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Creative accomplishments in science: definition, theoretical considerations, examples from science history, and bibliometric findings

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Abstract Science studies have not yet provided a conceptual scheme that distinguishes creative accomplishments from other research contributions. Likewise, there is no commonly agreed typology capturing all important manifestations of innovative science. This article takes up these two desiderata. We argue that scientific creativity springs from the fundamental tension between originality and scientific relevance. Based on this consideration, we introduce a conceptual scheme that singles out creative research accomplishments from other contributions in science. Furthermore, this paper shows that creative contributions are not only advances in theory but also new methods, new empirical phenomena, and the development of new research instrumentation. For illustrative purposes, the article introduces examples from science history and presents results from bibliometric studies.

Keywords Creativity · Originality · Heuristic · Research accomplishment · Philosophy of science · Bibliometrics

Introduction

This paper addresses the question of scientific creativity. While consensus exists in the literature about how to define creativity in general, a clear definition of creativity in scientific research is missing. Particularly vague is how creative research achievements differ from other research contributions. Furthermore, creative research has often been equated with theoretical progress whereas empirical findings, the development of new methods, and the construction of new research instruments have received little attention. Therefore, two constructs are missing: a conceptual scheme that differentiates between creative and other research achievements and a typology that captures all major manifestations of creative research achievements.

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This paper addresses both. Following a general definition of creativity, we first argue that research is considered to be creative when ideas and artifacts are both scientifically valuable and plausible (=scientific relevance) and novel and surprising (=originality). Second, we build a coordinate system by cross-tabulating scientific relevance with originality. This coordinate system helps to differentiate creative scientific achievements from three other types of research contributions. Based on examples from science history and with the help of bibliometric studies, we describe this coordinate system in more detail. Third, we show that creative research is not necessarily equivalent to the formulating of new or improved theories. Following recent work on the epistemic significance of experiments, we argue that new methods, the discovery of new empirical phenomena, and the development of new research instruments are equally important as types of creative accomplishments in science. Finally, we discuss opportunities for future research.

What is scientific creativity?

Two characteristics generally define creativity: *relevance* and *novelty* (e.g. Fleming et al. 2007: 466; Boden 1999: 351; Sternberg 2003: 89). Ideas and artifacts are considered to be creative when they are useful and relevant and at the same time novel and surprising in the context of a particular social practice. Creativity is not an inherent characteristic of things that will be discovered anyway but rather is socially constructed by an evaluation process (Csikszentmihalyi 1999; Westmeyer 2001). Furthermore, creativity is typically perceived as context specific. Thus, it can be observed and evaluated only within fields or systems of social practice, such as the arts (e.g. music, literature) or the sciences (e.g. physics, chemistry). According to this view, creativity does not exist across fields or social systems (Simonton 1999, 2004; Westmeyer 2009).

The general concept of creativity requires a specification for the research system. Regarding *relevance*, Luhmann (1992) argues that publications are typically connected with established wisdom, thereby placing new knowledge claims into a horizon of disciplinary knowledge. The criterion of relevance in the general creativity definition can therefore be specified as *scientific relevance*. On the other hand, Luhmann argues that publications typically aim at expanding the collective knowledge base by presenting original and surprising thoughts and results. Therefore, the criterion of *novelty* in the general creativity definition can be specified as *originality*.

According to Polanyi (1969): 53–55, two criteria serve for the evaluation of the scientific relevance of publications: *plausibility* and *scientific value*. Both criteria are formally institutionalized in the peer review process (Bornmann and Daniel 2010; Cicchetti 1991; Merton and Zuckerman 1973). Clearly, the peer review process also examines whether a new contribution is truly original. According to Polanyi (1969): 55, one major criterion is used for the evaluation of originality: *surprise*. Contributions that obtain unexpected and thus surprising results are usually classified as original.

In a next step, Polanyi points out a fundamental tension between scientific relevance and originality. A publication is expected to build on previous knowledge but at the same time to go beyond existing knowledge. The tension between scientific relevance and originality pervades, according to Polanyi, the entire institutional structure of scientific research: “Both the criteria of plausibility and scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. The professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it” (Polanyi 1969: 55).

