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A critique of Latour and Woolgar's Argument for the Social Construction of Scientific Facts in Laboratory Life: The Construction of Scientific Facts (1986)

This paper is an attempt to critique *Laboratory Life* by Bruno Latour and Steve Woolgar. This is done by: presenting a summary of the arguments in the text; contextualising the text (and the authors) in the scholarship of the time; and assessing whether the authors have succeeded in carrying out the overall purpose of the book. Suffice to say that much as they argue for the social construction of scientific facts, their account of the social construction of facts is probably unconvincing to those researchers who still conceive the social (or human affairs) and the scientific as two incompatible worlds.

Keywords: argument, social construction, laboratory, scientific knowledge, incompatible, ethnographic, exogenous, phenomenotechnique.

1. Introduction

Over three decades ago, Bruno Latour, a French philosopher-sociologist, and Steve Woolgar, an English sociologist, undertook a project which culminated in co-authorship of a book titled *Laboratory Life: The Construction of Scientific Facts* (first published in 1979 and later revised in 1986). This book constitutes an anthropological study that was conducted by Bruno Latour from October 1975 through August 1977 in Professor Roger Guillemin's laboratory at the Salk Institute for Biological Studies, California. The members of this laboratory were preoccupied with neuroendocrinology research. In the study, Latour who became a participant observer, is portrayed as a "an outside observer" or "stranger." The publication of *Laboratory Life* was indeed groundbreaking work for what has become known as 'laboratory studies.' A central feature of laboratory studies is its insistence on *in situ* observation of scientific activity; that is, it is characterised by an ethnographic description of science *as it happens* (Woolgar, 1982: 482). The book presents one main argument: that scientific facts are socially constructed. Through the use of what they call "an anthropology of science," the authors, based on Latour's observations at particular laboratory, attempt to shed more light on the process of fact production and on the similarities with their approach of constructing this account. For a greater part of the book the authors describe systematically the way in which the daily activities of working scientists in one science laboratory lead to the construction of facts.

Within a broader context, Latour and Woolgar's work can be viewed as reacting to perspectives of science prevalent at the time as espoused by philosophy of science and sociology of science. The book was also intended to denigrate the authoritative position scientific

knowledge had at the time. Latour and Woolgar's work overlaps with a research tradition in social studies of science referred to as Institutional Sociology of Science (ISS). This book attacks philosophers of science for being preoccupied with the abstract elements of science i.e how scientists discover truth; rather than what happens in the laborator. Sociologists of science are also faulted for their focus on the large-scale effects of science which led to increased knowledge of the external effects and reception of science; and for studying science using approaches that resulted in giving a picture that the scientific world was completely different from the social world (or human affairs). Ironically, Latour and Woolgar's work was influenced by some philosophers of science, such as Thomas Kuhn, and some sociologists of science including Mertonians, and the Edinburgh school i. e., Barry Barnes, David Bloor and Steven Shapin (Shapin, 1995).

Most probably, the authors are targeting multiple audiences to read their book (Anthony and Dirk, 2011, online). According to Latour and Woolgar, "what the text says," "what really happened" and "what the authors intended" are now very much up to the reader. It is the reader who writes the text" (p. 273). In other words, the authors are saying that the book's audiences include scientists and non-scientists.

This paper is a critical essay; that is, it is an attempt to critique *Laboratory Life*. It examines both the merits and demerits of the book. In this vein, three tasks will done: Firstly, presenting a summary of the main arguments in the text; Secondly, contextualising the text (and the authors) in the scholarship of the time; and thirdly, evaluating, in the light of other secondary sources and commentaries, whether the authors have been successful in carrying out the overall purpose of the book.

