

*Recent work has brought to light so many cases, historical and contemporary, of women scientists who have been ignored, denied credit or otherwise dropped from sight that a sex-linked phenomenon seems to exist, as has been documented to be the case in other fields, such as medicine, art history and literary criticism. Since this systematic bias in scientific information and recognition practices fits the second half of Matthew 13:12 in the Bible, which refers to the under-recognition accorded to those who have little to start with, it is suggested that sociologists of science and knowledge can add to the 'Matthew Effect', made famous by Robert K. Merton in 1968, the 'Matilda Effect', named for the American suffragist and feminist critic Matilda J. Gage of New York, who in the late nineteenth century both experienced and articulated this phenomenon. Calling attention to her and this age-old tendency may prod future scholars to include other such 'Matildas' and thus to write a better, because more comprehensive, history and sociology of science.*

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## The Matthew Matilda Effect in Science

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In his autobiography *Enigmas of Chance*, mathematician Mark Kac described his 1980 trip to Poland to deliver the Marian Smoluchowski memorial address in honour of the almost forgotten physicist of the past. Kac attributed Smoluchowski's near obscurity in 1980 not to his death at age 45, nor to his long name, nor to his career in Eastern Europe, but to the 'Matthew Effect' of having been eclipsed by Albert Einstein, who also worked on Brownian motion at about the same time.<sup>1</sup> In 1968, Robert K. Merton named for Matthew of the New Testament a kind of 'halo effect' experienced by well-known scientists who find work attributed to themselves which they did not do (or did not do totally alone), or as written in the Gospel according to Matthew:

For whomsoever hath, to him shall be given, and he shall have more abundance; but whomsoever hath not, from him shall be taken away even that he hath. (13:12)

The interesting thing about Kac's use of this concept was that he employed the second portion. More often the term 'Matthew Effect' refers simply to the former – the over-recognition of those at the top of the scientific profession. That this second use has wide application among the other have-nots of scientific history, including especially women in science, has not been generally recognized.<sup>2</sup>

Although Merton's original article of 1968 had mostly anecdotal documentation (with numerous footnotes to Harriet Zuckerman's 1965 doctoral dissertation), subsequent citation studies have confirmed this skewed pattern.<sup>3</sup> It is in fact quite common in scientific journalism, and in 'information' in general. Because readers easily recognize names they already know (a certain visibility factor, as in political campaigns), reporters and editors take various shortcuts, even though they thereby routinely distort something as central (we are told) to scientific morale and reputation-building as the proper attribution of scientific credit.<sup>4</sup> Most of Merton's evidence came from the unequal fame and reputation accorded collaborators and (independent) simultaneous discoverers: that is, the favoured collaborator gets lasting fame in prizes, textbooks and eponymy into posterity, while the other, if he or she escapes total posthumous obscurity, may be barely mentioned in the other's Nobel Prize acceptance speech or in a footnote in his biography. Subsequent Biblical research has provided additional evidence that this part of the effect is particularly well named, for we now know that, even though it was named for him in the second century, Matthew himself did not write this Gospel. It was not written until two or three generations after his death.<sup>5</sup>

Merton attributed this unequal division of glory to the somewhat circular process of 'accumulation of advantage' – those remembered often already had charisma, previous reputations, positions in large 'research schools' and many well-placed disciples, all of which helped them to get even more fame. The 'losers' in the process are, by contrast, often marginal figures with no solid position, central location or established disciples to battle for them or protest their exclusion, thus demonstrating that in scientific myth-making politics can play at least as important a role as the work itself. But Merton did not deplore the unfairness of this rather systematic misallocation of credit. Rather, he went on to describe how 'functional' it was, and even to suggest that little-known scientists learn how to take advantage of the asymmetry by communicating their ideas to the already-well-known who would then publish it for them (with or without credit). Yet such cynical advice – on how to capitalize on the

prevailing system rather than to change it – can only increase the morale problems of postdoctoral fellows and others whose achievements are today routinely subsumed into the reputation of their team leader.<sup>6</sup>

### Examples

Since 1968, a lot has been written on the history of women in science, much of it on obscure individuals previously present only in footnotes, if even that. Yet their rediscovery raises the question of just how well-known should essentially rank-and-file scientists be? Should someone who, for example, taught botany at Wellesley College for forty years, be remembered by all scientists? By all botanists? By alumnae of Wellesley? What if she was also a president of the Botanical Society of America? Or won an important prize? Or what about a government employee who worked for forty years on chestnut blight disease? She might be known around the world for this by certain other specialists, and yet be unknown in her home town. Just how specific and widespread should a scientific reputation be? If we had some scale or measure, then we might know how upset or outraged to be when a deserving scientist is ignored or forgotten. After all, not everyone can or should be remembered by everyone else. Yet whatever the hierarchy, if science is to be meritocratic and the history of science to reflect this, similar or equal achievements should receive similar reputations or recognition.