What distinguishes creative from other research achievements?

Polanyi (1969) does not discuss how the fundamental tension between scientific relevance and originality can be balanced. Rather, he conceptualizes both as distinct and opposing orientations. Therefore, Polanyi (1969) misses the important point that scientific relevance and originality can be conceived of as variables with different values. Colleagues perceive some contributions as highly relevant but others as rather esoteric. Some contributions are considered to be particularly original but others as only moderately original. Following this consideration, one can build a coordinate system in which scientific publications can be localized according to their degree of scientific relevance and their degree of originality (Fig. 1). It is obvious that depending on how they are evaluated, publications may be located at quite different places in this coordinate system. Some contributions are close to each other (A and B) whereas others lie far apart (A, C, D, and E).

Localizing scientific contributions in this coordinate system raises the question of what A and B have in common and what distinguishes, in contrast, A, C, D, and E from each other. To answer this question, dividing the coordinate system into four quadrants appears useful (Fig. 2). These four quadrants make it possible to differentiate among publications and thus to determine what distinguishes creative scientific work from other research contributions. Starting left at the bottom (Q1), we find contributions that are neither scientifically relevant nor particularly original. These are published contributions that have not received further attention. At the top left (Q2), we find contributions that formulate original ideas but which colleagues openly reject. In contrast to Q1, contributions in Q2 are not simply ignored but also encounter active resistance. At the bottom right (Q4), we find contributions of little originality but with high levels of acceptance among colleagues. These contributions fit well into existing theories and methods. The top right (Q3) contains original ideas that peers applaud at the same time. These contributions effectively connect scientific relevance and originality, they balance the tension between both.

Clearly, the four-quadrant typology takes Polanyi argument one step further. In fact, the typology spells out the types of scientific products resulting from different combinations of the two opposing orientations. If scientists consider scientific relevance as more important than originality, or likewise, if research organizations establish procedures that give more

Fig. 1 Scientific relevance and originality

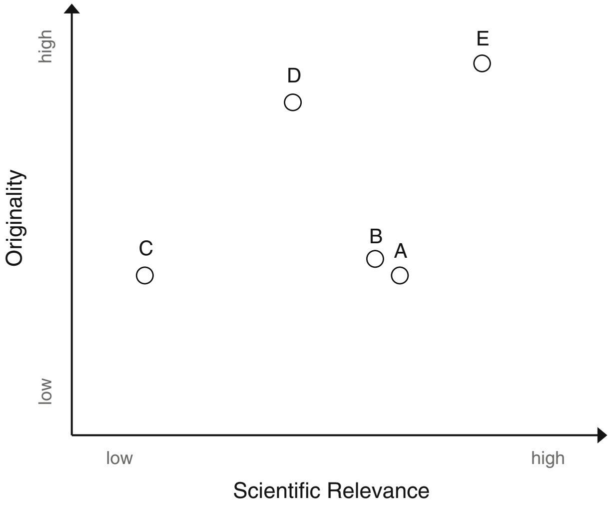
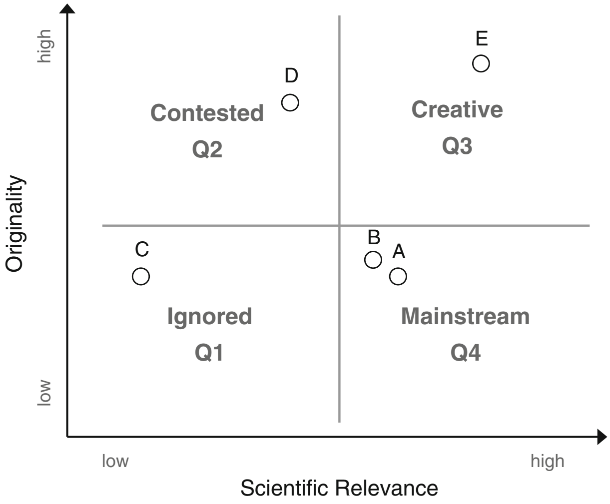


Fig. 2 Four categories of research accomplishments



weight to scientific relevance than to originality, then they will likely engage in research that leads to results that are judged as mainstream by their peers. In contrast, if scientists or research organizations regard originality as more important than scientific relevance, their results are likely to be rejected or resisted by their peers. The key point is that creativity requires a double-orientation that gives plausibility/scientific value and originality/surprise equal weight.

