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History of the Calculus of Variations in Economics
História do Cálculo Variacional em Economia

Vinícius Oike Reginatto
Prof. Dr. Pedro Garcia Duarte

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Prof. Dr. Ariaster Baumgratz Chimeli
Coordenador do Programa de Pós-Graduação em Economia

VINÍCIUS OIKE REGINATTO

HISTORY OF THE CALCULUS OF VARIATIONS IN ECONOMICS

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Orientador: Prof. Dr. Pedro Garcia Duarte

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ABSTRACT

In this dissertation work, I present a broad historical account of how the calculus of variations was applied in economics in the 1920s up until the 1940s. In the interwar period, mathematical economics was a vibrant and plural community of authors. Previous historical works on this period have focused on specific points of these authors. The present dissertation focuses on the mathematical technique, i.e., the calculus of variations and how it was used in economics. This history also encompasses the early mathematization of economics, the early history of econometrics, and the struggles to devise a dynamic theory of economics in a general equilibrium framework.

I follow mainly the works of American mathematician Griffith C. Evans (1887-1973) whom I argue is a seminal author in this literature. In 1924, Evans used the calculus of variations to put forward a dynamic version of A. Cournot's classic analysis of monopoly. In the following decades, a handful of authors followed Evans's approach and used the calculus of variations to research depreciation, business cycles, optimal savings, and general equilibrium. In the late 1960s, similar mathematical formulations became commonplace in the form of optimal control and dynamic programming. These new mathematical techniques shared intimate relations with the calculus of variations.

RESUMO

Neste trabalho de dissertação, apresento uma história geral de como o cálculo variacional foi aplicado na economia no período dos anos 1920 até 1940. Durante o período do entreguerras, havia uma comunidade plural e vibrante de autores trabalhando com economia matemática. Trabalhos históricos sobre esse período se debruçaram sobre pontos específicos desses autores. O presente trabalho tem como foco a técnica matemática, i.e., o cálculo variacional e como ele foi utilizado na economia. Minha história também abarca o início da matematização da economia, os primeiros anos da econometria, e os desenvolvimentos de uma teoria dinâmica de economia dentro de um modelo de equilíbrio geral.

Este trabalho segue de perto a obra do matemático estadunidense Griffith C. Evans (1887-1973), um autor seminal nesta literatura. Em 1924, Evans usa o cálculo variacional para dinamizar a análise clássica de monopólio de A. Cournot. Nas próximas décadas, a maior parte dos autores que usaram o cálculo variacional em economia seguiram a abordagem de Evans: eles encontraram aplicações para o cálculo de variações em teorias de depreciação, ciclos de negócios e equilíbrio geral. No final da década de 60, modelos matemáticos usando controle ótimo e programação dinâmica se popularizaram em economia. Estas novas técnicas matemáticas têm íntima relação com o cálculo variacional.

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Introduction

In economics, the first successful attempt to overcome the limitations of statical equilibrium theory used the calculus of variations to frame the typical consumer and firm problems into an intertemporal framework. Vilfredo Pareto (1848-1923) had attempted to analyze these problems in 1901 by rewriting the equations of general equilibrium, derived by Léon Walras (1834-1910), using differential equations. A more satisfactory approach, however, was presented by American mathematician Charles F. Roos (1901-1958) in 1927 who generalized the previous work of Griffith C. Evans (1887-1973), his PhD advisor. Evans, in 1924, had presented a dynamical version of the monopoly analysis stated by Augustine Cournot (1801-1877) in 1838, using the calculus of variations to allow for price variation through time and intertemporal optimization. The following year, Roos managed to generalize this problem to the competitive case (when an arbitrary number of firms are involved) and later, in 1927, embedded it in a general equilibrium framework. Harold Hotelling (1895-1973), another American mathematician, was the first to acknowledge the magnitude of Evans's and Roos's theory. He claimed that their “dawning economic theory” lay the groundwork “upon which future contributions to political economy of first-rate importance may be expected to be based” Hotelling 1931a, p. 109. In Hotelling's view, a broad array of questions in economic theory could be reduced to problems in the calculus of variations. By his own account, he also used the calculus of variations in 1931 in a now-famous paper on the optimal extraction rate of a finite natural resource.

These mathematicians brought an advanced mathematical formalism — the calculus of variations — to economic theory at a time when mathematical economics itself was a relatively “uncultivated” discipline. Their work in some senses was restricted to the small community of those with a shared interest in economics and who possessed a strong enough mathematical background to understand it. American economics, at this time, was living its pluralistic period¹: economists tackled issues using a wide array of methodological approaches; Morgan & Rutherford (1998) claim

1. Pluralism here indicates the variety of methods, beliefs, and policy advice present throughout American economic theory throughout the interwar period. I use the term in the same sense as Morgan & Rutherford (1998).

that there was a variety in beliefs, methods, and policy advice amongst economists². In contrast, postwar economics is characterized as the mathematized neoclassical era - in which intertemporal optimization problems became central. The calculus of variations, by this time, would be transformed into dynamic programming by Richard E. Bellman (1920-1984) and into optimal control theory by Lev S. Pontryagin (1908-1988) and his associates. Both would find their ways into economic theory and the calculus of variations became somewhat of a stepping stone towards these more modern techniques.

The present work studies the history of how the calculus of variations came to economic theory and how it developed. I focus mainly on the works of Griffith C. Evans and Charles Roos who used the calculus of variations to build a dynamic theory of economics. Evans is a central figure in this literature not only for his pioneering work on monopoly in 1924 but also for his 1930 mathematical textbook. Evans's 1930 book, *A Mathematical Introduction to Economics*, was the first to present the calculus of variations to economists and became a standard reference for other authors who used this technique in economics. Roos was Evans's most important follower. He generalized Evans's work to a general equilibrium framework and found applications of the calculus of variations in the study of business cycles, depreciation, and economic forecasting. Roos was also a significant figure in the early years of two institutional landmarks: the Econometric Society, founded in 1930, and the Cowles Commission, founded in 1932. These would become influential centers for research in both mathematical economics and econometrics. Economic dynamics, in the 1940s and 1950s, however, did not use the calculus of variations. Samuelson's influential *Foundations of Economic Analysis* framed dynamics as the limiting point of a system of differential equations Weintraub 1991, 101-104. Dynamics became the analysis of the stability of equilibrium points.

In the 1960s and 1970s, the optimal growth and real business cycle theories gained traction in macroeconomics. These theories were built upon intertemporal utility maximization problems, solved using either Hamiltonians (optimal control) or Bellman's equation in a recursive fashion (dynamic programming). At this time, a historical narrative emerged that connected the works of the British mathematician Frank P. Ramsey (1903-1930) in the calculus of variations with the growing literature of optimal growth. Duarte (2009) offers an appraisal of this historical account that crystallized in the now widespread Ramsey-Cass-Koopmans model. Ramsey's 1928 *A Mathematical Theory of Saving* used the calculus of variations to solve the intertemporal problem of a society that wished to make its well being a

2. There were, of course, debates on the proper methods of inquiry in economic theory. See, for instance, the debate between Joseph Mayer and Frank Knight (1885-1972) published in the first volume of *Econometrica* in 1933.

maximum. The narrative argued that because of its advanced mathematics, it was not adequately understood, and it took 30 years for the economics profession to catch up with Ramsey: in fact, the portrait of “Ramsey as a genius ahead of his time’s became “well entrenched among postwar economists” (164). Hotelling’s 1931 *The Economics of Exhaustible Resources*, that used the calculus of variations to find the optimal extraction rate of a finite natural resource, was also “rediscovered” in the 1970s (Erreygers 2009, 264-266). Postwar mathematical economics, however, has marginalized Evans’s figure (Weintraub 1998). Authors such as Dimand and Veloce (2007) contend similarly that both econometrics and mathematical economics, in the postwar period, have ignored both Evans and Roos.