But in women's history this is rarely the case. Certainly there are some exceptions, as Madame Marie Curie is well remembered, though whether for her two Nobel Prizes, the best-selling biography by her daughter, her power as head of the Radium Institute in France or the 1943 movie starring Greer Garson is not clear. Recently Helena Pycior has written on Curie's early reputation and publication pattern. Unlike many wife-collaborators, she was not Pierre Curie's silent and invisible co-worker, nor even his second author very often: more frequently she was the sole author of important papers or first with him second.<sup>7</sup> Another case of a woman scientist who got almost as much credit as she deserved would be the well-connected Maria Goeppart Mayer, co-worker with Enrico Fermi at the University of Chicago in the late 1940s, who shared the Nobel Prize in 1963 with Eugene Wigner and simultaneous co-discoverer of the shell model of the nucleus Hans D. Jensen of Germany. Because of anti-nepotism

rules at the University of Chicago, where she did the work in 1947–49, she was officially an unpaid ‘volunteer’ faculty member, and her contributions might easily have been overlooked by the Nobel Prize committee in Stockholm. But her inclusion in the Fermi group, regardless of her formal title, helped her overcome this seemingly marginal position.<sup>8</sup>

Although Dean Keith Simonton has recently stated that scientific reputations once made persist for generations, this is often not the case for women in science.<sup>9</sup> Not only have those unrecognized in their own time generally remained so, but others that were well-known in their day have since been obliterated from history, either by laziness or inertia, or by historians with definite axes to grind. Probably the most outrageous erasure or transformation of the history of science or medicine was the case of Trotula, an eleventh-century physician in Salerno, Italy, recently described by Margaret Alic in her book, *Hypatia's Heritage*. We know such a person existed and was famous for her cures of women's diseases and writings about them, through the various mentioning by her husband and son, both physicians as well. But in the twelfth century a monk, assuming that such an accomplished person must have been a man, miscopied her name on one of her treatises, giving it the masculine form in Latin, a mistake which has confused the issue of her sex ever since. In the twentieth century, Karl Sudhoff, a German medical historian intent on upgrading the status of his specialty, reduced Trotula to the level of a midwife, on the erroneous grounds that the treatises were so important that they must have been written by a male physician! Consequently she is not included in the *Dictionary of Scientific Biography (DSB)*.<sup>10</sup>

Among the simultaneous discoverers of unequal reputation was the uneducated German Agnes Pockels, who in 1890 sent to Lord Rayleigh of the Cavendish Laboratory in England her observations on surface tension. Although he (properly) arranged to have her letter published, still it is he who generally gets the full credit for discovering the phenomenon, one of the lesser accomplishments of his long and illustrious career.<sup>11</sup> Similarly, in 1905, the geneticist Nettie Stevens, then of Bryn Mawr College, and Edwin B. Wilson of Columbia University both discovered the chromosomal nature of sex determination. Yet he gets the credit in textbooks and the *DSB*, while, she, who died at age 51, does not.<sup>12</sup>

Among unmarried collaborators there are many cases of unequal credit for co-discoveries. This is especially notorious at the Nobel or

near-Nobel level of recognition. For example, Frieda Robscheyt-Robbins, the associate for thirty years of pathologist George Hoyt Whipple and the co-author of nearly all of his/their publications, did not share his Nobel Prize for Medicine in 1934. (Two men at other institutions did.) But Whipple, realizing his indebtedness to her and recognizing the awkwardness and injustice of the award, praised her lavishly and even shared the prize money with her and two other female assistants.<sup>13</sup> A later, better known but less generous case of credit denied, was that of the crystallographer Rosalind Franklin, who died before her 'collaborators' (to stretch a loose concept to its utmost limits) won the Nobel Prize in 1962, and whose essential contribution was then further minimized in the survivors' distorted autobiographical account of 'their' discovery.<sup>14</sup> In another case from the 1950s, no one seems to have protested that C.S. Wu, the physicist at Columbia University whose experiment proved that parity was not conserved, might have shared the Nobel Prize in physics with theorists T.D. Lee and C.N. Yang.<sup>15</sup> Similarly, in the 1960s and 1970s, younger women associates, such as astrophysicist Jocelyn Bell in England,<sup>16</sup> and pharmacologist Candace Pert in the United States, collaborated in important scientific work but did not share in the resulting Nobel and Lasker Prizes accorded these discoveries. By then, however, there was sufficient feminist criticism for these decisions to be termed 'controversial'.<sup>17</sup>

But perhaps the most notorious theft of Nobel credit is the case of Lise Meitner, who worked for decades with Otto Hahn in Germany and who, in 1939, realized that what they had done but could not explain was in fact nuclear fission. She must have been stunned to learn in 1944 that he alone had been awarded the Nobel Prize for one of the biggest collaborative discoveries of the century. She did not complain publicly but when, in December 1946, he went to Stockholm, her place of residence after 1938, to pick up his prize, he reported in his autobiography,

I had a rather unhappy conversation with Lise Meitner, who said I ought not to have sent her away from Germany when I did. That discord was probably the result of some disappointment because it was only I who was awarded the prize. I did not mention the point myself, but a number of her friends alluded to it in a rather unkind manner in conversation with me. Yet I really had no responsibility for the course events had taken. When I had organized my deeply respected colleague's escape from Germany, all I had had in mind was her welfare. And then, too, the Prize had been given to me for work I had done either alone or with my colleague Fritz Strassmann, and for her achievements Lise Meitner had been given a number

of honorary degrees in the U.S.A. and had even been declared the 'woman of the year'.<sup>18</sup>

This last would have been a particularly unkind reference, since these awards were often created as a kind of 'compensatory recognition' for those women who had been overlooked by the mainstream prize committees. *She* would not have considered a 'WOTY', here awarded by the Women's National Press Club of Washington, DC, itself the product of exclusion from the regular Press Club, anywhere near the equivalent of a major award from a scientific society, let alone the Nobelstiftung.