Examples from the history of science

Are there examples from the history of science that illustrate the four quadrants? Searching for an example for Q4 proves rather difficult because contributions of the scientific mainstream have hardly been closely examined so far, and if they have, it has been for the most part as a collective category. Kuhn's (1962, 1970) *normal science* is such a collective category that, because of its ubiquity, can be applied only with difficulty to selected examples. Furthermore, normal science is not congruent with Q4. However, for the other three quadrants, examples from the history of science are easy to find.

First, an example for Q1: Polanyi mentions a publication of a well-known member of the Royal Society. In this publication, released in 1947, the author alleges that hydrogen atoms that are shot at a metal wire pass on an energy charge of several hundred electron volts to the wire. "This, if true, would have been far more revolutionary than the discovery of atomic fission by Otto Hahn. Yet, when I asked physicists what they thought about it, they only shrugged their shoulders. They could not find fault with the experiment yet not one believed in its results, nor thought it worthwhile to repeat it. They just ignored it" (Polanyi 1969: 53f). The example shows that the publication is neither considered to be useful nor comes as a surprise. Therefore, it was simply ignored. Polanyi's colleagues do not contradict the author; they do not actively oppose his statements but instead simply slur over his publication without further comment.

An example for Q2: In the early 1980s, the two physicists Gerd Binnig and Heinrich Rohrer developed the scanning tunneling microscope (STM) with which they obtained experimental results that leading colleagues doubted considerably. The STM is based on a tip made of tungsten or platinum–iridium that moves line-by-line over a surface and thereby enables imaging at an atom-scaled resolution. Binnig and Rohrer stated that the distance between the tip and surface could be reduced to one atom, an assertion that met with immediate opposition (Binnig and Rohrer 1982: 57). According to quantum mechanics, a resolution at the size of an atom is, because of Heisenberg's uncertainty principle, not possible (Hessenbruch 2004: 138). In addition to this theoretical contradiction, the STM, however, also aroused opposition among the scientific establishment because it threatened the investments already made in existing microscopes. In fact, it did not initially produce reliable results either. Although the originality of the STM as a research instrument was not in question, some questions persisted about whether it had scientific value for furthering scientific research.

Finally, an example for Q3: at the beginning of the 20th century, the French mathematician Henri Poincaré published the so-called Poincaré theorem, which for a long time was among the great puzzles in mathematics because it could not be formally proven. About 100 years later, the Russian mathematician Grigori Perelman succeeded in finding a proof.[1] For his pioneering work, Perelman was awarded with the Fields Medal in 2006, the highest academic award in mathematics, as well as with the first Millennium Prize in 2010 of the Clay Mathematics Institute. In the laudation for the Millennium Prize, the high scientific value and the high degree of originality were emphasized as distinguishing features of Perelman's solution: "First, (…) it solves an outstanding, century-old problem: a problem that has done much to drive the development of topology from its inception. Second, the work is, to the highest degree, original and profound. (…) Perelman developed a host of extremely subtle and novel arguments: blending partial differential equations, differential geometry and the theory of convergence of spaces. The whole edifice he created in his proof is something unmatched, in its scope and depth, in this general area of mathematics."[2]

Frequency of creative research: bibliometric evidence

These three examples from science history serve to illustrate the conceptual scheme (Fig. 2); however, they do not answer the question of how frequently creative scientific thoughts or results occur. Using bibliometric studies, though, allows for a rough estimate of how frequently contributions fall into the four quadrants. We stress at this point that bibliometric studies provide estimates for the quantitative importance of the four quadrants but that they do not represent precise measurements (Fig. 3).

