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The dialectics of differentiation: Marx's mathematical manuscripts and their relation to his economics

Peter Hans Matthews

Department of Economics, Middlebury College, Middlebury, VT, USA

ABSTRACT
The notion that Marx neither understood nor advocated the use of mathematics is a persistent one. His interest in both commercial and abstract mathematics spanned more than two decades, however, and culminated in two 'contributions' to the foundations of the calculus: 'On the Concept of the Derived Function' (1881) and 'On the Concept of the Differential' (1881). A detailed examination of these and other technical notebooks suggests that Marx's economics both motivated and informed his studies in mathematics and that these, in turn, influenced his understanding of economic phenomena.

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> [Your paper on the derived function] has got such a hold over me that it not only keeps going round in my head all day, but last night I actually had a dream in which I gave a fellow my shirt buttons to differentiate and he made off with the lot.
>
> Letter from Engels to Marx, 18 August 1881 (*Works* V46: 131–132).

1. Introduction

It is unfortunate,[1] perhaps, that recent debates concerning the use of 'formal' or technical methods within the Marxian tradition seldom refer to Marx's own

CONTACT Peter Hans Matthews pmatthew@middlebury.edu

[1] The current version of this now mature paper owes much to the encouragement of participants at a recent Analytical Political Economy workshop in London, especially Roberto Veneziani and Peter Skott, and two meticulous reviewers. Earlier versions benefited from the comments of Antonio Callari, Richard Cornwall, Carolyn Craven, John Geanakoplos, David Weiman and Paul Zarembka, as well as Peter Hardin, who introduced me to Brokmeyer's (1985) reading of the manuscripts, and Michael Meo, one of their often unacknowledged English translators. The interest of Alain Alcouffe, who translated the manuscripts into French, is also acknowledged. Whatever errors remain, however, are mine alone.

mathematical manuscripts, which were inaccessible to most scholars until the English translation (Marx, 1983), more than three decades ago, of an obscure Soviet (Marx, 1968) collection.[2] The translation was the result of a serendipitous and seldom acknowledged collaboration between Charles Aronson, a sometime mathematics professor, and Michael Meo, a graduate student in history, at the University of California at Berkeley, and was first published by a Trotskyist splinter group.[3] In fact, these manuscripts, almost a thousand handwritten sheets in all, have seldom attracted much attention outside the former Soviet Union, despite the efforts of some contemporaries and later scholars. In his speech at Marx's funeral, for example, Engels himself offered what must have seemed a surprising tribute to some of the mourners:

> [T]wo such discoveries [the laws of historical development and economic motion] would be enough for one life time. Happy the man to whom it is granted to make even one such discovery. But in every single field that Marx investigated – and he investigated very many fields, none of them superficially – in every field, *even in that of mathematics*, he made independent discoveries. (Works, n.d. V24: 468, emphasis added)

Soon after, Engels would note in the preface to the second edition of *Anti-Dühring* (1885, p. 17) that he intended to publish some further reflections on the natural sciences 'in conjunction with the extremely important mathematical manuscripts left by Marx,' a promise he never fulfilled. In his subsequent role as the principal editor of the third volume of *Capital* (1894), he would discover a second set of technical notebooks, which included Marx's first efforts at 'mathematical economics':

> For the third chapter, there was not only a whole series of incomplete mathematical drafts, but also an entire notebook from the 1870s, almost complete, which presented the relationship between the rate of surplus value and the rate of profit in equations. (Marx, 1894, p. 94)

The notebook, entitled 'The Mathematical Treatment of the Rate of Surplus Value and the Rate of Profit,' is the principal concern of Smolinski's (1973) remarkable paper, one of the few studies of the technical dimensions of Marx's economics to draw on the archives at the Institute of Social History of Amsterdam.

The contents of Marx's notebooks would remain obscure, however, until the 1930s, when brief selections first appeared, in Russian translation, in *Unter dem Banner des Marxismus* and *Markism i Estestvoznanie*. At about this time, a

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[2] *The Mathematical Manuscripts of Karl Marx* (1983) includes brief but annotated selections from the more complete *Mathematischeskie Rukopsii*, not least of which are the drafts and final versions of Marx's two 'contributions' to the calculus, 'On the Concept of the Derived Function' and 'On the Concept of the Differential,' as well as drafts of the unfinished 'On the History of Differential Calculus' and notes on Lagrange's treatment of infinite series and d'Alembert's limit-based definition of the derivative. It also includes the commentaries of Kol'man and Yanovskaya (1931), Kol'man (1968), Yanovskaya (1968) and Smith (1983).

[3] This account is based on personal correspondence with Michael Meo.

surprise Soviet delegation to the Second International Congress on the History of Science and Technology, led by Nicholai Bukharin, presented a series of papers, one of which 'announced' the existence of the manuscripts and other notebooks, while a second (Kol'man, 1931b, p. 237) noted that 'the hitherto unpublished writings of Marx dealing with mathematics ... which will shortly be published by the Marx-Engels Institute, are of tremendous methodological importance.'[4] A complete and annotated *Mathematischeskie Rukopsii*, in facsimile and also in Russian and German translations, was not published until 1968, however. Smolinski (1973) speculated that the cunctation was the unfortunate consequence of the official suppression of 'mathematical deviation' in Soviet economics after the 1930s, itself a manifestation of the persistent suspicion that formal models often concealed 'bourgeois tendencies.'[5]

This does not account for the treatment of the notebooks outside the Soviet Union after the 1930s, however. With few notable exceptions – Struik (1948), one of the first mathematicians outside the Soviet Union to discuss the manuscripts, and the aforementioned (Smolinski, 1973), for example – the published selections excited little attention in the west and the archives in Amsterdam were seldom consulted. There are at least three possible explanations for this, the simplest, and most charitable, of which appeals to the sheer size of the archives: most of the numerous drafts, notebooks and letters that Marx left behind were neither 'recovered' nor published for decades. (It might be recalled, for example, that the complete *Grundrisse*, a manuscript at least as important as the mathematical notebooks, was not published until 1953.) A second, more specific, reason lies in Marx's durable but somewhat undeserved reputation as a poor mathematician, one for whom even arithmetic was a challenge. The notion that Marx was an innumerate of sorts, often associated with the critiques of Bortkiewicz (1890) and Pareto (1903), owes much to the computational errors and uncertainties familiar to modern readers of *Capital*, especially the posthumous third volume. The manuscripts reveal, however, that if, as Marx himself once confessed, 'I have never felt at home with arithmetic' (Works V40: 244), the principles of more advanced mathematics

[4] The Soviet contribution to the conference, which drew considerable attention from the scientific media (see Greenwood, 1931, for example) would be collected and published as *Science at the Cross Roads* (1971). This influential volume included Boris Hessen's (1931) 'The Social and Economic Roots of Newton's *Principia*,' which Needham (1971, p. vii) would later describe as the 'classical statement of the Marxist historiography of science.' Werskey's (1971) introduction to the reprinted conference proceedings contains biographical sketches of the contributors, including the aforementioned Ernst Kol'man, the mathematician most associated with Marx's manuscripts (Kol'man 1931a). He is described in ambiguous terms, as someone who, in the 1930s, managed to be 'simultaneously a partisan of Lysenko and a spokesman for the "liberal" defense of Einsteinian physics,' but then disappeared until the 1950s, when he 'emerged ... as an advocate of cybernetics' (Werskey, 1971, p. xvi). For much of his life, Kol'man seems to have 'command[ed] a schizophrenic [sic] reputation as a liberal in Russia [but] a rigid ideologue in his native Czechoslovakia' (Werskey, 1971, p. xvi). The promise of a Marx-Engels Institute volume was, like Engels' original promise, never realized.

[5] This is consistent with what little is known about the career of Ernst Kol'man, the mathematician who first announced the existence of the manuscripts. For more details, see the previous footnote.

were much less elusive and, as outlined in the fourth section, provide some support for Shaikh's (1977) claim that his own resolution of the transformation problem was more incomplete than incorrect, *and that Marx understood this*.

