Междисциплинарная магистерская программа

Технонаука, инновации, экономика / Technoscience, Innovation, Economy

(TIE Program)

(Университет ИТМО совместно с Европейским университетом в Санкт-Петербурге)

(двойной диплом)

Направления подготовки: 38.04.01 Экономика & 39.04.01 Социология

Список рекомендуемой литературы для ответов на вопросы экзаменационного билета

Основная литература:

Учебники и учебные пособия

1. Акоев, М.А., Маркусова, В.А., Москалева, О.В., Писляков, В.В. (2014) Руководство по наукометрии. Индикаторы развития науки и технологии.Из-во Урал. Ун-та.
2. Букки М., Тренч Б. Пособие по общественным связям в науке итехнологиях. СПб.: Альпина нон-фикшн, 2018.
3. Гребенников П.И. Микроэкономика : учебник и практикум для вузов / П. И. Гребенников, Л. С. Тарасевич, А. И. Леусский. - 8-с изд., перераб. и доп. — Москва: Издательство Юрайт, 2021. 547 с.
4. Девятко И. Ф. Методы социологического исследования. Екатеринбург:Изд-во Уральского университета, 1998
5. История экономики / Заславская М.Д. М.: Дашков и К°, 2020.
6. Ленуар Р. и др. Начала практической социологии. М.; СПб.: Институт экспериментальной социологии; Алетейя, 2001.
7. Макроэкономика : учебник для вузов / А. С. Булатов [и др.] ; под редакцией А.С.Булатова.— 3-е изд., испр. и доп.— Москва: Издательство Юрайт, 2021.— 333с.
8. Макроэкономика : учебник для вузов / под общей редакцией В. Ф. Максимовой. — Москва : Издательство Юрайт, 2021. — 171 с.
9. Макроэкономика : учебник для вузов / С. Ф. Серегина [и др.] ; под редакцией С. Ф. Серегиной. — 4-е изд., испр. и доп. — Москва : Издательство Юрайт, 2021. — 477 с.
10. Микроэкономика: учебник для вузов / под редакцией А. С. Булатова. — 3-е изд., испр. и доп.— Москва: Издательство Юрайт, 2021.— 358 с.
11. Мировая экономика. Учебник /Пашковская М.В., Господарик Ю.П. М.:Издательский дом Университета "Синергия", 2019.
12. Национальная экономика : учебник и практикум для вузов / А. В. Сидорович [и др.] ; под редакцией А. В. Сидоровича. — 2-е изд., перераб. и доп. — Москва : Издательство Юрайт, 2021. — 576 с.
13. Радаев В. Как организовать и представить исследовательский проект. М.:ВШЭ, 2001.
14. Ритцер Д. Современные социологические теории. СПб: Питер, 2002. 5-еизд.
15. Российская экономика. Курс лекций. В 2 книгах. Книга 1. Истоки и панорамарыночных реформ / Ясин Е.Г., Агамирова М.Е., Бирюкова С.С. М.: ГУ ВШЭ,2019.
16. Филиппов А.Ф. Элементарная социология: введение в историюдисциплины. М.: ГК «РИПОЛ Классик» / «Панглосс», 2019.
17. Штейнберг И., Шанин Т., Ковалев Е., Левинсон А. Качественные методы.Полевые социологические исследования. СПб: Алетейя, 2009.
18. Экономика. Учебник для студентов естественных и гуманитарныхфакультетов. М.: Издательство МГУ, 2019.

