

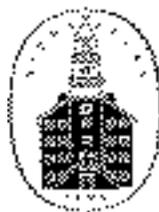
“The Dialectics of Differentiation”

by

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**THE DIALECTICS OF DIFFERENTIATION:
MARX'S MATHEMATICAL MANUSCRIPTS
AND THEIR RELATION TO HIS ECONOMICS**

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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.

JEL Classification Numbers: A12, B14, C60

The Dialectics of Differentiation: Marx's Mathematical Manuscripts and their Relation to his Economics*

[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 shirtbuttons 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, perhaps, that recent debates concerning the use of “formal” or “technical” methods within the Marxian tradition seldom refer to Marx's own mathematical manuscripts, which were inaccessible to most scholars until the translation (Marx 1983), almost two decades ago, of an obscure Soviet (Marx 1968) collection.¹

* The earliest versions of this paper benefitted from conversations with Antonio Callari, Cal Craven, John Geanakoplos and David Weiman. An abridged version, which owed much to Richard Cornwall's expertise in mathematics, was presented at the AESA Voices and Languages of Marxism conference in Amherst in December 1996, where Peter Hardin also introduced me to Brokmeyer's (1985) reading of the manuscripts. The current revisions were completed as a visiting scholar at UC San Diego, with generous leave support from Middlebury College. The usual disclaimers hold.

¹ *The Mathematical Manuscripts of Karl Marx* (1983) include brief but annotated selections from the more complete *Mathematisches Rukopsii*, not least of which are the drafts and final versions of Marx's two “contributions” to the calculus,

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 V24: 468)

Soon after, Engels would note in the preface to the second edition of *Anti-Dühring* (1885: 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 was unable to fulfill. 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

“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 Yanovskaya and Kol'man (1931), Kol'man (1968), Yanovskaya (1968) and Smith (1983).

the rate of surplus value and the rate of profit in equations. (Marx 1894: 94)

This notebook, entitled “The Mathematical Treatment of the Rate of Surplus Value and the Rate of Profit,” is a 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 in Amsterdam.

The contents of Marx’s notebooks remained 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 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: 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.”² A complete and annotated *Mathematischeskie Rukopsii*, in facsimile and also in Russian and German

² 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 “The Social and Economic Roots of Newton’s *Principia*,” which Joseph Needham (1971: vii) would later describe as the “classical statement of the Marxist historiography of science.” P. N. Worsley’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. He is described in ambiguous terms, as someone who, in the 1930s, managed to be “simultaneously a partisan of Lysenko and a spokesman

translations, was not published until 1968, however. Smolinski (1973) speculates 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.”³

This does not account for the treatment of the notebooks outside the Soviet Union after the 1930s, however. With few notable exceptions - Struik (1948) 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 involves 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 should 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 undeserved reputation

for the ‘liberal’ defense of Einsteinian physics,” but then disappeared until the 1950s, when he “emerged ... as an advocate of cybernetics” (Worsley 1971: xvi). For much of his life, Kol’man seems to have “command[ed] a schizophrenic reputation as a liberal in Russia [but] a rigid ideologue in his native Czechoslovakia” (Worsley 1971: xvi). The promise of a Marx-Engels Institute volume was, like Engels’ original promise, never realized.

³ 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.

as a poor mathematician, one for whom even arithmetic was difficult. The notion that Marx was an innumerate of sorts, often associated with the critiques of von 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 (Works V40: 244) himself once confessed, “I have never felt at home with arithmetic,” the *principles* of more advanced mathematics 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: 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 that of 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 often implicit. The third section examines the two “papers” that Marx considered his principal contribution 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 these 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), for example - to the more familiar *Capital*.

2. Marx and the “Golden Zero”⁴

Marx’s mathematical studies would span more than two decades, 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 of 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 Phillips (Works V41: 514-516), for example, includes a brief discussion of Boethius’ *De Arithmetica* and the historical development of household and commercial accounts. As the computational burdens of *Capital* became heavier, however, the scope of his studies widened:

In elaborating the principles of economics I have been so damnably

⁴ Bell (1940: 284) 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. Léonard 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.

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 evinces Marx's initial fascination with infinite series representations of familiar functions, about which more below.

