Theoretical Practice in Innovation-driven Research Environment

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The political assault on pure science and theoretical production imperils the very foundations of both 'soft' and 'hard' sciences. What positions are scientists taking towards the changing conditions of research? I will look at the epistemological position and self-reflection in scientific practices, the material conditions of research (particularly publishing and scientific impact measuring), the responses to external demands and the social positioning of science.

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In this article, I will try to shed some new light on the age-old question of possible epistemological convergences between the so-called 'soft' and 'hard' sciences: the humanities and social sciences on one side and natural sciences on the other. Relations between the two kinds of science have so far been understood rather as discrepancies between two rigid systems of knowledge, that is, as the difference between nomothetic and idiographic sciences. However, the recent political assault on 'pure science' and theoretical production in general seems to imperil the very foundations of both kinds of science.[1] Simultaneously, the whole research milieu is changing: from the university, which is constrained to submit to corporate governance and to engage in market-oriented 'services', to research institutions, which are pressed to justify their existence by providing innovations for enterprises. All these transformations are supposed to improve the global competitiveness of states. At the same time, exceedingly quantitative selection criteria for funding or personnel decisions are being introduced under the aegis of the so-called 'scientific excellence', that is, the international competitiveness of local scientists. Academic publishing, a decisive institutional setting for presentation, distribution and exchange of research findings, is being taken over by profit-driven multinational corporations, which impose their own conditions on the access to pub-

lishing. These external pressures pose a series of questions, specifically the questions regarding the epistemological position and self-reflection in scientific practices, the comprehension of material conditions of research work, the responses to external demands and the social positioning of science. I see these questions as closely interrelated: the capacity of scientists to analyse their own material conditions of research and to take a stance towards these conditions depends on their epistemological positioning. And vice versa, if scientists are not capable to cope with their own conditions of research, with what other task can we entrust them?

Counter-epistemology in social sciences

The notions of nomothetic and idiographic science were introduced by Wilhelm Windelband in 1894 as part of his critique of positivism; however, the debate itself goes back to Giambattista Vico and his polemics against Cartesianism as the critical method of the Moderns, which Vico contrasted with the topical method of the Ancients. G. H. von Wright described nomothetic sciences as examination of events that repeat themselves and can be anticipated; such events can be moreover isolated with the aim of carrying out experiments whose observation may engender scientific general laws. On the other side, idiographic sciences such as historiography study transient events, which we comprehend in the form of description. Wright (6) stressed that Georg Simmel compared historiography to theatre and defined the method of comprehending past events as empathy. During the modern age, the prestige of the nomothetic approach give birth to many new disciplines by annexing research areas that initially 'belonged to' the idiographic sciences (in this way, the nomothetic sociology 'appropriated' the field that had been covered by historiography). Rastko Močnik (188–191) even claims that social sciences are a compromise formation resulting from the pressure of the Galileian paradigm upon the humanities. However, tensions between nomothetic and idiographic sciences not only stem from competing scientific disciplines implementing one approach or the other, but they are also inherently present in every research or scientific work. The success of every discipline and of each research depends on a suitable combination of both approaches.

Idiographic sciences recently responded to these tensions with what seems to be a counter-attack based on Clifford Geertz's anthropology and his 'native's point of view' approach. Geertzian perspective has probably been the most widely accepted approach in the humanities since the publication of *The Interpretation of Cultures* in 1973, with an impact comparable

to the impact of Saussure's linguistics (published in 1916) upon structuralism.[2] Geertz's idea that we can speak about societies only in their own languages, and that theoretical apparatuses invalid our comprehension of the functioning of societies, disqualifies the use of sociological approaches or 'reified' concepts such as structure, class and class struggle in the humanities. Traces of this disqualification can still be found all over the spectre of the humanities, including literary studies, historiography and, say, cultural studies. In Geertz's 'counter-epistemology' *isolated and fixed social representations* are presumably the only acceptable intermediaries in research examination. As a consequence, his approach impedes the basic principle of idiographic science, due to which Vico called this science the 'topical method', according to which concepts should be able to provide a meeting point of various possible perspectives on a certain problem as well as space for their confrontation and comparison. Needless to say, this particular 'post-Geertzian' epistemology also takes for granted a vision of society as consistent and pacified community in which only 'soft social divisions' may exist (Breznik 285). Epistemology is therefore also a political statement.

