SCIENCE IN THE SYSTEM OF WORLD SOCIETY[1]

I

Modern society can only be described as one worldwide societal system.[2] There are national states, of course. But even they, as often has been argued,[3] are only subsystems of one "world polity" which means a system of interacting states closely shaping the features of individual states and intruding on historical identities and cultural differences. And politics, furthermore, is only one among the numerous *function systems* of world society. Research and theorizing on world society should therefore primarily focus on these function systems and their processes of globalization. If you look at the literature, however, it is easily to be seen that there is much general theorizing on "world-systems" and "globalization" but not many specific studies on more circumscribed function systems have been published yet.[4]

The same diagnosis seems to be generally true for the sociology of science. There exists, on the one hand, an extensive and very interesting literature on the internationalization of research and development in multinational corporations. But in this research tradition the main interest is focussed on technology and its role in the internationalization of economic systems. In sociology of science itself very few studies concentrate on the globalization of scientific research and communication.[5] Only Thomas Schott of the University of Pittsburgh has made this subject somehow his specialty. In an essay from 1991 on "The World Scientific Community" he seems to opt for a Mertonian perspective, defining the world scientific community as a community which "comprises all scientists who participate in the more general norms of scientific work".[6] Two years later his point of view is nearer to that of John W. Meyer. "World Science" is related to a worldwide "science policy regime" which seems to be a specification of Meyer's concept of a "world polity".[7] The worldwide homogenization of the institutional structures of science is then explained by this science policy regime. Two further analytical foci are to be identified in Schott's essays. First, he is interested in describing science and technology regimes in terms of centres and peripheries,[8] thereby relating his studies to Edward Shils and Joseph Ben-David on the one hand

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1Published in Social Science Information 35, 1996, 327-340.
2Luhmann 1971; Robertson 1992; Stichweh 1995.
3See esp. Meyer 1987.
4A good recent exception is Meyer et al. 1992.
5See for an overview Ancarani 1995 and the collection of essays Crawford/Shinn/Sörlin 1993.
6Schott 1991, 442.
7Schott 1993, esp. 197-8.
8Cf. Schott 1994; id. 1994a.

and to the world-system theories of Immanuel Wallerstein on the other hand.[9] Second, he tries to identify the structures of the emerging system of world science by observing structures of collaboration among scientists from different countries, thereby making use of the slowly growing stream of studies on collaboration and coauthorship originating in the field of scientometrics.[10] In terms of sociological theory and method this is akin to the approach inaugurated by network theory and structural analysis.[11]

II

What seems to be absent from the sociological literature on science in the system of world society is an approach which allows to explain the dynamics of the process of globalization of science. What are the driving forces of this process of astonishing rapidity? It would be far too simple and tautological to point to the *universality of science* as a self-realizing grammar of science reproducing itself in a plurality of particularistic and heterogeneous cultural systems. Furthermore such an explanation would not recognize the basic *paradox* of the development of science which seems to recur in other function systems (e.g. the legal system), by the way: The path to modern global und universal science leads via an intermediate phase of strong *nationalization* of science.[12]

Early modern science (16.-18. century) was universal science, and science with a specific claim to universality (Newton's *Principia* in contradistinction to his *Optics*) was often communicated in an international medium of communication: Latin. But beginning in the eighteenth century and parallel to the internal differentiation of natural philosophy a strong nationalization of science arose. In nineteenth century Europe the place of the *res publica literaria* of early modern Europe[13] was assumed by *national scientific communities*. The strange thing to be explained is that the genesis of the national scientific community went along with the genesis of the scientific discipline, i.e. with a progressive internal differentiation of science.[14] How is this possible that a shrinking of the reference group for scientific communications is accompanied by a progressive subdivision of the specialized subsystems of scientific communication. My suggestion is that the nationalization of reference groups is compensated for some time by the *inclusion* of new units into the possibilities of participation in scientific communication. The units included are *organizations*, *roles* and *persons*. This may be seen in early 19th-century Germany as an example which is a pertinent choice as it is in Germany that the institutional infrastructures of the modern system of science are first realized.[15] There the organizations included are primarily universities. Universities are now strictly and exclusively described as scientific institutions, and that means that