Before the first volume of *Capital* (1865) 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 a much easier part of mathematics ... than the higher parts of algebra, for instance” (Works V41: xx). 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. [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 that is the example I am going to use for you ... (Works V42: 208-209)

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 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. 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.

Yanovskaya (1968) also 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.⁵ 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 of the first volume of *Capital* and Engels’ assistant in the eventual “reconstruction” of 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. On one such occasion, Marx had written to Moore with an intuition about the decomposition of economic time series into the sum of periodic functions⁶ and, after Moore had (mistakenly) declared the

⁵ The preface to G. H. Hardy’s landmark *Course in Pure Mathematics* (1937) includes a blunt assessment of the Cambridge curriculum before 1900.

⁶ 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.

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 by saying, *in practice*, no essential change has been brought about in ... differential calculus. (Works V46: 380)

None of this should diminish 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 Leibnizian principles. He was soon drawn, however, to Boucharlat’s *Elementary Treatise on the Differential and Integral Calculus* (1828), the English translation of an influential (almost ten 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, including Lacroix’s monumental *Traité du Calcul Différentiel et du Calcul Intégral* (1819) and Hind’s *The Principles of the Differential Calculus and Curve Surfaces* (1831), a Cambridge

course manual.⁷ 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* (1813).⁸

The absence of Cauchy's *Cours d'Analyse* (1822), whose "new approach led to [its own] difficulties and generated problems of rigorization for [his] successors" (Kitcher 1984: 254), problems that culminated in Weierstrass' 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, Dirichlet and Weierstrass had overtaken it! That Marx was also unfamiliar with Weierstrass' "epislontics"⁹ then comes as no surprise at all. Historians of mathematics - see

⁷ Bell (1940: 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 was not."

⁸ A complete list of Marx's sources can be found in *The Mathematical Manuscripts*, pp. 273-274).

⁹ The term derives from the now standard $\epsilon - \delta$ definitions of limit and derivative. For more details, see, for example, Kitcher (1984).

the accounts in Bell (1937, 1940) or Kline (1972), for example - have characterized the dissemination and refinement of Weierstrass' 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, despite 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* (1744), an important precursor of the *Cours*. Yanovskaya's (1968) inference is a fragile one, however: there was an enormous difference - even a paradigmatic shift - between d'Alembert and Cauchy on the one hand and, on the other, Weierstrass, and in the context of Marx's professed interest in “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: 285). Nor does an appreciation for Marx's achievements in mathematics turn on this sort of “defense” : there are other senses in which his intuitions could 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: 260). Engels would come to believe that Marx alone understood the Hegelian critique of the infinitesimal. In March

1865, he would tell Friedrich Lange:¹⁰:

There is a remark about . . . Hegel which I cannot let pass without comment: you deny him any deeper knowledge of the mathematical sciences. Hegel knew so much 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 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 be of varying powers and at least one of them must be 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 coeur* of a crude polemicist.¹¹

¹⁰ 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).

¹¹ Bell (1937: 239, 469) believes that the immature Hegel’s ill-considered criti-

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 served much the same role in his mathematical studies as *Theories of Surplus Value* did in his economic ones, albeit on a much smaller scale. The terms and 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" there will remind some readers of Bishop Berkeley's infamous jeremiad in *The Analyst* (1734), published more than a century before. 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 decades¹² - despite the fact that neither mentions it. To recast

cisms of astronomers - he once announced, for example, that the search for additional planets was doomed to failure - secured an unfair reputation as an innumerate of sorts. He also notes, however, that the influential Krönecker was an important exception.

¹² 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, who interests were often more "practical." It would be the anomalies in practical uses of the calculus, however, that in the end stimulated the search for alternative foundations, while mathematics in Britain languished in comparison.

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 $b(dL) + 2cL_0(dL) + c(dL)^2$. If the squared term is much smaller than the first two, then $(b + 2cL_0)dL$ becomes 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 feature of Berkeley’s critique in *The Analyst*. Nor could these difficulties be resolved if, as textbooks of the time often recommended, the ratio of $b(dL) + 2cL_0(dL) + c(dL)^2$ to dL were calculated *before* the latter was set equal to zero because, as Marx (1983: 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 $[Q_1 - Q_0/dL = b + 2cL_0]$. Only then may the finite differences be annulled. The differential coefficient $[dQ/dL = b + 2cL]$ therefore also must have been previously developed ... before we may obtain the differential $[dQ/dL = b + 2cL]$.

