

# Contradictions of Commercialization: Revealing the Norms of Science?

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The proponents of the entrepreneurial university have claimed that it implies adjustments in the normative structure of science. In this article, I will critically examine whether a qualitatively new kind of academic ethos can emerge from the commercialization of academic research. The traditional conception of norms of science as institutionalized imperatives is distinguished from the constructivist conception of norms as strategic or ideological resources. An empirical case study on the commercialization of the research of one academic language-technology group is presented. The case study does not support the constructivist conclusion that the norms of science are malleable at will.

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**1. Introduction.** In the last 2 decades, universities in the Western world have become increasingly commercialized. This has involved, among other things, more funding gained from the private sector, ownership and management of intellectual property, establishment of technology transfer offices in campuses, as well as a growing number of university spin-off companies. Side by side with the commercialization of academic research a new body of science policy-oriented literature has emerged that targets the ongoing transformation of the university system. This transformation—which is already assumed to be underway—is conceptualized in terms such as “mode 2” knowledge production (Gibbons et al. 1994; Nowotny, Scott, and Gibbons 2001), “triple helix” of university-industry-government relations and “entrepreneurial university” (Etzkowitz and Leydesdorff 2000; Etzkowitz 2003), “academic capitalism” (Slaughter and Leslie 1997), and “postacademic science” (Ziman 1994, 2000). Although these studies differ from one another as regards their programmatic character and empirical grounding, as well as their prescriptive-cum-descriptive nature, they all address the need for more societally responsive science. However, this generally accepted requirement

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of more socially relevant scientific knowledge has more often than not boiled down to the commercialization of university research.

While there is already ample evidence on how commercialization can threaten the quality and integrity of scientific research (e.g., Krimsky 2003), this has not really bothered the proponents of the mode 2 thesis and the entrepreneurial university as they envisage a new mode of scientific research that also covers an accompanying change in the normative structure of science. The mode 2 theorists claim that due to its contextual problem orientation and new forms of organization, mode 2 knowledge production introduces novel forms of quality control and academic dissemination (e.g., Gibbons et al. 1994, 31–34; Etzkowitz 2011, 552). The ardent and visible spokesman for the entrepreneurial university, Henry Etzkowitz, even envisages a rise of “an entrepreneurial academic ethos” as “an increasing number of scientists and research organizations have sought simultaneously to advance and capitalize on knowledge, calling their full adherence to the Mertonian norms of communality and disinterestedness into question” (553). One particularly strategic site, according to Etzkowitz, for the emergence of this new ethos is a university spin-off firm, which “hybridizes” entrepreneurship with academic research (Etzkowitz 2003).

In what follows, I will critically examine whether and on what grounds a qualitatively new kind of academic ethos can be seen to emerge from the commercialization of academic research. My focus is on the nature of the social norms guiding scientific practice.<sup>1</sup> First, I will distinguish the traditional conception of norms as institutionalized imperatives from the constructivist conception of norms as strategic or ideological resources that can selectively be used to further various kinds of aims. Second, I will present the case study of a language-technology research group, whose members sought to commercialize academic research through spin-off companies. Although the group was successful in both academic and commercial terms, its simultaneous engagement in academic and commercial activities engendered various normative conflicts, which eventually led to the separation of the two activities. The case of the language-technology group does not support the constructivist conclusion that the norms of science are malleable at will; instead, such traditional Mertonian norms as communism, disinterestedness, and originality were found to be operative in this case.

**2. Mertonian Norms as Institutionalized Imperatives.** That Etzkowitz invokes a Mertonian ethos in his propagation of a normative change in science is by no means an accident. The most famous articulation of the normative structure of science was provided by the sociologist of science Robert K.

1. By “social norms,” I refer to customary, often unplanned rules that regulate and coordinate various social activities.

Merton (1942/1957).<sup>2</sup> As a functionalist sociologist, Merton was interested in how a social institution such as science is able to support its function. He proposed that science is characterized by a particular kind of cultural ethos that furthers the goal of science—the extension of certified knowledge. “Although the ethos of science is not codified, it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings and in moral indignation directed toward contraventions of the ethos” (Merton 1942/1957, 551–52). The four famous norms of science initially set forth by Merton were universalism, communism, disinterestedness, and organized skepticism. Later on, the norm of originality was added by Merton (1957), and the norms became known as the CUDOS norms, referring to the similar-sounding term “kudos,” which is derived from classical Greek and means fame or praise for an exceptional achievement.

