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NOTES & DÉBATS

TELLING THE LIFE OF A MATHEMATICIAN: THE CASE OF J.J. SYLVESTER

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Biography as a genre for studying the history of science increasingly came under attack as the social history of science began to dominate the field following the publication of Thomas Kuhn's provocative book, *The Structure of Scientific Revolutions* (1962). As is well known, the approach to the history of science that developed tended to stress the collective over the individual and to emphasize the *production* of scientific knowledge over an analysis of that scientific knowledge *per se*. Even more recently, social constructivists like Steven Shapin [1992, p. 352] have trumpeted the "*irreducibly social character of scientific activity*" and have argued for the analysis of science through observation of "*the processes by which community judgement coalesces around one or other boundary-frame, including the deployment of more stable cultural elements*" [*Ibid.*, p. 353]. The emphasis here is clearly not on the individual and the idiosyncracies of the individual experience but on the culture at large.

Writing in 1979, American historian of science and biographer of Sir William Rowan Hamilton, Thomas Hankins already had a sense of this more extreme position when he lamented that "*today's historian of science ... is either positively anti-biographical, immersing himself entirely in the subject matter of the science without reference to anything outside of it, or he seeks the origin of scientific ideas in a context much broader than the individual scientist's mind*" [Hankins 1979, p. 3]. In Hankins's view, this dichotomy was a false one; biography does have a place in the history of science, "*and it comes precisely at the juncture between science and its*

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cultural and intellectual context" [*Ibid.*, p. 4]. By focusing on an individual and that individual's life course, biography provides a cross-sectional view of historical development and "*gives us a way to tie together the parallel currents of history at the level where the events and ideas occur*" [*Ibid.*, p. 5]. Moreover, "*if biography is honest,*" Hankins argued, "*we can learn a great deal about the way in which science works, and we can also be protected from too-hasty generalizations*" [*Ibid.*, p. 5].

Hankins was not alone in his defense of biography. Two years later in 1981, Larry Holmes also argued for the potency of biography as a methodology in the history of science, and he specifically cautioned against a one-sided emphasis on context over content. He stated "[*f*]*irst, that the study of what is variously referred to as the 'intellectual' history of science, the 'internal dynamic', or the 'cognitive' side of scientific development is as fresh and new, as underdeveloped, as urgently in need of more concentrated, penetrating analysis as is the study of the 'social dimension'. Second, ... it is only through a profound understanding of these subjects that we can know what it is that the various contexts surround*" [Holmes 1981, p. 60]. In Holmes's view, closely studying the individual provides the historian of science with insight into the creative process that the more sociologically oriented methodologies cannot. At the same time, he acknowledged that while "[*t*]*he programme which I have outlined differs from those recently set forth by social historians of science who are directing our attention to institutions, the social milieu, prosopography, and the audience for science,*" "[*i*]*t is not however a competitive alternative direction, for these are complementary endeavours. We can never return to the situation in which theories, concepts, and discoveries were followed without regard for these other dimensions*" [*Ibid.*, p. 69.].

In 1988, Charles Rosenberg, then the editor of *Isis*, devoted an editorial to similar issues and concerns. Entitled "Woods or Trees? Ideas and Actors in the History of Science", the editorial stressed that "[*a*]*n actor-oriented approach seeks to appropriate the individual in the service of transcending the individual and thus the idiosyncratic*: *it seeks to use an individual's experience as a sampling device for gaining an understanding of the structural and normative*" [Rosenberg 1988, p. 569]. As Rosenberg was quick to add, however, "[*t*]*his is not to denigrate biography as genre* *or prescribe a particular style of biography*" [*Ibid.*, p. 569, note 3]. He merely aimed to make the point that "*unless the would-be biographer tries to reconstruct a protagonist's social world ... —to write a life in a very particular time—he or she will hardly succeed in explaining that subject's actions or evaluating the motives that impelled them*" [*Ibid.*].

L. Pearce Williams, author of a ground-breaking biography of Michael Faraday, concurred, as he took the case for biography even further. Writing in 1991, he held that the history of science aims to come to an understanding of three basic aspects of science and its development—the scientific culture at any given point in time, the process of scientific innovation, and the dissemination of scientific knowledge. He contended "*that biography is absolutely essential in each of these stages, although it is curiously rejected by both historians and the social constructivists*" as too particularistic [Williams 1991, p. 203]. Specifically, Williams argued that "[*t*]*he description and explanation of the creative work of the giants of science* ... [*is*] *fundamental to the history of science. Quite simply,*" he continued, "*the life of science depends upon minds with new insights and new theories*" [*Ibid*, p. 209]. Although this may seem like a throw-back to the "*bad old days*" when the history of science *only* concerned itself with the "*giants,*" Williams simply wanted to suggest that historians of science not allow the "*giants*" to be lost in what he immoderately termed the "*social swamp*" [*Ibid.*, p. 204].