1.1. Background Information

Latour and Woolgar's *Laboratory Life* is located in what has become known as the sociology of scientific knowledge (SSK). The sociology of scientific knowledge as a new approach to thinking about science emerged in the early 1970s (Pickering, 1992: 1). It is sometimes called social studies of knowledge or the new sociology of science, contrasting it to the traditional sociology of science. The focus in SKK is on the content of science, also described as "opening the black" of science (Hess, 1997). In fact, the advocates for SKK "have accused other sociologists of science of leaving the black box content unopened and examining only the exogenous, institutional aspects of science and technology" (Hess, 1997: 80). Thus, SKK is distinguished from other approaches of sociology of science in two major ways. Firstly, it holds that science is constitutively social. Secondly, SSK asserts that the only way to understand scientific activity which constitutes the content of science is to use an anthropological approach. Today, *Laboratory Life* is widely read by scholars in the field of study called Science and Technology Studies (STS) (Smith, 2011).

Latour and Woolgar's book was a landmark publication for the sociology of scientific knowledge (SSK), in the sense that it is the first book-length ethnographic study of laboratory life (Pickering, 1992: 2). The book demonstrates the social character of the scientific facts which is a remarkable shift from what was prevalent in the social studies of science at the time. In the Postscript to Second Edition (1986), Latour and Woolgar write: "When the first edition of *Laboratory Life* appeared in 1979, it was surprising to realise that this was the first attempt at a detailed study of the daily activities of scientists in their natural habitat" (p. 274).

Following the publication of this influential book, other theorists including Karin Knorr-Cetina and Susan Leigh Star, adopted the methodology used by Latour and Woolgar to explain how scientists construct knowledge and facts in a laboratory. For instance, Knorr-Cetina also became a participant-observer during her study at a government-funded

research institute in Berkeley, California, conducted between October 1976 and October 1977. Subsequently, in 1981, Knorr-Cetina wrote a book titled *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* in which she discusses in detail how scientists produce and reproduce their knowledge. Latour and Woolgar's book was first published in 1979 with the title *Laboratory Life: The Social Construction of Scientific Facts*, and was later revised in 1986, this time the authors omitted the word "social" in the subtitle. Latour and Woolgar assert that the use of the word "social" in their first edition was ironic since in their considered view all interactions are social. They argue that the social study of science has rendered "social" devoid of any meaning (cf. Latour 1986a and b). Thus, they do not regard scientific knowledge as having a privileged position among knowledge systems. The 1986 revised version contains a Postscript, intended to respond to some of the reactions to their first edition. Other additions to the revised version include a detailed Table of Contents, Additional References, and an index.

According to the authors, the book is based on Bruno Latour's two-year study (by observing the day-to-day processes of scientific activity and participating as a technician) in Professor Roger Guillemin's laboratory at The Salk Institute for Biological Studies located in La Jolla, California, and which was subsequently written up in collaboration with an English sociologist, Steve Woolgar.

2. Summary of the book

Laboratory Life has six chapters, each of which builds on the argument of the book. In this section I highlight the key arguments of the text.

Latour and Woolgar argue that the use of an anthropological approach (participant-observation) is the only way of both penetrating the mystique of science and providing a reflective understanding of the detailed activities of working scientists (p. 18). This is in a way undermines the various approaches other researchers in social studies of science have used. Latour and Woolgar observe that other approaches used by scores of researchers since the turn of 20th Century have only focused on underpinning the large-scale effects of science. They argue that this has led to increased knowledge of the external effects and reception of science, but the mystery of science still remains. These approaches are also faulted for taking the products of science for granted, rather than attempting to account for their initial production. As a consequence of this, there have been a number of studies whose focus is on the size and general form of overall scientific growth, the economics of its funding, the politics of its support and influence, and the distribution of scientific research throughout the world (p. 17). In contrast to these approaches, Latour and Woolgar decide to construct an account based on the experiences of close daily contact with laboratory scientists over a period of two years in a particular setting. They call this approach "an anthropology of science." Using this approach, Latour's two-year study of daily experiences in a laboratory at The Salk Institute for Biological Studies gave him some insights into what scientists do, and clearly demonstrates the social construction of scientific facts.