The focus of bibliometric studies is on the analysis of publication and citation data by using statistical methods (Moed 2005; van Raan 2004). Citations are of particular interest because they are a suitable indicator of the intellectual interconnectedness of publications (Cole 2000; White 2004; Zuckerman 1987). Here, two limitations require mention. First, specific work contributions are systematically not quoted. For example in biology, individual research contributions about the geographical distribution of animal and plant species are recorded in large biogeographical databases, but then usually the database is cited rather than the individual authors (MacRoberts and MacRoberts 1996, 2009). Second, citation measures do not consistently coincide with other evaluations of interconnectedness, e.g. evaluations or science awards. Therefore, authors repeatedly suggested combining different indicators for evaluating the quality of published works (Heinze and Bauer 2007; Moed 2005: 239–245).

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[1] http://www.claymath.org/poincare/, last accessed 24 June 2012.

[2] http://www.claymath.org/poincare/laudations.html#donaldson, last accessed 24 June 2012.

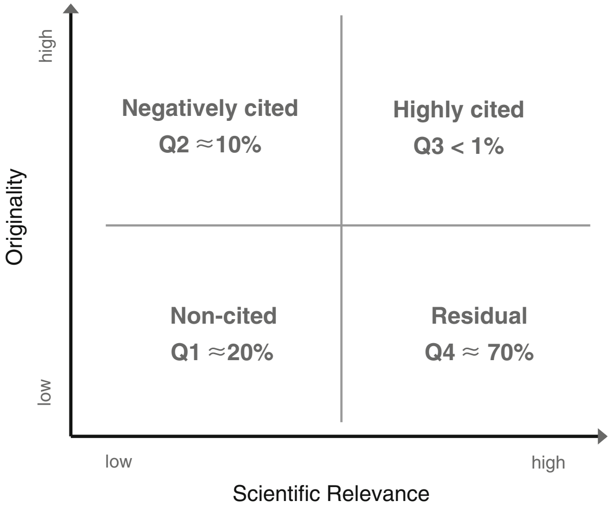

Fig. 3 Four categories of research accomplishments: bibliometric estimates

In line with the conceptual scheme (Fig. 2), the importance of ignored publications (Q1) can be determined on the basis of non-cited literature (NCL). Recently, numerous studies measuring the proportion of the NCL relative to the total publication volume of the Web of Science (SCI, SSCI, AHCI) have been published. Studies with a long time horizon, i.e. with a citation window extending over 10 or 20 years, determine with high accordance an NCL proportion of approximately 20 % (van Dalen and Henkens 2004; Glänzel et al. 2003; Sengupta and Henzler 1991; Stern 1990). Thus, one-fifth of all published contributions remain unconsidered in the long term.

One possible estimate for contested contributions (Q2) is the share of negative citations among all citations, i.e. references explicitly rejecting the contents of the cited source. In contrast to Q1, only a few estimates exist concerning such negative citations. Shadish et al. (1995) identify approximately 9 %, and Moed (2005) reports a magnitude of approximately 10 % for negative citations.

To estimate the proportion of creative contributions (Q3), highly cited publications (HCL) can be used. Publications are considered highly cited when they are cited with an above-average frequency, for example, 10 or 20 times more frequently than the average publication. The available bibliometric studies indicate that the proportion of HCL is significantly below 1 % (Aksnes 2003; Glänzel and Czerwon 1992; Glänzel et al. 1995). However, even this low value would probably overestimate the real proportion of Q3. Although creative achievements are frequently cited, the reverse does not apply: high citation scores do not reliably indicate groundbreaking contributions. Moed (2005: 83) reports about an initially frequently cited paper with results that could not be replicated; after some years, it fell into oblivion.

The proportion of mainstream contributions has not been determined by bibliometric studies so far. As was the case for the examples discussed above, a gap exists in the literature. However, Q4 can be approximately determined on the basis of the mentioned studies. Under the assumption that publications fall at any time into one of the four quadrants, Q4 can be determined by subtracting shares of the other three quadrants from 100 %. Under this assumption, the proportion of mainstream contributions would probably be about 70 %.

Relationships among the four quadrants

Social attributions, such as the evaluation of research achievements according to their scientific relevance and originality, are sometimes subject to change. A contribution that has initially remained ignored can turn out to be groundbreaking. Similarly, an initially contested contribution can finally gain recognition over time.