The sense that mathematical methods are somehow inconsistent with Marxian economics has no doubt also limited interest in the archives. Whether or not the recent successes of various strands of 'quantitative Marxism' have tempered this view, Marx himself believed, as Lafargue (1891, p. 13) quotes him, that 'a science becomes developed only when it has reached the point where it can make use of mathematics,' a proposition from which political economy was *not* excluded. The archives, as Smolinski (1973) further notes, contain no criticisms of mathematical economics *per se*, and there are none, as far as I have been able to determine, in his voluminous correspondence either.

The next section outlines the various influences on Marx's mathematics, with an emphasis on his detailed studies of the calculus. Some, like Lagrange's *Théorie des Fonctions Analytiques* (1813), are either obvious or explicit, while others, in particular those with roots in his economics, are subtle and implicit. The third section examines the two 'papers' that Marx considered his principal contributions to mathematics, 'On the Concept of the Derived Function' and 'On the Concept of the Differential,' both of which he finished in 1881, not long before his death. Neither of them can be understood, however, without reference to 'On the History of Differential Calculus,' his unfinished periodization of the pre-Cauchian period. The fourth and final section turns to the controversial role of mathematics in his economics, from the little known – 'The Mathematical Treatment of the Rate of Surplus Value and the Rate of Profit' or his correspondence concerning von Thünen's *Der Isolierte Staat* (1860) – to the more familiar *Capital*.

2. Marx and the 'Golden Zero'

Marx's mathematical studies would span more than two decades,[6] from the late 1850s to his death in 1883. His initial interests, practical and often narrow, were the result of research undertaken for the second and third volumes of *Capital*, some of the notebooks from this period concern commercial arithmetic – the methods used to discount various sorts of bills, for example, are reproduced in a series of exercises – as reviewed in Feller and Odermann's then standard text. There is some evidence that Marx was also interested in the *evolution* of business mathematics: a letter to his maternal uncle Lion Philips (*Works* V41: 514–516), for example, includes a brief discussion of Boethius' *De Arithmetica* and the historical development of household and commercial

[6] Bell (1940, p. 280) describes the evolution of pre-modern calculus as the Golden Age of Nothing, a reference to the power of the ambivalent – sometimes zero, sometimes not – differential. L*é*onard Euler's controversial 'calculus of zeroes' was one of several attempts, for example, to resolve the ambivalence with an ordered set of similar, but not identical, zeroes, one of which included the notion of the differential.

accounts. As the computational burdens of *Capital* became heavier, for example, the scope of his studies widened:

> In elaborating the principles of economics I have been so damnably held up by errors of calculation that in despair I have applied myself to a rapid revision of algebra. I have never felt at home with arithmetic. But by making the detour via algebra, I shall quickly get back into the way of things. (*Works* V40: 244)

This work often assumed the form of detailed notes on popular textbooks – Lacroix's *Complément des Eléments d'Algebre* (1818), for example – and evinced Marx's initial fascination with infinite series representations of familiar functions, about which more below.

Before the first volume of *Capital* (1867) was published, Marx had acquainted himself, for reasons that are *not* obvious, with the basics of differential and integral calculus, and would boast that 'it is much the easier part of mathematics ... than the higher parts of algebra, for instance' (*Works* V41: 483). In an appendix to a letter to Engels written soon after Marx's visit to Manchester in October 1865, for example, he promises:

> While I was last ... in Manchester, you ... asked me to explain differential calculus to you. [It] will be quite clear from the following example. The whole of differential calculus arose ... from the task of drawing tangents through a point on a curve. So this is the example I am going to use for you ... (*Works* V42: 208–9)

What follows is an illustrated explanation of the derivative as the slope of the line between two points that are 'infinitely near' one another on a curve, a definition he soon suspected was unsound. It is after this, Struik (1948) and others have noted, that Marx's studies acquired their rather more abstract character: the papers on the derived function and differential, neither of which includes practical illustrations, are perhaps the best example. This does not mean, however, that his mathematics became self-contained or recreational after 1870 – his interest in the foundations of the calculus should be understood in terms of a broader scientific enterprise, one in which his economics assumed a central role. Kennedy (1977) conjectures that Marx's interest in mathematical characterizations of dialectical processes in nature stimulated an interest in the dialectical foundations of mathematics itself. It should also be remembered that because he died before his 'technical studies' were completed, the connections to Marx's other work were never made explicit, and must remain, at least in part, a matter of informed speculation.

While Marx's Gymnasium teachers complimented his 'good knowledge of mathematics' (Kennedy, 1977; Yanovskaya, 1968) notes that Marx's formal studies in mathematics were oriented around the texts that Cambridge students used during this period. This is important inasmuch as the foundational contributions of continental thinkers in the 19th century – in particular, Cauchy in the 1820s and Weierstrass in the 1860s – would not be incorporated into the British curriculum for some time, a reflection of the much earlier schism between Newton and Leibnitz.[7] It seems reasonable to conclude that Marx's choice of texts reflects Samuel Moore's influence as a mentor of sorts. Moore, the Manchester barrister responsible, with Edward Aveling, for the first English translation for the first volume of *Capital* and Engels' assistant in the eventual 'reconstruction' of the third volume, had studied mathematics as a Cambridge undergraduate in the 1850s. Smolinski (1973) asserts that both Marx and Engels accepted Moore's judgment as the final word on technical matters, but this overstates his role: from time to time, Marx would complain that Moore did not understand the subtleties of a particular problem.[8] On one such occasion, Marx had written to Moore with an intuition about the decomposition of economic time series into the sum of periodic functions[9] and after Moore had (mistakenly) declared the problem insoluble, shared his dissatisfaction in a subsequent letter (*Works* V44: 504) to Engels. In another letter, written after Moore had read, and responded to, 'On the Concept of the Derived Function,' he complains that his tutor failed to appreciate the fundamental differences between his own 'algebraic' conception of the derivative, and the 'geometric' notion still familiar to beginning calculus students:

> Sam ... criticizes the analytic method applied by me inasmuch as he quietly sets it aside and instead turns his attention to the geometrical application, about which i said nothing at all ... I might [also] dismiss the whole of [the] historical development of analysis saying, in practice, no essential change has been brought about in ... differential calculus. (*Works* V46: 380)

None of this should dismiss Moore's contribution to Marx's studies, however: there is no indication that Marx ever consulted another mathematician, amateur or professional.

Marx's interest in the foundations of the calculus dates to his notes on Francoeur's *Cours Complet de Mathématiques* (1819), an antiquated but popular exposition of Leibnitzian principles. He was soon drawn, however, to Bourcharlat's *Elementary Treatise on the Differential and Integral Calculus* (1828), the English translation of an influential (almost 10 editions were published in his lifetime) French primer on the methods of d'Alembert and Lagrange. Marx was attracted to Lagrange's 'algebraic derivative' despite the fact that he believed, for reasons that will be outlined later, that its promise of rigor was never fulfilled. He would also compile detailed notes on several other well known texts,

[7] The preface to G. H. Hardy's landmark *Course in Pure Mathematics* (1937) includes a blunt assessment of the Cambridge curriculum before 1900.

[8] The surprise, perhaps, is that despite his own interest in the sciences, Engels' understanding of mathematics remained, at best, limited. As Van Heijenoort (1948) uncharitably characterized him, 'Engels does not show the slightest aptitude for mathematics.' (Heijenoort, better known to some as one of Trotsky's secretaries in exile, would himself become an accomplished mathematician.