The present work is organized as follows. The first chapter presents a brief history of the calculus of variations while also attempting to review its main features and some key results. While this is mostly an European history, its developments in the 20th century play out in the United States. Similarly to the many economists who flocked to Germany, seeking to pursue PhD level research, American mathematicians traveled to Berlin and Göttingen to study under luminaries David Hilbert (1862-1943), Karl Weierstrass (1815-1897), and especially Felix Klein (1849-1925). Klein had an enormous impact on the formation of many of the leading American mathematicians of this time. His insightful lectures and his broad domain over areas on the frontier of mathematical research attracted several talented students. The mathematics department of the University of Chicago, one of the leading research centers in America in the 20th century, was heavily influenced by Klein: two of its core teachers, Oskar Bolza (1857-1942) and Heinrich Maschke (1853-1908), were former students of Klein. The University also made arrangements to bring him overseas to Chicago during the city’s World Fair in 1903. The University of Chicago was *the* American center for the calculus of variations: first under Bolza and then under his student Gilbert A. Bliss (1876-1951) who was hired as a professor in 1908. Research and general interest in the calculus of variations seemed to dwindle in the late 1920s and 1930s but was rekindled in the 1950s by Bellman’s dynamic programming, a more operational approach to intertemporal optimization problems (McShane 1989).

The history of the calculus of variations, as a mathematical branch, reflects changes in mathematics itself. In the 18th century, when Leonhard Euler (1707-1783) and Joseph-Louis Lagrange (1736-1813) lay the groundwork for the calculus of variations, mathematics was much closer to physics. Physical experimentation served not only as motivation for new mathematical developments, but it also provided mathematics with rigor (Grabiner 2012, 16-17). Foundational questions and matters of proving the existence of solutions were commonly overlooked by mathematicians (Grabiner 1983, 187). The authors of the 19th century, on the other hand, had a much deeper interest in these matters. Karl Weierstrass and his fol-

lowers at Berlin, in particular, aimed to establish more solid foundations for the calculus. This meant proposing more precise definitions of key concepts such as continuity, real numbers, and convergence (Kleiner 1991, 298-302). Mathematics began looking inwards: the more abstract, formalist mathematics contrasted with the applied and more “loose” physical mathematics. Tensions between mathematicians on the proper way to do mathematics culminated in the Foundational Crisis, a collective effort to establish the foundations of mathematics. This parallel history of mathematics appears throughout my history of how the calculus of variations came to economics. Postwar mathematization was deeply influenced by the aftermath of the Foundational Crisis (Weintraub 1998); even earlier, the tensions between mathematical rigor, following either the formalist approach or a more physical approach, are still relevant in the 1920s. Evans, in particular, is deeply influenced by his post-doctoral advisor Vito Volterra (1860-1940). Volterra contended that mathematics — and the sciences more broadly — should be built upon observable and quantifiable concepts. This influence is seen in Evans’s critique of utility. Evans argues that empirical concepts should be the cornerstone of economic theory (Evans 1932).

Chapter two delves in Evans’s work with particular attention to his 1924 article on monopoly. To properly contextualize Evans’s work, one must go to the Italian mathematician Vito Volterra (1860-1940), his post-doctoral advisor and intellectual mentor. Volterra’s work reflected the ongoing debates on mathematical rigor (the Foundational Crisis), and his figure depicts an antiquated view on mathematics constrained to describing observable entities and tied to physics and mechanics. Volterra has a brief stint with mathematical economics and seemingly is responsible for Evans’s initial interest in mathematical economics. The time-frame of this chapter ranges from the 1920s to the 1950s, starting with the work of American mathematician Griffith Evans in 1924. I also contrast this paper with Evan’s 1922 *A Simple Theory of Competition* that presented a summary of Cournot’s analysis of monopoly and competition in a static framework. These first works also give insight into Evan’s view on how the mathematical method should be applied to economics. Evans argued that clarity of hypotheses should be paramount in economics. He also disagreed with the widespread usage of utility amongst economists. In his view, economics should be based upon more “concrete” concepts that could be measured and observed.

In the next section of this chapter, I analyze Evans’s 1930 textbook *Mathematical Introduction to Economics*. This was the first book to introduce the calculus of variations to an audience of economists. I analyze its reception to understand how contemporaries reacted to this new mathematical technique and also to Evans’s critique on utility. The book was criticized by many authors: most notably, British mathematical economists Arthur L. Bowley (1869-1957) and R. G. D. Allen, each

separately, disliked it. Weintraub (1998) shows that the book's mild reception bothered Evans. Dimand and Veloce (2007, 523) note how Evans became much less active in economics following the publication of his book and departure from Rice University to the University of Berkeley in 1933. Indeed, in the following decades, his academic output in mathematical economics diminishes — although he remained actively interested in economics even throughout the latter years of his career. I also analyze Allen's 1938 *Mathematical Analysis for Economists* an influential textbook that included two chapters on the calculus of variations. This textbook analysis leads into the 1950s where textbooks omit the calculus of variations and dynamics is now formalized with the use of differential equations following the presentation in Samuelson's *Foundations*. While it is difficult to access the impact that either Evans or Allen had on graduate education, I show that Evans's textbook was commonly mentioned by authors who applied the calculus of variations to economics.

In the following section, I discuss the work of Charles Roos. Roos's biography overlaps with the foundation of both the Econometric Society and the Cowles Commission. Roos served as the first secretary-treasurer of the Econometric Society in 1930 and as Cowles's first research director from 1934 to 1937. His most relevant work, *A Dynamical Theory of Economics*, was published in 1927 in the *Journal of Political Economy*. Roos showed how the calculus of variations could be used to formalize a dynamic theory of economics in a general equilibrium scheme à la Walras. His work in depreciation, published in 1928, relates closely to the first work in the calculus of variations by Hotelling published in 1925. He also found applications for the calculus of variations in business cycle theory and forecasting. Roos made contributions to econometrics, particularly in demand curve estimation and time series forecasting. Roos's work in econometrics is close to his work in dynamics but, since a complete analysis of this work would exceed the scope of this work, I present only a short analysis of it³. Ross's business interests lead him away from academia in 1937 when he starts his consulting firm.

In the last section of this chapter, I present other works that used the calculus of variations in economics. Evans's work was influential amongst a group of researchers in America and Italy. Italian economists Luigi Amoroso (1886-1965) and Giulio la Volpe (1909-1996), following the initial groundwork established by Pareto, used the calculus of variations to present dynamic versions of consumer and firm theory and attempted to establish a dynamic general equilibrium theory. In America, Hotelling became acquainted with Evans and Roos in the 1920s and wrote two papers using the calculus of variations. He was also the first American author to take notice of Frank Ramsey's optimal savings article, that used variational calculus. Austrian-American economist Gerhard Tintner (1907-1983) also used the calculus of variations in similar

3. Dimand and Veloce (2007) discuss in greater detail the econometric work of Roos.

lines to Evans's. Importantly, as did Hotelling, he used the calculus of variations in utility maximizing problems.