By 1996, then, there was a sense that biography was being or, in fact, had been passed by in the quest for the social construction of scientific knowledge at the same time that it was more and more in demand by a public "*hungry for news about science*" that "*consumes scientific biography with relish*" [Shortland and Yeo 1996, p. xiii]. This was how Michael Shortland and Richard Yeo described the seeming contradiction in the preface of their edited volume, *Telling Lives in Science*: *Essays on Scientific Biography.* Their quest, therefore, in bringing together ten historians of science to analyze scientific biography was to focus on it as a "*genre, its status and influence*" [*Ibid.*]. They sought to address questions such as: is biography a potent methodology for the history of science? what insights can it provide? what sorts of questions are beyond its methodological reach? how does it fit in the post-Kuhnian historiographical climate? how should it be done? But their over-arching objective was to provide a forum for self-reflection. In their words, "*very little criticism or comment has accompanied the recent resurgence of interest in scientific biography*" [Shortland and Yeo 1996, p. xiii].

In perhaps the most thought-provoking and self-reflective essay in the volume, Thomas Söderqvist argued strongly for what he called "*an existentialist approach to science biography*" [Söderqvist 1996, p. 60]. In his view, "*an existential approach does not mean a rejection of the importance of the social life of the individual, nor does it involve an uncritical individualist viewpoint*" [*Ibid*, p. 61]. Rather, it is "*an analysis of the life of a concrete, individual researcher, not a case study of what it means to be a scientist in general*" [*Ibid.*, p. 62]. Söderqvist also specifically contrasted existential biography with other predominant strains within the genre:

"*An existential reconstruction of the subject's life is therefore made from the inside, in an attempt to narrate the development of his life 'as it is directly experienced by the biographical subject.' Hence, existential biography is distinct from (a) social biography, in which the individual is contextualised with reference to his 'situatedness' in a certain time, a certain culture, etc.; (b) psychobiography, in which certain traits of the subject's personality or his achievements are explained with reference to psychological theory; and (c) biographical case histories aimed to generalise about genius, creativity, or the life cycle. All such approaches are external to the experiencing individual confronted with his existential choices*" [Söderqvist 1996, p. 73].

Whether existentialist biography in Söderqvist's sense can be achieved—especially given the incompleteness of the historical record—is a question open for debate. Of importance here is the fact that he so thoroughly analyzed and considered various methodological approaches and made an argument not only for biography as what he called "*an edifying genre*" [Söderqvist 1996, p. 45] but also for the particular approach that best seemed to fit his subject, the twentieth-century immunologist, Niels Jerne.

It is this kind of self-conscious reflection about methodology that historians of mathematics have tended not to do enough of. Relative to biography as a methodology, Hankins, Holmes, Rosenberg, and Söderqvist each engaged in this kind of reflection, analyzing its strengths and limitations. And while they may not agree on one style of biography as the "best", on the precise ends that biography most naturally serves as a methodology for the history of science, they *do* all agree that biography *is* an important genre for the field because it allows us to comprehend the individual scientist and scientific accomplishments within his or her cultural milieu in ways that more sociologically oriented approaches cannot. It allows us, among many other things, to follow the scientist's thought, to see how he or she conceived of a theory, to witness science in the making. That said, biography has limitations as a methodology. It concentrates on one particular life. Clearly, how the subject interacts in scientific and in broader society is a part of the story. Institutions, community opinion, the formation of consensus, all of these play a role in biography, too, but they are secondary to the person's life story. Biography can illuminate how broader sociological forces shape an individual, but it does not analyze those forces *per se*. Other methodologies serve that purpose. Still, as Hankins, Holmes, Rosenberg, and Söderqvist also tacitly agree, understanding an *individual*—in contradistinction to the dynamics of a collective—is important. I will take this as my starting point as well in looking at biography as a methodology for the history of mathematics.

