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Revisiting the "Quiet Debut" of the Double Helix: A Bibliometric and Methodological note on the "Impact" of Scientific Publications

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Abstract. The object of this paper is two-fold: first, to show that contrary to what seem to have become a widely accepted view among historians of biology, the famous 1953 first *Nature* paper of Watson and Crick on the structure of DNA was widely cited – as compared to the average paper of the time – on a continuous basis from the very year of its publication and over the period 1953–1970 and that the citations came from a wide array of scientific journals. A systematic analysis of the bibliometric data thus shows that Watson's and Crick's paper did in fact have immediate and long term impact if we define "impact" in terms of comparative citations with other papers of the time. In this precise sense it did not fall into "relative oblivion" in the scientific community. The second aim of this paper is to show, using the case of the reception of the Watson–Crick and Jacob–Monod papers as concrete examples, how large scale bibliometric data can be used in a sophisticated manner to provide information about the dynamic of the scientific field as a whole instead of limiting the analysis to a few major actors and generalizing the result to the whole community without further ado.

Keywords: DNA, J.D. Watson, Francis Crick, bibliometrics, co-citation mapping, citation analysis, scientific impact, Jacques Monod, François Jacob

In the field of the history of molecular biology we have observed, over the past ten years, a tendency to "revisit" the dominant narrative that made the double helix papers of 1953 by Watson and Crick a crucial event that transformed modern biology. Soraya de Chadarevian recently wrote: "it is now widely accepted that James Watson and Francis Crick's model of the structure of DNA did not make immediate *impact*."[1] Though the term "impact" is not defined, her statement is backed by a reference to a brief paper by Robert

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[1] De Chadarevian, 2006, p. 707.

Olby published in *Nature* on the occasion of the 50th anniversary of the publication of the DNA structure and to the author's own previous work where she argued, on the basis of interviews, press coverage and archival documents that the discovery of the DNA structure was not considered important until the 1960s. She writes, for example, that "a letter addressed to the secretary of the MRC by Bragg at the end of the 1950s *confirms the relative oblivion* surrounding Watson and Crick's work on the double helix."[2] Though it may be true that DNA was not a big thing in Bragg's laboratory, one can hardly generalize from such a local reaction to the whole community. In the same vein, Robert Olby titled his contribution "Quiet debut for the double helix." Its headline stated that "the historical record reveals a *muted response* by the scientific community to the proposal of [the DNA's] structure in 1953."[3] Three months later another historian, Bruno Strasser, repeated the same message that "we usually think that the double-helix model acquired *immediate and enduring success*," but that "on the contrary, it enjoyed only a 'quiet debut'," the author then referring the reader to Olby's piece.[4] Finally, a few years later, biologist Peter A. Lawrence, in an opinion piece criticizing "the mismeasurement of science" based on citation analysis, took that new wisdom as a fact and wrote that "the most important paper of the 20th century *was cited rarely for the first ten years*" again sending the reader to Olby's piece in *Nature* for the "proof" of that assertion.[5]

Reconstructing the Deconstruction

Given the central place held by Obly's brief piece in *Nature* as an argument for concluding that the original DNA paper did not have the immediate scientific impact most scientists and historians believed it had, we must first look more closely at the data he used to back such a counterintuitive conclusion and at their interpretation. Following that, we will provide a systematic analysis of the bibliometric data for the Watson–Crick paper and show that it did in fact have immediate and long term "impact" if we define impact in terms of comparative citations with other papers of the time. In this precise sense it did not fall into "relative oblivion" in the scientific community. We will also construct the co-citation network in which the DNA paper was incorporated and follow over the first ten years its circulation and diffusion in different specialized journals.

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[2] De Chadarevian, 2002, p. 243, our emphasis.

[3] Olby, 2003, p. 402

[4] Strasser, 2003, pp. 803–804, our emphasis.

[5] Lawrence, 2007, pp. R584–R585, our emphasis.

We think that this fine-grained methodological analysis is useful to give some content to terms like “impact,” “reception” or “success” often used but rarely defined in a way that makes their content testable. For what exactly mean “lukewarm reception” or “relative oblivion” if no comparison is made with other contemporary work? The example of the Watson–Crick paper thus provides a methodological illustration of the information that can be obtained from a more systematic analysis of bibliometric data that goes beyond the mere counting of citations and also takes into account the function of different journals in the dynamic of a scientific field. Such a global analysis also offers a way to take the notion of scientific community (or scientific field) as an object of analysis instead of looking only at a few major actors and then generalizing implicitly to the whole community. So, we will not ask here why a given individual did or did not cite Watson and Crick paper but we will look at the global reception of that paper in the scientific field, using tools appropriate to that level of analysis.

First, let us distinguish more clearly between the reception of a discovery by the general public through newspaper articles and by the researchers active in the scientific field.[6] The first indicator used by Olby to suggest a “lukewarm reception” of the DNA paper was the dearth of press coverage in the major British newspapers in the immediate aftermath of its publication. Being interested here in the analysis of the evolving visibility of the DNA paper in the scientific community, we will not discuss the reception in the press which is a different matter pertaining more to the complex question of the social determinants of media choices than to the scientific impact and reception of the DNA paper among scientists. In other words, press coverage says more about newspapers’ interests at the time than about scientists’ interests in DNA and thus cannot serve as a valid indicator of its impact among scientists.[7] The second indicator used by Olby is more germane to the scientific community itself as it is based on “the number of papers in *Nature* reporting on any aspects of DNA, and of these the number that mention the Watson–Crick model or cite any of the 1953 papers on DNA structure.” His results show that up to 1960, references in *Nature* were few, less than five each year. Noting that “the pattern of citation in *Science* is similar,” Olby concludes to the “muted reception of the structure” or its “lukewarm reception” and then goes on to explain why there was no special reason “for giving the DNA double helix more than passing attention.”[8]

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[6] For an analysis of the introduction of DNA in textbooks, see Winstanley, 1976.