An anthropology of science focuses on studying scientific activity in a particular setting. Thus the material Latour and Woolgar are discussing in their book was gathered from *in situ* monitoring of scientists' activity in one laboratory. This endeavour demonstrates the social construction of scientific facts, that is to say, social factors come into play when scientists are constructing facts, just the same way non-scientific institutions construct knowledge. La-

tour and Woolgar argue that science is like any other social system. In fact, scientific realm can be conceived as the end product of many other operations that are in the social realm. This means that the tribe of scientists should be an object of study for anthropologists just like other social phenomena. Thus, the expertise of sociology is necessary to explain even the scientific phenomena.

Latour and Woolgar assert that the scientists are in the business of producing facts. This can also be understood as constructing reality. The production of facts is made possible by the use of inscription devices, such as a bioassay, a spectrometer, or an amino acid analyser. An inscription device is any item of apparatus which can transform a material substance into a figure or diagram which is directly usable by a scientist to produce a paper. Scientists spend two-thirds of their time working with inscription devices to produce inscriptions. Inscriptions such as diagrams, graphs, curves, or sheet of figures become the focus of discussion between participants, and all the intermediary steps which made their production possible are forgotten or taken for granted as being merely technical matters. They are regarded as having a direct relationship to the "original substance" under study. In some sense, the inscriptions embody reality. Latour and Woolgar claim that not only does the production of the phenomena (inscriptions) depend on the inscription devices, but the phenomena are also thoroughly constituted by the use of inscription devices (p. 64). This becomes artificial reality which is essentially constituted by the use of inscription devices. Such a reality is what Bachelard terms the "phenomenotechnique" since it takes on the appearance of a phenomenon which has been constructed through material techniques (p. 64). Scientists use inscriptions to produce papers where some statements are re-affirmed while some are rejected. The aim of science is to create facts whereby statements are stripped away all modalities and traces of authorship. According to Latour and Woolgar, a fact is nothing but a statement with no modality and no trace of authorship. Scientists produce papers to persuade readers that the statements contained therein are facts. This explains why scientists compulsive and almost manic writers. In this light, Latour and Woolgar perceive a laboratory as a system of literary inscription in which scientists spend much of their time coding, marking, altering, correcting, reading, and wring.

Throughout *Laboratory Life* the authors repeatedly point out that scientific activity is not "about nature," but the construction of reality. Of course, defining reality is an age-old contested issue among philosophers. Latour and Woolgar's definition is contrasted to that held by many philosophers. According to the authors, "that which cannot be changed at will is what counts as real." However, they are quick to point out that their position is not relativist. Latour and Woolgar appear to be equating reality with facts. To build a case for their argument that facts are constructed, Latour and Woolgar invoke the etymological significance of the word "fact." Etymologically, the work "fact" is derived from the root *facere*, factum (to make or to do). For us to understand that scientific facts are constructed by scientists, the authors argue, we need to critically examine the microprocesses of negotiation which continually take place in the laboratory. This calls us to observe daily encounters, working discussions, gestures, and a variety of unguarded behavior among the scientists. These conversational exchanges provide a wealth of evidence of the intrusion of social factors in the daily exchanges between scientists which give rise to "logical" arguments, the implementation of "proofs," and the operation of "thought processes." The authors argue that fact construction is affected by conversational exchanges, but these have thus far been largely neglected in studies of scientific practice. The point being stressed here is that practicing scientists are engaged in conversational exchanges in the same way as do non-scientists, signifying that the nature of scientific activity is essentially not different from those practices of interpretation in non-scientific activity.