Evans's work is not only marked by its mathematical sophistication but also by its author's refusal to adopt subjective value theory. He sustains that utility functions bring along artificial mathematical constraints while not providing a concrete basis for economic analysis. His position is also seen in the work of Roos and is perhaps a significant factor as to why their work remained stagnant. In this regard, they were alone, as other authors - such as Hotelling and Ramsey - found no issue in pairing the calculus of variations with utility theory. One of Evans's main complaints had to do with the integrability conditions needed to guarantee that one could "recuperate" the underlying utility function from the demand curves. This critique appeared both in his 1930 book and in his 1932 article *The Role Hypothesis in Economic Theory*. The problem of integrability conditions first appeared in Volterra's appraisal of Pareto's *Manual of Political Economy* in 1909. Hotelling, together with American economist Henry Schultz (1893-1938) dealt with this issue when trying to estimate empirical demand curves in the 1930s. British economists John Hicks (1904-1989) and R. G. D. Allen (1906-1983) also worked on integrability conditions and were similarly interested in dynamical theories of economics. Their approaches were altogether different but were shown to be equivalent in a paper by Paul Samuelson (1915-2009) published in 1950. I present a brief discussion on this debate at the end of this chapter.

In the third chapter, I analyze several historical accounts involving Evans, Roos, and other authors who used the calculus of variations in economics. Fox (1988) claims that Evans, Roos, and Tintner were underappreciated at their time because they were too mathematically advanced. Also, postwar general equilibrium theory strayed away from the calculus in favor of convex analysis and set theory. Pomini (2018) similarly argues that the calculus of variations became unfashionable in post-war mathematical economics. He also notes that the rise of Keynesianism obfuscated the works of Evans, Roos, and Amoroso. Samuelson's *Foundations*, in particular, modeled dynamics using different mathematical tools: his approach not only required less sophisticated mathematics, but it was also more compatible with economic reasoning (76). Dimand and Veloce (2007) argues that both Evans and Roos were forgotten by econometrics and mathematical economics. Weintraub (1998) argues that Evans's style of mathematics was incompatible with the axiomatized post-war mathematical economics. In this sense, he argues that Evans was marginalized: mathematical economics drifted away from Evans's empirically-based approach.

A point that is not mentioned in these accounts is that the calculus of variations had some shortcomings that were better dealt with dynamic programming and optimal control. Bellman (1957) emphasized how dynamic programming was a new

formalism for the calculus of variations that made it easier to work with discrete problems and inequality restrictions. Dynamic programming, importantly, lent itself better to computer implementation. Optimal control also had similar advantages in relation to the calculus of variations, especially when dealing with discrete controls and inequality restrictions. I show comments of authors that support this view, and also some works such as Thompson et al. (1971) that formalized Evans's monopoly problem using optimal control.

Chapter 1

A Brief History of the Calculus of Variations

The calculus of variations is the branch of mathematics concerned with finding functions — of a specific class — that either maximize or minimize a particular integral. In many cases, this translates geometrically to determining curves or surfaces that satisfy optimal properties, e.g., the shape of the curve that allows the quickest descent or the shape of the surface of least air resistance. A typical problem in the calculus of variations is that of finding the geometrical shape that encompasses the largest possible area while simultaneously satisfying some criteria. This problem, a specific case of an *isoperimetric* problem, was first investigated by Greek mathematicians in the 2nd century BC. One of their findings was that the circle covers a larger area than any other polygon of *equal perimeter*. The systematized study of these kinds of problems began only in the 18th century following the publication of Leonard Euler’s mathematical treatise *Methodus Inveniendi*¹ in 1744. The term “calculus of variations” first appeared in the correspondence exchanged between Euler and Lagrange from 1754 to 1756. In these letters, Lagrange presented a novel approach to the problems previously tackled by Euler (Cooke 2013, 394). To find the function that made an integral a maximum/minimum, Lagrange analyzed a variation of a candidate function. His approach also significantly simplified Euler’s more geometrical analysis. As is also true with differential and integral calculus, the calculus of variations developed parallel to physics, and many of its results were solutions to particular physical problems.

1. The full original title of the book is *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentis, sive solutio problematis isoperimetrici latissimo sensu accepti* (“A method for finding curved lines enjoying properties of maximum or minimum, or solution of isoperimetric problems in the broadest accepted sense”).

1.1 The 17th and 18th Century: Euler and Lagrange

In 1696, Johann Bernoulli (1667-1748) proposed the *brachistochrone* curve problem in the *Acta Eruditorum*. The *brachistochrone* problem is that of finding the shape of the curve along which a particle will fall in the shortest amount of time (influenced only by the force of gravity). That is, put more abstractly, suppose that a curve joins two points at different heights and that a small bead is placed on the higher point: which curve shape will allow for the bead to reach the lower point in the shortest amount of time? Solutions were put forward by Johann Bernoulli himself, his brother Jakob (1655-1705), Gottfried Leibniz (1646-1716), and Issac Newton (1643-1727). John Bernoulli would, a couple of years later, again posit a mathematical challenge in the *Acta*: that of finding the shape of the curve, encapsulating the largest area, that satisfied a certain set of criteria. This problem belongs to the aforementioned class of *isoperimetrical* problems; these are questions of finding the shape of the plane figure with the largest possible area, given a specific length for its boundary. Johann Bernoulli, his brother Jakob, and mathematician Brook Taylor (1685-1731) explored a series of isoperimetrical problems in the following years².

Euler was close to the Bernoulli family and also became instigated by variants of isoperimetrical problems. He wrote two articles on this subject that were published by the St. Petersburg Academy of Science in 1738 and 1741. His most important work, *Methodus inveniendi* was published in 1744. In it, Euler not only classified and solved variational problems but also formulated them in a very general form and successfully derived equations for the solution of these problems. Using a geometrical method of breaking a curve into arbitrarily small pieces, he was able to derive the first general expression for a (necessary) solution for finding the maximum/minimum of an integral, a defining problem in the calculus of variations. These problems were dealt both with specific restrictions and when free of them. He also, as mentioned previously, related his work with the “principle of least action” in physics, showing how motion always acted in a way that minimized the action integral. A similar statement had already been stated by Pierre de Fermat (1607-1665) and later by Pierre L. Maupertis (1698-1759). Fermat’s studies in optic lead him to a unifying principle: the path taken by a ray of light is that which can be traversed in the

2. These early works reveal the common “problem-driven” motivation behind many of the developments in the calculus of variations. On this point, Jellett (1850) notes that the origins of the calculus of variations are not to be found “in any systematic treatise, but in the investigation and solution of some particular problem” (Jellett 1850, i). More modern accounts of the history of the calculus of variations also share this sentiment as in Carathéodory (1935), quoted in Pesch and Bulirsch (1994): “One can affirm that the charm, exerted all along by the calculus of variations on so many first rate greatneses [sic] of mind, is chiefly traceable to the role which particular problems have played and are playing even today in the development of this theory.” (221).

least time. Maupertis enunciated a more general law stating that the trajectory exhibited by a moving particle was that which minimized kinetic energy (although he used the term *vis viva*). Euler restated this principle using a mathematical formalization more akin to its modern presentation and also without the many theological implications drawn up by Maupertis³. The variational principle, stated by Euler, allowed for a unified view of mechanics and would remain central in its study throughout the following centuries. Quantum mechanics, in particular, would heavily rely on variational principles. The last chapters of the book deal extensively with isoperimetrical problems⁴.