[7] For an analysis of science coverage in the media see LaFollette, 1990 and Nelkin, 1994.

[8] Olby, 2003, p. 402.

Interestingly, the nature and characteristics of this indicator (using *Nature* as a gauge of scientific interest) are never discussed and thus implicitly taken as an obvious measure of "impact". But as with any indicator, one must look carefully at the meaning of the numbers obtained. *Nature* is a multidisciplinary journal covering potentially all scientific fields, not the standard scientific journal of a discipline or a specialty. Its social function in the scientific field is to provide an outlet for a short announcement of a major discovery whose details will then usually be published in the more standard central journals of the discipline or specialty. So, in order to decide if there was a "muted" or "lukewarm" reception of a given scientific paper originally published in *Nature* (or *Science* for that matter as this magazine occupies a similar position in the field of scientific journals) one should look at all the journals associated with the research domain of that paper.[9] Also, given that the practice of citation varies greatly between fields and depends also on the number of active researchers in the field (which can be quite small), absolute numbers of citations have no intrinsic meaning. Hence, one must construct a population of comparable papers in order to see if many (or any) other papers have been more cited than the one under study (here the Watson and Crick papers in *Nature*).[10]

Methodology and Sources

Using the Thomson Scientific Web of Science bibliographic database,[11] which contains 3,697 journals in all scientific disciplines for the period

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[9] This role of generalist journal concentrating on major discoveries dates back at least to the beginning of the 20th century and is not recent nor linked to the multiplication since the 1990s of specialized *Nature* journals devoted to special topics like *Nature Genetics*, *Nature Immunology*, etc. For example, the publication in *Nature* in 1932 of the discovery of the neutron by James Chadwick was taken on immediately and cited in physics journals and not much in *Nature* itself. Thus, in the period 1932–1939, only two of 21 citations are in *Nature*, all others coming from various physics journals.

[10] In his paper, Strasser uses the distribution of citations to the DNA paper over the period 1953–2003 to conclude that it became "immensely popular in the scientific community" only in the 1990s. But such a conclusion is not really warranted because the author compares the ups and down in the curve without looking at comparable papers of the time. Moreover, it is obvious that after 1970, many of the citations are of a historical nature. Notwithstanding the ambiguity of the term "popular," the only way to define it in a meaningful way at a given time is to compare the results for a given period, say 1953–1970, with other papers of the same period, and to take into account the natural obsolescence of papers as we do in our analysis. One also has to take into account the specialty of the citing journals.

[11] For details see www.thomson.com.

1953–1970, citations to the Watson–Crick original paper were first searched in all the journals.[12] The scientific community is here defined as all the authors of scientific papers contained in the Web of Science over the period 1953–1970. This first search was done on 3,842,584 publications containing 40,700,360 references in all disciplines. As could be expected, the citations were found essentially in the specialties related to crystallography, microbiology, biochemistry, genetics and the likes: 230 journals covering 1,024,752 papers containing 12,643,436 references. This very large sample is certainly representative of the global scientific activity at the time and thus provides an operational definition of the scientific field, as defined by the sociologist Pierre Bourdieu as, in essence, the space in which scientists evolve and compete for the accumulation of symbolic capital.[13] As we will see below, the structure of this space can be mapped using co-citation analysis and social networks techniques. In short, our database, which acts as a kind of virtual laboratory for history and sociology of science, includes all scientists whose research is published in major scientific journals of the times. It thus covers questions related directly or indirectly to DNA, molecular genetics and biochemistry more generally as well as most other research topics like virology, biochemistry, crystallography, general and theoretical biology.

Nature's Most Cited 1953 Papers

In order to measure the impact of the Watson–Crick original paper identifying the structure of DNA (hereafter "DNA paper" for short) we must compare the evolution of its citations over time with potentially comparable papers.[14] I have thus followed the citations over the period

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[12] In about a dozen cases, the paper was attributed to Crick as first author, so that all combinations of authors have been checked when looking for citations at multi-author papers.

[13] See Bourdieu, 2004.

[14] One could of course try to distinguish "visibility," directly given by citations, and reserve "impact" for a more profound effect on scientists. The problem with that distinction is that "impact" is the term the most frequently used in practice even by those who used citations to conclude to the weak impact of the DNA paper. More importantly, it is in fact difficult to distinguish the two terms as a very highly visible paper without "impact," even if only short term, seems improbable. Having an impact is having an effect on something and even in cases where scientists do not agree with a paper they cite, this paper is having an effect and thus some impact. One should also stress that having an impact in a community is not related to being right: a paper that happens to be wrong can have a great impact (even if only in the short term), as the example of the "cold fusion" episode shows. It is only in hindsight and after some years of further research that one can say it was "wrong." .

1953–1970 to all *Nature* papers published in 1953, the year the first two DNA papers by Watson and Crick appeared. Given that *Nature* usually contains only major contributions, it is fair to compare Watson and Crick's papers to other potentially major scientific announcements of the same year. This sample has also the advantage of containing the second paper of Watson and Crick as well as those of Maurice Wilkins and Rosalind Franklin. We stop the analysis in 1970 since it is safe to say that the discovery of the DNA structure has by then become part of accepted science and the ''reception'' period is closed.