Latour and Woolgar also discuss the process of stabilisation of a statement to become a “hard” fact. In Chapter 3, the authors take the reader back in time by discussing the construction process of TRF (H) (expanded as Thyrotropin Releasing Factor (Hormone) in). Special attention is given to the period between 1962 and 1969. The aim is to use a historical perspective to explain the construction and stabilization of a hard fact, TRF (H). The members of Guillemin’s laboratory were known for their research in neuroendocrinology, and their major breakthrough was in 1969 when Burgus, a chemist, who collaborated with Guillemin, determined the structure for TRF. The historical treatment in Chapter 3 shows clearly that the acceptance of a scientist’s findings by fellow scientists is influenced by social factors, that include age and reputation of the scientist, and availability of material and other resources to conduct scientific research. By analysing events that led to the rejection of Schally’s plausible findings and acceptance of Guillemin’s mistaken results, Latour and Woolgar conclude that the logic of deduction cannot be isolated from its sociological grounds. The authors argue the inclusion of Chapter 3 to show the process of the social construction of TRF (H) is justifiable. They argue: “If the process that demonstrating the process of social construction can be demonstrated for a fact of such apparent solidity, we feel this would provide a telling argument for the feasibility of the strong programme in the sociology of science” (p 106). The authors elucidate that at the point of stabilisation as a fact, two things happen: first, there is splitting between an object and a statement about the object; and second, an inversion of a statement takes place. For instance, TRF whose structure is Pyro-Glu-His-Pro-NH₂ transforms such that scientists now say “TRF *really* is Pyro-Glu-His-Pro-NH₂.” Thus, at the end of the stabilisation reality is now attributed to the object rather than to the statement about the object, and subsequently the object becomes the reason why the statement was formulated in the first place. According to Latour and Woolgar “...the statement becomes the mirror image of the reality “out there” (p. 177). The splitting and inversion of a statement are crucial processes in the stabilisation of a fact. Interestingly, all human aspects were eliminated at the end of the process of constructing TRF, such that TRF appeared unconstructed by either by Schally or Guillemin. This is a paradox that Latour and Woolgar discuss in their book.

The argument for the social construction of scientific facts is incomplete without discussing the importance scientists attach to the notion of credibility. But of what significance is the notion of credibility to the argument for the social construction of scientific facts? Well, this notion does help us to make sense of the scientists’ careers and the solidity of their production. Specifically, this notion helps us understand the real motivation for scientists. Latour and Woolgar argue that it is wrong to regard scientists as only being motivated by the receipt of reward. They argue that scientists do not just pursue reward; rather they are more of investors of credibility. If this is true, then we are in a better position to explain the sociological factors that influence scientists’ behavior and their ability to do science. What is clear though, is that a scientist’s ability to get research grants is dependent on his credentials (i. e., academic qualification especially a Ph.D, position in the field, and research experience). Thus, for researchers are constantly under pressure to be credible, and be productive at the same time. This is essentially a public relations side of scientific activity. Latour and Woolgar liken scientists’ credibility to a cycle of capital investment, and the notion of credibility makes us appreciate the conversion by scientists of one form of credibility into another. This cycle of credibility constitutes recognition, money (or research grants), equipment, data, prestige, credentials, problem areas, argument, papers, and so on. Latour and Woolgar assert: “the conversion between one type of capital and another is necessary for a scientist to make a move in a scientific field” (p. 201).

As a way of concluding their argument, Latour and Woolgar draw parallels between their own account of laboratory work and that used by scientists to construct scientific facts. Perhaps the only major difference is that scientists create order in a laboratory while sociologists like Latour and Woolgar write texts. They argue that scientists and non-scientists alike are in the business of constructing order out of disorder. They further argue that fabricating order out of chaos has its own challenges, and the onus is on investigators to demonstrate that this fabrication has been done correctly. This process is compounded when “an outsider” attempts to make sense of what “insiders” do. The authors submit that in order for any investigator to make sense of his observations, the investigator should adopt some kind of theme by which he is able to construct a pattern. Thus, the observer’s selection of a theme constitutes his method. In this book they demonstrate how they have used a theme to make sense of laboratory life. In the same way scientists try to persuade readers to accept what in their papers as facts, any investigator would like to convince others of the existence of a pattern of what has been observed. The authors show how they have employed methodological reflexivity in their approach to study science. The authors are not really sure if they have been able to convince their readers about the fabrication of their account on the social construction of scientific facts contained in their text. They pose this question: “How much further research, investment, redefinition in the field, and transformation of what accounts as an acceptable argument are necessary to make this account more plausible than its alternatives?” Hopefully readers of *Laboratory Life* will attempt to answer this question, and this write-up is an attempt to respond to the same.