[9] The fourth section contains a more detailed discussion of this important letter. The fact that Moore was unfamiliar with Fourier's celebrated *Théorie Analytique de la Chaleur* (1822) and other relevant works reveals his limitations as a tutor.

including Lacroix's monumental *Traité du Calcul Différentiel et du Calcul Intégral* (1819) and Hind's *The Principles of Differential Calculus and Curve Surfaces* (1831), a Cambridge course manual.[10] Marx's eventual dissatisfaction with textbook simplifications and inconsistencies would also lead him to consult a small number of classic works, from Newton's *Analysis per Quantitatum Series Fluxiones et Differentias* (1711) and Taylor's *Methodus Incrementorum Directa et Inversa* (1715) to Lagrange's *Théorie des Fonctions Analytiques* (1818).[11]

The absence of Cauchy's *Cours d'Analyse* (1822), whose 'new approach led to [its own] difficulties and generated problems of rigorization for [his own] successors' (Kitcher, 1984, p. 254), problems that culminated in Weierstrass's modern foundations for the calculus, from this list is conspicuous. There is no evidence that Marx was familiar with the *Cours* or that he understood its importance for analysis, a surprise until it is recalled that Moore had in all likelihood never studied it. Even where the *Cours* was better known, its contents would not be incorporated into textbooks for decades: Struik (1948) notes that the first such text, Jordan's own *Cours d'Analyse*, was not published until 1882, long after the contributions of Abel, Drichlet and Weierstrass had overtaken it! That Marx was also unfamiliar with Weierstrass's 'epsilontics'[12] then comes as no surprise at all. Historians of mathematics – see the accounts in Bell (1937, 1940) or Kline (1972), for example – have characterized the dissemination and refinement of Weierstrass's work as a slow and often tentative process, from a series of obscure papers in the 1840s to its *professional* acceptance in the 1880s.

Because the influence of continental 'innovations' in mathematics on Marx's studies was at best limited, some of his notebooks seem archaic, even primitive, to modern readers, notwithstanding Yanovskaya's (1968) forceful argument that he would have rejected such innovations, an assertion rooted in his critique of d'Alembert's *Traité de l'Equilibre et du Mouvement des Fluides* (1844), an important precursor of the *Cours*. (This said, Struik, 1948 also notes that Marx's appreciation for d'Alembert's accomplishments is more sophisticated than most.) Yanovskaya's (1968) inference is a fragile one, however: there was an enormous difference – even a paradigmatic shift – between, on the one hand, d'Alembert and Cauchy and, on the other, Weierstrass, and in the context of Marx's professed interest in the 'algebraic foundations' for the derivative, it is not obvious that he would have dismissed the latter's 'wish to establish the calculus ... upon the concept of number alone' (Boyer, 1949, p. 285). Nor does

[10] Bell (1940, p. 72) observes that the

> "chasm between the old and new that opened up in 1821 [with the publication of Cauchy's *Cours d'Analyse*] is plainly visible on comparing two classics: Cauchy's just cited, and the third edition, in 1819, of Lacroix's [treatise]. If the later work is mathematics, the earlier work was not."

[11] A complete list of Marx's sources can be found in The Mathematical Manuscripts, pp. 273–274.

[12] The term derives from the now standard $\epsilon - \delta$ definitions of limit and derivative. For more details, see, for example, Kitcher (1984).

an appreciation for Marx's achievements in mathematics turn on this sort of 'defense': there are other senses in which his intuitions might be considered sophisticated.

Kol'man and Yanovskaya (1931) and Smith (1983) have also drawn attention to the influence of Hegel's *Science of Logic* (1822) – in particular, its treatment of 'quantitative infinities' – on Marx's studies. The correspondence between Marx and Engels reveals that both were conversant with Hegel's efforts 'to demonstrate that the infinitely small ... [did not] ... have the negative, empty meaning of a non-finite, non-given magnitude' (Smith, 1983, p. 260). Engels came to believe that Marx alone understood the Hegelian critique of the infinitesimal. In March 1865, he would tell Friedrich Lange:[13]

> There is a remark about ... Hegel which I cannot less pass without comment: you deny him any deeper knowledge of the mathematical sciences. Hegel knew so much about mathematics that none of his disciples was capable of editing the manuscripts he left behind. The only man who, to my knowledge, has enough understanding of mathematics and philosophy to be able to do so is Marx. (*Works* V42: 138)

Much later, after Engels had read 'On the Concept of the Derived Function,' he wrote to Marx, to tell him that it confirmed one of Hegel's intuitions in *Science of Logic*:

> So old Hegel was quite right in supposing that the basic premise for differentiation was that most variables must of varying powers and at least one of them must be of the power of at least 2 or 1/2. Now we also know why. (*Works* V46: 131)

This is an awkward reference to Marx's discussion of the derivative in terms of the coefficients in series expansions, part of his broader interest in algebraic foundations for the calculus, the same interest that had motivated Lagrange.

This said, if Marx understood and, for the most part, endorsed Hegel's criticisms, he shared none of the latter's obvious disdain for mathematics, a posture that, in the end, limited both Hegel's audience and his influence. Mathematicians could, in turn, dismiss the sometimes impenetrable dialecticism of *Science of Logic* as the *cri de couer* of a crude polemicist.[14]

To understand Marx's desire to recast the foundations of the calculus, one must turn to 'On the History of Differential Calculus,' the unfinished periodization that serves the same role in his mathematical studies as *Theories of Surplus Value* does for his economic ones, albeit on a much smaller scale. The terms and

[13] Lange, the philosopher and sometime political economist, was also a prominent member of the First International, and is perhaps best remembered for the neo-Kantian History of *Materialism and Criticism of its Present Importance* (1877).

[14] Bell (1937, pp. 239, 489) believes that the immature Hegel's ill-considered criticisms of astronomers – he once announced, for example, that the search for additional planets was doomed to failure – secured him an unfair reputation as an innumerate of sorts. He also notes, however, that the influential Krönecker's generous reading was an important exception.

notation are sometimes eccentric, but the periodization is sensible and modern: Bell, in his *Development of Mathematics* (1940), for example, identifies the same three pre-Cauchian schools. In Marx's language, these are the 'mystical differential calculus' of Newton and Leibnitz, the 'rational differential calculus' of Euler and d'Alembert, and the 'algebraic calculus' of Lagrange.

The criticism of Newton's 'fluents' and 'fluxions' is reminiscent of Bishop Berkeley's acerbic appraisal in *The Analyst* (1734), published more than a century earlier.[15] It is difficult to believe, in fact, that neither Marx nor Moore were unfamiliar with it – he was a source of preoccupation in British mathematics for decades[16] – despite the fact that neither mentions it. To recast the critique in terms of an example familiar to both Marx and modern readers, let $Q = F(L) = a + bL + cL^2$ be the output of 'corn' on a particular plot of land, a quadratic function of the number of laborers L hired. If L increases from L_0 to $L_1 = L_0 + dL$, where dL is Leibnitz's differential, the increase in output $Q_1 - Q_0 = dQ$ is equal, after some simplification, to $b(dL) + 2cL_0 + c(dL)^2$. If the squared term is much smaller than the first two, then $b(dL) + 2cL_0$ is a reasonable approximation for dQ, that is, $dQ \approx (b + 2cL_0)dL$ or $(dQ/dL) \approx (b + 2cL_0)$. This did not mean, as Marx and others before him understood, however, that dQ and $(b + 2cL_0)dL$ were equal unless dL was itself zero, an essential element in Berkeley's critique in *The Analyst*. Nor could these difficulties be resolved if, as textbooks of the time often recommended, the ratio of $b(dL) + 2cL_0 + c(dL)^2$ to dL were calculated *before* the latter was set equal to zero because, as Marx (1883: pp. 82–83) explains it:

> The nullification of [dL] may not take place prior to the first derived function of [L], here [$b + 2cL$], having been freed of the factor [dL] through division, thus [$Q_1 - Q_0 = b + 2cL_0$]. Only then may the finite differences be annulled. The differential coefficient [$dQ/dL = b + 2cL$] therefor also must have been previously developed ... before we obtain the differential [$dQ/dL = b + 2cL$].