Transitions from Q1 to Q3 are very rare because in such cases, the evaluation of the two criteria is revised. In the literature, these cases are referred to as *premature discoveries* (Stent 1972, 2002) or as *delayed recognition* (Cole 1970; Garfield 1980, 1989a, b, 1990). Glänzel et al. (2003) and van Raan (2004) have presented a bibliometric estimate of the transition from Q1 to Q3. Both studies conclude that such a transition occurs in less than 1 in 10,000 publications. It is therefore very unlikely for a publication to emerge from lengthy obscurity and suddenly become famous. However, Van Raan (2004) reports a case in which a publication received no attention for 10 years until suddenly it was cited 15 times per year on average.

Transitions from Q2 to Q3 seem to occur more frequently: Initially disputed works gain high recognition after a certain, sometimes very long time. Unfortunately, no bibliometric estimates are available for transitions of this kind. Nevertheless, the existence of a whole series of works that were initially contested but eventually honored with a Nobel Prize indicates that such transitions do exist (Campanario 1996, 2009; Campanario and Acedo 2007), including the case of the STM. Although the STM was initially disputed for technical reasons, when awarding the prize to Binnig and Rohrer, the Nobel committee explicitly referenced the technical capacity of the STM, which is an indication for the re-evaluation of its scientific value: “Binnig's and Rohrer's great achievement is that, starting from earlier work and ideas, they have succeeded in mastering the enormous experimental difficulties involved in building an instrument of the precision and stability required.”[3] At the same time, Hessenbruch points out that reservations against the STM from quantum theorists turned out to be unfounded, which is an indication for the re-evaluation of plausibility: “Nowadays STM users will learn that the uncertainty principle does not apply for the case of atoms embedded in a solid and that the examples used to explain the uncertainty principle apply only to free atoms” (Hessenbruch 2004: 139).

The transition from Q3 to Q4 seems to occur particularly frequently. A research breakthrough is usually followed by a run on the new topic. Rabinow (1996), for example, reports about such a run subsequent to the development of the polymerase chain reaction (PCR), for which Kary Mullis was awarded the Nobel Prize in Chemistry in 1993. PCR is a method by which small quantities of DNA are copied and amplified in number, allowing production of specific sequences in great quantities (Mullis et al. 1994; Mullis 1998). Rabinow (1996) writes: “Thousands of scientists and technicians around the globe began using PCR, multiplying the modifications and feedback—nested PCR, inverse PCR, single-molecule amplification, universal primers, direct DNA sequencing, multiplex amplifications, quantitation, single-gamete genotyping, cUTP/UDG, combinatorial libraries, aptamers, isothermal amplification, sequence-tagged sites, ancient DNA, in situ PCR, single enzyme Rt-PCR, long PCR etc.” (Rabinow 1996: 169). PCR has become such an integral part of mainstream research that the multitude of research areas where it is applied is difficult to survey.

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[3] http://nobelprize.org/nobel_prizes/physics/laureates/1986/press.html, last accessed 24 June 2012.

Manifestations of creative research

Studying the various manifestations creative contributions can take allows further elaboration of the statement that creative research contributions connect scientific relevance and originality. Our starting point is the widespread belief in the philosophy and sociology of science that formulating new theories is at the center of scientific progress. This position is represented not only by Popper (1959) but also by Kuhn (1962, 1970), Merton (1968), and Lakatos (1970). In particular, Merton argues that empirical discoveries, new observation, and measurement data and methodological innovations put pressure on the improvement of theories. However, he does not consider these as independent categories of research achievements; rather, they have a "stimulating effect upon the further development of theory" (Merton 1968: 512). This perspective of theory's holding a privileged position in the evolution of science has also been endorsed in the constructivist tradition of science studies, which actually sprang from strong criticism of Merton and his followers (Collins and Pinch 1998).

Equating outstanding scientific achievements primarily with theoretical progress has been repeatedly criticized since the 1980s (Hacking 1983; Heidelberger 2003; Rheinberger 1997; Shapin and Schaffer 1985; Shinn and Joerges 2002). This criticism arises from the fact that experimental settings and empirical breakthroughs were largely ignored in the traditional philosophy and sociology of science (Radder 2003) and has been expressed particularly clearly by Rheinberger (1997) and Shinn and Joerges (2002).