[9] Wallerstein 1991.
[10] See among others Leclerc/Gagné 1994; Luukkonen et al. 1993; Narin/Stevens/Whitlow 1991.
[11] Cf. Wellman/Berkowitz 1988.
[12] See for two case studies on two different function systems: Stichweh 1984; idem 1990 and cf. Crawford/Shinn/Sörlin 1993a.
[13] Cf. Stichweh 1991, Ch. 6.
[14] Cf. for the following argument Stichweh 1984.
[15] Cf. Stichweh 1984, esp. Ch. 1.

any communicative act in universities is a way of participation in science.[16] This implies that the role of university teacher is a scientific role and this understanding was even extended on the role of school teacher. For teachers at Gymnasia in 19th-century Germany scientific publication was an administrative expectation and a relevant precondition for school careers. Finally, many persons were temporarily included in science. The naturalist with an amateur interest in science is enabled by the possibility of publication in the national language and by the easy accessibility of new and often specialized journals. This last one was a short time effect, of course.

The conclusion is that the nationalization of reference groups is made compatible with a progressive internal differentiation of science by a growth of scientific communication due to inclusion effects into national scientific communities. The theoretical concept of inclusion should perhaps be supplemented by the concept of *penetration* which comes from modernization theory.[17] Penetration means that global structures are more closely connected to local situations and are thereby intensified in their social effects. The nationalization of scientific communication seems to be important for the interpenetration of science and other social systems, and therefore the increased technological relevance of scientific research which in Germany becomes visible since the 1870s may be one of the side effects of this *coevolution* of national scientific communities and their internal differentiation into disciplinary communities.

The genesis of national scientific communities did not go along with science simply becoming parochial. If you look at the national disciplinary journals arising in Germany in the first half of the 19th century you see that they are *translation journals* to a considerable amount. That means there was some closing off of national communication circles. But nonetheless national disciplinary communities eagerly observed their foreign scientific environments and it is impossible to overlook the considerable effect of the endless row of Faraday translations in Poggendorff's Annalen on German electrical science. Even in nationalized science *informational openness* was obvious although it was inextricably intermingled with *informational closure* which may be seen by Poggendorff not only *translating* Faraday's papers but even stripping Faraday's natural philosophy from them.

III

What are the mechanisms leading from the national scientific communities of 19th century Europe to modern science as one global function system in world society? In asking this question one essential distinction should be taken into account. There exist at least two core meanings of the concept of globalization: *global diffusion* and *global interconnectedness*. Most definitions of globalization are not sufficiently precise

[16] If you look at the presumed "unity of teaching and research" even the student role is defined as a scientific role. See Stichweh 1994, Ch. 10.

[17] Cf. for the current use of "penetration" (sometimes "interpenetration") Luhmann 1984, 290; Meyer 1989, 403ff; Giddens 1990, 19.

in distinguishing these two aspects,[18] and you can even classify theories of world society by making use of this distinction: John W. Meyer's version being more on the side of global diffusion,[19] Niklas Luhmann, Immanuel Wallerstein and Anthony Giddens being primarily interested in global interconnectedness.[20] If you now look at the genesis of national scientific communities in the 19th century you can see that the structural change brought about by them is not simply a retrogression from a former state of internationalized science. The national scientific community in itself seems to be a potent stimulus to the global diffusion of science and its core institutions. This argument is valid for the national state itself[21] and can be repeated for many of its aspects which are specific to emerging function systems of modern society. The mechanism always seems to be the same: social inventions made in contexts which define and restrict themselves as national contexts are perceived by observers foreign to these national contexts and then - if they appear successful - they try to imitate them.