The first striking result of our analysis is that the Watson–Crick paper published in the April 25th issue of *Nature*, stays the most cited paper over all the 3-year periods spanning the years 1953–1970 among the 1,737 items published in that journal in 1953 (Table 1).[15]

As the average citations per paper published that year in *Nature* show, the most cited papers are high above the average for all periods though they suffer the usual decline over time known as obsolescence.[16] Interestingly, their second paper published in the May 30th issue of the same year, raised from a fifth position in the period 1953–1955 and 1956–1958 to a third position during the next period and kept the second place from the year of the Nobel Prize (1962) to 1970. As can be seen from Table 1 Wilkins' and Franklin's papers which follows immediately Watson and Crick's first paper in the same issue of *Nature*, are much less cited over the period, but they stay among the top 20 list over all the periods. Note also that Wilkins' contribution is always higher in ranking that Franklin's. Among the most cited *Nature* 1953 authors, we also find F.S. Sjöstrand paper on electron microscopy of mitochondria and cytoplasmic double membranes as well as that of R.E. Billingham and his collaborators L. Brent and P.B. Medawar on actively acquired tolerance of foreign cells.

From this first set of data, it is clear that the first DNA paper was cited early and continually and that it cannot be considered as a ''sleeping beauty'': a paper never cited for a long period and then suddenly rediscovered and highly cited.[17] The same is true for Watson and Crick second paper in *Nature*.

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[15] Among the 1,737 items, 72% (1,252) were cited at least once, the rest having no citation over the period studied. The average is calculated on the total number of published papers. We use three years periods to get better statistics but it is obvious that despite some annual fluctuations, the paper is still among the first two most cited *Nature* 1953 papers every year. Hence, over the 8-year period 1953–1960, the paper is first 5 times and second 3 times. It then stays first every year until 1974.

[16] Line, 1993; Larivière et al., 2008.

[17] On ''sleeping beauties,'' see Van Raan, 2004.

Table 1. Most cited papers published in *Nature* in 1953

Cited paper	Number of citations per period						
	1953–1955	1956–1958	1959–1961	1962–1964	1965–1967	1968–1970	Total
WATSON-JD_NATURE_1953_171_737	88	137	139	192	122	105	783
BILLINGHAM-RE_NATURE_1953_172_603	40	104	107	102	56	46	455
WATSON-JD_NATURE_1953_171_964	35	59	65	120	62	54	395
SJOSTRAND-FS_NATURE_1953_171_30	86	93	40	32	22	6	279
LEMBECK-F_NATURE_1953_172_910	23	64	40	28	26	19	200
SMITH-I_NATURE_1953_171_43	14	21	38	30	56	40	199
JEPSON-JB_NATURE_1953_172_1100	15	31	31	40	35	22	174
MUNCHPETERSEN-A_NATURE_1953_172_1036	26	48	29	22	16	9	150
SLATER-EC_NATURE_1953_172_975	15	22	25	18	31	34	145
WADE-HE_NATURE_1953_171_529	15	24	30	26	28	18	141
BENNETCLARK-TA_NATURE_1953_171_645	25	25	22	19	24	23	138
WILKINS-MHF_NATURE_1953_171_738	34	29	17	22	13	13	128
PAULING-L_NATURE_1953_171_59	20	21	22	26	20	15	124
HANSON-J_NATURE_1953_172_530	12	29	15	22	24	21	123
MITCHISON-NA_NATURE_1953_171_267	12	28	22	24	11	11	108
NEUBERGER-A_NATURE_1953_172_1093	24	35	12	9	9	12	101
CHARGAFF-E_NATURE_1953_172_289	23	37	19	11	5	6	101
BROWN-GL_NATURE_1953_172_339	25	34	20	11	4	5	99
ALBERT-A_NATURE_1953_172_201	14	20	13	15	22	10	94
FALK-JE_NATURE_1953_172_292	36	18	15	10	6	7	92
FRANKLIN-RE_NATURE_1953_171_740	26	16	15	9	12	12	90
Average citations per paper (1,737 papers)	2.44	2.08	1.49	1.29	1.09	0.96	9.35

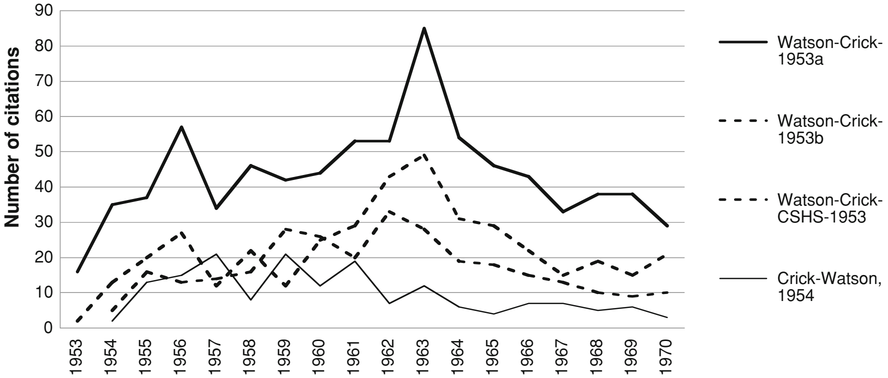

Figure 1. Citations to Watson and Crick four papers on DNA (1953–1970).

Another way to analyze the impact of Watson and Crick's identification of the structure of DNA is to look at the comparative citations of the other papers associated with this event, that is the other *Nature* paper (Watons–Crick 1953b); the *Proceedings of the Royal Society*'s piece first authored by Crick and published in 1954 (Crick–Watson 1954) and the 1953 presentation at the Cold Spring Harbor Symposium (Watson–Crick—CSHS 1953). As we can see in Figure 1, the two *Nature* papers are the most frequently cited followed by the Cold Spring Harbor one. In fact, among all the Cold Spring Harbor papers, the Watson and Crick contribution ranks 16th among the list of most cited items from that journal.[18] The four Watson and Crick papers are well above the average of the cited papers in *Nature*. We also clearly observe in these graphs the "Nobel effect": a surge in citations immediately after the attribution of the prize to Watson, Crick and Wilkins in 1962. The decline that follows after 1963 is related to the usual obsolescence of scientific papers whose content become incorporated into the scientific community and needs no more explicit citations.[19] As can be seen from Table 1, this Nobel effect also applies to Wilkins but has no effect on Franklin's paper since she did not get the prize though her work was closely related to the ones of the winners.[20]

Scientific papers are thus characterized by a citation history and we can analyze the specificity of the life-history of the DNA paper by looking at its citations over a long period and compare them to the average citation life of the other *Nature* papers and also to another famous paper in the same field, for example that of François Jacob and

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[18] Over the period 1945–1988; see Witkowski, 1990.