This is Berkeley’s “ghosts of departed quantities” argument in dialectical terms: 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 did not mean that the achievements of the “mystics” were underestimated, however. In his *Anti-Dürhing*, Engels (1885: 98) would echo Marx’s admiration thus:

With the introduction of variable magnitudes and the extension of their variability to the infinitely small and infinitely large, math-

ematics, in some respects so strictly moral, fell from grace; it ate from the tree of knowledge, which opened up to it a career of most colossal achievements, but at the same time a path of error. The virgin state of absolute validity and irrefutable certainty of everything mathematical 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 “harder” 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 that 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.¹³

¹³ Marx’s assessment of von Thünen’s work, contained in two letters written in

It is at least conceivable, then, that the abstractness of Marx's mathematics after 1870s 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 particular significance: Boole's calculus is *prima facie* more consistent with Marxian priors. It is unfortunate, therefore, that he never found the time to read *Calculus of Finite Differences* or, for that matter, Boole's interest in the separation of the "symbols of mathematical operations from [the objects on which these] operate" (Bell 1937: 438), which has similarities to 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 earlier critique, for example, as the unsophisticated outburst of someone who would 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"¹⁴ (Kitcher 1984: 239). Marx's periodization merits

the late 1870s, is also discussed in the fourth section.

¹⁴ Kitcher's (1984) qualification is based on several ambiguous passages in Newton's *Treatise on Quadrature*, which seem to anticipate the limit arguments of

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 recent interest in so-called “non-standard methods” in mathematical economics, in which an arithmetic of the infinitesimal has been axiomatized.¹⁵

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 $Q = F(L)$, $dQ/dL \equiv \lim_{\Delta \rightarrow 0} \Delta Q/\Delta L = \lim_{\Delta L \rightarrow 0} (Q(L + \Delta L) - Q(L))/\Delta L$. 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: 97) one that mathematicians had resisted for some time. Marx (1983: 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.

d’Alembert’s “solution” is seen to be flawed, however:

[As] with the mystics, [the derivative function] already existed as

d’Alembert and others.

¹⁵ For an introduction to non-standard methods and their uses in economics, see Anderson (1993). 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 neo-classical economists than others.

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 a strictly algebraic manner from its other companions. It is no *development* but rather a separation of $[f'(L)] \dots$ from its factor h and from the neighboring terms \dots 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 $[dQ/dL] = 0/0 \dots$ which in turn once more generates certain metaphysical shudderings, although the symbol has been mathematically derived. (Marx 1983: 96)

The problem *seems* to lie in the machinations - more reasonable than those of Newton and Leibnitz, 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 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 - to conclude that Marx anticipated Hadamard's so-called "operator calculus." Even if the similarities are overdrawn, however, comparisons of this sort underscore the relative sophistication of Marx's critique.

One of the appendices to this notebook 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 lead him to propose “absolute minimal expression” as a substitute for “limit value,” with little expectation that it would be adopted.

Marx (1983: 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 or actual 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: 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, “realistic” or not, is an admissible one. The provenance of this critique can be found, I believe, in the sometimes forced representation of economic (and other social) relationships in terms of continuous functions: the production function $F(L)$, for example, is perhaps 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" of constant capital - but whether or not he was *conscious* of the role of "practical discontinuities" in his mathematics is much less certain.

Marx (1983: 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 in fact is 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 $Q = F(L + h)$ about 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 , it is then 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: 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.¹⁶ It

¹⁶ We now know, of course, that the existence of the relevant derivatives is a *prerequisite* for this expansion. Kline (1972: 426-434) reviews the literature in more detail.

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: 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 senses, prescient: Bell (1940: 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 of the derivative failed to account for it, but he provided no rigorous proof. Marx (1983: 116-118) would also claim that the treatment of p, q, r, \dots was careless inasmuch as Lagrange *assumed*, with little justification, that the coefficients were finite.¹⁷

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

¹⁷ The criticism was perhaps unfair. Lagrange dismissed as "exceptional cases" infinite-valued functions, infinite-valued derivatives and fractional powers, all contentious problems in analysis then.