In his defense of biography, Thomas Hankins isolated perhaps the most important criterion the genre must satisfy in order for it to serve as a methodology for the history of science: "*it must deal with the science itself*" [Hankins 1979, p. 8]. Consider the biography of a scientist that ignores the science. If it develops the scientist's personality or character, then it may be enlightening as a psychological study and hence as methodology for the psychology of science. If it illuminates the day-to-day aspects of the scientist's social—as opposed to creative—scientific life, then it may be a useful methodology for the sociology of science. In order, however, to be a fruitful methodology for the *history* of science, it must shed light on that aspect of a scientist's life that makes him or her a scientist and not a novelist or a business tycoon or a political figure, namely, the actual science done. This is not to imply that the psychology of science or the sociology of science do not or should not inform the history of science. Nor is it to imply that the aspects of the scientist that these subfields treat are easily separated and clearly delineated in the process of trying to come to terms with the individual as a whole. It is, however, to take a stand on the role played by the creative process of doing science and by the scientific ideas ultimately produced in the life of the scientist. It is to say that that process and those ideas are fundamental to understanding the individual's life course. They must not be shoved into a "black box" and ignored, if what we want to do is understand the individual. Hence, they must form an integral part of what Hankins termed an "honest" biography of a scientist.

This said, Hankins specifically singled out mathematics as a field "*especially difficult for the biographer*" [Hankins 1979, p. 11]. After quoting George Sarton to the effect that "*the main reason for studying the history of mathematics, or the history of any science, is purely humanistic*" [Sarton 1937, p. 4], [Hankins 1979, p. 12], Hankins continued that "*it is devilishly difficult to find any humanistic setting for mathematics. Mathematics seems to have a life unto itself. Physical theories reflect a world view that has a cultural context, while mathematics reflects nothing but itself. Much mathematical progress comes in response to the demands of physical theory (Fourier's series being a prime example), but it is only the physics that allows one to build a broader intellectual context for the mathematician's work*" [Hankins 1979, p. 12].

Mathematicians may make difficult subjects for the biographer, but the reasons Hankins gave in 1979 are hard to justify in today's historiographical climate. One obvious humanistic setting for mathematics is the very human mathematician who formulates its concepts, proves its theorems, and develops its theories. Without entering into the philosophical debate over whether mathematics is created or discovered, the human agent, the mathematician, is required for the creation or for the discovery. Mathematics cannot have a life without that active, human agent. Moreover, to imply that mathematical theories do not reflect a worldview shaped by a cultural context, is to deny that mathematical actors are affected by philosophical, educational, religious, and other factors in pursuing their scientific work. Although they are very different types of mathematical biographies, Joseph Dauben's [1979] *Georg Cantor* and Jesper Lützen's [1990] *Joseph Liouville* both show the extent to which their cultures influenced these mathematicians in their educations, in their intellectual values, in their choices of what sort of mathematical ideas to pursue, and in their approaches to those ideas. The strong role of the broader culture at work in mathematics is particularly reflected in the goal that Dauben voiced in the introduction to his Cantor study, namely, to address the issue of "*how does a theory mean*" [Dauben 1979, p. 5] since "how" a theory "means" depends on those doing the interpreting, on the culturally shaped intellectual biases and presuppositions they bring to the subject matter.

Even if work in the history of science over the past two decades has rendered spurious the reasons Hankins gave for why mathematicians make difficult biographical subjects, it is nevertheless the case that biography does present at least two special challenges as a genre for the historian: the mathematics and the audience. In order to situate a subject's work within the broader mathematical context, it is often the case that a fair amount of mathematical background must be given. Unless carefully done, the incorporation of this kind of material can break the flow of the narrative and detract from the telling of the actual life story. Jesper Lützen had one solution to this problem in his biography of Liouville. He wrote the book in two parts. The first chronicles Liouville's life and career and includes brief, non-technical mention of his mathematics. As Lützen put it, "*one can consider the first part as an explanation of how Liouville's various works fit into a global picture of his life*" [Lützen 1990, p. viii]. The second part gives the fuller technical treatment in mathematical context of Liouville's varied mathematical accomplishments. Bruno Belhoste had a different solution in his 1985 biography of Cauchy. Subtitled *Un mathématicien légitimiste au XIX*e *siècle*, Belhoste's biography focuses on Cauchy the ardent Catholic royalist and traces both the formation of these views and their implications on his life story. Cauchy's mathematics is neither ignored nor treated completely. Rather, indications are given of some of his main mathematical accomplishments in five separate sections called "*thèmes*" that follow each of the book's first five chapters. As Jean Dhombres noted in his preface to the work, "*c'est bien là une gageure à la mesure de la demande de notre temps: présenter à un large public à la fois une vie et des exemples de ce qui en fait la valeur mémorable pour la science. Certes il ne pouvait être question de tout présenter. ... Il fallait choisir*" [Dhombres 1985, p. 8]. In justifying Belhoste's sampling of Cauchy's work, Dhombres put his finger on the other problem faced by biographers of mathematicians, the audience. Belhoste wanted to reach as wide an audience as possible, an audience that would read with interest a book about a nineteenth-century royalist who left and later returned to France on account of his strong political convictions, but an audience that would not necessarily relish discussions of mathematical rigor, convergence, the theory of substitutions, complex analysis, or elasticity theory. By reining the mathematics into self-contained sections, Belhoste gave his broader audience the opportunity to read or to skip over the more technical material, as their interests dictated, but he did not neglect the fact that the "*légitimiste*" he was interested in was also very much of a "*mathématicien.*"