[19] On the phenomenon of obliteration by incorporation see Merton, 1968.

[20] For more details on Franklin's contributions, see Maddox, 2002 and Elkin, 2003.

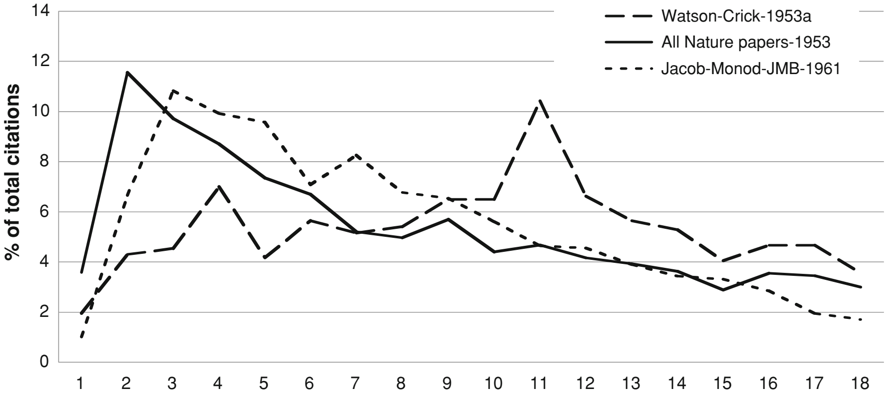

Figure 2. Comparative distribution of the % of citations obtained by each paper over a 18-year period from publication year.

Jacques Monod on "genetics regulatory mechanisms in the synthesis of proteins" published in the *Journal of Molecular Biology* in 1961 and for which they shared the Nobel prize in 1965 with André Lwoff. Figure 2 shows that the "average paper" in *Nature* shows the usual rapid peak about one year after publication followed by a rapid and continuous decline. Though the Jacob–Monod paper is highly cited, its general curve follows the standard one but with a longer initial peak.[21] If we consider their "half-life," that is the number of years it takes to gain 50% of their total citations, we see that both the Jacob–Monod and the average paper have a half-life of about 7 years. By contrast, the Watson–Crick paper has a half-life of about ten years and its pattern of evolution is very distinctive as it does not really decline over the first twelve years. This suggests that it has rapidly acquired a special status as a kind of "iconic" or "totemic" paper that makes it being cited well after the usual period of decline.[22]

Journals Citing Watson and Crick

Having established that Watson's and Crick's *Nature* papers were taken on and discussed as soon as they were published and that they never went into relative oblivion during the decade following their publication, let us look now at the journals which cited the first paper. As could be expected,

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[21] We use a distribution normalized to the total number of citations (100%) to facilitate comparisons between papers. The citations for each year are expressed as a proportion of the total number of citations received over the 18-year period.

[22] Ahmed et al., 2004.

the first citations in 1953 are mainly from *Nature* – which publish rapid communications and thus minimizes the time lag between reading a paper in a journal and writing one that could cite it – and from the *Cold Spring Harbor Symposium* where Watson and Crick cite their first paper, then just published. Not surprisingly, we also find *Acta Cristallographica* among the first citing journals. Also, the second paper by Watson and Crick obviously refers to the first. Over the whole period however, self-citations remain very low as the two authors contribute only 8 citations to the total of 783 citations over the period.[23] In 1954, *Nature* is second behind *Biochemica and Biophysica Acta*, the third being the *Proceedings* of the US National Academy of sciences. These stay the top three journals in 1955. In 1956 the *Journal of the American Chemical Society* comes first, followed by the two previously mentioned journals while *Nature* drops in 9th place. As shown in Table 2, the distribution confirms the fact that *Nature* is a journal where to "trumpet" a major discovery whose discussion then continues in the specialty journals of the field. *Nature's* proportion of total citations is 16% in the first period (1953–1955) and 7% in the period just preceding the Nobel Prize in 1962 (1959–1961). The diffusion of Watson's and Crick's paper is obvious also in the fact that, as time flows, more and more journals are citing it: from 7 in 1953 to 19 the following year, 22 in 1955 and 39 in 1956. In 1961, the year before the Nobel Prize, 35 different journals cited the original paper at least once. In 1963, probably as an effect of the Nobel prize, 55 different journals cited the paper, that number went down to 36 in 1964, closer to the average of 30 citing journals for the whole period. Note that since the citing journals vary from year to year a total of 230 different journals did cite the paper at least once over the period 1953–1970. But given the high concentration of the citations in a few journals, 50% of the citations are contained in only 22 journals that is 9% of the total. That is to be expected as there are always core journals for a given specialty or discipline.[24] If we define the core as journals having cited the paper at least twice for every three-year period, we get an average of 23 journals over the 1953–1970 time span.

Growth and Structure of the Field

It is obvious that the whole field of biology at the time is very diverse and that researchers interested in DNA comprise only a very small part

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[23] That is much less than the usual 10% of self-citations observed for the decade of the 1980s; see Snyder and Bonzi, 1998.

[24] In fact, the distribution tends to follow Zipf law of concentration; see Zipf, 1949.