3. Evaluation

In this section I briefly discuss my own assessment of the positive and negative aspects I find in *Laboratory Life*. I highlight how well the book has achieved its overall purpose, how it compares with other works on the subject, what has been left out, and specific points that are not convincing, among other things.

3.1. How well the book has achieved its goal

It is easy to follow through the argument of the book. Latour and Woolgar’s description of the daily activities in the laboratory is clear. It is a book that is interesting, especially to non-scientists who are unfamiliar with what scientists do in the laboratory and what they aim to achieve. The authors have tried so hard to argue for the social construction of scientific facts. However, their argument is weakened with the inclusion of Chapter 3, which I think is out of place. The inclusion of this chapter is tactfully done by the authors to build on their argument. One may argue that the historical narrative of the construction of TRF (H) has nothing to do with the ethnomethodological perspective that laboratory studies advocate for.

3.2. What possibilities are suggested by *Laboratory Life*

There are two possibilities that are suggested by the book. First, the authors have demonstrated that it is possible for non-scientists to study and understand scientific activity. They argue that scientific activity, like any other social activity, should be studied by sociologists. They have demonstrated that using an anthropological approach, sociologists can help us understand science better. Second, the authors suggest that scientific practice is no different from other human affairs having discussed how social factors permeate the production of scientific knowledge.

3.3. What the book has left out

The authors argue that studying the politics of science and economics of science does not help to demystify science. But this proposition can be challenged. Latour and Woolgar do not discuss how the social construction of scientific knowledge relates to politics and economics of science. One can argue that these three i. e. social, politics and economics are interlinked and not separate from each other as the authors portray. The authors intimate that the politics and economics of science are not related to the construction of scientific facts. They do not even discuss the politics scientists are engaged in the course of construction of scientific facts. A critical reading of the text reveals that scientists are engaged in some form of politics.

Further, Latour and Woolgar do not acknowledge the strides made in social studies of science by philosophers and sociologists of science. It is as if philosophy of science and sociology of science are irrelevant to understanding of science and what scientist do. Ironically, Latour and Woolgar borrow a lot of ideas from some philosophers of science and sociologists of science. However, they do not seem to explicitly acknowledge this contribution (Pickering, 1992; Shapin 1995).

3.4. How the book compares to others on the subject

As pointed out earlier, *Laboratory Life* is considered a landmark publication in the sociological of scientific knowledge. It is considered a precursor to publications in 'laboratory studies.' Laboratory studies emphasise the ethnographic description of scientific activity *in situ* (as it happens). The idea is to describe 'what really goes on in science' as opposed to 'what philosophers say about science' (Woolgar, 1982: 484). But "in style and in sympathy it is similar to Ludwik Fleck's resurrected *The Genesis and Development of a Scientific Fact* (1935)" in which he investigated the construction of scientific facts, knowledge, and practices, and argued that scientific facts are invented, not discovered (Shapin, 1981: 342). Fleck had argued that the process of social construction makes scientific facts appear as if they have been discovered. Woolgar and Latour support Fleck's argument. Another influential piece in laboratory studies is Knorr-Cetina's book *The Manufacture of Knowledge* (1981). Knorr-Cetina conducted her anthropological study at a government-funded research institute in Berkeley, California, between October 1976 and October 1977. Reading Knorr-Cetina's book, (which is a second book in 'laboratory studies'), one gets the impression that it is a replica of Latour and Woolgar's book. Just like Latour and Woolgar, Knorr-Cetina argues that close observation of scientific activity in a laboratory setting reveals that a scientist is practical reasoned. The contents of the book are consistent with those Latour and Woolgar presented in their 1979 edition.