As the decisions made by Lützen and Belhoste reflect, it is hard to write the biography of a scientist, and mathematicians surely present special problems, but they are not in some class by themselves. They are living, breathing, creative human beings who happen to do mathematics. The goal of the biographer of a mathematician is to understand them as such, to analyze the forces that shaped them as individuals and as mathematicians, to examine their reactions to those forces, to illuminate the life they led and how they led it. As a methodology for the history of mathematics, then, biography can provide a window not only onto the creative process of the mathematician but also onto the mathematician as a participant in and filter of his or her culture. Let me now try to provide further evidence for these claims through reference to my own work toward a biography of James Joseph Sylvester.

There are first some key questions to address: why a biography of Sylvester? why a biography of a nineteenth-century mathematician? why a biography of a nineteenth-century English mathematician? The trite answer is, of course, that Sylvester is intrinsically interesting, has received relatively little scholarly attention, and has never been the subject of a biography. This, though, is not reason enough for a biography of Sylvester to contribute to our deeper understanding of the history of science. Let me begin by sketching very briefly some of the highlights of Sylvester's life in an effort to convince you not only that he is a worthy subject for a biographical study but also that his biography can shed new light on the historical contours of nineteenth-century science.

James Joseph Sylvester (1814–1897) was, along with his friend and mathematical confidant, Arthur Cayley (1821–1895), arguably the most important mathematician Great Britain produced in the nineteenth century. As a researcher, Sylvester worked primarily on algebraic topics, developing the British school of invariant theory with Cayley [Parshall 1989] and making seminal contributions to the theory of algebras, to matrix theory [Parshall 1985], and to combinatorics [Parshall 1988]. From a technical point of view, a careful reading of his published works as well as his working papers and correspondence allows for the documentation and investigation of the development of fundamental mathematical theories at the same time that it provides fresh insights into broader historical issues like the changing conception of mathematical rigor. Moreover, in a very real sense, Sylvester was a pure algebraist a good half-century before such a category is generally thought to have existed and at a time when most of his contemporaries concentrated on questions in geometry and analysis [Parshall 1999]. A consideration of the technical aspects of his work thus also sheds light on the evolution of the disciplinary boundaries which have become institutionalized in mathematics during the twentieth century.

On the non-technical side, Sylvester lived in the nineteenth century, the century that witnessed the creation of much of the professional infrastructure of the sciences and other academic disciplines. Sylvester, however, was from Great Britain, a country *not* at the forefront of mathematical developments in the nineteenth century. This reflected itself in at least two key ways in his life. First, there was no real mathematical community in England at mid-century. Englishmen looking to the Continent, however, saw a variety of structures in place—research-level journals, positions, seminars, societies, etc.—that supported communities of high-level mathematical practitioners. Sylvester participated actively in establishing some of these same professional accoutrements in his homeland. Second, as an Englishman, Sylvester was effectively outside the Continental circles that set the research standards and, in so doing, conferred reputation. (This outsider status was only compounded by the fact that, as a Jew, the prestigious professional venues within Great Britain—namely, at Oxbridge—were also closed to him for most of the century.) Sylvester, a man of no small ego and true mathematical talent, thus "presented his case" before international judges in order to establish his reputation [Parshall and Seneta 1997]. His "fringe" status in British academic society also motivated two bold moves to the United States, one at the beginning of his career in 1841 and the other nearer its end in 1876. This second transatlantic foray resulted in the establishment of America's first true graduate program in mathematics and paved the way for the emergence of the American mathematical research community during the closing quarter of the nineteenth century [Parshall 1988], [Parshall and Rowe 1994]. With even this brief sketch as evidence, it seems clear that we can learn much about the history of mathematics from biography in general and from a biography of Sylvester in particular. I now turn to some of the specifics of the case of researching and writing that biography.