Although the mathematical importance of the *Methodus* is clear⁵, it was quickly surpassed by the work of Joseph-Louis Lagrange (1736-1813), presented to Euler himself through a series of letters they exchanged from 1754 to 1756. Lagrange's work succeeded in overcoming the geometrical limitations of Euler's work with the innovative idea of the variational method. This approach is still employed today and consists basically of using an auxiliary function to cause a small "variation" along an original curve (an operator defined as δ)⁶. One arrives at the solution by noting that no perturbation should be able to increase/decrease the value of the desired integral expression. Thus, one derives the *Euler-Lagrange equation*. The intuition behind this result is similar to that of finding optimum points in differential calculus: instead of a small variation dy of a specific point on a curve, one investigates a small variation δy of the entire curve. Lagrange's operator allowed for very convenient manipulation with integrals and differentials expressions and thus simplified the many encumbrances of Euler's method⁷. Lagrange also reformulated a wide class of

3. These differences, added to the fact that Euler arrived independently to his principle, lead to disputes as to the which author had priority over the *principle of least action*. The first enunciation of the *principle* is usually attributed to Maupertis in his works published in 1740 and 1744. In them, he claims that nature acts in such a way as to minimize the *vis viva* (kinetic energy) and then applies this result to the work of Fermat. Fermat had previously shown that light travels through mediums in such a way as to minimize the passage of time. Maupertis demonstrates that Fermat's principle is a special case of his broader one. In 1744 Euler arrives at a similar result: he shows that motion minimizes the "action integral". Thus Goldstine (1980, 101-109) argues Euler should have priority as the relation between kinetic, potential and action was not clear until later, and that Euler's presentation is thus more precise. Other authors (Kreyszig 1994, 675-676, Ferguson 2004, 11-13) argue similarly. Nonetheless, Euler himself gave priority to Maupertis for the first enunciation of the *principle*.

4. Fraser (1994) argues that these chapters are not as tight as the previous ones, stating the Euler seemed to give isoperimetrical problems only a marginal importance.

5. Carll (1881) in a brief history of the calculus of variations says that it "displays an amount of mathematical skill almost unrivaled." (545). Kreyszig (1994) calls it "a landmark in the development of the subject" (675).

6. This is the standard presentation of the Euler-Lagrange necessary conditions for an extremal and is present in almost every textbook on the subject. For more details see, for instance, Kamien and Schwartz (1981, 14-19). The name "calculus of variations" was introduced by Euler because of these "small variations".

7. This was not unanimously seen as an advance for Woodhouse (1810) notes that while Lagrange overcame the inconveniences of Euler's method it also "deprived its principles of a considerable

problems with restrictions using the now called *Lagrange multipliers* to essentially solve a restricted problem as an unrestricted one. Lagrange's later work, in special his *Mécanique analytique* (1788), is also a noteworthy contribution to mathematical physics and analytical mechanics.

To put things more clearly, a bit of mathematical formalism may be welcome. The typical problem in the calculus of variations is to find the *extremal* (maximum or minimum) of a definite integral of a functional. A functional is to a function what points are to functions: a functional associates functions to (real) values - in the same sense that a real-valued function maps points to real numbers. This functional will usually depend on time (t) a function ($x(t)$) and on the changes of this function ($x'(t) = \frac{dx}{dt}$). So the problem is to find $x(t)$ that renders

$$\int_{t_0}^{t_1} F(t, x(t), x'(t)) dt \quad x(t_0) = x_0, x(t_1) = x_1, t_1 \text{ is fixed} \quad (1.1)$$

a maximum or minimum. Lagrange's method involves defining an auxiliary function $\phi(t) = x(t) + \xi(t)$ where $\xi(t)$ acts as a small perturbation on $x(t)$. If $x^*(t)$ is the solution for (1.1) then it can be shown that it satisfies⁸:

$$F_x(t, x^*(t), x^{*'}(t)) = \frac{d}{dt} F_{x'}(t, x^*(t), x^{*'}(t)), \quad t_0 < t < t_1 \quad (1.2)$$

Where F_x and $F_{x'}$ denote partial derivatives of F with respect to its second and third arguments respectively. Equation (1.2) is the Euler-Lagrange equation and it is a necessary condition for a solution. It can be more succinctly stated by omitting the arguments as $F_x - \frac{d}{dt} F_{x'} = 0$. Goldstine (1980) notes how Euler and Lagrange understood their equation to be both a necessary and a sufficient condition for a solution. Proper second order conditions, however, would only be enunciated by Karl Weierstrass many years later. The works that followed Euler and Lagrange sought to fill this gap by presenting sufficient conditions of varying degrees of generality. Adrien-Marie Legendre (1752-1833) presented second-order conditions for an extremal in a memoir to the Paris Academy in 1786 entitled *Sur la manière de distinguer les maxima des minima dans le calcul des variations* (On the method of distinguishing maxima from minima in the calculus of variations). For a problem such as that in (1.1) the conditions for a maximum are $F_{x'x'} \leq 0$ and for a minimum that $F_{x'x'} \geq 0$, where $F_{x'x'}$ is the second partial derivative of F with respect to its third argument.

Legendre worked out his theorem starting from equation (1.1) and, via a second-order Taylor expansion, arrived at what is called the "second variation". This pro-

portion of their plainness and perspicuity" (Woodhouse 1810, 79).

8. For more details see, Kamien and Schwartz (1981, 14-19)

cedure is analogous to finding second-order conditions for a maximum/minimum in differential calculus. Although Legendre tried to state a stronger version of his theorem, with $F_{x'x'} > 0$, he was unable to do so correctly; while he successfully managed to simplify his analysis by working with a quadratic form a technical detail slipped by him that was pointed out by Lagrange in 1797, that showed the result to be necessary but not sufficient. Legendre's argument relied on finding the solutions to a quadratic differential equation over an entire interval. Lagrange noted that, for some choices of functions, there may exist no solutions in that interval; this gap, however, was only bridged by Carl Jacobi (1804-1851) nearly 50 years later. In correcting this argument, Jacobi provided the first sufficient condition for a local extrema⁹. He also had a pivotal role following the pioneering work of William Hamilton (1805-1865) in mechanics. Hamilton's *Philosophical Transactions*, published in 1834 and 1835, greatly simplified the study of motion by reducing it to the differentiation of a single function, that simultaneously satisfies a pair of differential equations. A few years later, Jacobi polished many of Hamilton's demonstrations and also further simplified his analysis by dropping one of the differential equations to be verified¹⁰.

Other important authors for the development in the calculus of variations include - but are not limited to - Siméon Poisson (1781-1840), Mikhail Ostrogradski (1801-1862), Otto Hesse (1811-1874), and Isaac Todhunter (1820-1884). These works were summarized and systematized into textbooks as early as Robert Woodhouse's (1773-1827) *A treatise on isoperimetrical problems, and the calculus of variations* published in 1810. Further textbooks include treatises by Richard Abbatt (1800-1884) in 1837, by John Jellett (1817-1888) in 1850, and by Lewis Carll (1843-?) in 1881. These books served the dual purpose of teaching a novice student the fundamentals of the calculus of variations, as to allow him to comprehend the texts of the aforementioned mathematicians, and also of presenting the subject's historical development. Indeed, the historical presentation of the works of Euler and Lagrange up to Jacobi and Hamilton remained common even in the textbooks written in the 19th and early 20th century¹¹. These textbooks also hint to the relative obscurity of the calculus of variations at this time. Carll (1881), in particular, notes how

9. Intuitively, Legendre's condition specifies a local criterium to verify optimality. To reach sufficiency some "global" consideration needs to be added to the analysis. To achieve this, he introduced the novel concept of *conjugate points*. A point $c \in [a, b]$ is said to be *conjugate* to a if both a and c are solutions to Jacobi's auxiliary equation (and c is not identical to zero). An alternative definition is to say that if different neighboring extremals (both starting at a) become very close to each other at c , then c is conjugate to a . While this may seem very abstract, it is analogous to verifying the sign of the Jacobian matrix of a quadratic form via principal minors. For more details, see Gelfand and Fomin (1963, 105-117, 125-129).