However, we need to understand the context of the scholarship at the time when Latour and Woolgar wrote their text. Latour and Woolgar most probably were reacting to some ideas propounded by philosophers of science and sociologists of science. Jonas Salk, writing an Introduction to *Laboratory Life* observes that the authors are not in agreement with the approaches researchers in social studies of science and philosophy of science have used to study science (p. 11). But at the same time, it is apparent that the authors also want to advance some arguments from philosophy of science and social studies of science. Arguably, Latour and Woolgar benefited from work of such writers as Thomas Kuhn, Peter Mettugh, Jeff Coulter, Harvey Sacks, and Melvin Pollner who argued that the making of scientific knowledge could be accounted for by human cognitive capabilities and ordinary forms of social interaction (Shapin, 1995: 305). For instance, Kuhn in his *The Structure of Scientific Revolutions* (1962) argues that sociological factors come into play when scientists chose theories. Latour and Woolgar discuss this fairly in their Chapter 3. Thus, by the late 1970s science had almost lost its privileged position as an authoritative system of knowledge (Shapin, 1995). Certainly Latour and Woolgar's work was an attempt

to demonstrate that science is not superior to other systems of knowledge. In fact, *Laboratory Life* argues that social factors are at play in the process of knowledge production in a laboratory, such that “human affairs” are not different from “scientific production” (p. 13).

Another positive aspect of *Laboratory Life* is how Latour and Woolgar aptly articulates the important role publications play in science. This is not a new idea in social studies of science at the time. One may argue that they are advancing Merton’s conceptualisation of the ethos of science. In his *The Sociology of Science* (1973), Merton had emphasised on the need for scientists to make their findings public. He argued then that scientists should communicate their findings to fellow scientists and the public for the advancement of science (Merton, 1973). Latour and Woolgar submit that the production of papers is acknowledged by scientists in the laboratory they studied as the main object of scientific activity. Indeed, Latour and Woolgar’s emphasis on scientific publications is consistent with the conceptualisation of the norms of science.

It is clear that Latour and Woolgar’s book, is responding to some of the implications of Kuhn’s work. Thomas Kuhn’s work coincided with a fundamental re-evaluation of preconceptions about the “special” character and place of science. Certainly, they are advancing this view. The authors have clearly explained how scientific facts are socially constructed by scientists, and yet no human agency is referred to in the statements of facts (Shapin, 1981). Latour and Woolgar also concur with Kuhn (1962) that scientists are not concerned about discovering the truth about nature. Since the publication of Kuhn’s *The Structure of Scientific Revolutions*, it has become philosophically unacceptable for scientists to claim to that they are endeavour to know the truth about nature (Bird, 1987: 255). As Latour and Woolgar point out, “scientific activity is not “about nature,” it is a fierce fight to construct reality” (p. 243). This means that scientists can only claim to know relative truth about nature, “one whose meaning and articulation are governed by can claim to know a particular scientific paradigm” (Bird, 1987: 225).

3.5. What specific points are not convincing

Laboratory Life is somewhat attacking philosophers of science who tend to be abstract and do not concern themselves with what happens in a laboratory. The authors insinuate that all science is done in a laboratory. But a question to be answered is: Is science only practiced in a laboratory, and not outside of the laboratory? Philosophers of science investigate the nature of science. It can be argued that some philosophers of science have discussed the nature of scientific activity and scientific methods as it is practiced outside of the laboratory. The authors’ perspective of what science should be is narrow. What is not clear in *Laboratory Life* is whether Latour and Woolgar’s description of social construction of scientific facts in the laboratory they studied can be universalised to other science practices that are done outside of the laboratory.