### The Special Case of Marriage

If unmarried female collaborators often receive less credit, the pattern is even more pervasive among collaborative married couples. There, as in the recent flurry of articles about Albert Einstein's first wife,<sup>19</sup> the spouse (usually it is the wife) is systematically under-recognized, either deliberately for strategic reasons or unconsciously through traditional stereotyping. In the case of the British physicists Hertha and W.E. Ayrton, she performed her husband's work and deliberately published it under his name rather than her own, after he, considerably older, became ill but wished to preserve the impression that he could still complete his own studies.<sup>20</sup> Another couple in which the husband got more than his share of the credit may be the case of Gerty and Carl Cori, biochemists who shared the 1947 Nobel Prize in Medicine with Bernardo Houssay of Argentina. Rumours rather than written accounts of their work have indicated that she was by far a better scientist than he.<sup>21</sup> In fact for some possibly threatened scientists, marrying one's collaborator may be a strategy for under-cutting a serious rival in the race for recognition. Something like this happened in the initially distinct but later collaborative work of Ruth Hubbard and George Wald. Although she had done independent work on the biochemistry of vision in the 1950s, she found that after she married Wald, who worked on similar problems, and he won the Nobel Prize in 1967, all her previous work was retrospectively attributed to him and their later collaboration.<sup>22</sup> More recently, Isabella Karle, a crystallographer who worked beside her husband Jerome for almost fifty years, was stunned to learn in 1985 that his Nobel Prize in chemistry was to be shared with a German colleague and co-author.

Their daughter, herself a scientist at Brookhaven National Laboratory, protested in a *New York Times* interview.<sup>23</sup>

### In the Aggregate

Although one could go on listing types of examples of this overall phenomenon, the pattern applies not only to individual cases, those of whom we have heard despite it all, but occurs systematically in populations as well. One prime example of this undercutting, undercounting and minimizing of the presence of women was in the naming of the *American Men of Science* of 1906, its first edition. There were women, hundreds and later thousands of them in each edition, and yet the name chosen deliberately minimized this. Critics as early as 1920 protested that it should have been *Men and Women of Science* or just *Scientists* or even *Scientific Worthies*, as one directory in England was called, but to no avail – until 1971, when the decision was made to broaden the name of the upcoming twelfth edition in six massive volumes to *American Men and Women of Science*.<sup>24</sup> Even more exclusive were the two volumes of the *McGraw-Hill Modern Men of Science*, which not only specified the scientists' sex in its title but omitted most women from its text. Of the 426 living scientists in its first volume (1966), only nine were women (six Americans and three British); in the second volume of 1968 there were only two women among its 420 'leading' scientists (Meitner and Honor Fell). Not even such usual honorees as Barbara McClintock made this biographical dictionary.<sup>25</sup> Even the *Dictionary of Scientific Biography* (1970–80), with about 2000 entries of scientists and others, included just 25 women, eight of whom were astronomers, including four from the Harvard College Observatory (which may reflect the presence of I. Bernard Cohen and Gerald Holton on one of the planning committees).<sup>26</sup>

Moving up the scale from directories and lists to what has passed as social science in the recent past, the dominant practice for many studies in the 1950s and 1960s was in fact to omit women and certain types of men entirely. One influential book that did this was *The Making of a Scientist* (1953) by psychologist Anne Roe (Simpson). Because, as she explained in an early chapter, she wished to reduce the chances for variation in her subjects' test scores, she eliminated such otherwise eligible persons as the foreign-born, those over age 61, full-time administrators, her husband and his friends, and women. The

last was particularly easy since there were in the late 1940s only two women members – Barbara McClintock and Cecilia Payne-Gaposchkin, who had been born in England – of the National Academy of Sciences or the American Philosophical Society, a requirement for most of her subjects. Thus purified, her sample of 64 prominent male researchers could stand as respectable social science. Only later would the educational implications of such a limitation – the insensitivity and lack of diversity – seem serious. At the time such an ‘ideal type’ seemed adequately to represent the whole. In fact there is some later evidence that Roe’s own consciousness rose a bit over time, for in 1966 she published material on these men’s more chauvinistic and sexist practices that she, seeking to idealize them, had omitted at the time.<sup>27</sup>