This is Berkeley's 'ghosts of departed quantities' argument in different language: if dL is zero, then dQ must also be zero, in which case the ratio $dQ/dL = 0/0$ is nonsensical, but if dL is non-zero, then it cannot be made to 'disappear.'

This does not mean that the achievements of the 'mystics' were underestimated, however. In his *Anti-Dühring*, Engels (1885, p. 98) would echo Marx's admiration of both:

> With the introduction of variable magnitudes and the extension of their variability to the infinitely small and infinitely large, mathematics, in some respects so strictly moral, fell from grace: it ate from the tree of knowledge, which opened

[15] Bishop George Berkeley was the eighteenth century Irish philosopher best remembered for his defense of 'immaterialism' and associated criticisms of Locke and Descartes.

[16] This preoccupation proved to be ironic. As Kitcher (1984) observes, it meant that Newton and his followers were far more concerned with the issue of rigor than Leibnitz and his continental heirs, whose interests were often more practical. It would be the anomalies in applications of the calculus, however, that in the end stimulated the search for alternative foundations, while mathematics in Britain languished in comparison.

> up to it a career of most colossal achievements, but at the same a path of error. This virgin state of absolute validity and irrefutable certainty of everything was gone forever ... [We] have reached the point where most people differentiate and integrate not because they understand what they are doing but from pure faith, because up to now it has always come out right.

Read from this perspective, 'On the History of Differential Calculus' is also a meditation on the strengths and weaknesses of 'practical science,' science that risks being misled, to paraphrase Marx's later dismissal of Hume's economics, by the 'appearance of things.'

How much, if at all, Marx's critique of Newton and Leibnitz reflects his prior work in economics is an important but to date overlooked question. As alluded to in the introduction, the influence is implicit, but it seems impossible that he never visualized these 'variable magnitudes' in economic terms. The texts with which he was acquainted often borrowed from the 'hard(er) sciences' to illustrate and, in some cases, demonstrate basic principles, and the possibilities for 'translation' must have seemed irresistible. It is still the case, for example, that economics students with even minimal mathematical skills perceive opportunities to formalize elements of Ricardo's *Principles* (1817), a text with which Marx was of course familiar, in differential terms. The same students often find it difficult, however, to conceptualize the 'infinitesimal increment' in terms of land or labor, a problem Marx never discusses but could well have contemplated. He believed, for example, that von Thünen's *Der Isolierte Staat* (1860), which did so, was an important, if not landmark, contribution to the classical tradition.[17]

It is at least conceivable, then, that the abstractness of Marx's mathematics after 1870 was in some small measure attributable to the 'discontinuities' in his notion of economic movement, which would have underscored the shortcomings of smooth and well-behaved differential representations. In this context, Smolinski's (1973) observation that Moore would recommend George Boole's *Calculus of Finite Differences* (1880) to Marx acquires special significance: Boole's calculus is *prima facie* more consistent with Marxian priors. It is unfortunate, then, that he never found the time to read *Calculus of Finite Differences* or to contemplate Boole's interest in the separation of the 'symbols of mathematical operation from [the objects on which these] operate' (Bell, 1937, p. 438), which is reminiscent of Marx's concern with 'strategies of action.'

Marx's account of the 'mystical' origins of the calculus also raise another question: to what extent did his characterization of the differential reflect a suspicion of the 'unseen' in mathematics? Philosophers and historians of mathematics have sometimes dismissed similar passages in Berkeley's much earlier critique, for example, as the unsophisticated outburst of someone who would

[17] Marx's assessment of von Thünen's work, contained in two letters written in the late 1870s, is also discussed in the fourth section.

have also rebelled at the notion of a complex number, another 'invisible' construction. While there is little doubt that 'suspicions of the invisible' animate *The Analyst*, Jesseph (1993) and others have demonstrated that Berkeley was no less convinced that Newton's 'fluxions' were inconsistent on their own terms. Furthermore, despite Berkeley's iconographic status as the embodiment of clerical resistance to science, his reading of Newton was 'competent but uncharitable'[18] (Kitcher, 1984, p. 239). Marx's periodization merits a similar verdict: his discussion of the 'effective operation' of differentiation seems to reveal some of the same suspicions but, in the end, underscores the internal contradictions of pre-Cauchian calculus. One is therefore tempted to wonder how Marx would have responded to more recent interest in so-called 'non-standard methods' in mathematical economics, in which an arithmetic of the infinitesimal has been axiomatized.[19] In this context, it is remarkable that in the late 1970s, Chinese mathematicians rationalized a new research program in non-standard methods in explicitly Marxian terms (Dauben, 1998).

Marx locates the transition to the 'rational differential' calculus in d'Alembert's *Traité* (1744), one of the first monographs to define the derivative in terms of the ratio of limits: for the previous production function $Q = F(L)$, $dQ/dL \equiv \lim_{\Delta L \to 0} \Delta Q/\Delta L = \lim_{\Delta L \to 0}(Q(L + \Delta L) - Q(L))/\Delta L$. The substitution of ΔL for dL as one of the definition's primitives 'stripped the mystical veil from the differential calculus and [so constituted] an enormous step forward' (Marx, 1983, p. 97), one that mathematicians had resisted for some time. Marx (1983," p. 97) further indicates that despite d'Alembert's achievement, the 'Leibnitzian method continued to prevail for years in France' while 'Newton prevailed in England until the first decades of the nineteenth century,' a verdict with which most historians of mathematics would more or less concur (Kline, 1972).

d'Alembert's 'solution' is also seen to be flawed, however:

> [As] with the mystics, however, [the derivative function] already existed as given, as soon as [L] became [$L + h$], for [$a + b(L + h) + c(L + h)^2$] in place of [$a + bL + cL^2$] produces [$(a + bL + cL^2) + (b + 2cL)h + ch^2$], where the [derivative, $b + 2cL$] already appears as the coefficient of h, raised to the first power. The derivation is therefore essentially the same as in Leibnitz and Newton, but the ready-made derivative is *separated* in strictly algebraic manner from its other companions. It is no *development* but rather a separation of [$F'(L)$] ... from its factor $h$and from the neighboring terms ... in the second series. What has on the other hand really been developed is the left-hand, symbolic, side, namely [dL], [dQ] and their ratio, the symbolic differential coefficient [dQ/dL] $= 0/0$ ... which in turn generates certain metaphysical shudderings, although the symbol has been mathematically derived. (Marx, 1983, p. 96)

[18] Kitcher's (1984) qualification is based on several ambiguous passages in Newton's Treatise on Quadrature, which seem to anticipate the limit arguments of d'Alembert and others.

[19] For an introduction to non-standard methods and their uses in economics, see Anderson (1991). To the extent that the rehabilitation of the infinitesimal allows the notion of an 'atomistic' actor to be formalized, these methods have resonated more with some economists than others.

The problem seems to lie in the machinations – more reasonable than Newton's and Leibnitz's, but still unsound – needed to disentangle derivatives that are known *a priori* from the finite and non-zero increment h but if so, Marx's preferred definition (see below) is vulnerable on the same grounds. More important, however, Marx locates the real contribution of *Traité des Fluides* in its treatment of dQ/dL as an operator or, as he describes it, a 'strategy of action' rather than as the ratio of ill-conceived infinitesimals. From this perspective, the notation dQ/dL embodies a finite series of well-defined arithmetic or algebraic operations, in which the 'numerator' dQ and 'denominator' dL are not independent entities and so do not call for interpretation as such. The frequent references to 'strategies of action' would lead some overenthusiastic Soviet mathematicians – Struik (1948) cites Glivenko's (1935) paper as an example of such excess – to conclude that Marx anticipated Hadamard's so-called operator calculus.[20] Even if the similarities are overdrawn, however, comparisons of this sort still underscore the relative sophistication of Marx's critique.