Rheinberger (1997) shows for biomedical and microbiological research that the core structure in which scientific activity evolves is not so much theories but experimental arrangements, in which the knowledge objects and the technical conditions of their generation are inextricably connected. Thus, the development of a certain problem horizon cannot be grasped correctly without the experimental texture. In contrast to classical epistemology, Rheinberger does not conceive of experiments as testing instances for theories that are conducted individually to either confirm or reject a theory. Rather, experiments are embedded in a system of other experimental practices that, as a whole, is aimed at constantly producing novel and surprising knowledge. Research in experimental systems represents an independent type of scientific activity with dynamics that can be explained only by reference to the system of the experimental practices. Experimental systems typically cannot be assigned to an academic discipline because their dynamics take place in various disciplinary and institutional contexts.

In addition, Shinn and Joerges (2002) argue that the development of new research instruments represents an independent category of scientific activity. They show that the inventors of new instruments typically focus their attention less on the laws of nature than on the technical regularities that play a role for the conceptualization, construction, and operation of precision machinery. The authors show that new research instruments are not developed exclusively in traditional academic settings but also in companies. They also point out that research instruments often cannot be assigned to one academic discipline.

Following this criticism, the position represented here is that an adequate description of creative scientific contributions needs to take into account non-theoretical work as well, including new methods and techniques, discovery of empirical phenomena, and the development of new research instruments. The examples introduced below are meant to illustrate these three types of creative scientific contributions (Table 1). As we show, capturing the entire landscape of intellectual innovations requires going beyond disciplinary research at universities and the narrow spectrum of academic research.

Table 1 Categories and examples for creative research accomplishments

Category	Example (Name, publication year)
New theory	Quantum hypothesis (Planck, 1900) Theory of superconductivity (Bardeen, et al. 1957) Critical phenomena in phase transition (Wilson, 1971)
New methods	Partition chromatography (Martin and Synge, 1941) Radio carbon method (Libby, 1947) Polymerase chain reaction (Mullis, 1985)
Empirical discoveries	Mobile genetic elements (McClintock, 1944) Carbon fullerenes (Kroto, et al. 1985) High temperature superconductivity (Bednorz and Müller, 1986)
New research instrumentation	Electron microscopy (Ruska, 1933) Bubble chamber (Glaser, 1952) Scanning tunneling microscopy (Binnig and Rohrer, 1982)

All examples were awarded the Nobel Prize in either Physics, Chemistry, or Physiology/Medicine

New methods and techniques

An example for new methods and techniques is PCR, already mentioned above. From a scientific perspective, PCR is a groundbreaking method because it significantly simplifies the sequencing, cloning, and amplification of DNA sequences. Furthermore, viral and bacterial infections can be identified without time-consuming cultivation of microorganisms. PCR can also be used for pinpointing sequence changes that underlie genetic diseases and is thus crucial for gene therapy.

Clearly, PCR is not a theoretical innovation. Rabinow argues: "PCR did not emerge as a solution to a growing set of theoretical anomalies in a scientific discipline. (…) the historical distinctiveness of PCR lies less in theoretical advances that it has facilitated (…) than in the practice that accompanied it" (Rabinow 1996: 168). Rather, PCR is a methodological innovation that was neither theoretically inspired nor has led to the formulation of new or revised theories. PCR is representative for innovative methods and techniques that constitute an independent category of scientific creativity.

Moreover, PCR has been developed in one of the first biotechnology companies worldwide and has—as a basic technology—considerable commercial value. Clearly, the idea that intellectual progress involves theoretical innovations has led traditional sociology and philosophy of science scholars to focus on academic settings and a certain cliché of what a scientist is. According to Shapin (2008): 215–226), Kary Mullis's appearance and his public self-expression, however, contradict the traditional cliché of highly educated scientists who, following an inner calling, work ascetically and thus create new knowledge. The facts that Mullis does not comply with the conventional cliché of a scientist and that PCR was not developed in a traditional academic setting are not indicators for low scientific creativity. On the contrary, although institutional context and cliché do not fit the standard account of where and by whom pathbreaking contributions are accomplished, PCR has had a tremendous scientific impact in various disciplinary contexts.