As a consequence, Geertz's epistemology is helpless when confronting the teleological viewpoint that is currently imposed upon science by the ideologies of 'innovation', service to industry, economic efficiency and the like. By removing the topical method from research, this viewpoint renounced the dialectical comprehension of human affairs, which is the most important contribution of the humanities to the interchange between nomothetic and idiographic sciences. Topical or dialectical approaches were a solid impediment against the teleological ideologies that easily undermine scientific efforts. After having given up these approaches, the humanities gave way to the 'spontaneous ideology of scientists' (to use Louis Althusser's concept) that frequently manifests itself as the idea of 'progress'.

It is precisely by way of the idea of progress that capitalist interests can grasp the scientific practice, something they are doing very efficiently at the moment.[3] It would be short-sighted to consider the ideology of progress merely as a 'spontaneous' component of scientific practices;[4] while the ideology of progress emerges as the spontaneous ideology of scientific practices only in specific and well determined situations,[5] it is the dominant ideology of the apparatuses of capitalist state: it secures the unity of scientific ideological apparatuses and articulates them onto other apparatuses of the capitalist state.[6] State apparatuses rigorously enforce this ideology and accelerate the re-orientation of scientific work into innovations for the development of the capitalist articulation of productive

forces. Looking at the European Commission's latest administrative creation, *Horizon 2020*, or at the latest Slovenian national research strategy, *Drzna Slovenia* (Daring Slovenia), we see that the politics of the European Union and its national epigones compel science to work exclusively on innovations as economic factors for raising economic 'growth' (concealing the fact that economic growth actually means securing and increasing profits for capital owners).[7] In this framework, the humanities and social sciences are assigned the task of pacifying conflicts that inevitably arise from the 'innovative' restructuring of the labour processes and generally from exploitative and repressive relations produced by economic growth itself; the humanities and social sciences are being encouraged to specialise in identity ideologies in order to maintain social cohesion for the accumulation of capital.

Old and new revolutions

The material conditions of research work, especially in the publishing area, are now being redesigned so as to comply with the idea of progress. The development of worldwide establishments for the publication, distribution and exchange of printed texts was certainly a great leap forward, but this development hardly guarantees access to printed texts greater than in the early modern print culture. Academic publications (the so-called 'academic electronic publishing') are restricted to the members of universities that can afford to pay expensive subscriptions, while remaining inaccessible to the majority of scientific workers. Similarly, access to literary and other printed works is increasingly restrained due to progressive dissolution of public cultural programmes. The new models of publishing impose a certain elitist social position of science. Moreover, these models dictate a certain 'epistemology' (focusing on novelty, experiments and innovation), prescribe research topics (such as social 'cohesion' and 'exclusion', 'identities', etc.), or, according to Bill Cope and Mary Kalantzis, bring 'epistemological disruption' into scientific work (see Cope and Kalantzis). It is therefore hidden publishing structures that nowadays 'write' scientific articles, novels and poems; they efficiently impose norms upon writers as they assume a neutral look with respect to scientific or artistic practices and operate in a common-sense way, like the forces of nature, not as a product of human decisions and actions.

The new print revolution, electronic digital publishing, is associated with a belief that technological possibilities may open a larger general access to printed works and promote greater social equality. With a brief

digression into Gutenberg's print revolution I would like to show that the persistent belief in technologically determined progress can be problematic. The following case is moreover instructive because it shows that without the topical approach we would not be able to grasp unexpected social correlations.