Other studies of the 1950s and 1960s included women in the data collection but omitted them from the text. One such study, by R.H. Knapp and H.B. Goodrich of Wesleyan University, was on the undergraduate origins of those scientists in the classes of 1924 through 1934 later listed in the *American Men of Science*. It included women in its institutional totals or raw data, but then, finding that women were only 2% of the science PhDs listed, dropped them from its further calculations and the text. Thus, when later readers looked to see where American scientists had gone to college, they were steered toward the private liberal arts colleges like Oberlin, Swarthmore and Reed, but the highly productive women’s colleges, such as Mount Holyoke, Vassar, Wellesley and Smith, were not mentioned. One suspects that the omission of women graduates also diminished the relative standing of those major coeducational universities, like Cornell, Berkeley or the University of Michigan, that had trained many women scientists. By thus omitting part of the historical record, this seemingly authoritative report (with important implications for vocational guidance) perpetuated the notion that women did not do science, and denied the considerable contribution of some women’s colleges and major universities. (One might also note that the resulting bias was in favour of liberal arts colleges for men like Wesleyan, which sponsored the study.)<sup>28</sup>

Another such study that included women in the data collection but then omitted them from the final text was *The Professional Scientist: A Study of American Chemists* by two prominent sociologists, Anselm Strauss and Lee Rainwater, in 1962. Based on a contract with the American Chemical Society to survey a representative sample of the field and report back on what attitudes the members held, the



researchers sent a questionnaire to nearly 10,000 chemists, carefully chosen to include 7.5% women, and interviewed 200 chemists, again carefully balanced to include 15 women. But the final volume blithely omits the women (as well as the retirees and the foreign-born), so that it can, as the authors say in Chapter 1, focus more intensively on the variations among the (white) men. There was not even a separate chapter on the women or the foreign-born, though this was a time of growing numbers of both groups among American chemists.<sup>29</sup> Perhaps their views were divergent enough to be different, but if so a separate chapter might have been revealing, especially since the ACS had commissioned the study in order to help its leaders navigate its future.

From such omission and exclusion it was an advance for Jonathan Cole to devote a whole volume to women scientists in his mistitled *Fair Science*, of 1979. But if Robert K. Merton in 1968 could jocularly call attention to the inequities (i.e. unfairness) in the recognition system for the already eminent, and name the 'Matthew Effect' on the basis of anecdotal evidence, Cole went to great pains to try to prove – to three decimal places at times – that the same system was fair to women in science. He claimed that, because the women PhDs of 1957 published so much less and were cited so much less often than men in the same field and year of degree, they therefore deserved their uniformly lower academic ranks and status. Most critics were unconvinced, however, and most were sure that academia (where most of this science took place) was unfair.<sup>30</sup> Since then, the sociology of science has not progressed much beyond this impasse; in fact, it is only since Sara Delamont wrote in this journal six years ago that the sociology of science, which presumes to study the practices of all kinds of scientists, has focused much attention on women at all.<sup>31</sup> Even philosophers of science, who used to focus on impersonal abstract thought, have given gender more attention than have sociologists.

A better and less contentious explanation for the above phenomena than any provided by sociology of science to date comes from the field of literary criticism. Joanna Russ has presented a systematic summary and critique of the many ways women's contributions to literature have been (consciously or not) undervalued over the centuries. She thus provides a good summary of the many ways in which accomplishments can be and have been assessed, but then diminished and dismissed. The cover to the paperback edition of her book, *How to Suppress Women's Writing*, summarizes, while also

caricaturing, her critique, which may be new to sociologists sticking strictly to science, as:

She didn't write it.

She wrote it, but she shouldn't have.

She wrote it, but look what she wrote about.

She wrote it, but she wrote only one of it.

She wrote it, but she isn't really an artist, and it isn't really art.

She wrote it, but she had help.

She wrote it, but she's an anomaly.

She wrote it, BUT . . . <sup>32</sup>

### **In Search of a Name**

If one accepts the fact that there is such a systematic undervaluing of women's contributions to science as well as literature (and history and medicine),<sup>33</sup> and that it is common enough in various forms in the history and sociology of science as to need a name, we should press on to find a suitable one. One could call it the 'Lise Effect', for Meitner, as mentioned above, one of the best-known examples of the phenomenon, among Nobel also-rans. Or as a courtesy to Robert K. Merton, one might call it the 'Harriet Effect' in honour of his invisible collaborator Harriet Zuckerman, who did most of the work on which the 'Matthew Effect' was based and should have been acknowledged as co-author (as he has himself since admitted).<sup>34</sup> But as a full professor at Columbia and past president of the Society for Social Studies of Science, she can hardly be considered obscure and exploited or downtrodden by history.<sup>35</sup>

Perhaps as befits the second generation history and sociology of science, one might stand on Merton's shoulders, so to speak (to use another phrase he has made famous), and find a religious or semi-religious name suitable for the eponymy. Here two possibilities come to mind, because the phenomenon is prevalent in the Bible and Biblical scholarship, as well as in other fields. Although we don't know the names of any of Matthew's invisible female assistants, Biblical scholars have found that an obscure figure known as Priscilla (or Prisca) wrote certain parts of the Scriptures.<sup>36</sup> A second choice would be the Biblical figure, Martha, the sister of Mary. Enough is known about her to suggest that she would be particularly appropriate, for unlike Mary, who as the mother of Christ was well treated in her lifetime and historically ever since, Martha was relegated to a

lot of unappreciated housework and no credit. (In fact when, at one point in the Bible [Luke 10: 40], she continued doing housework, even though Christ had come to talk to his mother, he rebuked her for such rudeness, thus adding insult to unappreciated drudgery.) Another factor in her favour is the tradition of poetry in her honour, as in the 1907 poem 'The Sons of Martha', by Rudyard Kipling. This is a paean to the joys of comradeship in worthwhile but under-appreciated work – here the British engineers in India who, rather than get the royal treatment befitting the sons of Mary, have as the sons of Martha to labour continually, to build railroads, dams and roads – all kinds of under-appreciated infrastructure, for little recognition or credit.<sup>37</sup> Years later, American psychologist-turned-engineer Lillian Gilbreth built upon the poem in a banquet address on 'The Daughters of Martha' to the Society of Women Engineers, where she used the occasion to call attention to how even more under-appreciated their work was.<sup>38</sup>