As a subject for the mathematical biographer, Sylvester offers an extensive printed and archival record with which to document the evolution of his mathematical thought. His collected works [Sylvester, *Papers*] run to some three thousand printed pages, and the vast majority of the surviving correspondence—well over twelve hundred known letters—shows Sylvester often hour-by-hour working through his latest ideas [Parshall 1998].[1] Both in print and in correspondence, moreover, Sylvester peppered his mathematical arguments with comments on, for instance, the impulses that led him to consider this or that mathematical topic, the thinking that lay behind his choice of this or that new mathematical term, even his broader philosophical stances (for an example, see [Parshall 1997]). The richness of the historical record thus allows the biographer to provide a nuanced account not only of Sylvester the working mathematician but also of the evolution of the mathematics he developed.

By way of example, consider some of Sylvester's earliest invariant-theoretic work in the first half of the 1850s. Guided by Cayley in his reading, Sylvester delved into the geometric works of George Salmon, Otto Hesse, Siegfried Aronhold, and others, and strove to find more purely algebraic approaches to the geometric questions arising there.[2] As Hourya Sinaceur noted in her book *Corps et modèles* in the context of Sylvester's work on Sturm's theorem in the late 1830s, Sylvester desired an algebrization of analysis [Sinaceur 1991, p. 126]; his work in the early 1850s showed him hard at work to affect an algebrization of geometry as well. Sylvester stated his position firmly in a footnote to an 1851 paper on "An Enumeration of the Contacts of Lines and Surfaces of the Second Order". "*Geometry, to be properly understood, must be studied under a universal*

[1] All page references to Sylvester's papers refer to the pagination in [Sylvester, *Papers*].

[2] The remainder of this paragraph comes from [Parshall 1998, p. 25].

point of view ...," he argued. "*In this way only (discarding as but the transient outward form of a limited portion of an infinite system of ideas, all notion of extension as essential to the concept of geometry, however useful as a suggestive element)* [in this way only] *we may hope to see accomplished an organic and vital development of the science*" [Sylvester 1851, p. 219]. The "*universal point of view*" that developed was precisely the algebraic theory of invariants.

By 1852, Sylvester had made his first major contribution to that theory in his multipartite paper, "On the Principles of the Calculus of Forms".[3] He opened this work with an explicit statement of the general and overarching nature of the theory. "*The primary object of the Calculus of Forms,*" he wrote, "*is the determination of the properties of Rational Integral Homogeneous Functions or systems of functions: this is effected by means of transformations; but to effect such transformation experience has shown that forms or form-systems must be contemplated not merely as they are in themselves, but with reference to the ensemble of forms capable of being derived from them, and which constitute as it were an unseen atmosphere around them*" [Sylvester 1852, p. 284]. To get a sense of Sylvester's conception of his mission here, briefly consider one of the problems he tackled in the work.

He took a binary $2n$-ic form and asked under what conditions is this transformable into the sum of n $2n$ powers of linear expressions in x and y. Typically, he actually considered not the general case but rather a specific small-order example, the binary quartic

$$ax^4 + 4bx^3y + 6cx^2y^2 + 4dxy^3 + ey^4. \tag{1}$$

The question was when can it be written as

$$(\beta_1 x + \gamma_1 y)^4 + (\beta_2 x + \gamma_2 y)^4,$$

or equivalently, after suitable change of notation, as

$$\beta_1(x + \gamma_1 y)^4 + \beta_2(x + \gamma_2 y)^4, \tag{2}$$

[3] This and the next two paragraphs present the same example as given in [Parshall 1999].