Table 2. Journals citing Watson–Crick paper most frequently (1953–1970)

Citing journal	Number of citations per year									
	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962
NATURE	5	4	5	1		2	3	2	5	5
BIOCHIMICA ET BIOPHYSICA ACTA		10	5	5	3	4	3		3	3
PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES, USA		4	4	4	1	2		1	3	2
JOURNAL OF MOLECULAR BIOLOGY							4	2	4	2
JOURNAL OF THE AMERICAN CHEMICAL SOCIETY		1	1	6	3	2	3	1	1	1
SCIENCE		1	1		1	1		1	2	
JOURNAL OF BIOLOGICAL CHEMISTRY		1		1	2		3	3	2	2
ACTA CRYSTALLOGRAPHICA	2		1	1		2	3		1	
ANNUAL REVIEW OF BIOCHEMISTRY		1	2	1	1			2	2	1
BIOCHEMICAL JOURNAL	1			2		1		1		2
ARCHIVES OF BIOCHEMISTRY AND BIOPHYSICS	1		1	1	1	1			1	1
BIOPOLYMERS										
COLD SPRING HARBOR SYMPOSIA ON QUANTITATIVE BIOLOGY	5			1		2				
JOURNAL OF THEORETICAL BIOLOGY										
NATURWISSENSCHAFTEN								1	1	
JOURNAL OF BIOPHYSICAL AND BIOCHEMICAL CYTOLOGY			1	2	3	1	1		2	
Total number of citing journals	7	19	22	39	26	31	26	34	35	37

Table 2. continued

Citing journal	Number of citations per year								
	1963	1964	1965	1966	1967	1968	1969	1970	Total
NATURE	7	3	5		2	2	2	2	55
BIOCHIMICA ET BIOPHYSICA ACTA	5	1	3	1	1		1	1	49
PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES, USA	3	4			2	1	2	2	35
JOURNAL OF MOLECULAR BIOLOGY	3	5	6	3	2	2	1		34
JOURNAL OF THE AMERICAN CHEMICAL SOCIETY	1		1		1	3		1	26
SCIENCE	3	2	2	2			1	2	19
JOURNAL OF BIOLOGICAL CHEMISTRY	2				1				17
ACTA CRYSTALLOGRAPHICA	2		2	2					16
ANNUAL REVIEW OF BIOCHEMISTRY	1	1	1	1	1				15
BIOCHEMICAL JOURNAL	2	1	1		1	1			13
ARCHIVES OF BIOCHEMISTRY AND BIOPHYSICS		1	2	2					12
BIOPOLYMERS	2	5				2	2	1	12
COLD SPRING HARBOR SYMPOSIA ON QUANTITATIVE BIOLOGY	2						1		11
JOURNAL OF THEORETICAL BIOLOGY	1	1		4	3		1		10
NATURWISSENSCHAFTEN	3				2	1	1	1	10
JOURNAL OF BIOPHYSICAL AND BIOCHEMICAL CYTOLOGY									10
Total number of citing journals	55	36	31	34	26	30	33	24	230

of the field in which many other topics are studied. As in all disciplines, some specialties are more populated than others and the field is thus structured into specialties of different sizes. This of course affects the potential citations and must be taken into account when making comparisons between specialties. We can get a sense of the size of the group interested in DNA by noting that, in 1953, ten different authors cited the DNA paper (excluding self-citations), a number that went up to 28 the next year and to 33 in 1955. In the following years until 1961, the year before the Nobel Prize, the annual number of researchers citing it was on average, 42 with a range between 31 and 51. The effect of the Nobel prize made the number of people citing the paper go up artificially to 79 in 1963, the next year going down to 47, a number closer to the average (42) of the period. Over the 18-year period (1953–1970), this amounts to 557 authors having cited the original DNA paper at least once. If we define the core of researchers by looking at those who cited their paper at least twice, the number of researchers rapidly goes down to 124. For the period before 1962, we get only 30 authors (excluding Watson and Crick). In a sense we can say the invisible college[25] closely interested in the structure of DNA is not larger than 50 and is probably of the order of 30.

Let us compare this group with the one we can define around the work of Monod and Jacob on genetic regulation in the synthesis of proteins. The year before Watson and Crick got the Nobel Prize, François Jacob and Jacques Monod published a major paper in the *Journal of Molecular Biology*, for which they will receive (with André Lwoff) the 1965 Nobel Prize in Physiology and Medicine. We have already seen that their paper follows a more standard pattern of citations than the one of Watson and Crick. In absolute numbers however, coming nearly a decade after the identification of the DNA structure, their paper, which discusses the mechanism of the synthesis of proteins, is of interest to a much larger community and thus gets citations from a larger group of researchers. While already 20 authors cited it in the year of its publication, that number surged to 150 in 1962 and 250 in 1963 and then fell to 228 and 224 in the next two years and stayed at 165 in 1968 and 1969. The average over the period 1962–1970 is 187, much larger than the 42 citing the original DNA paper. Here also we can restrict the core to those who cited their paper at least twice over the 18 year period; we then get a total of 378 researchers compared to 124 for Watson and Crick.

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[25] On invisible colleges see Crane, 1972.

Table 3. Journals citing Jacob and Monod's *Journal of Molecular Biology* 1961 paper most frequently (1961–1970)