10. For more details, see Goldstine (1980, 176-186).

11. Liberzon (2012) is an example of a contemporary textbook that still holds, in some aspects, this sort of presentation. The discussion of sufficient conditions for an extremal, in particular, follows its historical development from Lagrange and Legendre, to Jacobi and finally to Karl Weierstrass.

the Jellett's 1850 *An Elementary Treatise on the Calculus of Variations*, although "justly deemed the only complete treatise [on the calculus of variations] which has ever appeared in English" was seldom available as to "not be found in the majority of the college libraries of the United States" (Carll 1881, iii). Similarly, the works of the authors just mentioned were also out of print and inaccessible¹².

By the end of the 18th century, the calculus of variations had accrued a voluminous body of work. In parallel to developments in physics, and especially in mechanics, variational principles were being applied to a wide array of natural phenomena. Further developments, in the 19th and 20th century, however, became ever more motivated by internal aspects (as opposed to external motivations such as from physical experimentation). Instead of seeking for novel results and new applications, mathematicians now sought to verify the soundness of past findings: previously established results were reexamined and effort was made to show that these logically followed from precisely stated definitions. Lagrange was the first to consider questions of this kind seriously. Following his years as a professor at the École Polytechnique, he tried to make the calculus rigorous by translating its results to algebra¹³. This reflected a change not only in style and approach but more fundamentally of rigor and logic that also extended to other branches of mathematics.

As we shall see, an important historical event to understand the introduction of the calculus of variations (and advanced mathematics, in general) to economics is the timing of the turn of the century Foundational Crisis. At that time, the beginning of the 20th century, questions of the basis of mathematics came to the forefront: questions of how to properly prove a mathematical theorem; or even more generally, of how to prove that the entire body of mathematical knowledge was logically consistent (i.e., that it did not lead to contradictions). These type of contentions were either absent or confined to the introductory sections of 18th-century mathematical treatises. Foundational questions, matters of rigor in the demonstrations of theorems, were not a major preoccupation nor a research interest for 18th-century mathematicians with few exceptions. These authors were not oblivious to matter of rigor, their attention, however, was directed to solving specific problems¹⁴. More

12. Carll (1881) reiterates this point in the preface to his textbook on the calculus of variations. While composing his book, he notes that he "has never enjoyed the acquaintance of anyone who had made the Calculus of Variations the subject of extensive study, and has consequently been obliged to depend solely upon his own judgment ..." (Carll 1881, vii).

13. Lagrange believed algebra to be a more rigorous medium to express mathematics. For more on his algebraic vision of the calculus and his changing approach to the calculus of variations, see Fraser (1985) and Fraser 1987.

14. Rigor, at that time, was closely tied to symbolic notation (i.e., the symbols used to represent mathematical entities), understood to be a key method for discovery Kleiner 1991. The success of algebra and calculus bestowed an enormous trust upon the correctness of symbols and the proper manipulation of them - to demonstrate a theorem - was considered satisfactory (see Grabiner (1974) and Grabiner (1983)).

often than not, questions of rigor could fruitfully be put aside because the physical problems being dealt with “guaranteed” that solutions would be well behaved. Also, much of the work of Euler — and of some of his distinguished contemporaries — was marked by an uncanny intuition that helped him avoid any serious logical pitfall in his demonstrations (Grabiner 1983, 188). Lagrange, as already mentioned, was an important transitional figure. His lifelong interest in making the calculus rigorous through the translation of its results to algebra sparked the initial discussion around foundations¹⁵. Lagrange’s *Théorie des fonctions analytiques* and *Leçons sur le calcul des fonctions*, published in 1797 and 1801, respectively, helped make foundational topics legitimate research inquiries¹⁶. These would be seized many years later by Augustin-Louis Cauchy (1789-1857) who, in many regards, is responsible for making the calculus rigorous and for shaping the style that predominated in the works of mathematicians of the late 19th and early 20th centuries¹⁷. Cauchy’s *Cours d’Analyse* founded the calculus on inequalities, limits, and the now widespread $\delta - \varepsilon$ argument to refine the notions of continuity of functions, of the convergence of infinite series, and of derivatives¹⁸. His work represents a significant departure from Newton’s *fluxions* and the loose treatment of the foundations of calculus; in this sense, Cauchy lay the groundwork for the rigorization of analysis by the school of Weierstrass (Grabiner 2012, 77-78).

15. For more on Lagrange’s algebraic vision of the calculus, see Fraser (1987).

16. Grabiner (1983, 188-190) and Grabiner (2012, 31-46) argues that interest in foundations during the 18th century came about for at least three reasons: (1) some mathematicians, such as Lagrange and Jean-Baptiste D’Alembert (1717-1783), shared the sentiment that an insurmountable wall had been achieved in the development of the calculus - new findings became scarcer and this perhaps led some to look back at previously established results, seeking to unify and generalize them; (2) British philosopher Bishop Berkeley (1685-1753) called out the calculus for not being rigorous in his 1734 *The Analyst, or a Discourse Addressed to an Infidel Mathematician*, mocking the ethereal and vague definitions of limits and fluxions (Newton’s term for the differential quotient $\frac{dx}{dy}$); while many mathematicians dismissed Berkeley’s contentions, Lagrange considered them valid and attempted to address them - most notably, by his suggestion the Berlin Academy proposed the question of the foundations of the calculus as mathematical prize in 1784; (3) at this time mathematicians were being incorporated into the professorial rank: the need to teach led both Lagrange and Cauchy to begin to write on foundational topics during their time at the École Polytechnique.

17. On the importance of Cauchy to the foundations of calculus see Grabiner (1983) or Grabiner (2012).

18. A simple example of the $\delta\varepsilon$ argument is in the definition of the continuity of a function. A function f from $R \rightarrow R$ is continuous on a point $p \in R$ if given a $\varepsilon > 0$ there exists a $\delta > 0$ such that if $|p - x| < \delta$ then $|f(p) - f(x)| < \varepsilon$.

1.2 The 19th Century: Weierstrass and Oskar Bolza

Cauchy's program to make the calculus rigorous was fully embodied in the second half of the 19th century in the works of Karl Weierstrass (1815-1897) and his disciples at both Berlin and Göttingen. Amongst his widespread contributions to mathematics, he devoted serious considerations to the calculus of variations. Weierstrass managed to compare global characteristics between extremal candidates using a novel excess function (or "E-function") that encompasses the total variation of the comparison curve and whose sign (either non-negative or non-positive) was proven by him to be a sufficient condition for an extremal¹⁹. He also devised alternative necessary conditions for an extremal that dealt with corner points, allowing for analysis of a much wider class of piecewise smooth functions. He essentially split the function around this corner (or discontinuity), analyzed the impact of small neighboring perturbations, and arrived at additional conditions to determine the extremal²⁰.