In one of the appendices of this notebook, Marx then reviews the confusion between 'limit value' and 'value at the limit,' one that subverted textbook presentations of the so-called rational calculus until the 1840s. Hind's *Principles of Differential Calculus* (1831), for example, the Cambridge course book mentioned earlier, defined the former as the 'last value' of a function evaluated at the 'last value' of its argument. Marx's suspicion that the terms in common use exacerbated this confusion led him to propose 'absolute minimal expression' as a substitute for 'limit value,' with little expectation that this nomenclature would ever be adopted.

Marx (1983, p. 88) then identifies the 'false idealization' of movement as a second weakness of the rational calculus. One could understand this as the claim that the motion embodied in d'Alembert's notion of limit is smoother than observed motion: for the production function $Q = F(L)$, the evaluation of $F'(L_0)$ seems to require that labor power L approach L_0 in a manner inconsistent with the realities of economic life: an infinite sequence of smaller and smaller values ΔL in the open interval $(L_0, L_0 + \Delta L)$ is assumed. Ever the generous reader, Yanovskaya (1968, p. 13) detects in Marx's subsequent notebooks the seeds of a more robust definition, in which 'one does not need information about the entirety of values of any such variable for the complete expression of the derivative function $[F'(L)]$ from the given $[F(L)]$, but that it be sufficient to have the expression $[F(L)]$.' It is not obvious, however, that this resolves the 'false idealization problem' and, regardless, both the inference and the original argument are tenuous. The source of the uneasiness is not difficult to uncover: if $F(L)$ is a continuous function on the real numbers – and all the particular functions he considers are members of this class – then d'Alembert's sequence,

[20] Operator calculus allows for the reduction of differential equations to algebraic equations, through the conversion of differentiation and integration as an operator on functions.

"realistic" or not, is an admissible one. The provenance of this critique can be traced, I believe, to the sometimes forced representation of economic and other social relationships in terms of continuous functions: the production function $F(L)$, for example, is better defined on some, much smaller, subset of the real numbers. There is no doubt that even if these are not identified as such, Marx's economics involves functions of this sort – under most interpretations, his account of the 'anarchy of production,' for example, exploits what later writers would call the 'lumpiness' or indivisibilities of constant capital – but whether or not he was conscious of the presence of 'practical discontinuities' in his mathematics is much less certain.

Marx (1983, p. 99) then introduces Lagrange's *Théorie des Fonctions Analytiques* (1813) as the first 'algebraic' treatise on the calculus, algebraic inasmuch as its 'starting point ... [is Taylor's Theorem] which is in fact the most general, comprehensive [statement] and at the same time operational formula of differential calculus.' For the production function $F(L)$, the relevant incarnation is the expansion of $Q = F(L + h)$ around h:

$$Q = F(L) + ph + qh^2 + rh^3 + sh^4 + \dots$$

where $p, q, r, \dots$ are functions of L but not h. If $F'(L)$ is equal to p, then it is not difficult to show that $F''(L) = 2!q$, $F'''(L) = 3!r$, and so on, an observation that inspired Lagrange to claim that this approach had 'the advantage of showing how the terms of the series depend on each other, and especially how when one knows how to form the first derivative function, one can form all the derivative functions which enter the series' (Kline, 1972, p. 431). What remains to be shown, of course, is that $F'(L) = p$, and here the *Théorie* stumbles: Lagrange asserts that for 'small' h, all but the first two terms in the equation can be ignored, in which case $F(L + h) - F(L) = ph$, the sort of 'proof' he, and later Marx, would disavow.[21] It was the expansion itself that concerned Marx, however:

> [T]he equation ... is not only not proved but indeed knowingly or unknowingly assumes a substitution of variables for constants, which flies in the face of all the laws of algebra ... the derivation of this equation from algebra ... appears to rest on a deception. (Marx, 1983, p. 117)

As much as it can be deciphered, the criticism seems misdirected but the intuition – that the conventional demonstration of the theorem was flawed – is sound and, in some sense, prescient: Bell (1940, p. 285) reminds us that Taylor's first proof in *Methodus Incrementorum Directa et Inversa* (1715) was 'nonsense' despite the fact that its modern 'equivalent [could be found] in texts on the calculus as late as 1945.' Lagrange's restatement of Taylor's Theorem was one of the first to append the now familiar remainder term, even if his definition

[21] We now know, of course, that the existence of the relevant derivatives is a *prerequisite* for this expansion. Kline (1972, pp. 426–434) reviews the literature in more detail.

of the derivative failed to account for it, but he provided no rigorous proof. Marx (1983, pp. 116–118) would also claim that the treatment of $p, q, r, \ldots$ was careless inasmuch as Lagrange assumed, with little justification, that the coefficients were finite.[22]

Marx nevertheless admired *Théorie des Fonctions Analytiques* as the first 'modern' text of its kind, in the sense that it presented the calculus without recourse to infinitesimals or limits. There is no reason to believe that he doubted the truthfulness of the series expansions under well-specified conditions, but preferred, it seems, to seek alternative foundations. Given his own limitations as an amateur mathematician, however, these new foundations would assume the form of a simple but narrow definition of the derivative consistent with his commitment to the 'algebraic school.'

It is unfortunate, however, that Marx did not read *Methodus Incrementorum* in more detail. The introduction to infinite series there followed an inceptive treatment of the calculus of finite differences that anticipated Boole's much later work, the same work Moore had earlier recommended to Marx.

3. Marx's 'contributions' to the foundations of the calculus

3.1. On the concept of the derived function

The search for better foundations for the derivative, for a definition consistent with the rules of arithmetic and his own priors on the abstract representation of motion, became the principal focus of Marx's mathematical studies after the mid-1870s. There is some evidence, however, that the search concluded before he otherwise planned: in an earlier notebook, for example, he reminds himself to read Landon's *The Residual Analysis* (1754), which Hind (1831) and Lacroix (1818) both characterized as a forerunner of *Théorie des Fonctions Analytiques*, but there is no evidence in subsequent manuscripts that he ever found the time to do so. There is little reason to believe, however, that Landon's text would have exerted much influence on 'On the Concept of the Derived Function,' the first of the companion papers Marx finished in the fall of 1881. There, Marx (1983, p. 6) outlines, with little elaboration, an algebraic and algorithmic notion of differentiation, one illustrated here with the same production function $Q(L) = a + bL + cL^2$ introduced in the previous section. Suppose that L increases from L_0 to L_1. The difference $Q_1 - Q_0$ can be factored into the product of $L_1 - L_0$ and $b + c(L_0 + L_1)$, which then implies that[23]

$$\frac{Q_1 - Q_0}{L_1 - L_0} = b + c(L_1 + L_0)$$

[22] The criticism is perhaps unfair. Lagrange dismissed as 'exceptional cases' infinite-valued functions, infinite-valued derivatives and fractional powers, all contentious problems in analysis during his lifetime.

[23] The arithmetic is straightforward: the difference $Q_1 - Q_0$ is equal to $b(L_1 - L_0) + c(L_1^2 - L_0^2)$, and the last term can be factored into $(L_1 - L_0)(L_1 + L_0)$.

Marx calls this the 'preliminary derivative' of $F(L)$ and asserts that it is 'the limit value of the ratio of the finite differences ... [and not] the limit value of the ratio of differentials' (Marx, 1983, p. 6). The derived function obtains when L 'reaches the limit of its decrease' and L_1 'is changed into' L_0: if $L_0 = L_1$, in other words, the previous expression becomes the desired $b + 2cL_0$, from which he draws two basic conclusions. First, because L_0 and L_1 are set equal to one another, 'in the *strict mathematical sense*, [their difference is zero, and there is] no subterfuge about merely approaching infinitely [closely]' (Marx, 1983, p. 7). Second, despite the equalization of L_0 and L_1,

> [N]othing symbolic appears in the derivative. The quantity [L_1], although originally obtained from the variation of [L_0], does not disappear: it is only reduced to its minimum limit value, [L_0]. It remains in the original function of [L] as a [new] element which, by means of its combinations partly with itself and partly with the [L] in the original function, finally produces the 'derivative,' that is, the preliminary derivative reduced to its absolute minimum quantity. (Marx, 1983, p. 7)

This is the derivative as dialectic, the 'negation of the negation': when L_1 is set equal to L_0, we find that 0/0 on the left hand side is now equal to the newly created function, $b + 2cL_0$.