Citing journal	Number of citations per year										
	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	Total
JOURNAL OF MOLECULAR BIOLOGY	2	8	14	20	11	16	13	13	10	9	116
BIOCHIMICA ET BIOPHYSICA ACTA		19	28	16	16	11	13	6	5	2	116
PROCEEDINGS OF THE US NATIONAL ACADEMY OF SCIENCES	6	25	18	19	8	7	9	8	6	4	110
NATURE	1	8	14	14	10	8	8	3	6	2	74
JOURNAL OF BACTERIOLOGY		5	5	6	8	5	8	12	10	7	66
JOURNAL OF BIOLOGICAL CHEMISTRY		4	4	11	9	8	10	2	5	1	54
GENETICS		4	4	4	3	3	9	5	4	4	40
COLD SPRING HARBOR SYMPOSIA ON QUANTITATIVE BIOLOGY	10	1	13	3		4		1		6	38
BIOCHEMICAL AND BIOPHYSICAL RESEARCH COMMUNICATIONS		11	6	3	3	2	7	1	1	2	36
SCIENCE		2	7	6	9	3	2	3	2	2	36
BIOCHEMICAL JOURNAL		3	3	6	3	5	6	5	4		35
JOURNAL OF THEORETICAL BIOLOGY		3	3	1	5	3	2	5	2		24
VIROLOGY	1	1	4	3	3	1	3	3	3	2	24
JOURNAL OF GENERAL MICROBIOLOGY		3	2	5	2	2	4	1	3	1	23
BULLETIN DE LA SOCIETE DE CHIMIE BIOLOGIQUE		2		7	5	2	4	2		1	23
CANCER RESEARCH		1	4	5	2	2	2	1	1	2	20
ANNALS OF THE NEW YORK ACADEMY OF SCIENCES		1	6	3	2	2		3	3		20
ANNUAL REVIEW OF BIOCHEMISTRY		2	3	3		2	2	2	2	1	17
MOLECULAR AND GENERAL GENETICS							3	5	3	6	17
ANNUAL REVIEW OF MICROBIOLOGY		3	5	1		1	4		1	2	17
EXPERIMENTAL CELL RESEARCH		1	5	1	3	2	2		1	1	16
NATURWISSENSCHAFTEN			2	4	6	1	2		1		16
COMPTES RENDUS DE L' ACADÉMIE DES SCIENCES		5	5	4	2						16
Total number of citing journal	10	62	103	98	118	97	101	96	95	85	537

From the point of view of citing journals, we have a similar pattern than the one observed above for Watson and Crick. Instead of the average mentioned above of 30 journals citing Watson and Crick original paper, we have about three times more with an average of 86 different journals citing the Jacob–Monod paper each year at least once. That number goes down to an average of 33 journals a year if we count only those who cited it at least twice a year, just ten more journals than for Watson and Crick whose average was 23. As is to be expected, 50% of the total of citations are contained in only 23 journals; that is only 4% of the citing journals. Those can be considered as the core journals in that field (Table 3). Not surprisingly, many of the citing journals (10) are the same for Jacob–Monod and for Watson–Crick.

The difference in levels of citations between the two groups can be explained in large part by the growth in the active population of publishing researchers during the decade between the DNA paper (1953) and the paper on genetic regulation (1961). Using the bibliometric data, we can get a good idea of the number of researchers active in the field of biology by counting the number of researchers publishing in a set of core journals over the period. Figure 3 shows the trend in the growth of papers published and number of distinct authors involved in the 16 major journals in which we find the major part (nearly half of the total) of the citations to the DNA papers. The number of publishing researchers more than doubled over the period, growing from about 7,500 in 1953 to more than 18,000 in 1970. Of course if we extend the field to include more journals and thus more subfields of biology we get more people but the overall trend is the same with the number of active researchers being multiplied by 2.5 between 1953 and 1970. Since we are interested here in the subset which is the nearest to the topics addressed by Watson and Crick Figure 3 present the data for this sub-group of 16 journals.[26] The number of researchers involved grow faster than the number of papers as most of them are the fruit of collaborations: 70% of the papers are signed by more than one author, a proportion that goes up to 80% by the end of the period, and about 25% of the papers have 3 or 4 authors. It is thus normal to observe a growth in the absolute number of citations received by papers as time goes on and this fact must be borne in mind when comparing papers whose date of publications differ by more than a couple of years.

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[26] If we use 108 journals, the number of authors grows from 20,391 in 1953 to 51,847 in 1970. With 230 journals we go from 27,227 authors to 70,340 over the same period. In all the cases the growth ratio is about 2.5.

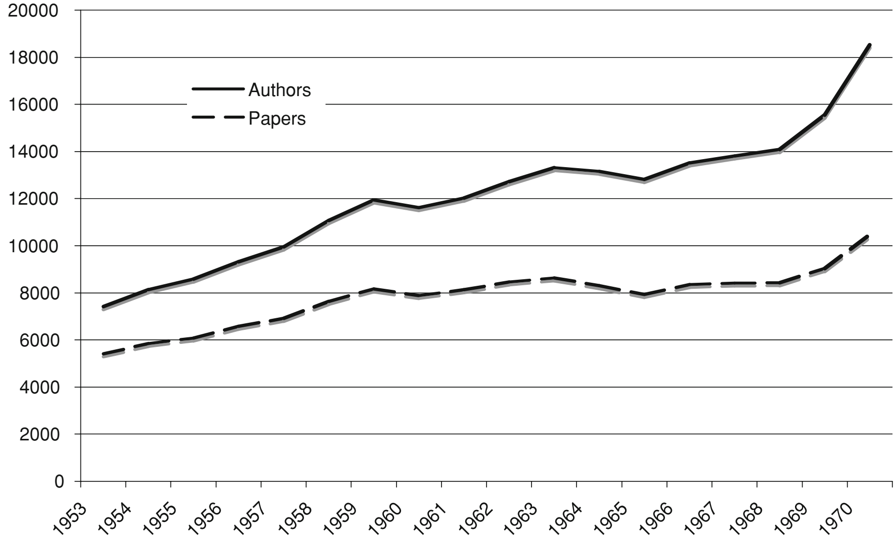

Figure 3. The growth of research in biology as measured by number of papers and distinct authors in the 16 major journals citing the DNA paper (3-year moving average).