Let me briefly go back to the fifteenth century, to the time of Gutenberg's invention of print. French historian Christian Bec reconstructed Florentine family libraries from this period on the basis of the inventories of *Magistrato dei Pupilli*. The 'Magistrate of the Pupils' exercised custody over Florentine orphans and maintained a detailed registry of family heritage for each child. This inventory included the list of books of every household, on the basis of which Bec examined which books the households possessed. He divided the inventories into two periods, divided by the invention of print. According to his findings, in the first half of the fifteenth century, family libraries contained a relatively small number of books, which were, however, equally distributed among Florentine households regardless of the economic strength of families. The list of the most frequent works in Florentine households of that period is quite surprising: these were either writings in Italian (with Dante and Boccaccio prevailing) or translations (especially Donatus), while religious books were not as frequent as one might expect. In the second half of the fifteenth century, the amount of books in family libraries as well as the preferences of readers changed. The most frequent authors were Petrarca, Cicero, Dante, Virgil, Ovid, Boccaccio, Donatus and Tit Livy in Latin language, a selection more suitable for educated humanists. The number of small family libraries was progressively decreasing, while big family libraries with hundreds of books appeared in that period. We should know that Italy surpassed even Germany, where the invention of print technology took place, in the number of printing presses soon after their invention; and as for the number of printed books in that period, Florence occupied the fourth place in Europe (Febvre and Martin). Even though the accessibility of printed books in Florence was wide, Bec concludes that after the invention of print, most Florentines had a more limited access to books than during the preceding period, when books had been copied by hand. Only wealthy and well educated individuals could afford printed books, and they were buying a lot of them, particularly in Latin. Additionally, a new cultural barrier appeared with the books printed in Latin, which most people could not read.

Another historian, Samuel K. Cohn, Jr., drew a similar conclusion from his examination of commissions of paintings in testaments. During the first half of the fifteenth century, many people were commissioning

religious paintings *post mortem* for a low price, while during the last years of the fifteenth century, big commissions of large frescos started to prevail, while small commissions almost disappeared.

These two conclusions contradict Richard Goldthwaite's claim that after having lost the world economic leadership in basic products (such as wool), Italy successfully substituted this regression with the export of luxury goods. According to Goldthwaite's interpretation of this substitution, the peak period of the Renaissance arts sponsorship is actually the moment when the first proto-cultural industry was established. Goldthwaite tries to prove his claim by noting that during this period, the share of wealthy population in Florence was greater than in other parts of Europe. However, this argument is correct only if we compare the Florentine wealthy social strata with other wealthy social groups in Europe; that is, the argument holds if we look at the society from the top down. But if we change the perspective and look from the bottom up, a mass of very poor people appears before our sight. The poverty and deprivation of the poor in Florence in 1427 were incomparably worse than the situation of the poor in Great Britain in 1688 (at the presumably brutal beginning of industrial capitalism) or, say, in 1962 (Cippola 5–17). As a consequence, it seems impossible to see in the high Renaissance a period when a considerable portion of the population could participate as consumers on an accessible cultural market. The process would be much better defined as 'conspicuous consumption' by a relatively small social group. From this observation we can draw the conclusion that there is no necessary correlation between artistic prosperity and social equality; various socio-economic and cultural factors (such as economic inequalities, social stratification and learned culture in Latin inaccessible to the majority of people) may generate contradictions between productive forces and relations of production. Technological development may enlarge the group of those who benefit from it, while expropriating many others who have already taken advantage from the 'socialisation' of previous technology. It is also appropriate to note here that the idea of an irreversible and continuous 'civilising process' is illusory; the history of society is actually full of disruptions, and simultaneous heterogeneous processes result in re-compositions and re-articulations of social practices and institutions for which it is not possible to say whether they have anything to do with progress.

Electronic publishing was a revolutionary innovation comparable to the introduction of print technology in the fifteenth century. It is still developing, so we cannot anticipate all possible publishing and distribution models. However, it is already very clear that the new technology, assisted by extra-economic forces such as copyright regulation, will certainly im-

pose new relations of production, which will support the appropriation of technology and its results for individual capital gains. Like print five centuries ago, electronic publishing may have increased the number of its beneficiaries. But it also imperils the general access to published works, particularly by undermining the public library system. Electronic publishing has eliminated the institution of 'public lending', which had been offering general access to printed works. Public libraries are only allowed to offer to their members distant access to e-published works if they pay costly licences. Moreover, copyright holders have the right to limit access to their works to a special group of library members, while being entitled to request exorbitant compensations. Hence, libraries usually restrict the access to their e-collections (academic e-journals, e-books and databases) to the exclusive group of their members. This was inconceivable in the previous period, when public libraries were tightly associated with general access they had to facilitate.

The conclusions we usually draw from our research depend, as we can see, on the perspective we take. There is no objective approach or quantifiable data that would by themselves make possible any reflection without theoretical elaboration, which starts with the first determination of the object of research. The humanities cannot examine human affairs without taking into consideration a plurality of perspectives. This is why a rejection of the topical method exposes research to imprudent simplifications, if not ideological deception.