Empirical discoveries

An example for empirical discoveries is high-temperature superconductivity (HTS), developed by Alexander Müller and Georg Bednorz at the IBM Research Center in

Rüschlikon (Switzerland). Superconductivity occurs when a material is cooled below a critical point at which the electrical resistance disappears. Bednorz and Müller discovered in 1985 a ceramic based on copper oxide, which becomes superconducting at −238 °C (Jansen 1998: 21–32). After this breakthrough, hundreds of laboratories around the world began to search for similar and better materials (Felt and Nowotny 1992; Jansen 1998: 32–42).

The discovery by Bednorz and Müller joins a long list of research breakthroughs concerning superconductivity, several of which have been honored with Nobel Prizes (Felt and Nowotny 1992; Jansen 1998; Karlsson 2000). However, the reason that the HTS story is interesting is because it conflicts to this day with the prevailing standard theory of superconductivity, the so-called BCS theory. Even today, a conclusive theoretical explanation of the HTS phenomenon is lacking. Many physicists argue that the difference that Müller and Bednorz identified in the ceramic superconductors compared to conventional superconductors is so great that a completely new theory needs to be developed.[4] In fact, BCS theory, developed in the 1950s, could explain the materials developed at that point but did not inform the search for new materials. For this reason, no theoretically predetermined search strategies for new materials existed; there were only recommendations on the basis of former experimental findings. Bednorz and Müller stepped into this theoretical vacuum and identified a new copper oxide-based ceramic with HTS characteristics. Since the 1985 breakthrough, new materials have been developed that become superconducting at −140 °C.

New research instruments

We have already introduced STM as an example of research instruments. Another related invention is the electron microscope (EM). Both instruments are connected by the fact that the inventor of the EM, Ernst Ruska, was awarded the Nobel Prize in Physics in 1986, together with the STM inventors Gerd Binnig and Heinrich Rohrer. The primary scientific importance of the EM is successfully overcoming the limits of the conventional light microscope. The conventional light microscope can register objects only up to the size of the wavelength of light. In contrast, the wavelength of electrons is much smaller. Consequently, the EM allows a much higher resolution (Erni et al. 2009; Pennycook 2005). A further scientific importance of the EM is its broad disciplinary applicability, useful not only in material sciences but also in medicine, biology, chemistry, and physics for the determination and characterization of material samples (Marassi and Nobili 2009; Potter and Love 2004; Williams and Carter 2010).

The story of how the EM was invented shows that the production of new research instruments does not have to be theoretically inspired. During his development work, Ruska knew nothing about the wave theory of de Broglie from 1924, which states that electrons have a wavelength smaller than the wavelength of light (Ruska 1986: 360–361). Furthermore, the invention of the EM shows that new instruments can be developed in an academic as well as in an industrial context. Although Ruska produced the first EM prototype at the Technical University of Berlin, regular EM production was possible only with support from Siemens and Carl Zeiss, two leading companies in instrument development. After World War II, Ruska continued his activities at Siemens and presented in

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[4] UZH News from 28 March 2006: Symposium in honor of the Physics Nobel Prize winner 1987. Superconductivity with a future. Available at: http://www.uzh.ch/news/articles/2006/2125.print.html; last accessed 24 June 2012.

1954 a significantly improved device that was sold more than 1,000 times (Lambert and Mulvey 1996). Thus, Ruska's biography fits well with the description by Shinn and Joerges (2002), who point out that research technologists change in their professional careers between university and companies.

Discussion

The examples from science history serve to illustrate four major categories of creative research achievements and thus specify Q3 (Fig. 1). However, this does not address the question of how frequently the four categories actually occur. This paper, unfortunately, cannot answer this question because no relevant studies about this subject are available. For estimating the quantitative importance of the four categories, for example, one could classify all the scientific works awarded the Nobel Prize (Table 1). The result of such an analysis would not only be a quantification similar to Fig. 3 but also would be an answer to the question of whether all intellectual innovations can be clearly assigned to the four categories or whether there are certain overlaps and ambiguities, even further categories not included in Table 1 could exist, but such an analysis remains to be performed.