I will not present a comprehensive argument here on this interrelation of national systems and the global diffusion of the institutions of science. But it may be recorded that the close functional association of higher education and science which develops first in Germany and then in other national systems is one central aspect of this interrelation. Higher education is in many cases seen as a stronghold of "national culture" and as the final stage in a system of "national education". On the other hand it is a specific type of higher educational institution - the European university – which since the Spanish conquests of the sixteenth century is diffused worldwide. And that means that there is always in modern society a considerable probability of the teaching roles in institutions of higher educations being redefined in a sense closely related to the ideals of scientific research. The university as one of the most successful European inventions is a strong independent support of the emergence of globalized science.

But what about global interconnectedness? Perhaps one should return to a basic problem stated in part II of my argument. How can we explain the coincidence of the genesis of national scientific communities and the progressive disciplinary differentiation of science? In part II I sketched an answer which was based on the theoretical concepts of inclusion and penetration (i.e. which postulated a kind of intensive growth of science in an artificially restricted communicative space). But one further point should be raised. If the dynamics of the genesis of national scientific communities leads to the worldwide diffusion of the institutions of science - how do we have to locate the dynamics of disciplinary differentiation?

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[18]See two recent definitions: Worthington 1993, 178: "the scope of social relations in any area of human endeavor is global when most people in most places are affected by them at least some of the time." - Schott 1994, 28: "Most commonly ... globalization denotes a process of increasing density of long-distance interaction."

[19]See as an interesting essay Strang/Meyer 1993.

[20]You may furthermore distinguish *lateral connectedness* (across spatial distances in a network structure) from *vertical connectedness* (across system levels and system boundaries). Penetration or interpenetration is conceptually equivalent to vertical connectedness. Even the Polanyi-Granovetter concept of *embeddedness* (see Granovetter 1985) is obviously one more synonym for penetration.

[21]Cf. Stichweh 1994a.

My suggestion is that the dynamics of the internal differentiation of science, i.e. the sequence of disciplinary differentiation, subdisciplinary differentiation, subsubdisciplinary differentiation, is the most important cause of the globalization of science in its sense of global interconnectedness. That is the global interconnectedness of science is not the result of one worldwide community of scientists with a shared set of normative and cognitive presuppositions emerging.[22] Instead it is the incessant proliferation of ever new communities of scientists with progressively restricted jurisdictions[23] which organizes the social and cognitive space of science in a way[24] which is incompatible with the boundaries of national scientific communities. The decomposition of the problem space of science makes it progressively improbable that relevant and necessary collegial relationships should accidentally be coextensive with national contexts.

Only an argument of the type just outlined allows to establish the explanative and descriptive relevance of international collaboration and coauthorship as an indication of globalization.[25] To see this more precisely we must introduce a further distinction which correlates with the system/environment distinction. You have to distinguish *collegial affinity* which motivates collaboration by a near identity of problem formulation from *collegial complementarity* which demands collaboration due to the insufficiency of the cognitive resources of each singular researcher. Collegial affinity is a relation internal to a scientific communication system, collegial complementarity involves a plurality of systems which are environments to one another. The multiplicity of system/environment-distinctions in modern science slowly devaluates the informational parsimony in restricting collaborative relations to the national scene.

But what becomes of the national scientific community? It would be superficial to say that it is a nonexistent entity in modern science. There is an interesting parallel in the discussion on *national innovation systems*.[26] In both cases globalization is the conceptual challenge. In both cases the answer seems to be that national systems can only be identified if you concentrate on the interface of the respective function system and the political system of a national state.[27] Then a national scientific community would primarily function as a policy community and its social relevance even has been somehow enlarged in recent times because of the enormous growth of the dependence of scientific research on state finance since World War II.[28]

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[22]In a slightly romantic vein Lewis Thomas (1984, 966-7) still postulates this: "there is in being a worldwide community ... of working scientists who do their work together, across oceans and national borders, without any awareness of national or ethnic or social identities. They make up, in the aggregate, the largest and most cohesive of underground movements to be found anywhere on the globe; subversive in the literal meaning of that word, which is to turn things upside down." Thomas Schott (1993, 205) diverges from this position, "the global community of all scientists actually consists of a global Kuhnian community for each discipline, which can be contrasted to one another," without ever really drawing the consequences from his insight.
[23]An interesting use of the concept of jurisdiction is to be found in Abbott 1988, esp. 65ff.
[24]See as a still suggestive essay Campbell 1969.
[25]Cf. Fn. 9.
[26]Cf. Nelson 1993.
[27]See the definition cited in Niosi/Bellon 1994, 175: "A national system of innovation is the system of interacting private and public firms (either large or small), universities and government agencies, aiming at the production of science and technology within national borders."
[28]See for some remarks Stichweh 1994, Ch. 6.