Дополнительная литература:

1. Maxwell J. A. Qualitative Research Design: An Interactive Approach Applied.Social Research Methods Series. 1996.
2. Блур Д. Сильная программа в социологии знания // Логос. 2002. № 5- 6(35). С. 162–185.
3. Волков В., Хархордин О. Теория практик. СПб.: Изд-во Европейского ун-та в С.-Петербурге, 2008.
4. Земнухова Л. В. Социальные исследования технологий: эволюция и взаимодействие подходов // Экономическая социология. 2018. Т. 19. № 5.С. 113–129
5. Квале С. Исследовательское интервью. М.: Смысл, 2003.
6. Корбут А.М. Этнометодологические исследования науки // Эпистемология и философия науки. 2013. Т. 35. № 1. С. 151–166.
7. Кун Т. Структура научных революций. М.: АСТ, 2003.
8. Латур Б. Наука в действии: следуя за учеными и инженерами внутриобщества. СПб.: Изд-во Европейского ун-та в С.-Петербурге, 2013.
9. Латур Б. Об акторно-сетевой теории. Некоторые разъяснения, дополненные еще большими усложнениями // Логос. 2017. Т. 27. № 1. С.173–200.
10. Малкей М. Наука и социология знания. М.: Прогресс, 1983.
11. Фирсова Н.И. Предвестник исследований диффузии инноваций ГабриэльТард //. Социология власти. 2012. № 6-7. С. 298-313.
12. Широков А.А. Концепция больших технологических систем томасаХьюза - между технологическим детерминизмом и социальным конструктивизмом // Социология науки и технологий. 2016. №4.
13. Шумпетер, Й. А. Теория экономического развития. М.: Прогресс, 1982.(Глава 2). С. 148-205.
14. Этический кодекс Российского общества социологов (РОС) // https://www.ssa-rss.ru/index.php?page_id=84

Словари и справочники:

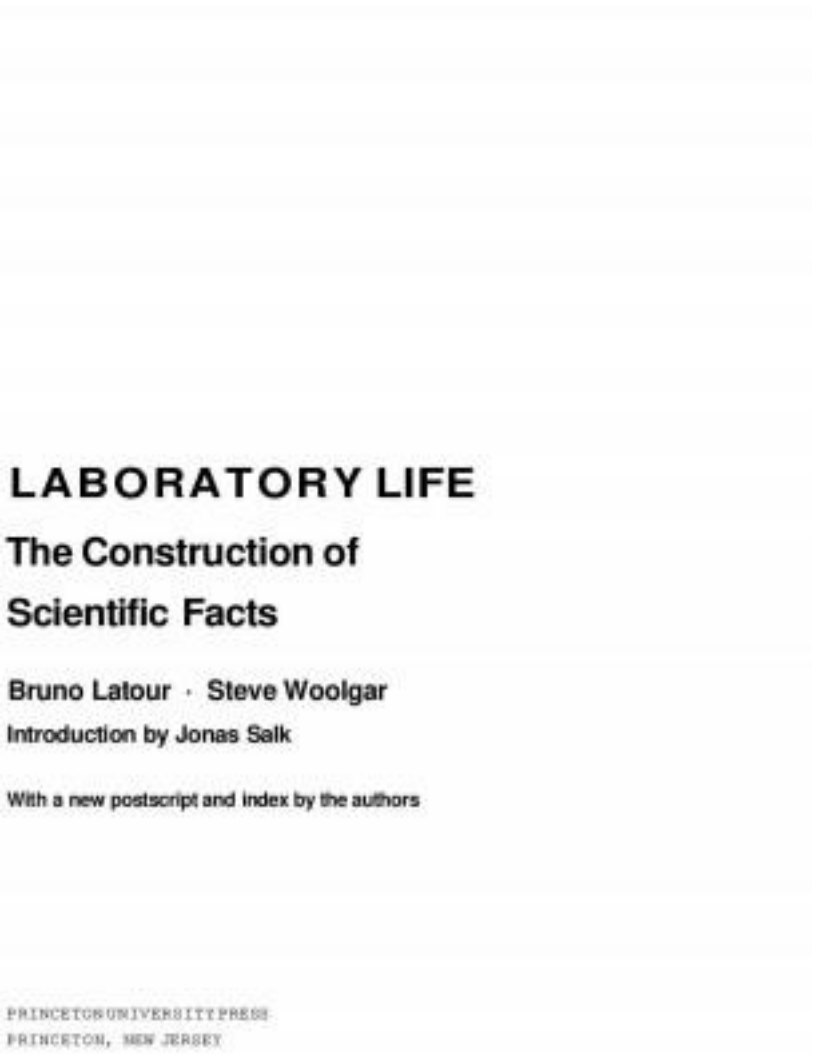
1. International Encyclopedia of the Social & Behavioral Sciences / под ред. J.D.Wright: Elsevier Science, 2015.
2. Социологический энциклопедический словарь / под ред. Г.В.Осипов. М.: Норма, 2000.
3. Социология: Энциклопедия / под ред. А.А. Грицанов. Мн.:Книжный Дом, 2003.
4. Райзберг, Б.А. Современный экономический словарь / Б.А. Райзберг, Л.Ш. Лозовский, Е.Б. Стародубцева. – 6-е изд., перераб. и доп. – Москва : ИНФРА-М,2019. – 512 с. – (Библиотека словарей «ИНФРА-М»). - ISBN 978-5-16-105386-7 (online). - Текст : электронный. - URL: https://znanium.com/catalog/product/1003268 (дата обращения: 16.02.2021). – Режим доступа: по подписке.