Furthermore, it must noted that the typology discussed can basically be applied on all quadrants (Fig. 1). Just as there are, for example, creative methodological research accomplishments, of course, there are also method publications that belong to the mainstream (Q4) and that do not find proper consideration (Q1) or are disputed (Q2). It would therefore be interesting to find out whether the four quadrants differ from each other regarding the distribution of theoretical, methodical, instrumental or empirical discoveries. In this regard, one possible research strategy would be the bibliometric analysis of a large number of publications, say all SCI papers of five consecutive years, by determining their share of uncited papers (Q1), negatively cited papers (Q2), and highly cited papers (Q3)—and indirectly also mainstream papers (Q4), and second, by estimating the share of theoretical, methodical, instrumental or empirical contributions in these using sophisticated keyword and phrase search algorithms.

How does this paper stimulate further research, apart from suggesting possible strategies for estimating the distribution of theoretical, methodical, instrumental or empirical contributions in the four quadrants (see above)? First, the article argues that science progresses not only through the development of new theory but to significant parts also through advances in experimentation and instrumentation. While this is indeed a completely uncontroversial statement in contemporary science studies, it is under-used as basis for studies of scientific achievements. Those interested in studying the difference between creativity in either empirical discovery or instrumentation would be perhaps be best advised conducting historical case studies on particular experimental or instrumental breakthroughs combined with supplementary bibliometric analysis both on the individual and organizational level. In this way, for example, the history of the EM, qualitatively described above, could be complemented by insights about Ruska's lifetime publication profile, and also bibliometric analyses of the disciplinary spectrum in which the EM became established.

Second, empirical studies on the level of research organizations can complement the conceptual analysis of this article. In this way, it is possible to address scientific creativity from an organizational sociology perspective including, for example, questions regarding which research organization produce creative research achievements particularly frequently, or which institutional conditions are particularly conducive to scientific creativity.

Third, the scheme (Fig. 2) connects a multitude of individual findings from the sociology of science, the history of science, and bibliometrics. This approach allows linking of different perspectives. In this way, knowledge gaps and questions that have received little attention can be identified.

Admittedly, this paper has limitations. First, the conceptual scheme is derived from two continuous but not discrete variables. Therefore, more than four categories are logically possible. Dividing the two variables in "low values" and "high values" and then combining them in a matrix clearly is a simplification. Therefore, the model is probably not the final answer to the question of how to define scientific creativity. Second, a challenge for the scheme is both operationalization and empirical measurement. Using estimates from the bibliometric literature can only be a first step. In particular, it seems difficult to identify mainstream publications as a separate category. In this paper, they are a bibliometric residual calculated from uncited, negatively cited, and highly cited work. Improvements both for alternative operationalization and better measurement are therefore highly welcome.

A final point: the four quadrants discussed here raise the question of how they relate to Kuhn's (1962, 1970) proposed distinction between *normal science* and *scientific revolution*. Certainly, the intention to equate creative research achievements (Q3) with scientific revolutions and mainstream research (Q4) with normal science would not go far enough. According to Kuhn, intellectual progress can be observed within the scope of a prevailing paradigm, as well, including the derivation of new laws, elimination of theoretical contradictions, and determination of important empirical facts (Kuhn 1970): 23–34. Although Kuhn's main interest lies in the intellectual discontinuities that lead to scientific revolutions, he explicitly mentions intellectual achievements as work products of normal science. It appears therefore appropriate to assign both Q4 and Q3 to normal science.

Revolutionary contributions at first sight belong to Q2 because they break up the consensus within scientific communities. However, concerning Q2, a proper differentiation is required. Contributions that after initial skepticism by the scientific community are finally accepted without fundamentally changing the scientific world view move from Q2 to Q3. In contrast, all Q2 contributions that trigger an extensive revision of the scientific world view have a revolutionary character. However, such revolutionary contributions go beyond the framework of the scheme; it is a heuristic tool is not aimed at describing the transition from an old to a new paradigm. Its focus is rather on the differentiated description of normal science. Therefore, Q1 falls within Kuhn's definition of normal science, as well. In sum, the paper argues that the conceptual scheme is a heuristic complementary to that of Kuhn (1962, 1970).

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