view that the objects and theories of science are completely constituted by human and social factors, leaving no role for objective nature in the determination of scientific beliefs?” (Ibid., pp. 176-177). Hands certainly sympathized with “an effort to salvage scientific rationality and normative epistemology from the threat of relativism and social constructivism” (Ibid., p. 367).

E. Roy Weintraub, in his historical studies[4; 35; 36], does not pay any attention to the nature of resistance of the research object to economists. In fact, mathematical economists considered by him did not experience at all this kind of resistance. In addition Weintraub with his co-author Till Düppelimit, the community of evaluators exclusively to members of the economists’ profession. Weintraub and Düppe rightly affirm that the Cowles Commission played a crucial role in the transformation of the profession of economists in the profession of mathematical economists², but they cannot accept the conclusion of Philip Mirowski[36a]that after 1947 “Cowles [had to] attend to its political priorities”, because Cowles was connected by contracts and ideas to RAND, which was “a cold war (anti-communist) institution”, and in this way “Cowles was corrupted by the military-industrial complex”. The list of literature (References) of Weintraub’s and Düppe’s working paper [35] contains the book by Amadae

S.M.[37] Rationalizing Capitalist Democracy. The Cold War Origins of Rational Choice Liberalism, but I did not find any quotation or reference to this book inside this working paper. It is quite natural because this book would confirm the Mirowski’s conclusion. After the presentation by Weintraub and Düppe of a chapter of their future book [36] at the 2012 ESHET Conference, in which they emphasized that the first Bank of Sweden Prize in Memory of Alfred Nobel in 1969 was a clear sign of the success of the Cowles’ culture in economics, I asked Weintraub, whether they investigated the process of the creation of this prize and the process of attribution of prizes. The answer was “no”. In fact, this prize has nothing in common with the Nobel Prize and can be characterized as an intellectual fraud³.

The crucial role in the communities of evaluators (see Fig. 2 above) of political and economic actors from outside of communities of economists at the period of creation of profession of economists in 19th century was quite well investigated by American historians Elizabeth Sage and Mary Furner. French political economists of the 19th century saw an important opportunity for professionalization of their discipline. In order to get “scientific status and power”, the discipline “protected itself from unwanted knowledge”, “elevated particular types of knowledge and disqualified others” [38, p. 6]. Unwanted knowledge primarily concerned the social question: “It was industrialists who offered the descriptions of la question social and proposals for its solution that economists in turn studied, promoted and grafted onto their science” (Ibid. p. 7). In order to do it, they relied upon fictional “Adam Smith and Jean-Baptiste Say, who they invented” (Ibid., p. 19) by ignoring in their teachings everything that contradicted the laissez-faire principle. The French government supported economists in this activity. The following quotation from a report dated 1864 of the French Minister of Education, Victor Duruy, to Emperor Napoleon III on the creation of the Department of Political Economy at Paris Faculty of Law shows governmental involvement in spreading ideas of economists as justifications of the existing social order: “Your Majesty once addressed these words to the national industry exhibitors: “Spread among your workers the sound doctrines of political economy”. You, Sir, also claimed that the duty of government is to propagate these necessary concepts, which, according to the English minister of that time, saved England from socialism. This ne-

¹ “All of us constructivists, I think, are what they call ontological realists: We believe in the existence of the material world “out there,” and we believe in the fact that this material world offers resistance when we act upon it. It will resist; we can’t just do everything with it. So in that sense we are all realists. ... Negotiating, for example, when they can stop the measurement, at what point they’ve got enough data, and at what degree or position they can say, “Now it is real!” ... This interpretative flexibility ... prompts me to doubt that you can ever get at the real world as it really is. You can get resistances in the laboratory; but in order for these resistances to make sense, they have to be interpreted. The very moment you interpret them, you enter the realm of the social world” [34, pp. 184-185].

² In France between 2000 and 2011, 84,2 % of recruited university professors of economics were mathematical economists (Le Monde, 3 April 2014).

³ <http://exiledonline.com/the-nobel-prize-in-economics-there-is-no-nobel-prize-in-economics>.

cessity proclaimed by the Emperor fourteen years ago, the country recognizes today. Public opinion demands that unfortunate gap in our general educational system is filled, and several cities have already claimed the opening of political economy courses' [39, p. 43-44].