Предлагаемые для перевода тексты

Примеры книг, из которых могут быть предложены фрагменты длянаписания изложения

1. Collier, S. Post-Soviet Social: Neoliberalism, Social Modernity, Biopolitics.
2. Hughes, T. Networks of Power: Electrification in Western Society, 1880-1930.
3. Jasanoff, S. The Fifth Branch: Science Advisers as Policymakers.
4. Latour, B., and S. Woolgar. Laboratory Life.
5. Minssen, T. et al. Cutting edges and weaving threads in the gene editing (Я)evolution : reconciling scientific progress with legal, ethical, and social concerns.
6. Shapin, S., and Schaffer. Leviathan and the Air Pump.
7. Shapin S. The Scientific Life. A Moral History of a Late Modern Vocation.

Пример фрагмента научного текста для подготовки к письменному изложению на русском языке



LABORATORY LIFE

The Construction of Scientific Facts

Bruno Latour · Steve Woolgar

Introduction by Jonas Salk

With a new postscript and index by the authors

PRINCETON UNIVERSITY PRESS
PRINCETON, NEW JERSEY

in that it raises the question of whether or not an equitable balance has been reached between the two sides of the dichotomy. This question remains, despite affirmations that "technical and social issues are intimately linked" (Mulkay, 1974: 114).

We should like to argue that it is not necessary to attach particular significance to the achievement of a "correct" balance between "social" and "intellectual" factors. This is for two main reasons. Firstly, as already mentioned, the distinction between "social" and "technical" factors is a resource drawn upon routinely by working scientists. Our intention is to understand how this distinction features in the activities of scientists, rather than to demonstrate that emphasis on one or the other side of the duality is more appropriate for our understanding of science. Secondly, our interest in the details of scientific activity cuts across the distinction between "social" and "technical" factors. We want to pay attention to "technical" issues in the sense that the use by scientists of "technical" and "intellectual" terminology is clearly an important feature of their activity. But we regard the use of such concepts as a phenomenon to be explained. More significantly, we view it as important that our explanation of scientific activity should not depend in any significant way on the uncritical use of the very concepts and terminology which feature as part of that activity.

The "Anthropology" of Science

The focus of our study is the routine work carried out in one particular laboratory. The majority of the material which informs our discussion was gathered from *in situ* monitoring of scientists' activity in one setting. Our contention is that many aspects of science described by sociologists depend on the routinely occurring minutiae of scientific activity. Historic events, breakthroughs and competition are examples of phenomena which occur over and above a continual stream of ongoing scientific activities. In Edge's (1976) terms, our most general objective is to shed light on the nature of "the soft underbelly of science": we therefore focus on the work done by a scientist located firmly at his laboratory bench.