As Smith (1983) summarizes it, Marx's 'definition' is not difficult to formalize: for the general primitive function $f(x)$, define the two variable function $F(x,z) = f(x) - f(z)$, and then factor $F(x,z)$ into the product of the difference term $x-z$ and a second multivariate function, $G(x,z)$, where the latter is now called the 'preliminary derivative.' The derived function $f'(x)$ is then equal to $G(x,x)$.[24]

Within its particular historical context, Marx's formulation is reasonable but incomplete. The conditions under which $F(x,z)$ could, or could not, be factored into $(x-z)G(x,z)$, for example, are never considered in detail: Marx (1983, pp. 10–12) relies on a familiar series expansion to calculate $G(x,z)$ in one illustration, for example, but then fails to consider, *on his own terms*, the connection between derived functions and the existence of such expansions.[25] Furthermore, Marx assumes, without cause, that $G(x,x)$ must be continuous at (x,x).

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[24] The use of factors in this context was not novel, and dates from Leibnitz and his immediate followers, who would express the difference $f(x+dx) - f(x)$ as $(A(x) + B(x,dx))dx$. For more details, see, for example, Kitcher (1984, pp. 234–236).

[25] If it exists, the series expansion of $f(x+h)$ around $h = 0$ has the form $f(x+h) = f(x) + ph + qh^2 + \dots$ which, for $h = z-x$, implies that $f(z) = f(x) + p(z-x) + q(z-x)^2 + r(z-x)^3 + \dots$ or, rearranging terms:

$$\frac{f(z) - f(x)}{z - x} = p + q(z-x) + r(z-x)^2 + \dots$$

When z is equal to x, the right hand side collapses to p, from which Marx *should* have inferred that the existence of a well-behaved series expansion was a sufficient condition for differentiation. That he would have been incorrect had he done so – recall that the existence of all relevant derivatives was later discovered to be a *prerequisite* for the expansion – is beside the point: from his perspective, this was a sensible proposition.

From the modern perspective, one constructed on Weierstrassian foundations, the limitations of Marx's mathematics are immediate. There is a temptation, then, to dismiss his 'contribution' to the calculus as outdated, even crude, but this would be unfair. First, and as a practical matter, Marx was able, following well-marked paths, to differentiate a number of familiar and important functions without recourse to limits or infinitesimals. The first of these, $f(x) = ax^3 + bx^2 + cx - e$, involves no technical challenges ($x^3 - z^3$ is factored into the product of $x - z$ and $x^2 - zx + z^2$) and serves to illustrate basic themes. Nor does the second function, $f(x) = ax^m$, from which the reader is expected to infer that all functions of the form $a_0 + a_1x + a_2x^2 + \ldots a_mx^m$ can be differentiated using this method. The third function (Marx, 1983, pp. 10–12) considers, $f(x) = a^x$, is much less tractable: the derivation of $G(x, z)$ relies on the (assumed) binomial expansion of a related function $(1 + (a - 1))^{z-x}$.[26] The fourth and final function, $f(x) = \sqrt{a^2 + x^2}$, also involves minor complications: the difference term $z - x$ must, in Marx's words, be 'manufactured' from $f(z) - f(x)$.

If 'On the Concept of the Derived Function' contains a *bona fide* contribution, it is, as Kol'man (1968) and others have suggested, Marx's algorithmic treatment of the derivative. Within the Marxian calculus, the derivative $df(x)/dx$ becomes a 'strategy of action,' a well-defined and finite sequence of algebraic operations on $f(x)$. It is, furthermore, an operator that can be extended to discrete functions – the production function $Q = F(L)$ when labor power is available in Ricardian 'doses,' for example – but whether or not Marx recognized this is not clear.

Yanovskaya (1968, p. 15) situates Marx's interest in algorithms for differentiation as part of a broader commitment to the 'effective operation' of mathematical and scientific principles. From this perspective, the Leibnitzian differential is a flawed primitive for the calculus because, to repeat a claim made earlier, it is inconsistent with both the rules of arithmetic and the realities of economic and other motion. This position is one that, in its extreme form, reduces to a crude variant of mathematical intuitionism, but there is little

[26] Marx first expresses the difference $f(z) - f(x) = a^x(a^{z-x} - 1)$ and observes that $a^{z-x} = (1 + (a - 1))^{z-x}$ has the binomial expansion:

$$1 + (z - x)(a - 1) + \frac{(z - x)(z - x - 1)}{1 \cdot 2}(a - 1)^2 + \ldots$$

from which it follows that

$$\frac{f(x) - f(z)}{z - x} = a^z\left((a - 1) + \frac{z - x - 1}{1 \cdot 2}(a - 1)^2 + \frac{(z - x - 1)(z - x - 2)}{1 \cdot 2 \cdot 3}(a - 1)^3 + \ldots\right)$$

For $z = x$, this becomes

$$\frac{dy}{dx} = a^x\left((a - 1) - \frac{1}{2}(a - 1)^2 + \frac{1}{3}(a - 1)^3 + \ldots\right)$$

Marx then observes that the infinite series inside the large parentheses is the power series expansion for $\log a$, and concludes, correctly, that $f'(x) = a^x \log a$.

textual evidence that Marx intended this.[27] This said, he then reiterates what he believes to be the foundational differences between his definition and the alternative(s):

> [Although the] reduction of x_1 to x_0 within the [derived function] changes the left hand side [from] $\Delta y/\Delta x$ to 0/0 or dy/dx, so that the derivative *appears* as the limit value of the ratio of the differential ... the transcendental or symbolic mistake which appears only on the left hand side has ... lost its terror since it now appears only as the expression of a process which has established its real content on the right hand side. (Marx, 1983, pp. 7–9).

The same principles also inform Marx's (1983, p. 8) subsequent discussion of higher order derived functions as members of a 'genealogical register' of derivatives, and a source of confusion when treated as 'the starting point of the exercise, instead of ... the expressions of successively derived functions of x.' Considered from this vantage point, d^2y/dx^2 is a 'purely symbolic equation' that represents no more, but no less, than the twice repeated application of the same operator.

3.2. On the concept of the differential

Like the first of Marx's finished papers on the calculus, 'On the Concept of the Differential' was completed in the fall of 1881. In it, he shifts the 'initiative ... from the right hand pole, the algebraic, to the left hand one, the symbolic' (Marx, 1983, p. 20) or, as he also describes it, the 'own ground: of the calculus, the differential, from where the 'dialectical nature' of the calculus can be better elucidated. He devotes the first half of the paper to a deconstruction of the familiar product rule: if u and v are both differentiable functions of x, and y is defined to be the product of $u$and v, then $dy/dx = u(dv/dx) + v(du/dx)$ or, in terms that he considered deceptive, $dy = u(dv) + v(du)$. Marx does so to illustrate mathematical propositions 'without real content' – that is, propositions on the operators themselves. Marx (1983, p. 15) first notes that the difference $y_1 - y_0 = u(x_1)v(x_1) - u(x_0)v(x_0)$ can be written:

$$\begin{aligned} y_1 - y_0 &= u(x_1)v(x_1) - u(x_0)v(x_0) \\ &= v(x_1)(u(x_1) - u(x_0)) + u(x_0)(v(x_1) - v(x_0)) \end{aligned}$$

from which it follows that

$$\begin{aligned} \frac{y_1 - y_0}{x_1 - x_0} \equiv \frac{\Delta y}{\Delta x} &= v(x_1)\frac{u(x_1) - u(x_0)}{x_1 - x_0} + u(x_0)\frac{v(x_1) - v(x_0)}{x_1 - x_0} \\ &= v(x_1)\frac{\Delta u}{\Delta x} + u(x_0)\frac{\Delta v}{\Delta x} \end{aligned}$$

[27] On the other hand, it cannot be coincidence that Kröneckernecker, the most influential advocate of intuitionism during this period, shared Marx's appreciation for Hegel's *Science of Logic* (1822).