But, as a historian of American women, my preference goes to a third candidate, Matilda, who was not an unappreciated and possibly mythical Biblical figure at all, but a nineteenth-century American feminist, suffragist, critic of religion and the Bible, and early sociologist of knowledge, who glimpsed what was happening, perceived the pattern, deplored it, but herself experienced some of the very phenomena described here. Matilda Joslyn Gage (1826–98) was born 10 miles outside of Syracuse, New York, and spent most of her life in the region. Her father was a strong force in her intellectual development – he was a physician who was also an abolitionist, a temperance advocate, a woman's rightist and free thinker, whose house was reportedly a stop on the Underground Railroad that took escaped slaves to Canada. He taught her Greek as well as mathematics and physiology, but her only actual schooling was at the Liberal Institute in nearby Clinton, New York. In 1845, she married Henry Gage, a local merchant who ran a dry goods store. They had five children, one of whom died, and she was often considered frail and sickly, but this did not stop her. Her first public meeting was the 1852 National Woman's Rights Convention in Syracuse, New York. It was rare then for a woman to speak up in public, but she did. Others remembered not only her weak voice but also her stylish clothes. She was more active after 1869 when the children had grown up, and in 1875, as president of both New York and national suffrage groups, she testified before Congress. Although chiefly a writer and editor of suffrage newspapers, in 1870 she published a short volume on women in the

history of technology or *Woman as Inventor*, which disputed among other things Eli Whitney's invention of the cotton gin. She claimed, as is still being done today, that Whitney had been instructed by Constance Greene on how to put the pieces together. In the 1880s, disheartened, as were others at the lack of progress in getting votes for en, she was one of the three co-editors of the premature but still massive *History of the Women's Suffrage* (1881–86).

Then, in the 1890s, she turned her efforts to (what makes her appropriate here) religion, especially to Christianity, which she thought especially downgrading to women. In 1893, she published her virulent *Woman, Christ and the State*, which shows how Christianity has justified and extolled the subjection of women – it urges them to work hard and to sacrifice, it takes their money, but in return it gives them little credit and even justifies men's exploitation of them.<sup>39</sup> She then actively participated in the group of twenty American women (including Elizabeth Cady Stanton) that wrote the *Woman's Bible*, published in two parts in 1895 and 1898. They felt that some feminist reinterpretation of the Bible was necessary, because all too often in nineteenth-century America, Biblical stories and analogies were still commonly used as a kind of guide to proper behaviour, or an etiquette manual. Since the Bible was quite patriarchal, these politically-aware women felt that its restrictions were overly prohibitive, particularly of women's behaviour. Thus, in the 1890s, when the suffrage campaign was in the doldrums, some intellectual women determined that it was worth the time to redo the Bible, not merely adding 'he or she' instead of 'he', but in publishing paragraphs of alternative texts on certain parts that seemed to sustain a newer, more feminist and permissive interpretation. This was a towering intellectual feat for women who had not been to college or theological school, and who had been trained to obey their clergymen. Unfortunately for our purposes here, the *Woman's Bible* of 1895 and 1898 does not discuss this Matthew 13:12 – probably because it was part of a lengthy series of 'parables' that were not expected to make much immediate logical sense. Christ was in one of his more cryptic or mystical moods on these particular travels, and clergymen over the years have explained and re-explained these passages.<sup>40</sup>

In many ways, Gage seems a more directly relevant candidate here for eponymy than does the Biblical Martha, for she was aware of, and denounced, the tendency of men to prohibit women from reaping the fruits of their own toil, and in fact noticed that the more woman worked the more the men around her profited and the less credit she

got. And since Gage is nearly unknown today, her life personifies this phenomenon. Dale Spender, an Australian feminist writer, described her,

Despite her analysis, her energy, her actions, she has been negated and denied. Unable to use her ideas, patriarchy has elected to lose them. It is as if she had not existed. Yet it seems to me from the few fragments I have been able to piece together, that she, more than any other woman of the past (with the exception of Mary Beard, 1946) identified and understood the process of denial of woman's existence, the theft of woman's being, in a male-dominated society.<sup>41</sup>