Science of science and scientific efficiency measuring

In my view, the implicit epistemological position, tacitly imposed upon scientific practices by the new methods of presumably objective evaluation of scientific work as well as by other regulations of academic and research establishments, increases the powerlessness of scientists, forces upon them certain epistemological choices while excluding others, obstructs the production of theoretical *problématique* and imposes ideological problems. It has deep social effects: it radically changes the conditions of research work, decides where and how scientists should publish their research results and atomises scientific communities (Močnik 441–510). Yet so far, the resistance of scientists to the newly imposed regulations has been weak and inefficient, which should be a matter of particular concern, since the erosion of the institutionalisation of scientific practices as a special social field abolishes the institutional separation of scientific work from ideological practices and processes. The production of the *epis-*

temological break is certainly the permanent task of any theoretical practice: however, in the present situation when the epistemological break is institutionally undermined, theoretical practices have to be exercised *against* their institutional conditions of possibility.[8]

The so-called objective evaluation of scientific work comprises several sets of procedures: the quantification of data for measuring scientific efficiency of every scientist by the number of publications and citations; the evaluation of academic journals by their 'impact factor'; university ranking; international innovation scoreboard; and international science ranking. Electronic academic publishing and information management made manageable enormous quantities of scientific data: this technological possibility alone seems to be a sufficient reason to impose the citation index and impact factor as the main elements of the evaluation of scientific work. However, most of other arguments speak against the use of such measurement, as the allocation of research funds and the distribution of university posts have been subjected to these controversial measuring procedures.

Electronic publishing of academic journals brought about numerous possibilities of collecting data from the published articles. It made possible a large-scale use of the citation index methodology. It is important to note that academic publishing is a profit-driven industry with the highest profit rates in the publishing sector. In 2009, three of the top five publishers with the largest turnover were publishers of academic journals (Reed Elsevier, Thomson Reuters and Wolters Kluwer). They have concentrated enough journals to take advantage of the new business opportunity and are now offering to their clients not only journals and articles, but also metadata about authors, publications, citations and impact assessments. Because of this double function of the quasi-monopolistic academic publishing corporations, authors are forced to cooperate with them for two reasons: the main two criteria for the evaluation of scientific work are publications in journals with the highest impact factor and citations of authors in the articles written by other authors for the same group of journals. So the authors have no other choice than to fight for publications in journals with high impact factors and for as many citations of their works as possible, if they want to keep working as researchers or university teachers.

The idea to measure citations in scientific articles, books or conference proceedings originates from the 1920s;[9] gradually, it was developed into 'unobtrusive measures that do not require the cooperation of a respondent and do not themselves contaminate the response (i.e. they are non-reactive)' (L. C. Smith, qtd. in Bornmann and Daniel 45). The citation betrays the information scientists' desire to forge an objective measurement of scientific efficiency. Eugene Garfield, the founder of the Institute

for Scientific Information, argues that this methodology has roots in the history of science (he cites Kuhn's *The Structure of Scientific Revolutions*) and its legacy:

> Certain presuppositions, both historical and sociological, underlie the idea of 'mapping' science by identifying key papers and events through citation analysis. The basic unit of analysis in mapping is the highly cited document. The assumption is that these articles and books are markers for critical scientific ideas or events, taken in the broadest sense. This includes theoretical formulations, speculative hypotheses, experimental results, procedures or methods, and any combination of these. The fact that some documents have been highly cited within a specified time-period confers upon them a special status as providing important 'ideas' in their respective areas or specialties. (Garfield et al. 181)

Citations certainly can, to a certain extent, indicate[10] ideas, procedures, methods and concepts of the current scientific production; for the history of science they are amazing tools for the reconstruction of invisible scientific currents and paradigms. These methods are probably quite applicable in cumulative nomothetic sciences, which rely on up-to-date experiments, procedures and discoveries. However, if we only take rough statistical indicators as information about scientific work, we tend to simplify the available data and reproduce the empiricist notion that all we can assess is there in the presumed objective reality and does not need any further intellectual intervention (which is the problem of Geertz's epistemology). By proceeding in this manner we also imply that scientific publishing in a particular publishing setting with certain power relations and profit-driven motives can be taken as an exact mirror of what we believe to be 'scientific production'. This type of reasoning is obviously very difficult to accept.