The Changing Centrality of Watson and Crick in the Structure of the Field of Biology

In order to follow the changing centrality of J.D. Watson over time in the fields of biology related to DNA, we have used the 50 journals that cited the Watson and Crick paper at least 4 times over the period studied (1953–1970). Using this database of citations we constructed a map of the cocited first authors in these 50 journals to see the general structure of the field of biology around the time of the publication of Watson and Crick's paper (1953–1955). Defining the field by the content of these journals provide a wide enough definition to cover research going on at the time in biochemistry and molecular biology, virology, as well as most, if not all, other topics in biology. Whereas the total number of citations received by a scientist is a measure of his/her global symbolic capital in the field, the number of co-citations between two scientists offer a measure of their conceptual proximity. The more two scientists are cited *together* in different papers (and thus by different scientists), the more they are conceptually linked to each other.[27] Once

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[27] For more details on co-citation analysis, see Small, 1977, 1978 and Gmür, 2003 for a recent review of the literature.

we have the matrix of co-citations, we can use different methods to represent the results and here we use the tools of network analysis.[28]

Figure 4 presents a section of the whole co-citation network for the period 1953–1955 centered on JD Watson (black dot). As we can see, the density of the links varies greatly and gives us an idea of the different specialties being developed in biology. As could be expected Watson, Franklin and Wilkins form a triangle of highly co-cited first authors. Looking at a similar map about a decade later (Figure 5) give us a different portrait with a much denser patch around JD Watson.

A useful quantitative indicator for following over time the relative position of a given scientist in the network is provided by the degree centrality of all the authors present in the network and ranking the results in decreasing order. Degree centrality measures the number of links an author has with all other scientists in the co-citation network. The larger the number of links with different scientists the more that person is central in the field.[29] A peripheral researcher in the field would be one with very few connections to others and thus situated on the periphery of the map. Drawing co-citation maps for 3-year periods provides a way of measuring the changing structure of the field over time and the changing position of researchers.[30] As time goes on and the network changes, a given scientist can become more central in the network for a period of time and then decline in centrality due to a transformation of the research front and the raising popularity of other research topics.

As Table 4 shows, among nearly 65,000 cited scientists distributed among more than 2,000 ranks (many are ex-aequo in the lower rankings), Watson rapidly become central, moving from the 116th position in the first period to the 51th position just before the year of the Nobel and up

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[28] The tools we used are presented in Borgatti et al., 2002, and Borgatti, 2002. For a general survey on network analysis see Wasserman and Faust, 1994. For more details on the uses of network analysis in history of science, see Gingras, 2007, 2008. For examples of citations analysis applied to historical cases, see Garfield, 1979; Garfield et al., 2003 and McCain, 2008. In his study of the early response to Avery’s 1944 paper on DNA, Deichmann 2004 also uses citation data but only to identify the citing papers for a detailed qualitative analysis of their content.

[29] For details on centrality, see Freeman, 1978/1979.

[30] There is a strong correlation between rankings in terms of centrality and rankings in terms of total citations received. However, whereas citations characterize the node (the scientist), centrality is a relational measure of the position of the scientist in the structure of the field. For examples of evolution in time of networks in the case of physics for the period 1900–1945, see Gingras, 2007 and Wallace et al., 2009 for an application of community detection methods to identify specialties in the whole network of a discipline.

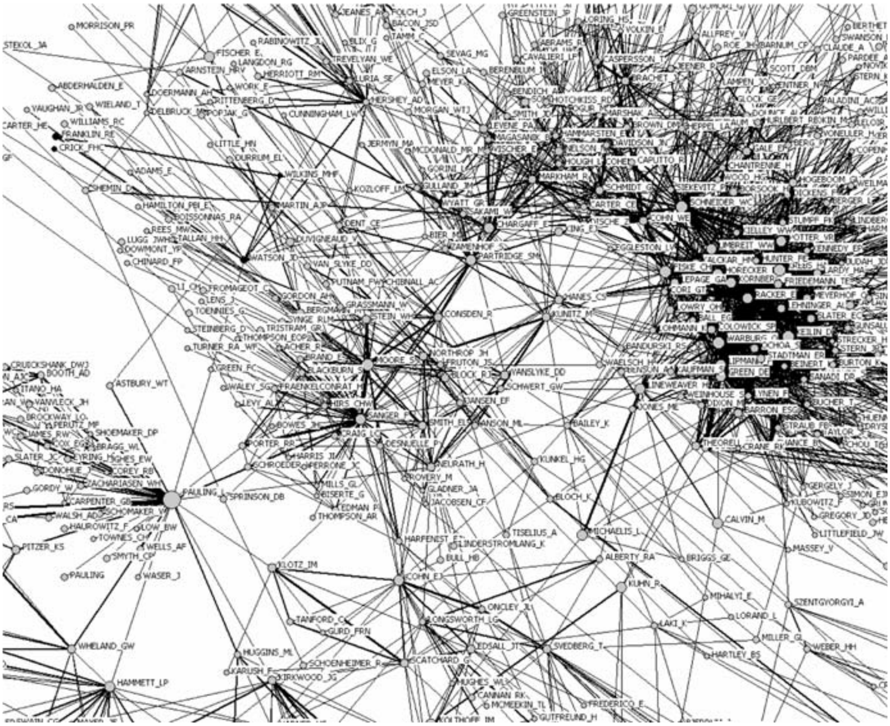

Figure 4. Close up on a section of the first-author co-citation network in biology for the period 1953–1955 based on the 50 journals citing the first Watson and Crick 1953 *Nature* paper at least four times between 1953 and 1970 (links with at last 13 co-citations are shown). The circles are proportional to the total number of citations received by authors during this 3-year period and the thickness of the links is proportional to the number of co-citations.

to 24th in the following period.[31] His centrality goes down after 1965 but stays among the top 100th among a list of 2,000. The rise of Jacob is striking as his research with J. Monod puts him at the center of the field for the whole of the 1960s. By comparison, Linus Pauling, whose contributions are much wider in the field and covers bio-organic chemistry, quantum chemistry and structural chemistry (to name a few), in addition to writing classic textbooks, stays among the top five most

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[31] Given that citations go to first authors, JD Watson (or F. Jacob) is here an index for the Watson–Crick papers (or Jacob–Monod papers). We could also have computed centrality for the paper itself but this would not have been comparable with the other scientists for which we have the citations to all their papers. So, here we use all citations to Watson in order to make rankings more meaningful, knowing that most citations are to the Watson–Crick papers.