Despite its importance, Weierstrass's contributions to the calculus of variations were seldom known outside of a restrict circle of mathematicians because none of it was formally published until 1927, 30 years after his death. His work was disseminated through his advisee's dissertations and the lecture notes compiled by his students. Adolf Kneser (1862-1930) was the first to present Weierstrass's contributions in a book in 1900. Kneser had been a student of Weierstrass and himself introduced many important concepts such as those of "field" and of "neighborhood in the wide and narrow senses" (Zermelo 2013, 518).

A noteworthy dissemination work on the Weierstrassian tradition in the calculus of variations is that of Hans Hahn (1879-1934) and Ernst Zermelo (1871-1953). Hahn and Zermelo (1904) was published in the *Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen* (Encyclopedia of the mathematical sciences with the inclusion of their applications) a project envisioned in 1895 by Felix Klein, Franz Meyer (1856-1934) and Felix Müller (1843-1928). Their goal was to compile a comprehensive reference work on the mathematics of the 19th century. The first of the planned seven volumes appeared between 1898 and 1901 — the entire work came about in 1935. Initially, the editors had considered commis-

19. The intuitive idea behind his proof (for a minimum) is to show that the functional lay entirely above a given plane (a linear approximation around a given point of interest). For a detailed presentation, see Gelfand and Fomin (1963, 146-150) or Liberzon (2012, 76-81).

20. A point c is said to be a *corner point* if the limit of it's derivative, approaching a from the "right", does not coincide with it's limit approaching from the "left". A simple example is the point 0 in the graph of $f(x) = |x|$ where $|x|$ is the modulus of x . For the simplest case, the one presented in (1.1), the condition would be that $F_{x'}$ and $x'F_{x'} - F$ are both continuous around each corner point. For a presentation of the Weierstrass-Erdmann corner condition, see Gelfand and Fomin (1963, 61-63).

sioning Kneser for the task of submitting an article on the calculus of variations. Even though Kneser already had a written piece ready in 1903 he was unwilling to acquiesce to the editor's demands to change it. The editors then went after Ernst Zermelo (1871-1953) and Hans Hahn (1879-1934). Hahn and Zermelo were well acquainted with the calculus of variations: the former had recently lectured on the topic in Göttingen, in 1902, and the latter had not only worked with the calculus of variations in his dissertation and PhD thesis but also had delivered a talk on Kneser's textbook in the Göttingen Mathematical Society in 1901. Their encyclopedia entry followed the tradition established by David Hilbert in Göttingen: they introduced the calculus of variations — in the nowadays conventional — “functional approach” rather than the geometric/parametric approach of Weierstrass and Kneser. This approach had the advantage of avoiding many technical difficulties that naturally arose in a geometrical framework, that had nothing to do specifically with the variational techniques being presented. Following the publication of this work, both Zermelo and Hahn would become prominent names in the international mathematical community.

The first English-language textbooks, grounded in the Weierstrassian tradition, came from two of Weierstrass's former students: the American Harris Hancock (1867-1944) and the German Oskar Bolza (1857-1942). Hancock studied mathematics at John Hopkins and like many of his contemporaries found no suitable opportunities to pursue his graduate education in America. This was especially true after the departure of James J. Sylvester (1814-1897) from the faculty who returned to his native England in 1883 to lecture at the University of Oxford. While Sylvester, during his time in the US, had begun to lay the foundations necessary for a mathematical research center at the Johns Hopkins, American mathematicians, desirous of PhD level research, were forced to seek their studies abroad. The vast majority of these opted to study in Germany; Hancock, in particular, worked his PhD at Berlin University where he studied under H. A. Schwarz (1843-1921), F. G. Frobenius (1849-1917), and Weierstrass. In 1892 he was hired to the nascent Chicago University to lecture on the calculus of variations. In 1904 he published *Lectures on the Calculus of Variations: the Weierstrassian Theory*. He notes in the book's preface that he delayed its publication in anticipation of an official release of Weierstrass's works on the subject; he eventually decided to publish his book and claims he followed Weierstrass's treatment of the subject as closely as possible.

Oskar Bolza had a challenging career in Germany. He began his higher education in Berlin in 1875 studying physics under Gustav Kirchhoff (1824-1887) and Hermann von Helmholtz (1821-1894). He then began his dissertation work under physicist August Kundt (1839-1894). The laboratory work in physics, however, bored him and he decided to move definitely into pure mathematics. He spent the following years,

from 1878 to 1881, traveling Germany. He studied under Elwin B. Christoffel (1829-1900) and Theodor Reye (1839-1894) in Strassburg, Schwartz in Göttingen, and Weierstrass in Berlin. Bolza's studies into elliptic functions and Abelian functions would spark Klein's attention. Bolza finished his doctoral thesis on this subject in 1886 under Klein's supervision at Göttingen.

During this time, Klein was beginning to build his reputation as a masterful teacher, to whom many of the most distinguished students gravitated towards, and that of Göttingen as the leading institution for mathematical research in the world. Indeed, many distinguished American mathematicians elected to work their doctoral research under Klein²¹. Klein had a broad domain over the growing fields of mathematics of his time²². He cultivated relations with many of the leading mathematicians of his time and openly shared insights with his students (Parshall 2006, 194). Klein's lectures and seminars attracted many talented American mathematicians. A number of these would take on prominent positions as scholars and become leading figures in their areas of research.

The job market for professors in mathematics looked particularly uninviting in the 1880s and 1890s in Germany²³ and Bolza, as well as many other talented mathematicians, struggled to find a position in a university. In 1888, Bolza decided to try his luck in America and, with only a letter of recommendation from Klein in his possessions, landed a temporary job at the Johns Hopkins and, after a couple of years, at Clark University. In 1892 he was approached by Eliakim Hastings Moore (1862-1932), from the University of Chicago, to join as a faculty member and, after back and forth negotiations, he accepted the position. The hiring of Bolza to the mathematical department of Chicago was part of a conscious effort from its president, William R. Harper (1856-1906), to build a top-tier University, inspired by German academia, founded on the principles of excellence in research and academic autonomy.

21. A short list of some of Klein's American students who wrote their doctoral dissertations under him include: Henry Burchard Fine (1858-1928) in 1886, Maxime Bôcher (1867-1912) in 1890, Henry Seely White (1861-1943) in 1891, Henry Dallas Thompson in 1892, Edward Burr Van Vleck (1863-1943) in 1893, and Virgil Snyder (1869-1950) in 1895. All of the aforementioned authors served as presidents of the American Mathematical Society and held professorships in top-tier American universities. For a complete list of Klein's seminars and doctoral students see Parshall and Rowe (1994, 255-259).

22. Parshall (2006) notes that "Klein's mathematics embodied many of the ideals characteristic of German scholarship in the nineteenth century. Even from his youth, he sought to attain a unified conception of mathematical knowledge that embraced the achievements of his predecessors" (193-194).

23. Parshall and Rowe (1994) comment that the "fragile institutional structures that supported research-level mathematics at the German universities" resembled a "medieval guild" (198). The aspiring professor had to spend several years working without a salary from the university and rising from this initial barrier became increasingly difficult throughout the 1880s and 1890s. Not only were positions in mathematics becoming scarcer, but the available pool of candidates also kept on growing.