for $\beta_1, \gamma_1, \beta_2, \gamma_2 \in \mathbb{R}$. Notice that, by equating like coefficients, the equation (1) = (2) reduces to

$$\begin{aligned} a &= \beta_1 + \beta_2, & b &= \beta_1\gamma_1 + \beta_2\gamma_2, \\ c &= \beta_1{\gamma_1}^2 + \beta_2{\gamma_2}^2, & d &= \beta_1{\gamma_1}^3 + \beta_2{\gamma_2}^3, \\ e &= \beta_1{\gamma_1}^4 + \beta_2{\gamma_2}^4, \end{aligned}$$

or five equations in four unknowns. In general, then, it is not the case that a binary quartic can be written as the sum of two fourth powers; some additional condition will have to be satisfied. A bit of algebraic manipulation yielded, however, that solving this system is equivalent to finding γ_1, γ_2 satisfying simultaneously the three equations

$$\begin{aligned} ax_2 + bx_1 + c &= 0, \\ bx_2 + cx_1 + d &= 0, \\ cx_2 + dx_1 + e &= 0. \end{aligned}$$

Finally, the necessary and sufficient condition for this to happen is clearly

$$\begin{bmatrix} a & b & c \\ b & c & d \\ c & d & e \end{bmatrix} = 0.$$

After much discussion with Cayley, Sylvester called this determinant the *catalecticant* [Sylvester 1852, p. 292–293], [Elliot 1964, p. 267–268]. Cayley noted that all catalecticants G are, in fact, invariants. This thus showed that, provided the catalecticant of the binary quartic is zero, the binary quartic can be linearly transformed into the sum of two fourth powers.

Now reconsider this in light of Sylvester's statement of the objectives of the calculus of forms. In order to find a transformational property of the binary quartic form, the focus shifted to an associated form derived from it, namely, the invariant called the catalecticant, one of what Sylvester termed "*the ensemble of forms . . . which constitute as it were an unseen atmosphere around*" the binary quartic form [Sylvester 1852, p. 284]. The invariant-theoretic approach allowed for the algebrization of what was fundamentally a geometrical question.

The mathematical biographer reconstructing the year 1852 in Sylvester's mathematical life thus has incredibly rich sources to draw from in tracing his thought, in documenting his research techniques and style, and in coming to terms with his mathematical persona. The mathematical biographer also has ample sources for tracing Sylvester as he crafted an economic niche for himself in a Victorian Britain that did not recognize the category "mathematician" as a wage-earning category distinct from "mathematics teacher" or "mathematics don" and as he established his reputation as a research mathematician both at home and abroad. The biographer—as opposed to the *mathematical* biographer—of Sylvester often faces more of a challenge, however, in obtaining a nuanced understanding of the non-mathematical aspects of the man.

Consider, in this regard, the problem of knowing Sylvester, the student of St. John's College, Cambridge. Sylvester pursued his studies in Cambridge from 1831 to 1837. Not surprisingly, no papers from this period appear in his collected works; he apparently kept no diary; there are no known letters to family or friends. How can the biographer penetrate this wall of silence and come to know the young man during what were presumably his formative stages as a scholar? Institutional records, institutional histories, accounts of contemporaries or near contemporaries, these types of sources can provide some insight. In Sylvester's case, we can piece together many aspects of his years at Cambridge from such sources. From contemporaneous accounts, we know how a so-called "reading man," that is, a serious student at Cambridge to study hard and to do well on the culminating Tripos examination, spent a typical day [Bristed 1874]. We have vivid descriptions of what sitting for the Tripos was like [Bristed 1874], [Rouse Ball 1889]. We know the sort of lectures Sylvester heard and the books he would have read to prepare for the various examinations. We know who his tutors and coaches were, and we know how some of them conducted their training sessions. We also know who some of Sylvester's friends were, and some of them left anecdotes both about College life and about their friend, Sylvester.

The archival record, moreover, is not totally barren in the case of Sylvester's Cambridge years. In 1836, he wrote and had privately printed a pamphlet entitled *A Supplement to Newton's First Section, Containing a Rigid Demonstration of the Fifth Lemma, and the General Theory of the Equality and Proportion of Linear Magnitudes* [Sylvester 1836]. As is well known, Newton opened Book I of the *Principia* by laying out the various mathematical lemmas needed in the analysis of the motion of point masses. Of the eleven lemmas he gave, only his fifth was stated without any further justification: "[*a*]*ll homologous sides of similar figures, whether curvilinear or rectilinear, are proportional; and the areas are as the squares of the homologous sides*" [Newton 1962, 1, p. 32]. What a tease for the serious student steeped in the logical rigors of Euclid's *Elements*! If the lemma is not an axiom, then it requires a rigorous proof. Newton's oversight was thus Sylvester's challenge. What better way to begin establishing one's mathematical reputation than by filling in a gap in the work of the great Newton?