The similar situation took place in the United States: «members of so-called clerical school of academic economists such as Francis Bowen, John Bascom, and Arthur Latham Perry worked closely with a group of wealthy and prominent men of affairs that included Amasa Walker, David Ames Wells, Edward Atkinson, Horace White, Gamaliel Bradford, Charles F. Dunbar, John Murray Forbes, and Joseph Ropes. Their common goal was the installation of laissez-faire as an American system of economics. Walker, Perry, Bascom, and Bowen wrote the standard economics texts of the 1860s and 1870s» [40, p. 37]. As Mary Furner 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. British economist Bob Coats indicates, that “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” [41, 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 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).

French-American sociologist Marion Fourcade indicates: “The first American [economics] 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” [42, p. 64]. Later this type of economics was welcome by American businessmen who “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” (Ibid., p. 66). Fourcade underlines that in the United States “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” (Ibid., 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).

Now, the constructivist institutionalism in political science “draws attention to the role of business in proselytizing and sponsoring new and/or alternative economic theories” [43, p. 68]. Düppe and Weintraub ignore it totally. It is interesting to compare the Düppe-Weintraub’s vision of the post-war American economics profession with the vision of the same period by one of the leaders of heterodox economics, Frederic Lee: “In the post-war years, three different forces affected the landscape of American economics. The most dramatic of these was the anticommunist hysteria that silenced an entire generation of radical and progressive American academics, including economists. Moreover, the emerging conservative pro-business anti-government political and social climate affected liberal economists in terms of what they taught and what they wrote in textbooks. The final force was the modernization movement where economic departments consciously redesigned their programs to ensure that the most up-to-date versions of neoclassical economic theory were taught using the appropriate mathematical tools. As a result, all that was taught in this post-war period was neoclassical economic theory while the descrip-

tive-Institutional oriented approach became less emphasized and nearly disappeared” [43a, p. 35]. He describes it as a period when universities “administrators actively co-operated with the FBI and in many cases asked the FBI to vet potential hires, professors going up for tenure, and all tenured faculty and make recommendations about hiring and firing”, when some university “academic staff for a variety of reasons ...were American Legion and FBI informants and collaborators while others feared reprisal by the university administration”, and finally when “many of the professional associations to which academics belonged either collaborated with the FBI, such as the American Anthropological Association and the American Economic Association (AEA)” (Ibid., p. 37). In order “to avoid the withdrawal of research funds or escape attacks, harassment, social ostracism, or the inevitable dismissal or denial of tenure, many progressive academics left academia voluntarily, took academic positions outside the United States, restricted and censored the content of their lectures (such as not teaching Keynesian economics or as not to appear pacifistic, atheistic, or unpatriotic) since classes were monitored by students and police informers, advised graduate students to do safe, conventional dissertations so as to avoid red-baiting from committee members, avoided talking to student groups about socialism, and/or at the least metaphorically voluntarily blowing their brains out by re-directing their own research and publications to safe, more conventional areas” (Ibid.). This kind of history of economics describes very well the heterogeneous composition of the community of eval-

uators where decisive influences concerning destiny of ideas/theories come from outside of a community of academic economists. At the same time “A History of Heterodox Economics” by Frederic Lee shows also very well that heterodox economists deal with degenerated types of experimental situations where the research object cannot resist to the researcher because of its absence: all ideas/theories of heterodox economists come from their minds a priori without any real interaction between the researcher and the research object.

The constructivist approach to the history of science consists not only of the declaration that scientific ideas and theories are human constructions but also that these constructions are made by researchers on the basis of direct interactions between them and research objects in the framework of experimental situations constructed by researchers. Constructivists like Bruno Latour [11] and Karin Knorr Cetina [44] “discovered” this unalienable feature of scientific research by making anthropological type of studies of real scientific practices. This unalienable feature of scientific research provides us with the notion of the difference between science and non-science. Certainly it is not the notion used by E. Roy Weintraub and Frederic S. Lee: for the former, as for the majority of mainstream economists, the science is there where they apply mathematics; for the latter, as advocate of critical realism based on dualism, the science is there where they are looking for causalities on the basis of abstractions and not on the basis of interactions with objects of study in the framework of experimental situations.

(Part II of the article will be published in the next issue of the journal)

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