Universalism requires that scientific claims are subjected to “*preestablished impersonal criteria*; consonant with observation and with previously confirmed knowledge.” Furthermore, the acceptance or rejection of claims should not depend “on the personal or social attributes of their protagonist; his race, nationality, religion, class and personal qualities are as such irrelevant” (Merton 1942/1957, 553). According to communism, scientific knowledge is a product of social collaboration, and consequently it should be collectively owned by the scientific community. The “intellectual property,” which Merton understood differently than how it is conceived of today, is limited to recognition and esteem. Secrecy is the antithesis of the norm of communism. Disinterestedness requires that scientists disengage their interests from their claims and judgments, which should not be mistaken for a lack of individual motivation. Finally, organized skepticism refers to the tendency of the scientific community to disbelieve claims until they have been well established. This norm requires the “suspension of judgement” and “detached scrutiny of beliefs in terms of empirical and logical criteria” (560).

What is important to note about Mertonian norms is their institutional nature. They describe less any properties or dispositions of individual scientists than the functioning of the scientific practice as a whole. Thus, Mertonian norms can be regarded as institutionalized imperatives guiding scientific practice. Moreover, the Mertonian norms link closely together the social and the cognitive aspects of science. One reason for their persistent significance has been precisely their close connection to the philosophical reconstruction of science as a rational, empirically grounded pursuit of knowledge. For instance, in philosophical parlance “disinterestedness” and “organized skepti-

2. Several philosophers, e.g., Helen Longino, Philip Kitcher, and David B. Resnik, have provided accounts of scientific norms that are also sensitive to scientific practice. I will concentrate on Merton because, to date, his account has served as a steady reference point in the discussions on the normative structure of science.

cism” can be regarded as belonging predominantly to the context of justification (e.g., Radder 2010, 234). “Disinterestedness” can be associated with the goal of objectivity, and “organized skepticism,” to the requirement that scientific claims need to be fully tested and justified before accepted as knowledge. Thus, it is not surprising that the attacks against Mertonian values have often been combined, especially in the science and technology studies tradition, with the critique of the philosophy of science.<sup>3</sup>

Since its original formulation, Merton’s scientific “ethos” has been an object of intensive discussion, and especially since the 1970s this discussion has become increasingly critical. Mertonian norms have been criticized for their historical inaccuracy or ahistoricity or their not being peculiar to science (see Stehr [1978] for an overview). Any demarcation issues, or “boundary work” between science and other social practices, do not concern me here. The critics claiming that Mertonian norms do not stand historical scrutiny (see, e.g., Barnes and Dolby 1970) certainly have a case, but at least from the philosophical perspective one should take into account the different senses of the term “norm.” On the one hand, the term “norm” refers to the “normal” behavior of a group, and, on the other hand, a “norm” can be considered as an ideal standard for a certain kind of behavior (Resnik 2010; see also Etzkowitz 2011). These two senses of a norm were clearly present in Merton’s work at the very outset. Merton thought that the norms he formulated provide science an “institutional context for the fullest measure of development” (1942/1957, 552).

**3. Normative Ambivalence and Norms as Ideological Resources.** A highly relevant question in regard to the normative change of science concerns the other kinds of norms that scientists might follow simultaneously. Merton himself acknowledged the possibility of the dynamic alternation of norms and counter-norms (Merton and Barber 1963). A well-known empirical study on counter-norms was conducted by Ian Mitroff (1974) on the practices of Apollo moon scientists during the late 1960s. According to Mitroff, the scientists in question not only committed themselves to the four Mertonian norms, but they also followed a set of counter-norms, which were roughly the opposites of the Mertonian norms. Mertonian universalism was supplemented by particularism, communism by solitariness, disinterestedness by interestedness, and organized skepticism by organized dogmatism. One possible explanation Mitroff proposed for the normative ambivalence he found was that scientists tend to commit themselves to the Mertonian norms in cases of well-structured research problems, but they adhere to the counter-norms when problems are ill structured.

3. For example, “Merton’s position resembles that of those philosophers of science who attempt to describe a specific scientific method” (Barnes and Dolby 1970, 11).

Another more recent set of counter-norms, which takes into account the recent discussions of the mode 2 knowledge production and commercialization of science, has been suggested by John Ziman (1994, 2000). According to Ziman, the Mertonian norms provided “regulative principles” for science “as practiced in its heyday” (2000, 57). As a result of greater societal demands, academic science can no longer be defined by such norms only. Science in our age is being reconfigured, Ziman suggests, into a new form: “postacademic science.” This new kind of ethos is regulated by a set of norms that are the rough inverse of the Mertonian ones: proprietary, local, authoritarian, commissioned, and expert. Ziman refers to the ethos of post-academic science with the acronym PLACE and claims that proprietariness as opposed to communalism “is the cornerstone of any scientific career directed towards gaining an organizational ‘PLACE’ rather than personal ‘CUDOS’” (Ziman 1994, 182).