This unremarkable, of course, unless x_1 is set equal to x_0, 'so that $x_1 - x_0 = 0$, likewise $u(x_1) - u(x_0) = 0$, $v(x_1) - v(x_0) = 0$; so that the factor $v(x_1)$ [in the second expression] also goes to $v(x_0)$ [and] finally on the left hand side $y_1 - y_0 = 0$,' which leads to the product rule. This is a reasonable argument if, as outlined in 'On the Concept of the Derived Function,' *du/dx* and *dv/dx* are understood to be operators or 'strategies of action,' and not the problematic form 0/0 or, worse, 0. The first, 0/0, is misleading inasmuch as 'the numerator in this form remains inseparable from the denominator [while the second is because] even in the usual algebra it would be false' (Marx, 1983: 17), as the discussion of $f(x) = (x^2 - a^2)/(x - a)$ at $x = a$ that follows serves to illustrate.

The second half of the paper considers the differential form $dy = f'(x)dx$ in more detail. The expression 'appears ... more suspicious than the differential coefficient $dy/dx = f'(x)$ from which it is derived' because, as Marx (1983, pp. 24–45) repeats, 'in $dy/dx = 0/0$ the numerator and denominator are ... bound [but] in $dy = f'(x)dx$ they [seem to be] separated, so that one is forced [to conclude] that it is only a disguised expression for $0 = f'(x)0$ or $0 = 0$, whereupon "nothing's to be done ".' This was Boucharlat's (1828) judgment, for example, who believed that 'in order to facilitate algebraic operation, one [must] introduce a ... false formula which one baptises the differential' (Marx, 1983, p. 25). The confusion, presented as an inevitable consequence of the foundational crisis in mathematics, cannot be resolved until it is understood that *dy*, 'considered in isolation, that is, without its real equivalent ... assumes the same role as Δy in the algebraic method' (Marx, 1983: 27) and *dx* is treated likewise.

4. Marx and mathematical economics

Having explored the possible influences of Marx's economics on both the direction of his mathematical studies and the substance of his critique of the calculus, it remains to consider the possible effects of this technical work on his understanding of capitalism. 'The Mathematical Treatment of the Rate of Surplus Value and the Rate of Profit' (1875), the notebook mentioned in the introduction, is perhaps the most obvious manifestation of this influence.[28] The editorial decision to use a less 'abstract' version of the third chapter in the third volume of *Capital* was not, as hinted earlier, Engels' alone: in the preface, he notes that Moore's role in the reconstruction of the relevant arguments was crucial. Both had reason to believe that their choice was *a fortiori* consistent with Marx's wishes: he was once pleased to tell Engels, for example, that 'in the final elaboration, the [second volume of *Capital*] is, I believe, assuming a tolerably popular form, aside from a few unavoidable M-C's and C-M's' (Works

[28] This unpublished manuscript has remained inaccessible to most scholars. It is not included, for example, in his mathematical notebooks (Marx, 1983).

V 41: 488). *Capital* was written, in other words, for a diverse audience, and to infer from the absence of formal models an opposition to their use is to overlook authorial intention: the elaborate restatement of Quesnay's *Tableau* in a subsequent letter (Works V45: 263–265) is an eloquent demonstration of his interest in such models.

There was, however, a second sensible rationale for the choice Engels and Moore would make: Marx's 'Mathematical Treatment' is in one important sense a failure. Written in the mid 1870s, at which point Marx could claim to be familiar with (for example) differential equations, its relative unsophistication – in particular, its continued reliance on numerical examples – comes as a disappointment. His treatment of the manuscript's benchmark equation, as summarized in Smolinski (1973), is instructive: from the initial observation that $p = mv/(c + v)$, where p is the rate of profit, m is the rate of surplus value, c is constant capital and v is variable capital, Marx shows, after tedious calculation, that the rate of profit decreases, ceteris paribus, as the composition of capital c/v increases, a proposition that does not require arithmetic, as Moore later noted in the margin. His earlier confession – 'I have never felt at home with arithmetic' (Works V40: 244) – notwithstanding, it seems Marx never overcame his reliance on specific numerical examples.

Smolinski (1973) also claims, on the other hand, that despite computational errors, Marx's 'laborious methods' did achieve the same ends as the more sophisticated methods now familiar to all economists. This is somewhat premature, however. The connection between p and m can be elucidated using several approaches, some more laborious than others, but it should be recalled that subsequent efforts to formalize the various schemes of reproduction and to 'solve' the transformation problem, which subsumes the relationship between p and m, have often relied on the use of matrices, a tool uncommon in Marx's lifetime and unknown, it is reasonable to assume, to Moore. Where matrix methods are not used, a complete characterization of the properties of the Marxian model still calls for the abstract manipulation of at least 'departmental' equations. This raises a difficult question, however: are the ostensible defects of Marx's own solution to the transformation problem – in particular, his treatment of the means of production – in some measure attributable to his reliance on numerical illustrations? Is it possible, in other words, that his reliance on arithmetic methods predisposed Marx to 'solutions' with smaller computational burdens?

A complete discussion of the alternative solutions to the transformation problem – one that would distinguish between those Marx himself *might* have offered, and those he perhaps *should* have – is well outside the scope of this paper, of course, but even so, it is not difficult to argue that the answer is no.[29]

[29] I am not, however, a disinterested observer: Matthews (2000), for example, estimates a model of the circuit of capital that relies on the so-called 'new solution' or 'new interpretation' proposed by Foley (1982). It should be noted that some readers – in particular, those who find inspiration in Wolff, Roberts,

In particular, Shaikh (1977) finds textual evidence in *Capital* for the view that Marx understood the need to extend the 'price-value disproportionalities' but decided, for a number of reasons, to postpone whatever further calculations were required:

> [A]part form the fact that the price of [a particular] product . . . diverges from its value . . . the same situation also holds for the commodities that form the constant part of [its] capital and indirectly, also, its variable capital, as means of subsistence for the workers. (Marx, 1894, p. 261)

Furthermore, Shaikh (1977) confirms that if the particular methods Marx used to compute equilibrium prices are iterated, with the prices of outputs in each iteration become the prices of inputs in the next, the resulting sequence tends, in the limit, to the prices calculated using more conventional methods. If so, the treatment in the unfinished third volume should be understood as the first iteration in a convergent 'values to prices algorithm' that constitutes *one* solution to his infamous problem. Whether or not this algorithm satisfies all of the properties Marx and his followers have advocated is, in this narrower context, beside the point. On the other hand, the need to postpone calculation of successive iterations would not have arisen if Marx had been able to overcome his 'addiction to arithmetic.'

A second, perhaps more remarkable, manifestation of the influence of Marx's mathematics on his economics can be found in a letter to Engels dated 31 May 1873:

> I have been telling Moore about a problem with which I have been racking my brains for some time now. However, he thinks it is insoluble, at least *pro tempore*, because of the many factors involved, factors which for the most part have yet to be discovered. The problem is this: you know about these graphs in which the movements of prices, discount rates, etc, etc, over the year, etc, are shown in rising and falling zigzags. I have variously attempted to analyze crises by calculating these 'ups and downs' as irregular curves and believed (and still believe it would be possible if the [empirical] material were sufficiently studied) that I might be able to determine mathematically the principal laws governing crises. As I said, Moore thinks it cannot be done at present and I have resolved to give it up for the time being. (Works V44: 504)

Leontief (1938), one of the first economists to draw attention to this remarkable letter, discerned in these lines the elements of the 'statistical . . . approach to business cycles' later identified as the 'NBER approach' of Mitchell (1913) and others. Much later, Kol'man (1968) would advance an even more sophisticated, but also more tenuous, interpretation, one based on the resolution of economic time series into combinations of periodic functions of various frequencies – that is, the spectral decomposition of (in this case) prices and discount rates. Because Marx's published correspondence contains no

and Callari (1982) and later advocates of the 'single system' framework – there is no problem in the usual sense, and that the qualification described in the text is a red herring of sorts.

other references of this kind, Kol'man's (1968) judgment is difficult to substantiate, but it is at least conceivable that Marx, in his efforts to characterize the laws of motion, would have considered, and perhaps abandoned, the decomposition of prices, values and quantities into short, medium and long waves. If this was indeed Marx's intention, Moore's response was mistaken: most of the relevant mathematical principles had been articulated some time earlier, in Fourier's *Théorie Analytique de la Chaleur* (1822). Moore would not have alone in his assessment, however: empirical research in economics, mainstream or radical, would not experiment with these methods for some time.