IV

As Niklas Luhmann notes in a paper on universities as organizations there are two new mechanisms on which communications of societal relevance seem to be concentrated in modern societies: formal organization and telecommunication.[29] Therefore we should try to find out how these two mechanisms are related to the globalization of science.

There exists one obvious difference between the economy and the social system of science in the way these function systems make use of organizations in their respective processes of globalization. In the economy the *multinational corporation* is one dominant mechanism of globalization. Especially the international transfer of technology is a phenomenon largely internal to multinational corporations."[30] This internalization of the exploitation of technology by multinational corporations is seen by some theorists as the most important driving force behind the genesis of the multinational corporation.[31] And it is remarkable that since 1980 the growth rates of international patent applications are much higher than the growth rates of national patent applications which means that the international *exploitation of knowledge* by multinational corporations is a somehow more prominent feature of the present situation than the *growth of knowledge* in terms of national patent applications.[32]

The situation is completely different in the social system of science. Nearly all the important organizations of science (universities, research organizations as the *National Institutes of Health*, *Centre National de Recherche Scientifique*, *Max-Planck-Gesellschaft,* academies of science) are - with the occasional exception of a foreign affiliate - strictly national in their organizational reach. Even if you look at *Research & Development* in multinational corporations you will observe an interesting contrast: there is on the one hand the global exploitation of technological knowledge by multinational corporations, on the other hand the same multinational corporations still concentrate their R&D in their country of origin (90% for US corporations, 98% for Japanese corporations) and their foreign R&D facilities are often primarily occupied with technology adaptation, national patent or drug applications, evaluation of foreign technologies they intend to acquire etc.[33]

This persisting national basis of the most important organizations of scientific research however does not prevent the astonishing growth of international scientific

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[29]Luhmann 1987, 208. Cf. Luhmann 1971, 54, on organizational membership as the condition of access to worldwide contacts.
[30]See Wortmann 1990, 175, Fn. 3: "In the F.R.G. today ... about 80% of the expenditure for imports of patents and licences is paid for by subsidiaries of foreign companies." Cf. Senghaas 1994, 204.
[31]See Scaperlanda 1993, 608; Kogut/Zander 1993.
[32]See Archibugi/Michie 1995, esp. 123, 127.
[33]Freeman 1995, 17; Niosi/Bellon 1994, 183; Archibugi/Michie 1995, esp. the table on 137; Serapio/Dalton 1993; for an interesting case study Malnight 1995.

collaboration. In most of the countries which are important for scientific research today more than 20% of all coauthored papers document an international collaborative relationship. There are some exceptions - the U.S. with the most extensive internal market for scientific collaboration (13%), and Japan being still somehow isolated for linguistic reasons (10%) - but if you sum over 131 nations contributing authors to scientific papers the share of international collaboration rose from 11,3% in 1980 to 20% in 1990.[34] There is an even bigger growth in absolute numbers because of the rising share of coauthored papers on the total number of scientific papers. And finally there seems to exist a reputational advantage for internationally coauthored papers. This is especially true for Europe as papers with adresses from at least two European countries receive (worldwide) more as twice as much citations as papers with a single institutional adress.[35]

How is this growth of international collaboration possible in view of the national organizational basis of science. A first precondition is that many organizations of science try to institutionalize a representative sample of scientific disciplines and subdisciplines instead of specializing on some fields in which they are really strong. This is true for universities and academic departments but even for big scientific organizations as the Max-Planck-Gesellschaft. By choosing this principle of institution building scientific organizations refer scientific collaboration to the outside of the organization[36] as even collegial complementarity becomes improbable if the primary organizational goal is to represent a catalogue of academic specialisms. Or in a different formulation: if specialization is the driving force in the process of globalization of science as I tried to demonstrate above (part III) the absence of specialized organizations should imply that formal organization can not be the core mechanism of globalization in science.