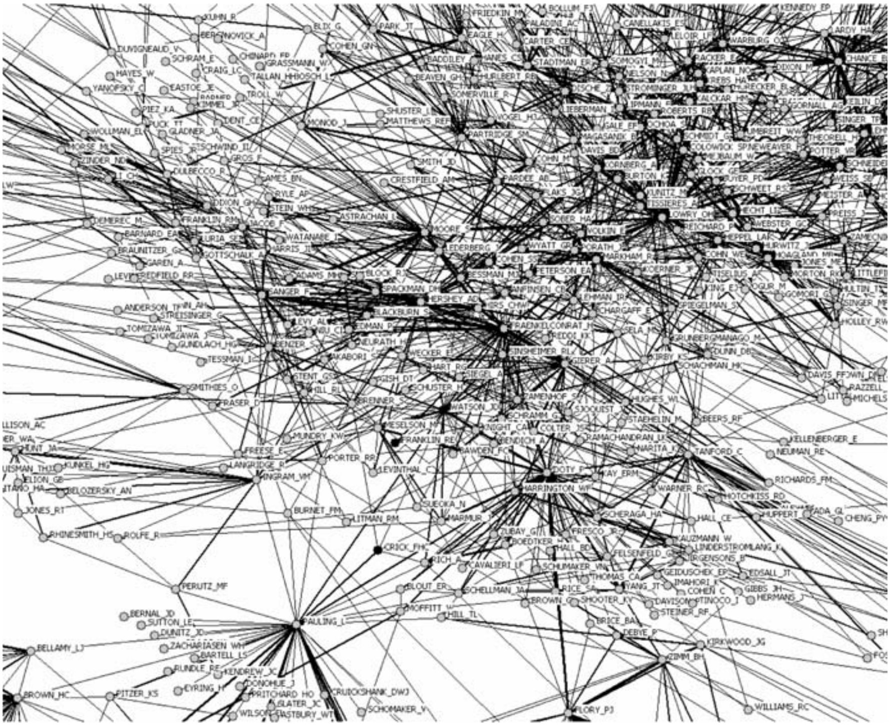

Figure 5. Close up on a section of the first-author co-citation network in biology for the period 1959–1961 based on the 50 journals citing the first Watson and Crick 1953 *Nature* paper at least four times between 1953 and 1970 (links with at last 17 co-citations are shown). The circles are proportional to the total number of citations received by authors during this 3-year period and the thickness of the links is proportional to the number of co-citations.

central scientists over the whole period. He kept being first for the second half of the 1950s and declined slowly as the field was moving toward new topics. For comparison, we also see the rise of Frederick Sanger, Nobel Prize in Chemistry in 1958 for his work on structure of proteins and Arthur Kornberg, Nobel Prize in Physiology and Medicine in 1959 for his work on the synthesis of DNA. Centrality is here closely linked to the total number of citations received and thus to the global symbolic capital of the scientist. By the very diversity of his work, Pauling stayed central in the field of biology (largely defined) for a much longer period than would be the case if his research had been more limited in scope, as is the case for most scientists who tend to focus on a given topic. One could of course measure the centrality of given papers and one would then obtain different maps and rankings.

Table 4. First author centrality ranking for some major scientists of the period

Name	1953–1955	1956–1958	1959–1961	1962–1964	1965–1967	1968–1970
Pauling, L	1	1	2	3	4	5
Kornberg, A	3	10	4	9	26	29
Sanger, F	21	26	29	98	47	47
Watson, JD	116	111	51	24	60	76
Jacob, F	668	719	130	2	6	13

Based on the co-citation networks by 3-year periods in the 50 journals citing the Watson–Crick paper at least 4 times between 1953 and 1970

The general rise of Watson in terms of degree centrality in the whole field of biology again confirms the conclusions obtained using citations: their contributions were immediately taken up by the scientific community and "peaked" around the year of the Nobel Prize. Of course, given the fact that their specialty did not comprise many researchers, their centrality could not be as high as that of Pauling for example or, ten years later that of Jacob whose work touched upon a larger research community. As is the case for the life history of citations, centrality also decline as time goes on, the field moving toward now topics with new researchers, while older central positions move toward the periphery of the field.

Conclusion

The object of this paper was two-fold: first, to show that contrary to what seem to have become a widely accepted view among historians of biology, the famous paper by Watson and Crick on the structure of DNA was widely cited – as compared to the "average paper" of the time – on a continuous basis right from the very year of its publication and that the citations came from more and more journals over the period 1953–1970. Our second aim was to show, using the case of the reception of the Watson–Crick and Jacob–Monod papers as concrete examples, how large scale bibliometric data can be used in a sophisticated manner to provide information about the dynamic of the scientific field as a whole instead of always limiting the analysis to a few major actors and generalizing the result to the whole community without further ado. If historians want to use concepts like "impact" in a meaningful manner and not simply as an empty metaphor or a tautology, it is necessary to provide an operational definition of that term.

The tools we have presented here can provide useful additions to the more standard ones used by historians, based on textual analysis of written documents (published papers, reports, and correspondences). Though it should be obvious that one cannot seriously propose to define or characterize a scientific revolution only by looking at citations, it remains that such quantitative methods make possible a macro-analysis of the scientific community by providing ways to map the changing structure of the field, something that is difficult to obtain from the standard methods, more attuned to the micro-analysis of local exchanges between scientists.

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