As the authors discuss the daily activities in the laboratory they include a discussion of the historical construction of TRF (H). However, the inclusion of Chapter 3 is misplaced and dubious. Woolgar discussing the features of ‘laboratory studies’ asserts: “Even where the scientific activity under study is not confined to a laboratory as such, the analyst takes something akin to a participant observer role, where he becomes part of the situation he attempts to observe” (Woolgar 1982: 482). Woolgar who co-authored *Laboratory Life* insinuates that the inclusion of a historical exposition of the construction of TRF (H) in their book is akin to being a participant observer. This is horrendous.

After a critical reading of the book and other commentaries, and contextualising the text in the scholarship of the time, I am tempted to think that Latour and Woolgar’s book project is either an attempt to quash the long held belief that scientific knowledge is special and superior to other knowledge or agreeing with Robert K. Merton, the founding father

of sociology of science, that sociology is also a genuine science. Not only did Merton work tirelessly to constitute the study of science as a legitimate branch of structural-functionalist sociology, but he also attempted to constitute sociology as “scientific” (Shapin, 1995: 294). Latour and Woolgar argue tactfully that indeed scientific knowledge is a product of social negotiation among scientists, thereby rendering science at par with other human activities. Shapin (1995) argues that the deletion of the word “social” from its original (1979) title was deliberately done to remove scientific knowledge from its status as credible knowledge. The authors also demonstrate that scientific activity is comprehensible even to those who have not been trained as scientists. Latour, a non-scientist, who studied scientists in their laboratory setting was able to understand what scientists do. Perhaps the big question that remains is: ‘Is scientific practice at the same level with other human affairs?’

4. Conclusion

Laboratory Life gives a reader a vivid picture of what goes on in a science laboratory. Latour and Woolgar, who in my view are very competent writers, present the most concrete and very detailed account for the social construction of scientific facts in an endocrinology laboratory by describing Latour’s daily experiences therein and his attempts to comprehend what scientists do. A critical analysis of the book points to the fact that it can be recommended not only to sociologists of science and practicing scientists, but also to non-scientists who would like to grasp how scientists produce knowledge. It is a well-written piece which is easy to read and puts together philosophy of science and social studies of science. Indeed, as observed by Shapin, “The book is without question the most concrete and detailed account we have of how scientists actually behave, how they talk with one another, and how they interact with their technological devices” (Shapin, 1981: 342). A word of caution though, to those interested in reading the book is that in order for them to fully comprehend the argument of the book, it is necessary that no chapter (the book has six chapters) is read in isolation. Suffice to say, that much as the authors attempt to show that they have gone an extra mile (in as far as social studies of science are concerned) by presenting a well-structured and forceful argument for the social construction of scientific facts, their account of fact production is probably unconvincing to a vast majority of researchers who conceive the social (or human affairs) and the scientific as two incompatible worlds.

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РОДНОЙ АЛЕКСАНДР НИМИЕВИЧ

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Некоторые тенденции в истории профессионального сообщества химиков

Новейшее более враждебно новому, чем устоявшемуся старому.
Эрвин Чаргафф

Деятельность людей, связанная с анализом и синтезом вещества, привела к формированию профессии химика. Для этого химикам понадобилось овладеть такими важными социальными функциями, как объясняющая (теоретическая), обучающая (передача знания) и инновационная (создание новых технологий).

Рассмотрено несколько сюжетных линий, связанных с возникновением и развитием профессионального сообщества химиков: химико-аналитическая и химико-технологическая деятельность до XIX века; появление первых национальных дисциплинарных сообществ химиков в XVIII — начале XIX века; химики-естествоиспытатели; наука и преподавание; наука и технологии; научно-практические и химические общества; российские химики в XX веке. Сделана попытка выявить некоторые тенденции развития сообщества химиков.

Ключевые слова: профессиональное сообщество химиков, дисциплинарное сообщество, научно-практическое и химическое общество, химико-аналитическая деятельность, химико-технологическая деятельность, история профессий.