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* (1764), which Hind (1831) and Lacroix (1863) 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, on the other hand, that Landon’s text would have exerted much influence on “On the Concept of the Derived Function,” the first of the companion papers finished in the fall of 1881. There, Marx (1983: 6) outlines, with little elaboration, an algebraic and *algorithmic* notion of differentiation, one that can be illustrated here with the production function $Q(L) = a + bL + cL^2$ used 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 $b + c(L_0 + L_1)$,¹⁸ which

¹⁸ The arithmetic is not difficult: the difference $Q_1 - Q_0$ is equal to $b(L_1 - L_0) + c(L_1^2 - L_0^2)$, and $(L_1^2 - L_0^2)$ is equal to $(L_1 - L_0)(L_1 + L_0)$.

implies that:

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

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: 6). The derived function he 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: 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: 7)

As Smith (1983) summarizes it, Marx’s “definition” is not difficult to formalize: for the primitive function $f(x)$, define the two variable function $F(x, z) \equiv f(x) - f(z)$, 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 the preliminary derivative. The derived function $f'(x)$ is then equal to $G(x, x)$.¹⁹

¹⁹ The use of “factors” was *not* modern, and dates from Leibnitz and his immediate followers, who would express the difference $f(x + dx) - f(x)$ as $(A(x) +$

Within its particular historical context, the Marxian calculus seems 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: 10-12) relies on a familiar series expansion to calculate $G(x, z)$ in one case, for example, but then fails to consider, *on his own terms*, the connection between derived functions and the existence of such expansions.²⁰ Furthermore, Marx assumes, without cause, that $G(x, x)$ must be continuous at (x, x) .

From the modern perspective, one constructed on Weierstrassian foundations, the limitations of Marx's mathematics are more immediate. There is a temptation, then, to dismiss his "contribution" to the calculus as outdated, even simplistic, but this would be unfair. First, and as a practical matter, Marx was able, following well-

 $B(x, dx))dx$. For more details, see, for example, Kitcher (1984: 234-236).

²⁰ 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 the relevant derivatives was later discovered to be a *prerequisite* for the expansion - is beside the point: from his perspective, this was a sensible proposition.

marked paths, to differentiate a number of familiar and important functions without *direct* recourse to limits or infinitesimals. The first of these, $f(x) = ax^3 + bx^2 + cx - e$, involves no technical difficulties - $x^3 - z^3$ is factored into the product of $x - z$ and $x^2 + xz + z^2$ - and serves to illustrate his basic themes. Nor does the second function $f(x) = ax^m$, from which the reader is supposed to infer that *all* functions of the form $a_0 + a_1x + a_2x^2 + \dots + a_mx^m$ can be differentiated using this method. The third function Marx (1983: 10-12) considers, $f(x) = a^x$, is much less tractable: the derivation of $G(x, z)$ relies on the (unproven) binomial expansion of a related function, $(1 + (a - 1))^{z-x}$.²¹ 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 $f(z) - f(x)$.

The *bona fide* contribution of "On the Concept of the Derived Function,"

²¹ Marx first rewrites the difference $f(z) - f(x) = a^x(a^{z-x} - 1)$ as $(1 + (a - 1))^{z-x}$ and observes that the latter has the binomial expansion:

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

It follows that:

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

For z and x equal, this becomes:

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

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

Kol'man (1968) and others have suggested, lies in 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 classical "doses," for example - but whether or not Marx understood this is not clear.

Yanvoskaya (1968: 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 Leibnizian 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 version of mathematical intuitionism, but there is little textual evidence that Marx intends this.²² 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: 7-9)

The same principles also inform Marx's (1983: 8) subsequent discussion of higher

²² It cannot be coincidence, on the other hand, that Krönecker, the most influential advocate of intuitionism during this period, shared Marx's appreciation for Hegel's *Science of Logic* (1822).

