

Perhaps, the paper by professors Collins and Evans (C&E) might be most plausibly regarded as a kind of diplomatic message belonging to the sphere of inter-disciplinary relations. After three decades of constant **encroachments** on the natural sciences' legitimacy, two chiefs of the warlike academic tribe of students of science and technology offer the victims of their raids a peace treaty. The cultural authority of scientific experts – the source of their economic prosperity and organizational autonomy - is generally recognized, albeit in a somewhat diminished form. The circles of still-too-realist scientists are assured that nothing threatens the superiority of their judgment (they are occasionally even told that – in total agreement with what they used to believe – “truth [is] grounded in scientific procedures” – p.250). Moreover, the chiefs even hint that the current level of participation of the lay public in esoteric scientific affairs might be too high, and the scientists' voices sometimes not given due weight. These concessions go at a price, however – the tribesmen (and tribeswomen) reserve for themselves the right to decide in each particular case, if something belongs to the sphere of expertise of the scientists. The latter's cultural authority is contingent on the former's approval, recognition of the science's expertise is traded for recognition of the STS's meta-expert authority (one might think here of Vikings settling at the territories formerly ravaged by their expeditions and usurping power from the local elites).¹

On the other hand, the chiefs have to calm down the possible opposition from the rows of their more conservative fellow-tribespersons who are not ready to give away traditional belligerent attitude. Some of the members of the STS movement might have consider this compromise as betrayal of its ideals; the reactions to C&E articles which followed in the next issues of SSS (e.g. by Brian Wynne) demonstrated that they were in fact actually perceived in this way. The authors attempted to pacify this part of their audience by repeatedly **reminding** that the major achievements of the Wave Two remain intact, and what the new political course adds is just a practical implementation of the familiar ideals, rather than a retreat.

There was only one side which was offered nothing under the conditions of the treaty – the so-called “Wave One” of science studies. C&E insist that the old, ‘Mertonian’, sociology of science is dead (“crashed on to the shore”, “intellectually bankrupt”, etc.), and totally replaced by the superior “Wave Two”. However, the relations between “waves” seem much more like relations between Kuhnian paradigms, than between successive Lacatosian programmes. “Wave One” asked questions which “Wave Two” did not, and vice **verse**. From the point of view of each of them the other was largely a failure. “Wave One” was essentially a sociology of academic institutions. The type of problems it tried to solved was finding general correlation between, on the one hand, patterns of social organization of intellectual production and, on the other hand, form and volume of scholarly achievements. Under what institutional and cultural conditions the modern science emerges (“the Merton thesis”)? What kind of institutional arrangements guarantee production of discoveries later universally recognized as major achievements (much of Ban-David's work)? How disciplines are internally organized, and what kinds of work different segments of academic professions are likely to produce (e.g. Bourdieu, Whitley)?

It might be true that representatives of the “Wave One” tended to accept uncritically the then-conventional definition of the explanandum – the basic understanding of what science is.² It

¹ This re-conciliating tendencies are even more pronounced in 2007 “Rethinking Expertise”, which starts with an epigraph from Ecclesiastes saying: “there is time to break down and time to build up”

² Although their tendency to do so should not be overestimated. Some of the “Wave One” findings turn out surprisingly similar to what “Wave Two” claims as its major achievements. Thus, a piece of research seemingly quite relevant to the matters C&E are discussing may be found in Ben-David's 1960 paper on ‘Scientific Productivity and Academic Organization in Nineteenth Century Medicine’ which demonstrates that scientific revolutions are usually produced by marginal figures staying somewhere in between of C&E's “core sets” and practitioners with wide non-academic **clientele** and experience. Excessive organizational autonomy of the “core sets” tends to result in sterile “pure science” which is eventually overthrown by non-academic experts appealing to