Without going into the details of the mathematics contained in Sylvester's essay, suffice it to say that the pamphlet provides a glimpse of the mathematics he studied at Cambridge in the 1830s and a glimpse, too, of the mind of a serious, reflective, cocky personality at work. Another aspect of this pamphlet, however, provides even greater insight into the young Sylvester. The copy held in the collection of University College, London, was heavily annotated by Sylvester in August of 1836 and contains musings on the content of the essay as well as theological, philosophical, and even existential reflections. "*Mind or a man's personality is said to be where that is in which motion must take place in order for our sensations to be produced in the mind,*" Sylvester stated with indebtedness to John Locke. "*∴ To say God knows all things is to say he is everywhere. Thus the idea of the omniscience of Diety comprehends the idea of his ubiquity*" [Sylvester 1836]. He also wondered

"*May (or is) not what is called coming to a consciousness of our own existence (recorded by others & experienced by the author to the best of his belief) be the coming to a consciousness of the separate existence of other persons no longer as images merely of the brain but as possessing wills akin to our own? a consciousness obtained from the production of effects in ourselves (similar to effects) known to be produced by our own will & generally not except by our will) without the action of our will as where Jesus saith 'who has touched me, for I know that the Virtue is gone forth from me'*" [*Ibid.*].

The Cambridge-trained student, quoting here from Chapter 5, verse 30 of the Gospel according to St. Mark, further revealed that the required reading of the New Testament had made a certain impression upon a young man raised in the Jewish faith. This 1836 work on Newton's fifth postulate—as well as the hand-written draft of ideas for what was apparently intended to be a subsequent work—thus allows the biographer to penetrate beyond the social aspects of the Cantabridgian's life and to glimpse his developing mind at work.

With these two examples from the biography of Sylvester now as cases in point, let me conclude with a brief analysis of the strengths and weaknesses of biography as methodology in trying to understand J.J. Sylvester, his life, work, and times. First, as the example of Sylvester's developing ideas on invariant theory illustrates, the biographical context naturally highlights various dimensions of the mathematician at work—interaction with colleagues, reading and writing research papers, communication with others through lectures and society meetings—as it allows for an analysis of the evolution of mathematical thought. This is undoubtedly one of the strengths of biography as a methodology for the history of mathematics in general and for an analysis of Sylvester in particular. Biography highlights the maker of mathematics as the mathematics is being made. Moreover, if, as Thomas Hankins urged, biography is "honest," it shows the mathematician's successes and failures, false starts and dead ends, thereby underscoring the complexity of the development of mathematics, a complexity that is often masked by methodologies of intellectual history. Given the extant historical record, Sylvester is a prime candidate for highlighting this aspect of what it means to do mathematics. But, as the example of Sylvester's student days at Cambridge underscores, Sylvester taxes the biographer because most of the remaining historical record deals with him *as a mathematician*; it is hard—but not impossible—to get a sense of him outside of his mathematics. The main weakness of biography as methodology for an analysis of Sylvester, then, is that it provides insights more into the mathematician than into the man, although the man still does come through. The remaining historical record does not permit for Sylvester the sort of sustained existential biography that Thomas Söderqvist called for, namely, one in which the life is narrated "*as it is directly experienced by the biographical subject*" [Söderqvist 1996, p. 73].

Sylvester's life course—and his ways of dealing with the obstacles he encountered in his path—provides a lens through which to view modern algebra during a crucial developmental period, England at a time of increasing professionalization relative to science, and Great Britain, Europe, and the United States as their mathematical communities formed and interacted internationally. In the case of Sylvester, biography as methodology also allows for a rich, multilayered look at a fascinating man as well as at mathematical theory and mathematical culture in the nineteenth century. Moreover, biography as methodology permits us to ground the development of mathematics and of mathematical communities *at the level of the individual*. It puts us "in the trenches," so to speak, and provides an intimate look at—not a detached, generalized view of—what it meant to be a mathematician and to do high-level mathematical research in nineteenth-century Britain. Biography puts a human face on mathematics, shows it as a human endeavor, demystifies it. And, although necessarily particularistic, biography in context also allows for generalization about what constituted a mathematical theory and about what the morays of the scientific and/or mathematical culture were at a particular point in time. These strengths of biography as methodology relative to Sylvester's life, work, and times far outweigh its weaknesses. Taking Sylvester as a case in point, moreover, underscores the potency of biography as methodology in the history of mathematics.

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