Harper was a Yale PhD who lectured on classical languages — mainly Hebrew — across the country. For many years he served as a principal director at the Chautauqua Liberal Arts College. In 1890, Harper was approached with the offer to preside the newly founded University of Chicago. At Chicago, Harper had now both a clear philosophical vision, that put teaching and original scholarly research as the pillars of upper education, as well as the funding and trust from Rockefeller to support it. He maintained a clear distinction between graduate and undergraduate practices, the former being more focused on research. Faculty members should be hired accordingly to this divide and their expected duties would be different: graduate teaching loads would be lighter, professors would be granted time to work exclusively on their own work, and promotions would be contingent upon research productivity (Parshall and Rowe 1994). Harper also heavily endowed his handpicked head professors with autonomy: all decisions that concerned the inner workings of their respective departments were to be figured out internally, although, after settled, these should be carried out methodically. For the mathematical department, Harper sought after E. H. Moore, a Yale PhD, who had also studied abroad in Germany at the University of Berlin, and that currently lectured at Northwestern University. Harper had met Moore as a Tutor in mathematics during his time at Chautauqua and had retained of him a positive image. Harper had also, during a series of personal interviews, found that Moore shared his ideals in graduate instruction and research. Delegating such responsibilities to a relatively unproven mathematician was, however, nothing short of a gamble by Harper. From over-watching entrance examinations, arranging and approving course offerings, to hiring new faculty, Moore could reign freely over the department following his own blueprint.

Moore initially sought to structure his department around five professors (himself included), two of whom should be hired under non-permanent conditions, and other two to be hired in the professorial ranks. While the latter would do “excess” work, the former would help shape the department and the advanced course offerings. Moore first went after his friend Henry Seely White (1861-1943), a former PhD student of Felix Klein, for one of the professorial positions. White, at the time, worked at Clark as an associate professor and negotiations never came through. Moore continued seeking after former Klein students residing in America. The next names in line were William Fogg Osgood (1864-1943) and Maxime Bôcher (1867-1918). Bôcher, however, had recently been admitted as a professor at Harvard and Osgood was expecting a position at Harvard as well. Moore then turned to Bolza who at the time was an associate professor at Clark. Negotiations, however, were not smooth as Bolza at first was entertaining plans to return to Germany and then later demanded that his old Berlin companion Heinrich Maschke also be hired for a position. Harper, while supportive of Moore’s plans, claimed that hiring

Maschke was not financially feasible. By the autumn of 1892 the whole quarrel settled: a timely financial grant came through, allowing Maschke to be hired as an assistant professor and Bolza, as an associate. Initially, Bolza feared that the strict regiments at Chicago (and its Catholic influences) reminisced closely to Oxford or Cambridge, that is, an “English training ground for passing exams” (Bolza quoted in Parshall and Rowe 1994, 294), and even of the absolute rule that Moore was entitled to by Harper. These concerns were quickly drawn away by Moore’s tactful and competent leadership: with Moore as head, these three figures would foster a prolific intellectual environment at Chicago. The department was complete with the hiring of H. Hancock²⁴, who was to lecture on the calculus of variations, and Jacob W. A. Young (1865–1948), a Clark PhD, who lectured on number theory and the theory of invariants.

Moore would be at the forefront of fundamental institutional changes in the American mathematical landscape. His amicable relation to Klein – no doubt bolstered by the presence of Maschke and Bolza at the department – fostered fruitful collaborations and brought to Chicago additional interest from European mathematicians. This would result in Klein’s participation in the Chicago Mathematics Congress, run together with the World’s Columbian Exposition in 1893, and in a following two week mathematical seminar hosted at Evanston by Henry White, in a cooperative effort between the University of Chicago and the Northwestern University. Both events were seen as a whopping success, and Moore aptly capitalized on this momentum to request the New York Mathematical Society to subsidize the proceedings from the Congress. He also insisted on the creation of the Chicago Section of the American Mathematical Society (AMS) thus centering the Midwestern American mathematical community around their department. He also pushed for the creation of a new mathematical journal, the *Transactions of the American Mathematical Society*, and served as chief editor of *Transactions* for its first eight years. During this time frame, Chicago was one of the *big three* together with Princeton and Harvard that were considered the top-tier departments for mathematical research in America. Indeed, through the efforts of E. H. Moore, Chicago rose to prominence during the 1900s and 1930s in the American mathematical community.

In 1898, Bolza was invited as a keynote speaker to the third Colloquium of the AMS to survey broad areas of research and to suggest potential paths for further work. He was supposed to deliver a talk on his familiar field of expertise, that of the theory of hyper-elliptic functions. Instead of this, he presented a series of “advances in the calculus of variations: the ideas of Weierstrass and others aimed

24. Contentions between Hancock and Moore lead to an uneasy relationship that ended with Hancock’s departure from Chicago to Cincinnati University in 1900. Bolza who previously shared lectures on the calculus of variations would fully take on these courses.

at rigorously establishing old eighteenth and early nineteenth-century results, the sufficiency proofs of both Weierstrass and Hilbert, Hilbert's existence proof, and the then-recent transversal theorem of Adolf Kneser" (Parshall and Rowe 1994, 394). Perhaps influenced by his doctoral advisee Gilbert A. Bliss, whom a year prior defended a thesis in the calculus of variations, entitled "The Geodesic Lines on the Anchor Ring", Bolza had reignited his interest in variational calculus. During the following years he would oversee other 7 PhD thesis on the calculus of variations, and, as mentioned above, compose his own textbook on the matter – a major reference at the time for introducing many Weierstrassian results in the English language. A few years later, in 1908, he also published an expanded German version of the same textbook.

1.3 The 20th Century: G. A. Bliss and the Calculus of Variations in America

Following the death of Maschke, Bliss was appointed a position at Chicago in 1908. While he initially is encumbered with the advanced geometrical courses previously lectured by Maschke, the departure of Bolza from the department in 1910 opened way for Bliss to teach in his preferred field of the calculus of variations. This was a return to familiar grounds for Bliss: his academic studies began at the University of Chicago, albeit not in mathematics proper but in mathematical astronomy. Indeed, Bliss's first graduate work was under professor F. R. Moulton (1872-1952) in astronomy entitled *The motion of a heavenly body in a resisting medium*. While he desired to further pursue this path, his request for a fellowship was not granted and he eventually decided to endeavor into pure mathematics²⁵. Although brief, Bliss states that this contact and this first work had “an influence on myself quite out of proportion to the value of the paper itself” (Bliss 1952, 595). His later career suggests that, albeit his academic work was restricted to pure mathematics, he remained interested in mathematics that could find applications and was at times even a bit critical of the apparent disinterest of the mathematicians of his time to seek for real-world applications to their work²⁶.

Bliss had been drawn to the calculus of variations through the influence of Bolza's lectures at Chicago. Bolza had also shared with him his personal notes of the Weierstrass's lectures he'd attended in Berlin in 1879. These notes were very precious as the pioneering work of Weierstrass, as mentioned above, had not been officially published at that time. A couple of years after he finished his PhD thesis in 1900, Bliss studied for a year abroad at Göttingen and, on his return, found work at the Universities of Minnesota, Missouri, and Princeton in the years from 1904 to 1908. In 1908, when Bliss was hired at Chicago, he was also appointed as the editor of the *Transactions*, a position he would retain until 1916. During the wartime, Bliss worked as a “scientific expert” in the Range Firing Section at Aberdeen upon the call of Oswald Veblen — himself a former Ph.D student of E. H. Moore at Chicago. There he greatly improved upon the current algorithms that had been devised by his former advisor Moulton together with Veblen and their associates as an extension from the previous theory of F. Siacchi (1839-1907) that calculated approximate solutions for

25. This transition may be more straightforward than might be imagined as Lane (1988) mentions that the astronomy department, that emphasized mathematical astronomy, was closely related to the mathematics department.