In line with this perspective, a project took shape which we called, for want of a better term, an anthropology of science. We use this description to draw attention to several distinctive features of our approach.[1] Firstly, the term anthropology is intended to denote the

preliminary presentation of accumulated empirical material. Without claiming to have given an exhaustive description of the activities of all like-minded practioners, we aim to provide a monograph of ethnographic investigation of one specific group of scientists. We envisaged a research procedure analogous with that of an intrepid explorer of the Ivory Coast, who, having studied the belief system or material production of "savage minds" by living with tribesmen, sharing their hardships and almost becoming one of them, eventually returns with a body of observations which he can present as a preliminary research report. Secondly, as has already been hinted, we attach particular importance to the collection and description of observations of scientific activity obtained in *a particular setting*. By our commitment to techniques of participant observation we hope to come to terms with a major problem which have thus far dogged understanding of science. Recently, there has been a growing dissatisfaction with outside observers' reliance on scientists' own statements about the nature of their work. Some participants have themselves argued that printed scientific communications systematically misrepresent the activity that gives rise to published reports (Medawar, 1964).[2] In a similar manner, Watkins (1964) complains that the "didactic dead-pan" style required of scientific reporting creates various difficulties in understanding how science is done. In particular, scientists who eschew the autobiographical form of reporting make it difficult for readers to appreciate the programme or context which provide the backdrop to reported work. Sociologists have noted that similar tendencies cause particular problems for the sociological understanding of historical context (Mulkay, 1974; Woolgar, 1976a; Wynne, 1976), although it is usually held that contradictory interpretations are reconciliable through sociological explanation (Mulkay, 1976; but see Woolgar, 1976b). These comments on the problems involved in the use of scientists' accounts find a parallel in discussions of the "craft" character of science. For example, Ravetz (1973) suggests that the nature of scientific activity is thoroughly misrepresented by the form of presentation which is used in the reporting of science. Not only do scientists' statements create problems for historical elucidation; they also systematically conceal the nature of the activity which typically gives rise to their research reports. In other words, the fact that scientists often change the manner and content of their statements when talking to outsiders causes problems- both for outsiders' reconstruction of scientific events and for an appreciation of how science is

done. It is therefore necessary to retrieve some of the craft character of scientific activity through in situ observations of scientific practice. More specifically, it is necessary to show through empirical investigation how such craft practices are organised into a systematic and tidied research report. In short, how is it that the realities of scientific practice become transformed into statements about how science has been done? We regard the prolonged immersion of an outside observer in the daily activities of scientists as one of the better ways in which this and similar questions can be answered. This also has the advantage that our descriptions of scientific activity have emerged as a result of the observer's experiences in the field. In other words, we have not chosen consciously to focus predominantly on any one of the technological, historical, or psychological aspects of what is observed. No attempt was made to delimit the area of competence prior to our discussion, and there was no prior hypothesis about a concept (or set of concepts) which might best explain what was to be encountered in the field. Thirdly, our use of "anthropology" denotes the importance of bracketing our familiarity with the object of our study. By this we mean that we regard it as instructive to apprehend as strange those aspects of scientific activity which are readily taken for granted. It is evident that the uncritical acceptance of the concepts and terminology used by some scientists has had the effect of enhancing rather than reducing the mystery which surrounds the doing of science. Paradoxically, our utilisation of the notion of anthropological strangeness is intended to dissolve rather than reaffirm the exoticism with which science is sometimes associated. This approach, together with our desire to avoid adopting the distinction between "technical" and "social," leads us to what might be regarded as a particularly irreverent approach to the analysis of science. We take the apparent superiority of the members of our laboratory in technical matters to be insignificant, in the sense that we do *not* regard prior cognition (or in the case of an ex-participant, prior socialisation) as a necessary prerequisite for understanding scientists' work. This is similar to an anthropologist's refusal to bow before the knowledge of a primitive sorcerer. For us, the dangers of "going native" outweigh the possible advantages of ease of access and rapid establishment of rapport with participants. Scientists in our laboratory constitute a tribe whose daily manipulation and production of objects is in danger of being misunderstood, if accorded the high status with which its outputs are sometimes greeted by the outside world. There are, as far as we know, no a priori reasons for