However, a crucial question about scientific production remains answered. What separates scientific work from ideology and enables science to make the epistemological break, which differentiates it from ideological practices? Louis Althusser taught us that we must know how to separate what he calls the real object, *l'objet réel*, which exists independently of our thoughts, from the object of knowledge, *l'objet de la connaissance*, which is the product of our reasoning and exists independently of the so-called real object. The two processes belong to different ontological realms: on one side, formation and duration of the real object pertains to reality by the working of natural and historical forces; on the other side, the object of knowledge is produced by our thoughts in accordance with specific cognitive processes that use concepts as their tools of production. Albeit concepts may 'reproduce' real objects, they do not belong to their realm of existence, and perform specific functions in the production of the objects of knowledge. Scientific practice is therefore theoretical production,

a special social production that produces objects of knowledge (Althusser et al. 3–79). This is an initial formal identification of scientific practice that of course needs further elaboration. Moreover, we have to take into consideration that there exist two different thinking processes: ideological and scientific thinking process. This is why we have to introduce the concept of epistemological break as the tool for differentiating scientific thought from ideological thought. This makes the epistemological break a necessary constitutive element of theoretical production.

Garfield and his colleagues took a quite empiricist shortcut and presented citation data as a possible instrument to survey and control scientific practices. This is how they presented their visionary claim: 'At the Institute for Scientific Information (ISI), we operate on the fundamental assumption that citation data can be used as indicators of present, past, and perhaps future activity in science.' (Garfield et al. 179–180)

However, what we can get from citations and the reconstruction of ideas, paradigms, methods and concepts (that is, from science mapping) is a mere factographic description of intellectual activities. This remains an empiricist project that tells little or nothing about scientific and/or theoretical production. It may inform us about the spread of ideas in scientific production, but it can also reveal merely a star system in science or uncover an elitist academic network. Within the framework of Garfield's method we cannot determine the nature of the 'facts' established by that method. The history of science in Garfield's sense yields factographic description rather than an examination of scientific production. In order to examine scientific production, we have to take into consideration the specificity of a scientific practice, and examine the way it produces the epistemological break, which differentiates the 'pre-scientific' history of a particular scientific area from scientific practice proper (Althusser 47). These are probably the minimal necessary steps to approach science as a special social production of scientific objects of knowledge.

Citation data are an amazing rough material for further research in the history of science, but cannot pretend to be an aim in itself. But this is precisely what happened. Since 1982, when Thompson Scientific & Healthcare bought Garfield's Institute for Scientific Information, the company offers a citation index and its derivatives (the impact factor and H-index) for evaluating the scientific work of a scientist, department, university or country. Since then, the supply has been extended: Elsevier offered the SciVerse Scopus Database, and Google offered Google Scholar with free access. Under the influence of well-argued criticism (see O'Segen, Cameron, as well as Bornmann and Daniel, for a review of arguments) the databases have been improved, but only partially: the time span in which

citations are collected was extended from two to five years, and H-index replaced the initial impact factor. However, the fundamental argument against the use of citation data for evaluating research has been put aside.

It is difficult to understand why scientific communities so easily accepted citation data for evaluating research, something that is contestable as a method and inappropriate for evaluating research work. This is surprising because the method itself is probably in many aspects inconsistent with the social science methodology; from the epistemological point of view, it contradicts the basic conditions of science as social practice; from the point of view of the people involved, it jeopardises the very existence and work of scientists. As we have seen, the citation data method combines a number of detrimental features: the weakening of epistemological reflection in scientific production (the question of topical method, the epistemological break); the scientists' incapacity to control and question the changing conditions of work (such as publishing and evaluation systems); and as a consequence, the servile attitude towards external ideological demands and expectations. These features are systematically interconnected and equally harmful to the 'soft' and the 'hard' sciences. They indiscriminately challenge all the sciences to reclaim their social role and to act socially and politically. There is no science without social and political involvement.