is definitely true that “Wave Two” offered new insight into the nature of scientific knowledge and expertise. The fact remains still that it gave no answers to the questions “Wave One” asked. “Wave Two” focus on historical/ethnographic case studies listing various irremediably external sources of ‘scientific truth’, from contingencies of laboratory practice to the prevailing ideas of gentlemanly conduct. Still, it rarely attempted to end this idiographic work with nomothetic statements, and the implicit theory of academic institutions it relied on was quite simplistic, at the very best.³ Although this is definitely a gross overstatement, it seems that STS experts generally paid much more attention to celebrating cases of corruptibility of scientists’ judgments, than to systematically theorizing about relations between types of biases and types of social organization. In a way, it greatly diminishes possibilities of STS a ‘knowledge science’. “Wave Two” focused on external legitimacy of the sciences vis-à-vis truth claims by other social groups. “Wave One’s” ambition was largely to establish social conditions of internal legitimacy of truth claims made by different groups of scientists. While “Wave Two” was interested in ‘external’ influences, it was implicitly defined by STS scholars as arising from the alliances between groups of scholars and various groups outside of the Academy (London eugenicists, French farmers, Edinburgh philanthropists, etc.) The systematic role of academic institutions as such in generating certain intellectual biases – the central theme of “Wave One” - is usually not recognized by the STS experts. (Returning to our Viking metaphors, the former raiders offer protection to their subjects and otherwise regulate their relations with neighboring communities, but do not attempt to judge their internal disputes, as a stationary bandit should, according to Olson).

One of the reasons academic institutions fall into blank spot of the STS was that students of scientific knowledge preferred to study careers of theories and discoveries, rather than careers of categories of academics or individuals. While that produced a number of exciting case-studies in social history of ideas, it also gave a somewhat distorted and, ironically, idealistic picture of what sciences are like. In a nutshell, they define any kind of controversies in sciences as essentially intellectual and associate success in them with having one’s definition of reality prevail, albeit sometimes by mobilization of political and other ‘external’ resources. Evidence of huge academic power by groupings, having no intellectual message at all, seems to escape their attention altogether. Similarly, the accumulation of ‘credit’ was associated by Latour and his followers with having one’s knowledge claims prevail, rather than with successful achievement of symbols of professional recognition. That was implicitly assumed that the former is both a necessary and a sufficient condition of the latter. It becomes obvious that this is not so as soon as we approach social (and most other ‘historical’) sciences, in which professional recognition is sometimes fairly consolidated in absence of any substantive consensus. The list of ‘most important contemporary British sociologists’ emerging from a BSA poll might turn out to be much more uniform, than a list of ‘the most important recent discoveries by British sociologists’. In fact, the latter will probably demonstrate total dissensus.

My paper is an attempt to describe a type of controversies which have so far escaped attention of STS authors. These controversies arise from gross disagreement among groups of scholars in ‘historical’ disciplines on distribution of what C&E call meta-expertise – an ability and willingness to identify expert powers in others. Such controversies become vigorous when conventional credentials are concerned. As C&S argue, that is through credentials that societal discriminatory abilities are organized. Anyone outside of “core set’ has to rely on signals coming from inside of it to find out, if a person’s opinion is likely to contain the best scientific

the sentiments and sense of the general public. Ben-David’s treatment of the Pasteur’s case is sometimes strikingly similar by Latour’s which followed 25 years later. Similar arguments about alleged “realism” of Merton’s work can be found in Gieryn (1982). “Wave One” could quite happily co-exist with philosophical relativism, and sometimes did so. One can but wonder, if the treatment it receives in “Wave Two” and “Wave Three” writings is not a rhetoric strategy adopted by a (mostly British) cohesive “theory group” to do away with (mostly US) predecessors.

³ Thus, C&E light-heartedly assume that scientists tend to avoid bringing their conflicts into public domain to save the face of their discipline (p. 264). Enormous cohesive work necessary to maintain such dramaturgical discipline is treated by them as a trivial achievement, which it, of course, is not.

judgment available.⁴ The emergence of credentials does not occur by itself, however. It is a result of following various costly and time-consuming procedures in which participation of many actors is required. The type of controversies I have in mind occurs with different academic groups dispute each others abilities to do this work properly. One the other hand, existence of such signaling institutions might consolidate meta-expert authority even in absence of any consensus on expert authority as such. The most interesting thing is that sociology degree from a world leading department tend to be recognized as a binding status symbol even although vast majority of sociologists would dispute any value the PhD research twchich they were awarded for might have. In my article I analyze the case of controversy dividing sociology (as well as most other social sciences) in Russia now. The most interesting applications of this study, however, in that it sheds some light on a set of sufficient conditions of successful consolidation of meta-expert authority in 'historical' disciplines.

⁴ There are great differences in socially distributed abilities to recognize varieties in 'knowledge status' symbols. Most people in Western societies would recognize Nobel prize or Harvard professorship, much less – hierarchy of journals in each particular field, presumably attesting quality of argumentation produced by author.