The alternation of norms and counter-norms paves the way for an idea that there actually is not, and never was, any unitary set of norms capable of defining scientific activity. This is the line of argument the adherents of the emerging sociology of scientific knowledge (later to be transformed into science and technology studies; i.e., STS) pursued already early on. On the basis of a short historical review of scientific practices, Barnes and Dolby made a distinction between *statistical norms*, which are “observable as a pattern of positively sanctioned activity,” and *professed norms* “directed mainly to out-groups” and “celebrated in a tract or speech” (1970, 8). According to Barnes and Dolby, statistical norms governing scientists are likely to be specific to particular “paradigm-sharing” communities and more technical in nature than the Mertonian norms that belong to rather the realm of professed norms. Thus, the Mertonian norms were converted from ideal standards guiding and controlling the behavior of scientists into an official ideology of science. An early and influential formulation of this idea can be found from Michael Mulkey (1976/1991).

Mulkey claimed that neither Merton nor Mitroff furnishes any evidence on how far norms or counter-norms were institutionalized, that is, “linked to the distribution of rewards” (1976/1991, 65). Although Mulkey was not altogether fair in his assessment of Merton, who in fact studied the reward systems of science in terms of priority disputes, the focus here will remain on Mulkey’s view of things (see also Radder 2010, 237). From Mulkey’s perspective, “the standardized verbal formulations which are used by participants” (1976/1991, 68) to describe and judge their own and their colleagues’ professional behavior should not be taken as evidence for the existence of some institutionalized norms that constrain the behavior of scientists. Instead, they are “vocabularies of justification” (77), which function as resources for practitioners as they describe, manage, legitimize, control, and question their own and others’ activities. For Mulkey as well as for many other STS scholars, Mertonian norms

present an overly idealized and historically situated image of science that has been used by scientists to justify the special status of science as an activity not to be interfered with from the outside. As such, these norms of science form an “occupational ideology” rather than any adequate description of how science works. It should be remembered, though, that this disagreement concerning the norms of science should also be contextualized inside the larger debate on the roles of norms in social theory. The kind of “discourse analysis” of Mulkay can be related to the ethnomethodological movement according to which norms should be considered not as something conformed to in action but rather as available resources for making sense of social actions. There is also a liberating aspect to this move, as social actors are supposed to retain their discretion and freedom of action.

As to the supposed normative transformation of science, it should not be too difficult to see how the conception of norms as resources for deliberative actions nicely suits the proponents of mode 2 knowledge production and entrepreneurial science. Commercialization need not threaten the basic values of academic research if norms are considered as discursive resources: various aspects of commercialization simply provide new items for the agenda of ongoing local “negotiations.” But is this really all there is to the normative ethos of science? And does a closer look at activities taking place at the grass-roots level necessarily impose on us the constructivist perspective of the contingent nature of facts, norms, boundaries, and so on? In what follows, I will take a cue from the ethnomethodologist Harold Garfinkel (1989), according to whom rules and norms are usually taken for granted by the actors involved and are recognized only when they are breached. Consequently, I assume that the contradictions involved in the commercialization of academic science can provide interesting material on the ethos of science because they can be expected to reveal the shared norms of science and the extent to which the scientific community is willing to renounce them.

**4. The Contradictions of Commercialization.** In addition to biotechnology, language technology has often been singled out as the branch that has advanced furthest in commercialization of academic research (e.g., Etzkowitz 1998). As an area of research, it displays many characteristics listed as properties of mode 2 knowledge production. Language technology is an interdisciplinary application-oriented field of study that gathers together researchers from both academia and the commercial world. In addition to new ideas, well-functioning tools are traded within this instrumentally oriented research community. The following discussion is based on an empirical study on a language-technology group that operated under the auspices of a comprehensive European public university. The group was very successful in both academic and commercial terms: it had done cutting-edge research in the field of language technology for 2 decades, simultaneously spinning

off three firms from out of its research. Two of the three spin-offs are still in business.<sup>4</sup>