## Conclusion

The 'Matthew Effect' as coined by Merton in 1968, applied chiefly to and applauded the first half of Matthew 13:12 – the over-recognition of the already prominent or prominently-placed. Yet the phenomenon described in the second half of the parable has (as befits its message) received less attention, though it is a fairly common occurrence, especially in women's long historic existence in science. Rather than denying that this is the case, as has been the sociologists' practice to date, the sexist nature of much of the women's systematic under-recognition should be acknowledged, noted and even highlighted in the sociology of knowledge or science, as in a named 'effect'. Since the Bible was so patriarchal, even Biblical women like the author Priscilla or the participant Martha, might not be as appropriate for our purposes as one of its later feminist critics who contributed to the *Woman's Bible* of the 1890s. Of these, the American Matilda J. Gage of New York best befits the honour for first articulating (but, alas, also experiencing herself) what we can call in her memory the 'Matilda Effect'. Perhaps if we call attention to her and this tendency, which goes back centuries, it will remind and help current and future scholars to write a more equitable and comprehensive history and sociology of science that not only does not leave all the 'Matildas' out, but calls attention to still more of them.

## ●NOTES

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1. Mark Kac, *Enigmas of Chance: An Autobiography* (New York: Harper & Row, 1985), 22. See also Andrzej A. Teske, 'Marian Smoluchowski', in *Dictionary of Scientific Biography*, Vol. 12 (1975), 496–98. His inclusion there assures him some minor fame, more than the many others not so favoured.

2. Judith Lorber, *Women Physicians, Careers, Status and Power* (New York: Tavistock Publications, 1984), 4–6, applied the second part to the dearth of women physicians at the top of that profession.

3. Robert K. Merton, 'The Matthew Effect in Science', *Science*, Vol. 159 (5 January 1968), 56–63. See also note 34, below. All investigators seem to accept the skewed citation patterns – some papers and some authors are cited far more often than others – but differ over its meaning. Perhaps the highly-cited papers or authors are 'better' in some sense, but maybe they are just better-placed or more powerful. If the latter but not the former – the key issue here – the fairness of the system and the weight of others' relative contributions remains open to interpretation: see the exchange between H.M. MacRoberts and B.R. MacRoberts, 'Testing the Ortega Hypothesis: Facts or Artifacts?', *Scientometrics*, Vol. 12 (1987), 293–95 and Harriet Zuckerman, 'Citation Analysis and the Complex Problem of Intellectual Influence', *ibid.*, 329–38.

4. See Rae Gooddell, *The Visible Scientists* (Boston, MA: Little, Brown & Co., 1977), 210.

5. Francis Wright Beare, *The Gospel According to Matthew* (San Francisco, CA: Harper & Row, 1981), 7–13.

6. Merton, *op. cit.* note 3, 59–60. See also Stephen Hall, *Invisible Frontiers: The Race to Synthesize a Human Gene* (New York: Atlantic Monthly Press, 1987), 88–89, 95–96, 113 and 142–44.

7. Helena Pycior, 'Reaping the Benefits of Collaboration While Avoiding its Pitfalls: Marie Curie's Rise to Scientific Prominence', *Social Studies of Science*, Vol. 23 (1993), 301–23.

8. Joan Dash, 'Maria Goeppart Mayer', in Barbara Sicherman and Carol Hurd Green (eds), *Notable American Women: The Modern Period* (Cambridge, MA: Harvard University Press, 1980), 466–68, has several sources.

9. Dean Keith Simonton, *Scientific Genius: A Psychology of Science* (Cambridge: Cambridge University Press, 1988), 91.

10. Margaret Alic, *Hypatia's Heritage: A History of Women in Science from Antiquity through the Nineteenth Century* (Boston, MA: Beacon Press, 1986), 54–55. See also Rolf Winau, 'The Role of Medical History in the History of Medicine in Germany', in Loren Graham, Wolf Lepenies and Peter Weingart (eds), *Functions and Uses of Disciplinary Histories* (Dordrecht, Holland: D. Reidel, 1983), 112–13.

11. See M. Elizabeth Derrick, 'Agnes Pockels, 1862–1935', *Journal of Chemical Education*, Vol. 59 (1982), 1030–31; Charles Tanford, *Ben Franklin Stilled the Waves: An Informal History of Pouring Oil on Water* (Durham, NC: Duke University Press, 1989), esp Chapters 10 and 11 ('Meticulous Miss Pockels'); and Jane A. Miller, 'Women in Chemistry', in G. Kass-Simon and Patricia Farnes (eds), *Women of Science: Righting the Record* (Bloomington, IN: Indiana University Press, 1990), 309–10; but she is not in Marilyn Bailey Ogilvie (ed.), *Women in Science: A Biographical Dictionary with Annotated Bibliography, Antiquity through the Nineteenth Century* (Cambridge, MA: MIT Press, 1986). Pockels is mentioned, however, in the DSB's entry for Henri Devaux, who in 1903 built upon her earlier work: A.M. Monnier, 'Henri Devaux', *DSB*, Vol. 4 (1971), 76–77.

12. Stephen G. Brush, 'Nettie M. Stevens and the Discovery of Sex Discrimination of Chromosomes', *Isis*, Vol. 59 (1978), 163–72; Marilyn Bailey Ogilvie and Clifford J.

Choquette, 'Nettie Maria Stevens (1861–1912): Her Life and Contribution to Cytogenetics', *Proceedings of the American Philosophical Society*, Vol. 115 (1981), 292–311; Ogilvie, (ed.), op. cit. note 10, 167–69; and G. Kass-Simon, 'Biology is Destiny', in Kass-Simon & Farnes (eds), op. cit. note 10, 225–26.