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, dy/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: 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: 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:

$$y_1 - y_0 = u_1z_1 - u_0z_0 = z_1(u_1 - u_0) + u_0(z_1 - z_0)$$

from which it follows that:

$$\frac{y_1 - y_0}{x_1 - x_0} \equiv \frac{\Delta y}{\Delta x} = z_1 \frac{u_1 - u_0}{x_1 - x_0} + u_1 \frac{z_1 - z_0}{x_1 - x_0} = z_1 \frac{\Delta u}{\Delta x} + u_0 \frac{\Delta z}{\Delta x}$$

This is unremarkable, of course, unless x_1 is set equal to x_0 , “so that $x_1 - x_0 = 0$, likewise $u_1 - u_0 = 0$, $z_1 - z_0$; so that the factor z_1 [in the second term above] also goes to z_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, just 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: 24-25) 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: 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

While I have 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.²³ 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, the latter 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 V41: 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 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

²³ This unpublished manuscript is *still* inaccessible to most scholars. It is *not* included, for example, in his mathematical notebooks (Marx 1983).

reliance on numerical examples - comes as a disappointment. His treatment of the manuscript's benchmark equation, reviewed in Smolinski (1973), is representative: 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 required little or no 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 that Marx never overcame his intuitive reliance on specific numerical examples.

Smolinski (1973) 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 a little premature, however. The connection between p and m can be elucidated from several perspectives, 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 in 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 examples? 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 various proposed solutions to the transformation

problem - one that would distinguish, for example, between those Marx himself *might* have offered, and those he perhaps *should* have - lies well outside the scope of this paper, of course, but even so, it is not difficult to argue that the answer is no.²⁴ 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 from 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: 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 “round” becoming the prices of inputs in the next, the resulting sequence tends, in the limit, toward the prices calculated with 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 the

²⁴ The fact that I am not a disinterested observer of the relevant debates - Matthews (2000), for example, estimates a model of the circuit of capital that relies on the so-called “new solution” (Foley 1982) - is (for me, at least) a further complication. It should also be noted that some redares - Wolff, Roberts and Callari (1982) and other “single system” advocates, for example - would counter that there is no problem in the usual sense, and that the qualification quoted below is a red herring of sorts.

infamous problem. Whether or not this algorithm satisfies *all* of the properties Marx and his followers have advocated is, in this narrow 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 those 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 analyse 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 to draw widespread attention to the 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, and perhaps 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 other references of this kind, Kol’man’s (1968) judgment is difficult to substantiate, but it seems conceivable that Marx, in his efforts

to characterize the laws of economic motion, would have at least considered the decomposition of prices, values and/or 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 been alone in his assessment, however: empirical research in economics, mainstream or radical, would not experiment with these methods for some time.

Kol'man (1968: 222) also finds 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 (controversial) 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 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 centre of oscillation, and on the average peaks and average troughs of oscillation above or below this centre. (Marx 1857: 137)

In econometric terms, the deviations of prices from "average values" are in effect treated as an autocorrelated process with mean zero and constant variance - that is, a stochastic version of the natural price doctrine of classical economics. It should

also be noted that the oscillations Marx describes are the *cause*, 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 recent 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 own 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 “scientific veneer” for their work: 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*, as well as the work of Bentham and Jennings, as an important influence.²⁵ While there is little doubt that Marx would have criticized these efforts, there is also little evidence that 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).

²⁵ Blaug (1985) documents Macleod’s limited role in the so-called “marginalist revolution.”

The pamphlet to which Marx refers is *A Contribution to the Critique of Political Economy* (1860) and it is Macleod's *Theory and Practice of Banking* (1856) that is discussed there, not the *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 1865: 153). The "banal tautologies ... in algebraic form" are 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."²⁶

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 no less than three references to von Thünen's *Der Isolierte Staat* (1860), however. The first occurs in a letter to Ludwig Kugelmann²⁷ 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 Kugelmann that:

[T]here is something touching about [it]. A Mecklenberg squire

²⁶ Schumpeter (1954: 115) notes that Macleod's explanation of bank credit formation, a precursor of modern orthodox models, 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."

²⁷ Kugelmann, a German physician, participated in the revolts of 1848 and served as a member of both the First International and Social Democratic Workers' Party.

... who treats his estate of Tellow as *the land* and Mecklenberg-Schwerin as *the town* and who, proceeding from these premises, constructs for himself the 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 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).

Nowhere, it should be underscored, does Marx object to von Thünen's extensive and innovative use of mathematics. Because *Der Isolierte Staat* contains few theoretical innovations, it is reasonable to infer that, *in some measure*, Marx's favorable impression reflects von Thünen's methodology. As Schumpeter (1954: 466-467) reminds us, he was one of the first to characterize economic phenomena in terms of the 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 that spanned more than two decades, however, 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 attempted to show, informed these studies, which in turn lead 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 abstract representation of economic relations a sensible ambition.

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