*4.1. Research versus Selling.* The language-technology research group in question started commercializing its research from its infancy in the beginning of the 1980s. This development was largely a direct consequence of its research agenda. The empirically based approach of the group toward developing language-independent theories and tools bred important early applications, and the group started to get orders from large domestic companies and government offices, which needed new language-technology applications. These orders were managed through the department's administration and written in the form of formal research contracts. The resulting money and equipment were more than welcome in the department, which had, like many other small departments in the humanities, constant problems in gaining funding for its activities. Nonetheless, the department had some difficulty in fitting the extra income attained from the contracts within its yearly budget, as no clear procedures existed for chargeable service and research at the university in those days. Simultaneously, the professor who led both the group and the department began to think that the commercial orders they received had nearly nothing to do with scientific research. In his opinion, the personnel at the university were expected to do research rather than to engage in commercial activities: "It was sort of selling. Those contracts were not genuine research contracts in the sense that we would have needed to do research to execute what stood in them. In fact, we just sold programs that were already made here. . . . Of course, some configuring work was done."

In 1987, the professor and the principal researcher of the group (who later also became a professor in the same department) decided to establish a company into which all commercial activities were transferred. Consequently, the need to externalize mere "selling" to the newly established spin-off company was directly related to the fact that the actors in question considered that it was not the kind of activity that belongs in a university. However, the activities of the department and the company were still closely entwined as the company directly used the research results of the department; this practice led to various kinds of difficult problems in the longer run.

*4.2. Economic Rewards and Originality.* The company grew rather slowly in the beginning, relying on projects of diverse kinds for which it hired researchers from the department typically for a few months at a time.

4. The data used in this case are gleaned from documents covering more than 20 years, including research proposals and reports, publications, and external evaluations. Moreover, 24 interviews and numerous informal discussions were conducted between 2000 and 2009.

In the mid-1990s, the company started to grow faster, due to large contracts with a big international information-technology company, but as a result of this commercial success internal tensions started to grow within the group. One problem concerned the ownership of intellectual property that eventually escalated into a disagreement concerning academic priorities. These disagreements can be related to the differences in the ways in which the “ownership” is attributed in the university research and commercial activity. Whereas the origin or “ownership” of ideas in science has traditionally been indicated by credits given to colleagues, the more formally defined intellectual property rights indicate ownership in business. In business, property ownerships convey to the owner both the exclusionary right, that is, the right to exclude others from using his or her property, and the right to appropriate economic returns (Owen-Smith 2006). In contrast to property ownership, scientific articles do not have such exclusivity, and others can use your findings to further their own research. Scientists establish their intellectual rights and gain esteem by “communizing” them (e.g., Merton 1942/1957, 556–57).

In the case of the language-technology research group, the disagreements concerning the economic rewards were largely due to a generational shift. As the research program of the language-technology research group advanced, the group hired several young scientists, who licensed the programs they developed in the academic projects to the company. Soon the younger generation began to think that their contribution to the economic activities of the professors’ company should have been institutionalized in the form of shares of the company. Despite some preliminary negotiations, this did not happen, which created a poisoned atmosphere within the group. The younger generation attributed the reluctance of the professors to accept them as shareholders to the professors’ inability to recognize that, despite their pioneering work, the technology was no longer the fruit of their research only. Thus, the disagreement on the commercial benefits sparked a struggle within the group concerning academic priorities and credits. In their research articles, the younger generation singled out predecessors for the professors’ innovations, and, second, they delineated some other forerunners to their own work than that of their professors. In Merton’s terms, the younger generation questioned the assumed “originality” of the professors’ work in relationship to their own work.

*4.3. Secrecy.* Frustrated with their exclusion from the professors’ company, the younger generation eventually decided to set up a company of their own in 1997. This created a secretive atmosphere within the language-technology group, whose members belonged to two partly rival spin-off companies. In an interview, the head of the department described the resulting situation in the following way: “For some years already we have



had the problem that the whole truth has not been laid out on the table either in our internal discussions or in our publications. . . . It is a big ethical problem, indeed. How much can you hide—and still act as a credible academic researcher?”

A disagreement concerning one doctoral dissertation provides a telling example of the contradictions the researchers of the group encountered while simultaneously trying to fulfill the requirements of both academia and business. The doctoral candidate in question belonged to the group of younger researchers who had established their own company. He was accused of being intentionally vague in describing a new parser he had been developing that had already become part of the business of the younger researchers' company. The university grading committee made the following remark concerning the thesis: “[XX] . . . has on some important points, especially when it comes to algorithmic descriptions and design principles, refrained from the scientifically detailed descriptions that would have been desirable. This is contrary to the principle of openness that is central to science.” In an interview, the doctoral candidate in question explained that he had only acted in a way that had become a departmental convention.