13. Margaret W. Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore, MD: Johns Hopkins University Press, 1982), 213–14.

14. James D. Watson, *The Double Helix* (New York: Athenaeum Press, 1968) and Anne Sayre, *Rosalind Franklin and DNA* (New York: W.W. Norton, 1975). There is still a need for a full biography of Franklin.

15. Gloria Lubkin, 'Chien-Shiung Wu, The First Lady of Physics Research', *Smithsonian*, Vol. 1 (January 1971), 52–57.

16. On Jocelyn Bell, see Nicholas Wade, 'Discovery of Pulsars: A Graduate Student's Story', *Science*, Vol. 189 (1 August 1975), 358–64, and George Reed, 'The Discovery of Pulsars: Was Credit Given Where It Was Due?', *Astronomy*, Vol. 11 (December 1983), 24–26. I thank Michele Aldrich for a copy.

17. J.L.M., 'Lasker Award Stirs Controversy', *Science*, Vol. 203 (26 January 1979), 341; William Pollin, 'Pert and Lasker Award', *ibid.*, Vol. 204 (6 April 1979), 8; Joan Arehart-Treichel, 'Winning and Losing: The Medical Awards Game', *Science News*, Vol. 115 (24 February 1979), 120 and 126. See also Solomon Snyder, *Brainstorming: The Science and Politics of Opiate Research* (Cambridge, MA: Harvard University Press, 1989); Jeff Goldberg, *Anatomy of a Scientific Discovery* (Toronto: Bantam Books, 1988); and Robert Kanigel, *Apprenticeship to Genius: The Making of a Scientific Discovery* (New York: Macmillan, 1986).

18. Otto Hahn, *My Life: The Autobiography of a Scientist*, trans. Ernst Kaiser and Eithre Wilkins (New York: Herder & Herder, 1970), 199. For more on Meitner's award, see 'Honor Dr. Meitner for Work on Atom', *New York Times* (10 February 1946), 13. She also lost credit for her 1922 discovery of what was later named the 'Auger Effect' for Pierre Auger's 1925–26 papers: Richard Sietmann, 'False Attribution, A Female Physicist's Fate', *Physics Bulletin*, Vol. 39 (1988), 316–17.

19. Jurgen Renn and Robert Schulmann (eds), *Albert Einstein/Mileva Maric – The Love Letters* (Princeton, NJ: Princeton University Press, 1922).

20. Ogilvie (ed.), op. cit. note 10, 32–34; Evelyn Sharp, *Hertha Ayrton, 1854–1923: A Memoir* (London: Edward Arnold, 1926); Joan Mason, 'Hertha Ayrton (1854–1923) and the Admission of Women to the Royal Society of London', *Notes and Records of the Royal Society of London*, Vol. 46 (1992), 279–300.

21. See the entry for Cori in Sicherman & Green (eds), op. cit. note 8, 165–67.

22. Judith Walzer, 'Interview with Ruth Hubbard', July 1981, typescript in Henry A. Murray Center, Radcliffe College, 98–102, and Patricia Farnes, 'Women in Medical Science', in Kass-Simon & Farnes (eds), op. cit. note 11, 289.

23. '3 Nobels in Science', *New York Times* (17 October 1985), 17.

24. Rossiter, op. cit. note 13, 112; 'Directory of Scientists Will Now List Women', *New York Times* (23 November 1971), 7. Only 2% of its 'starred' scientists, a practice stopped in 1943, had been women (Rossiter, op. cit. note 13, 291).

25. McGraw-Hill *Modern Men of Science* (New York: McGraw-Hill, 1966 and 1968), 2 Vols.

26. *DSB*, Vol. 1 (1970), front matter.

27. Anne Roe, *The Making of a Scientist* (New York: Dodd, Mead, 1953), 22–25. Some, for example, refused to have women graduate students: 'Women in Science', *Personnel and Guidance Journal*, Vol. 44 (1966), 784–87.

28. R.H. Knapp and H.B. Goodrich, *Origins of American Scientists* (Chicago, IL: The University of Chicago Press, 1952), 20 and Appendix 2. Their figure of 2% is far lower than the proportion of science doctorates actually awarded to women in the 1920s and 1930s, especially since psychology was included in the study. It is particularly odd, because women in the *AMS* held doctorates more often than the men. For another example of the deliberate omission of women's colleges, see *Research and Teaching in the Liberal Arts College: A Report* (n.p.: 1959), 15, which, despite its title, omitted all colleges for women, even though Mount Holyoke College long topped the list for women by a wide margin: see John R. Sampey, 'Chemical Research in Liberal Arts Colleges, 1952-59', *Journal of Chemical Education*, Vol. 37 (1960), 316; Alfred E. Hall, 'Baccalaureate Origins of Doctorate Recipients in Chemistry: 1920-80', *ibid.*, Vol. 62 (1985), 407; Mary L. Sherrill, 'Group Research in a Small Department', *ibid.*, Vol. 34 (1957), 466 and 468; and Emma Perry Carr, 'Research in a Liberal Arts College', *ibid.*, 467-70.