Under these premises the scientific organization has to accept the autonomy of the researcher in the choice of his collaborative projects. This way arises a constellation which is rather strange if you compare scientific organizations to state bureaucracies or industrial organizations. The scientific organization has nearly no control over the external ties of the organization members. Describing this situation Kreiner and Schultz speak of a *personalized collaboration*.[37] And they point to a certain amount of 'anarchy' and 'licence' being introduced into the organization by this: "The picture is one in which individual researchers informally appropriate organizational resources and divert these into unauthorized projects and relationships."[38]

But how do organization members conduct these collaborative projects, especially if they collaborate with foreign colleagues? Which are the organizational resources they appropriate? At this point it nearly suggests itself to look to media of communication, especially to telecommunicative media. If it is not organizational membership which functions as principle of worldwide connectivity, telecommunicative ties might

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[34]Leclerc/Gagné 1994, 267ff.; cf. Frame/Narin 1988, 208.
[35]Narin/Stevens/Whitlow 1991, 320-2.
[36]Cf. for a British case study Becher 1981, 118.
[37]Kreiner/Schultz 1993, 206, Fn. 1: "we are referring to the fact that researchers enact collaboration on their own, and not on a mandate from the university or the company."
[38]Kreiner/Schultz 1993, 204.

substitute for it. Therefore one should look to media of publication (e.g.: you prefer reading a journal article and not asking for the opinion of the colleague next door) as well as to telecommunicative media such as mail, telephone, fax and e-mail. In this respect one may once more note the well-known fact that the medium of e-mail was in economic organizations established in the form of (inner-)*organizational networks* and in science in the form of one *worldwide net* (internet). But I will not in this essay resume the rich literature on the effects of new telecommunicative media on the communication structure of science.[39] Instead I will only point to one well-established fact regarding the interrelationship of scientific organizations and telecommunicative media.

It is easily to be seen that the availability of computer mediated communication (e-mail, file transfer, bulletin boards, online publication) improves the possibilities of long-distance collaboration in science. This is supported by other telecommunicative media (cheaper long-distance calls, fax machines). The differentiation of science in centres and peripheries is weakened by these developments. That means they affect one dimension of globalization of science which has not been mentioned yet. Globalization also entails (besides global diffusion and global interconnectedness) *decentralization* in function systems.
But there is one finding regarding the limitations of telecommunication which recurs in many studies on telecommunicative media: You can *continue* a scientific collaboration via telecommunicative media for some time. But for *initiating* a collaborative project it seems to be necessary for the participants to be proximal to one another for a certain time. And even scientists who know each other well from previous collaborations experience serious difficulties in starting a new collaborative project if there is no possibility of interactional co-presence. Finally, the progress of telecommunicative projects is obviously slower than it would be the case in a localized project.[40]

You may explain this by the *uncertainty* of scientific research cumulating in the situation of problem choice and thereby demanding interactional copresence, and this because of the media richness of interaction systems (verbal and nonverbal communication, cooperative notations on a blackboard etc.).[41] This subject demands much further exploration. Instead I will only mention one final point which returns to the structure of formal organizations. If organizations can not mandate or even control the network of global collaborative links and if these global collaborative links influence the demand for the temporary presence of foreign researchers in a research organization, membership as a boundary criterion for the scientific organization becomes rather unprecise. Organizations can select those members which fill the permanent occupational roles. But they do not really control the steady flow of shorttime visitors, guests, collaborators etc. which represent in terms of mobility of personnel the fluidity of social and cognitive links in globalized science.

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[39]See for one preliminary discussion Stichweh 1989.
[40]See Carley/Wendt 1991; Stichweh 1989; Hoke 1994; Howells 1995.
[41]See Howells 1995, 176.

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