supposing that scientists' practice is any more rational than that of outsiders. We shall therefore attempt to make the activities of the laboratory seem as strange as possible in order not to take too much for granted. Outsiders largely unfamiliar with technical issues may severely jeopardise their observational acumen by initially submitting themselves to an uncritical adoption of the technical culture.

Our particular use of an anthropological perspective on science also entails a degree of reflexivity not normally evident in many studies of science. By reflexivity we mean to refer to the realisation that observers of scientific activity are engaged in methods which are essentially similar to those of the practioners which they study. Of course, debates about whether and in what senses the social sciences can be scientific are the familiar stock-in-trade of many sociologists. Frequently, however, these debates have hinged on erroneous conceptions of the nature of scientific method culled from philosophers' partial accounts of the way science is practised. Although, for example, much has been made of whether social science can (or should) follow Popper or Kuhn, the correspondence of the descriptions of science provided by these authors to the realities of scientific practice is somewhat unclear, to say the least.[3] In our discussion, we shall sidestep these general issues and instead concentrate on specific problems which the scientific practitioner and the observer of scientific activity may have in common. This will entail making explicit, particularly in the latter part of the discussion, our awareness of certain methodological problems which we face in the construction and presentation of our discussion.

We have attempted to meet the above requirements of an anthropological perspective by basing our discussion on the experiences of an observer with some anthropological training but largely ignorant of science. By using this approach we hope to shed some light on the process of production within the laboratory and on the similarities with the approach of the observer.

It is unlikely that our discussion will tell working scientists anything they do not already know. We would not presume, for example, to reveal hitherto undiscovered facts about the details of scientific work to the subjects of our study. It is clear (as we show) that most members of our laboratory would admit to the kinds of craft activities which we portray. At the same time, however, our description of the way in which such craft activities become transformed into "statements about science" might constitute a new perspective on what working scien-

tists know to be the case. We anticipate that hackles might rise where participants hold an obdurate commitment to descriptions of scientific activity formulated in terms of research reports. Often this commitment stems from the perceived utility of such statements in procuring funds or claiming other privileges. Objections will thus be forthcoming where our alternative version of the way science proceeds is seen potentially to undermine or threaten the securement of privileges. The investigation of the basis for beliefs or, as is a more accurate description of the present discussion, of the social construction of scientific knowledge, is frequently construed as an attempt to cast doubt on the beliefs or knowledge under study. Analysts often face this kind of mistaken perception in the sociological study of knowledge (for example, Coser and Rosenberg, 1964: 667). Our "irreverence" or "lack of respect" for science is not intended as an attack on scientific activity. It is simply that we maintain an agnostic position. We should emphasise, therefore, that we do not deny that science is a highly creative activity. It is just that the precise nature of this creativity is widely misunderstood. Our use of creative does not refer to the special abilities of certain individuals to obtain greater access to a body of previously unrevealed truths; rather it reflects our premise that scientific activity is just one social arena in which knowledge is constructed.

It might also be objected that the work of the particular laboratory we have studied is unusual in that it is relatively poor at the intellectual level; that its activity comprises routinely dull work, which is not typical of the drama and conjectural daring prevalent in other areas of scientific work. However, the Nobel Prize for Medicine was awarded to one of the members of our laboratory in 1977, soon after we began preparation of this manuscript. If the work of the laboratory is merely routine, then it is possible to receive what is perhaps the most prestigious kind of acclaim from the scientific community for the kind of routine work we portray.