29. Anselm L. Strauss and Lee Rainwater, *The Professional Scientist: A Study of American Chemists* (Chicago, IL: Aldine, 1962), 17-21.

30. Jonathan Cole, *Fair Science: Women in the Scientific Community* (New York: Free Press, 1979). Although his scientists were academically-employed, he omitted the whole matched-vita literature of the early 1970s on discriminatory hiring there: see, for example, Lawrence A. Simpson, *A Study of Employing Agents' Attitudes Toward Academic Women in Higher Education* (unpublished EdD dissertation, Pennsylvania State University, 1968); Lawrence A. Simpson, 'A Myth is Better Than a Miss: Men Get the Edge in Academic Employment', *College and University Business*, Vol. 48 (February 1970), 70-71; L.S. Fidell, 'Empirical Verification of Sex Discrimination in Hiring Practices in Psychology', *American Psychologist*, Vol. 25 (1970), 1094-98; and Arie Y. Lewin and Linda Duchan, 'Women in Academia, A Study of the Hiring Decision in Departments of Physical Science', *Science*, Vol. 173 (3 September 1971), 892-95. Reviews included Gaye Tuchman, 'Discriminating Science', *Social Policy*, Vol. 11 (May/June 1980), 59-64; Barbara Reskin, 'Fair Science: A Fair Test?', *Contemporary Sociology*, Vol. 9 (1981), 793-95; Margaret W. Rossiter, 'Fair Enough?', *Isis*, Vol. 72 (1981), 99-103; Patricia Yancey Martin, '"Fair Science": Test or Assertion?', *Sociological Review*, Vol. 30 (1980), 478-508; and Harrison C. White, 'Review Essay: Fair Science?', *American Journal of Sociology*, Vol. 87 (1982), 951-56.

31. Sara Delamont, 'Three Blind Spots? A Comment on the Sociology of Science by a Puzzled Outsider', *Social Studies of Science*, Vol. 17 (1987), 166-67; but see also Hilary F. Burrage, 'Women University Teachers of Natural Science, 1971-72: An Empirical Survey', *ibid.*, Vol. 13 (1983), 147-60.

32. Joanna Russ, *How To Suppress Women's Writing* (Austin, TX: University of Texas Press, 1983). See also Cynthia Ozick, 'We Are the Crazy Lady and Other Feisty Feminist Fables', *Ms.*, Vol. 1 (Spring 1972), 40-44.

33. Berenice Carroll, 'The Politics of Originality: Women in the Class System of the Intellect', *Journal of Women's History*, Vol. 2, No. 2 (Fall 1990), 136-63.

34. Robert K. Merton, 'The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property', *Isis*, Vol. 79 (1988), 607n2, which admits Zuckerman should have been a co-author. (I thank Mordechai Feingold for calling this article to my attention.)

35. See entry for Zuckerman in *Who's Who in America*, No. 46 (1990-91), 3591.

36. See Ruth Hoppin, *Priscilla: Author of the Epistle to the Hebrews* (Jericho, NY: Exposition Press, 1969).



37. I thank Sheila Jasanoff for suggesting Martha. For story on Martha, see Luke 10:38–42. Rudyard Kipling, 'The Sons of Martha', in Burton E. Stevenson, *The Home Book of Verse* (New York: H. Holt, 1949), 1, 3072–73. See also Vasant A. Shahane, *Rudyard Kipling, Activist and Artist* (Carbondale, IL: Southern Illinois University Press, 1973), 117–18.

38. Lillian Gilbreth, 'The Daughter of Martha', Program, Society of Women Engineers Banquet, New York City, 8 November 1961, 4–9, copy in Lillian Gilbreth Papers, Special Collections, Purdue University Library.

39. See entry for Gage in Edward T. James et al. (eds), *Notable American Women, 1607–1950* (Cambridge, MA: Harvard University Press, 1971), II, 4–6, and Sally Roesch Wagner, 'Introduction', in Matilda Joslyn Gage, *Woman, Church & State* (Watertown, MA: Persephone Press, 1980, reprint of 1893 text), xv–xxxix. Gage died in Chicago in 1898 at the home of her daughter, whose husband later wrote the *Wizard of Oz*. I thank Ruth Oldenzil for a copy of Matilda Gage, *Woman as Inventor*, Woman Suffrage Tract No. 1 (Fayetteville, NY, 1870).

40. On the Women's Bible and the group who worked on it, see Elisabeth Schüssler Fiorenza, *In Memory of Her* (New York: Crossroad Publishing Co., 1983), 7–14; Barbara Brown Zikmund, 'Feminist Consciousness in Historical Perspective', in Letty M. Russell, *Feminist Interpretations of the Bible* (Philadelphia, PA: Westminster Press, 1985), 23–25; and Barbara J. MacHaffie, *Her Story: Women in Christian Tradition* (Philadelphia, PA: Fortress Press, 1986), 113–16, as well as *The Woman's Bible*, Parts I and II (New York: Arno Press reprint, 1972).

41. Dale Spender, *Women of Ideas and What Men Have Done to Them* (London: Routledge & Kegan Paul, 1982), 252.

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