What is the proposition on the other side? To answer this question we should look at the final argument why citation data should nonetheless be considered an appropriate method for evaluating research, as it was proposed by Bornmann and Daniel. The authors meticulously present arguments for and against citation indexes. They conclude their examination with an assessment that at the micro-level, at the level of local scientific production, there is a greater possibility that a citation does not reflect the scientific impact of the work cited. Authors more often cite works by authors with whom they are personally acquainted, they may build up reciprocal exchange of citations to help each other, etc. Therefore, the low aggregation level of citation data is likely to produce, according to the authors, results that do not reflect the scientific impact of the work. But at the high aggregation level such distortions disappear, since the highly cited 'work […] is accepted by the relevant scientific community as important and correct (the core of research), and it is more or less uninfluenced by social variables and processes' (Bornmann and Daniel 70). This argument is a real acrobatic feast that makes possible a deduction from all the negative premises valid for local scientific communities a positive conclusion applicable to a larger scientific community, although this presumed larger scientific community consists but of local scientific communities. At the high level of aggregation, where institutional world hegemony really operates, academic con-

nivance and hierarchical conformism are washed out, and, presumably, the truth appears. The truth of the power relations, one should add.

Conclusion: Science on the stock exchange

A quite possible outcome of the processes outlined above is scientific work as an investment on the stock exchange. Publishers have transformed academic publishing into a close equivalent of a stock exchange with a system of quantification and valuation of items such as publication, citation, rejection of articles, impact factor, H-index, etc. The process of 'securitisation' transforms non-monetary values into quantitative values that can enter into the process of monetisation of scientific data. Quantitative values created in this process are exchanged by authors for university posts, research funding, rewards and prestige; the national funders use them as quantitative research funding criteria and as international score rates of national scientific competitiveness; and publishers use them to plunder public funds for education and research. The system seems to work well as it binds its agents together by a network of reciprocal obligations and benefits. Publishers have thus created a binding system of 'monetary dependence' where scientific work itself cannot find its appropriate price.

NOTES

[1] Recently, both the current Prime Minister and the Minister of Science justified severe cuts in the financing of public high education by claiming that Slovenian universities should make a better use of their 'internal reserves' and adding that the average university teacher works four to six hours per week.

[2] A suggestion that comes close to Geertz's idea was formulated by Claude Lévi-Strauss in his 1950 'Introduction à l'œuvre de Marcel Mauss' (Lévi-Strauss). For Lévi-Strauss, Maussian 'total social fact' should be conceptualised both as a 'Durkheimian thing' and as a 'native representation'. It should be noted that Lévi-Strauss proposed a way out of Geertzian dead-end even before its appearance.

[3] I use the wording 'capitalist interests' insofar as these are the real interested party behind the 'state interests'.

[4] In this case, it would pertain to the 'cumulative' phase of a scientific practice located between two 'ruptures' of the theoretical problematic: this is the typical epistemic situation of the applied sciences and also of peripheral practices in those sciences where epistemic 'ruptures' are conditioned by important financial inputs. Applied sciences are favoured by the capital interests in general, while the peripheral practices are politically and institutionally dominant in peripheral zones such as Slovenia.

[5] See footnote 4.

[6] Althusser's thesis that the dominant ideology unifies various ideological apparatuses should be extended into a thesis that the dominant ideology also unifies the field of regional apparatuses such as the scientific ideological apparatus in its various modes of material existence of (scientific) ideology: universities, academies of science, research institutes, etc. In particular, the dominant ideology determines the criteria of financing, the mode of financing (mostly 'by projects', in order to secure efficient control), criteria of recruitment of the personnel and modes of employment of personnel (mostly precarious, in order to undermine the solidarity between scientists and to subordinate them to the requirements of capital and the state). The way how the dominant ideology integrates the scientific field is increasingly in contradiction with the logic of scientific practices: it promotes individualism and competition where practices are collective and co-operative, it imposes short term utilitarianism for capital where practices are in principle long-term and have their own specific criteria of 'utility'. In the EU, integration by the dominant ideology has all but excluded theoretical practices and their agents from scientific institutions (including universities) and from the system of financing. (See Breznik and Močnik.)

[7] For more on the Slovene research strategy, see Žagar and Korsika, eds.

[8] The current tendency of scientific and academic institutions towards the abolishment of the institutionalisation of the epistemological break, that is, of the separation between theoretical and ideological practices, is destroying 'the separation of the principle of power, the principle of law and the principle of knowledge', considered by Claude Lefort as the central and 'unprecedented event' constitutive of modernity and its political emancipation (Lefort 65n8).

[9] For the first documented application of a citation index, see Gross and Gross.

[10] I say 'to a certain extent' because there are various reasons why one cites a certain work, and they do not always reflect scientific impact on the author.

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