*4.4. The Erosion of the Successful Research Program.* Apart from the problems concerning intellectual property rights, academic priorities, and a secretive atmosphere, difficulties were also brewing on other fronts. After a long period of academic and commercial success, the department received, in the late 1990s, a very disappointing research assessment evaluation. The evaluation report stated, “Given the high degree of excellence that the department achieved in the eighties and early nineties, the results for the period covered by this evaluation are disappointing. Considering the level of support and the number of people involved one would expect to see more interesting results and more scientific output.”

The evaluators were worried about the impact the commercial ties were having on the kinds of research being conducted and the overall research focus of the department. In their opinion, there was a real danger that the group's initial success in commercialization was shifting the research focus from “scientifically interesting but ‘difficult’ issues to problems whose solutions might be more financially rewarding,” although they admitted that the commercial success of the methods developed “validate the value of scientific work.” In an interview, the head of the department explained that, in his opinion, their very success in gaining external funding also contributed to the eventual erosion of the successful scientific research program. He thought that the external grant requirements and objectives as well as the mere number of projects that the researchers of the department were simultaneously involved in had prevented fresh and innovative initiatives.

As the time passed, the intermingling of academic research and business activity became problematical also from the commercial perspective, or as the former sales manager of the professors' company put it, "When the company started to grow out of its earlier research-group-like guise in the 1990s, *we found out that the interests of research and those of the company were not necessarily congruent*. . . . Those things that were 'hot' in academia, like speech technology, were still those days far from anything that could be commercialized, and things that would have been easily converted into marketable tools, such as terminology extraction and spell check, were no longer considered interesting on the research side."

The second professor of the department explained that the reason why terminology extraction and spell check were not considered interesting in the research side was that they were considered as "solved problems" that had advanced to the "engineering phase" of "small incremental advances." Indeed, even in such an application-oriented branch of research as language technology, the researchers and other actors had a clear conception of what they considered as genuine academic research. In attempting to explicate this, they referred to novel theoretical ideas, original solutions, and the principles of a "good way of doing things," as one of the interviewees put it. Consequently, although commercial activity clearly had an impact on the academic one, the contrary was also the case (e.g., Vallas and Kleinman 2007). In interviews, the executives of the professors' company complained about the perfectionist attitude of the employees—who were recruited from academia—as well as about their "slight contempt" for simple commercial tasks. This contributed to the financial problems of the professors' company in the beginning of the 2000s, which were further aggravated, as the professors, who still sat on the executive board, were reluctant to dismiss personnel who consisted largely of their former students. Eventually the company was sold off to a larger corporation, in which merger the professors withdrew themselves from business activity.

However, at the end of the 1990s, the key younger researchers—somewhat unwillingly—decided to give up their academic careers and moved to work for their newly formed company. Practically speaking, this meant the end of the language-technology research group. There was an element of irony in this development. The language-technology research group appeared to provide a prime example of entrepreneurial science, yet its development ran oddly against the tide of the official research policy: the group began its commercialization process long before the widespread notion of academic capitalism gained common currency in a European context, and the group dissolved in an entirely different institutional context in which the outspoken science policy was to enhance the "hybridization" of academic and commercial activities.

**5. Concluding Remarks.** The proponents of commercialized science have generally admitted that it implies adjustments in the normative structure of science. This has not been deemed problematical for them, however, as they typically rely on the constructivist understanding of science according to which existing institutions and organizations are local and historical constellations that may and will be molded into new kinds of forms at will. From this perspective, norms are treated rather as cultural resources that are strategically used by the participants to achieve various kinds of goals than as institutionalized imperatives able to guide and constrain scientific practice. The case study on the commercialization of the language-technology research group does not support such constructivist conclusions concerning the normative structure of science. Quite the contrary, despite the incentives and intentions of the participants to combine academic research and business activity, the language-technology group faced several problems that eventually led to its dissolution as the participants felt forced to choose either academia or business. The earlier mostly tacit normative stands revealed in the process concerned, first, the proper tasks of the university: selling was not supposed to be an activity taking place at a university department. Second, the results of scientific work are collective and so should be the rewards. Interestingly, as this norm was breached, the disagreements on property rights escalated into disagreements concerning academic priorities. Third, communication should be open within academia, and, fourth, academic research should be oriented toward novel and scientifically interesting, difficult issues. Ultimately, the Mertonian values of communism, disinterestedness, and originality were thus a force to be reckoned with and confronted in this specific case.

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