It is perhaps relatively easy to show the intrusion of social factors in cases of borderline, controversial science, or where secrecy and competition are evident. This is because it is precisely in these situations that scientists can offer evidence of nonscientific or extra-technical interference with their work. As a result, it is tempting in these cases to explain the occurrence of the "technical" in terms of the "social." The work of our laboratory, however, constitutes "normal" science which is relatively free from obvious sociological events. We

are less tempted, therefore, to try to tease out instances of gossip and scandal; no sociological muckraking is intended, nor do we claim that science devoid of such intrigue is unworthy of sociological attention.

So far we have discussed some ways in which our approach differs from many traditional sociological interests. In particular, we have adopted the notion of an anthropological study of science to denote the particular sense of our conception of the social. We are not concerned with a sociological analysis in the functionalist tradition which tries to specify norms governing scientists' behaviour. At the same time, we want to avoid a perspective which implicitly adopts a distinction between "social" and "technical" issues, however closely related these might be said to be. The use of such a distinction can be dangerous either because it fails critically to examine the substance of technical issues or because the effects of the social are only apparent in the more obvious instances of external disruption. More significantly, the use of this distinction fails to examine its importance as a resource for scientific activity. In addition, our collection of observations within the setting has led us to a kind of research primarily concerned with the details of scientific activity rather than with all-encompassing historical description. Our discussion concerns the social construction of scientific facts, with the proviso that we use "social" in a special sense which will become clear in the course of our argument. Obviously, we want to avoid the simplistic imposition of concepts in our attempts to make sense of our observations of science. For example, our concern with the "social" is not confined to those nontechnical observations amenable to the application of sociological concepts such as norms or competition. Instead, we regard the process of construction of sense implied by the application of sociological concepts as highly significant for our own approach. It is this process of construction of sense which forms the focus of our discussion. As a working definition, therefore, it could be said that we are concerned with the *social* construction of scientific knowledge in so far as this draws attention to the *process* by which scientists make sense of their observations.

Let us recap by using an example to illustrate what we mean by the process of making sense in the social construction of science. Sometime in late 1967, Jocelyn Bell, a research student at Cambridge radio astronomy laboratories, noted the persistent appearance of a strange section of "scruff on the recorded output from apparatus designed to produce a sky survey of quasars. This statement is itself a highly condensed version of an account gleamed from a variety of

sources, including discussions with Bell (Woolgar, 1976a). Sociologists of different persuasions and research styles would undoubtedly view this episode in a variety of different ways. Those primarily interested in norms, for example, might enquire how the communication of news of this finding was handled in the light of prevailing competitive pressures. To what extent did scientists live up to, or evade, norms of universality? Such an approach would leave intact the activity involved in Bell's perception. A more sophisticated approach might enquire as to the social circumstances prevalent at the time. What were the constraints in terms of availability of equipment which made Bell's observation appear remarkable? What were the characteristics of the organisation of radio astronomy at that stage of its development that gave Bell's observation a special significance? This approach would be more sophisticated in the sense that factors such as the organisation of research at Cambridge and participants' experience of past disputes would be examined for their influence on the observation and its subsequent interpretation. Given a different state of affairs, it could be argued, the observation would have been interpreted differently or might not have occurred at all.

In this particular example, it might be argued that if scrutiny of the recording had been automated or if Bell had been sufficiently socialised into realising that the persistent recurrence of scruff was impossible and hence nonnoticeable, the discovery of pulsars would have been much longer in coming. Technical events, such as Bell's observations, are thus much more than mere psychological operations; the very act of perception is constituted by prevalent social forces. Our interest, however, would be in the details of the observation process. In particular, we should like to know the method by which Bell made sense of a series of figures such that she could produce the account: "There was a recurrence of a bit of scruff." The processes which inform the initial perception can be dealt with psychologically. However, our interest would be with the use of socially available procedures for constructing an ordered account out of the apparent chaos of available perceptions.

The Construction of Order

Our interest in the way in which scientific order is constructed out of chaos arises from two main considerations. Firstly, from the fact that there are always available a number of alternative sociological