Kol'man (1968, p. 222) also hints, here and elsewhere, that Marx understood 'the statistical nature of economic mechanisms as mechanisms of large scale processes ... [a principle with great methodological significance for mathematical statistics.' The principle to which Kol'man refers is the proposition that specific realizations of economic variables can be described in probabilistic terms and that means, over both time and space, should 'dominate' deviations. He cites as further evidence a passage from *Grundrisse*:

> The value of commodities as determined by labor time is only their average value. This average appears as an external abstraction if it is calculated as an average figure of an epoch.. but it is very real if it is at the same time recognized as the driving force and the moving principle of the oscillations which commodity prices run through during a given epoch. This reality is not merely of theoretical importance: it forms the basis of mercantile speculation, whose calculus of probabilities depends both on ... price averages which figure as the center of oscillations, and on the average peaks and average troughs of oscillations above or below its center. (Marx, 1857–8, p. 137)

In econometric terms, the deviation of prices from their 'average values' is represented as a stationary but autocorrelated process with zero mean – that is, a stochastic version of the natural price doctrine of classical economics. It should be underscored, however, that the oscillations Marx describes are the cause, and not the effect, of 'mercantile speculation.'

(If one stretches a little, this could also, or perhaps instead, be understood as a statement of the 'law of value' in statistical terms. In this context, the work of Cockshott and Cottrell (1997), who find that market prices are better correlated with the sum of direct and indirect labor than other inputs, merits particular attention.)

What little there is to learn about Marx's response to the work of the 'mathematical economists' of his time must also be inferred from his correspondence. In a letter dated March 1868, for example, he refers to Henry Dunning Macleod as that 'stilted jackass who expresses every banal tautology (1) in algebraic form, and (2) constructs it geometrically' (*Works* V42: 543). Kol'man (1968) and others have concluded that Marx was therefore familiar with the efforts of neo-classical writers to provide their work a 'scientific veneer': Jevons, for example, in the preface to his 'Brief Account of a General Mathematical Theory of Political Economy' (1860), would acknowledge Macleod's *Elements of Political Economy* (1858), as well as the work of Bentham and Jennings, as an important influence.[30] While there is little doubt that Marx would have criticized these efforts, there is also little evidence he drew a connection between Macleod and Jevons or, for that matter, between Macleod and neo-classicism as it is now understood. In the same letter, he adds that 'I have already given [Macleod] a passing kick in the pamphlet published by Duncker ... his 'great' discovery is: credit is capital' (*Works* V42: 543).

The pamphlet to which Marx refers is *A Contribution to the Critique of Political Economy* (1859) but it is Macleod's *Theory and Practice of Banking* (1856) that is discussed there, not his *Elements*. Furthermore, Marx's criticism of Macleod, later repeated in the first volume of *Capital*, was more muted than his letter hints: Macleod, described as 'a successful cross between the superstitious mercantilists and the enlightened peddlars of free trade,' is said to have 'trick[ed] out the confused ideas of Lombard Street in their most learned finery' (Marx, 1867, p. 153). The 'banal tautologies ... in algebraic form' to which Marx refers are in fact crude balance sheets and, as Engels notes in a footnote, Macleod's error was the mistaken belief 'that money in general arises from its most advanced form, that is the means of payment.'[31]

In fact, neither Jevons nor Gossen, the pre-eminent practitioners of (orthodox) mathematical economics in Marx's lifetime, are mentioned in his letters. There are at least three references to von Thünen's *Der Isolierte Staat* (1860), however. The first occurs in a letter to Ludwig Kugelman[32] written in January 1868 (Works V42: 522), in which Marx asks that von Thünen's book be purchased for him in Hanover. Two months later, he would tell Kugelman that

> [T]here is something touching about [it]. A Mecklenberg squire ... who treats his estate on Tellow as *the land*, and Mecklenberg-Schwerin as *the town*, and who, proceeding from these premises, constructs for himself a Ricardian theory of ground rent, with the help of observation, differential calculus [and] practical accountancy. This is estimable and at the same time ridiculous. (*Works* V42: 543)

Almost a decade after this, he would write to Herman Schumacher, the German economist who had published *Der Isolierte Staat* and, as a companion volume, an introduction to von Thünen's life and work, in much warmer terms: 'I have always regarded [him] as something of an exception among German economists, since it is exceedingly rare for an objective and independent

[30] Blaug (1985) documents Macleod's limited role in the so-called 'marginalist revolution.'

[31] Schumpeter (1954, p. 155) notes that Macleod's explanation of bank credit formation, a precursor to modern orthodox treatments, attracted little attention in his own lifetime, despite Marx's 'passing kick.' He attributes this to Macleod's 'inability to put his many good ideas in a professionally acceptable form.'

[32] Kugelman, a German physician, participated in the revolts of 1848 and served as a member of both the First International and Social Democratic Workers' Party.

inquirer to be found in their midst' (*Works* V45: 90). What is most remarkable about the third letter, though, is Marx's declaration that he 'would endorse ... in its entirety' Schumacher's appreciative preface 'if our attitude to "wages" did not differ ... he regards wages as the immediate expression of a genuine economic relation [while] I regard them as a spurious form concealing a content materially different from the expression of that form' (*Works* V45: 90). One wonders, then, whether, under different circumstances, Marx might have adopted, and benefited from, a calculus-based treatment of differential rent in the third volume of *Capital* (Marx, 1894).

This raises another question, one outside the scope of the present paper but deserving of discussion: What other arguments in *Capital* might have profited from such an approach? One reviewer suggests at least two others: the discussion of the 'general law of capital accumulation' in the first volume (Marx, 1867) and the treatment of 'market prices and market values' in the third volume (Marx, 1894).

In broader terms, it should be underscored that nowhere does Marx object to von Thünen's extensive and innovative use of mathematics. Because *Der Isolierte Staat*contains few conceptual innovations, it is reasonable to infer that, at least in some measure, Marx's favorable impression reflects von Thünen's general methodological approach. As Schumpeter (1954, pp. 466–467) reminds us, he was one of the first to characterize economic phenomena in terms of calculus, to substantiate his various 'laws' with established tendencies in observed data and, with Cournot, to represent simple forms of interdependence in terms of simultaneous equations models.

5. Conclusion

The belief that Marx neither understood nor endorsed the use of mathematical methods has somehow persisted to the present. An examination of his mathematical and technical papers reveals an interest in commercial and abstract mathematics, however, that spanned more than two decades and evinces sufficient command over the calculus for Marx to have proposed an alternative, albeit narrow, definition of the derivative. His economics both motivated and, this paper has argued, informed these studies, which in turn led to at least one technical exposition of value-price relations, the failed 'Mathematical Treatment of the Rate of Surplus Value and the Rate of Profit.' This said, his response to the efforts of some mathematical economists – in particular, von Thünen – suggests that he considered the formal representation of economic relations a sensible ambition.

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Notes on contributor

Peter Hans Matthews is the Charles A. Dana Professor of Economics at Middlebury College in Vermont, and Visiting Professor of Economics at Aalto University and the Helsinki Graduate School of Economics in Finland.

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