26. On this matter Bliss wrote: “It does seem unfortunate, however, that for some reason the number of people in this country interested in the adaptation of mathematical theories to the pressing contemporary problems of the applications has been relatively small. I am hoping sincerely that cooperation of mathematicians with scientists in other fields will presently be effective in remedying this defect” (Bliss 1920, 344).

the trajectories of projectiles under “non-normal conditions” (differing wind effects, non-standard projectile weight, etc.). Bliss’s background in variational calculus provided him with just the tools to tackle this problem and he quickly arrived at both the mathematical foundations and the procedures to deal with trajectories under a variety of disturbances in an efficient manner. This work would also result in two papers in the Journal of U.S. Artillery and, much later, in his 1944 book *Mathematics for Exterior Ballistics*.

In 1927 Bliss was appointed chairman at Chicago and his tenure lasted until he retired in 1941. During the 1927–1937 period he oversaw 34 PhDs²⁷ in the calculus of variations (out of 117 Ph.Ds awarded during that period) and was considered to be one of the grand authorities on the calculus of variations in America (Parshall and Rowe 1994). This was particularly true of his field of specialty, the so-called “classical” calculus of variations: the theory of problems involving simple (in contrast to multiple) integrals — the most notorious cases being the problems of Bolza, Lagrange, and Mayer. A Bolza problem can be stated generically as that of finding the extremal of

$$\int_{t_0}^{t_1} F(t, x(t), x'(t))dt + G(t_0, x(t_0), t_1, x(t_1)) \quad (1.3)$$

subject to differential equations and end-conditions of the form:

$$\begin{aligned} \phi(t, x(t), x'(t)) &= 0 \\ \psi(t_0, x(t_0), t_1, x(t_1)) &= 0 \end{aligned}$$

If $G \equiv 0$, that is, if (1.3) is simply

$$\int_{t_0}^{t_1} F(t, x(t), x'(t))dt \quad (1.4)$$

then we now denominate it a Lagrange problem; similarly if $F \equiv 0$ then it is now a Mayer problem²⁸. As we shall see, the vast majority of economic applications, that employed variational calculus, classify into one these three simple integral problems. Bliss’s name will, indeed, be common reference amongst the authors that apply the calculus of variations to economics.

Two years prior, Bliss had written a more popular reaching book, the first monograph in the *Carus* series. The Carus Mathematical Monographs were founded on

27. Some notable advisees include: Alston S. Householder (1904-1993), David M. Smith (1884-1962), Edward J. McShane (1904-1989), Lawrence M. Graves (1892-1973), and Magnus Hestenes(1906-1991)

28. For more details on these problems and their classification see, for instance, Bliss (1946, 187–219).

the ideal of making advanced topics in mathematics accessible to broader audiences not only amongst scientists in distinct fields, but even to those with “only a moderate acquaintance with elementary mathematics”. Backed by a generous endowment from Mary H. Carus and Edward H. Carus, Bliss together with Chicago mathematician Herbert Ellsworth Slaught and David Raymond Curtiss worked as editors for the series during its initial years²⁹. The presentation of subjects in the 1925 *Calculus of Variations* is motivated by particular topics, each chapter devoted to a famous problem in the calculus of variations (trajectory of shortest distance, brachistochrone curve, etc.), and a general treatment is offered only in the book’s last section. Bliss found this approach to be justified on several grounds noting that a comprehensive account of these elementary subjects was only to be found scattered amongst various treatises aimed at the specialized public. While the book claims that its introduction to each subject and the theorems presented should be “perfectly intelligible to everyone” a complete understanding of the book required a familiarity with the differential and integral calculus, a prior knowledge that Bliss assumed the reader to possess. Perhaps, for this reason, Arnold Dresden (1882-1954), another former student of Bolza in the calculus of variations, wrote that the book would find its public – mathematicians aside – amongst engineers, physicists and chemists³⁰.

The late 1920s and 1930s were a period of decline in Chicago, and this trend would only revert under the leadership of Marshall H. Stone in the postwar period (Parshall and Rowe 1994; Parshall 1988; Lane 1988; Duren Jr. 1988). Princeton, under Veblen, and Harvard, under George D. Birkhoff (1884-1944) — two former students of Moore — continued to thrive and expand their areas of influence in the international mathematical landscape. During these harder years, Chicago was still a champion in two of its longstanding traditions: the calculus of variations of Bolza and the advanced algebra of Maschke and Moore; the latter torch was carried first by Leonard Dickson (1874-1954) and, afterwards, by his student A. Adrian Albert (1905-1972), while the latter was ostensibly carried on by Bliss. Even though the department cruised through the Depression relatively unscathed, Moore’s illness and subsequent retirement in 1932 were a major blow. While a full consideration of the differing philosophies between Moore’s and Bliss’s department far exceed the

29. The series is currently still ongoing, the latest release being in 2016. Bliss’s 1925 monograph was followed by David R. Curtiss’ *Analytic Functions of a Complex Variable* and by Henry L. Rietz’s *Mathematical Statistics*.

30. Dresden, in his review of the book, notes: “We have here a new type of mathematical book. It is not a textbook, neither is it addressed to the specialist, actual or *in spe*. It is intended for that very indefinite group, the intelligent general public. This group surely includes teachers of mathematics, even those who may have taken a course in the calculus of variations! Certainly engineers who expect to do more than routine work, physicists and chemists and other workers in natural science can profit largely from familiarizing themselves with the leading ideas set forth in this book” (Dresden 1925, 554).

scope of this work, it suffices to note that Bliss's tenure was a departure from the previous one of Moore. Bliss's vision of graduate work put a lesser emphasis on research and a higher one on training teachers³¹. Bliss's own research seemed to suffer as his role as chairman further consumed his time, particularly in the workings of the Eckhart Hall that was to serve as the new house to the mathematical department. Despite his solid background in physics, Bliss did not himself work on the pioneering field of quantum mechanics that heavily employed the variational formalism. Perhaps most notable is that the groundbreaking contributions in the calculus of variations in the 1930s did not come from Chicago mathematicians, but from Marston Morse (1892-1977) at Harvard³². Bliss's lectures, however, did not suffer as he was widely regarded as a masterful teacher even though many thought his subject was becoming "too narrowly defined as the study of local, interior minimum points for certain prescribed functionals given by integrals of a special form.". Additionally, generalizations "came only at the cost excessive notational and analytical complications" (Duren Jr. 1988, 179).

An indication of Bliss's general perception of the calculus of variations, as a mathematical discipline, was given in his 1920 survey "Some Recent Developments in the Calculus of Variations". The paper was a speech delivered at the joint meeting of the Chicago and Southwestern Sections, the Missouri Section of the Mathematical Association of America in 1919. He begins noting that:

I was discussing recently the title of this address with a fellow mathematician who remarked that he was not aware that there had been any recent progress in the calculus of variations. This was a very natural suspicion, I think, in view of the fact that the attention of most mathematicians of the present time seems irresistibly attracted to such subjects as integral equations and their generalizations, the theory of definite integration, and the theory of functions of lines. (343)

31. Bliss's views on graduate work is fairly broad. For instance, he does not consider it a failure, as does R. G. D. Richardson, in his national PhD report, that so many mathematics PhDs were not actively involved in research. Graduate training, for Bliss, was fundamentally about guiding the student in finding "the truth" in a specific topic; and this skill set could be employed in "teaching, or business, or whatever activity may claim the student's future interest" (Bliss 1952, 604.). He also argued that the success of mathematical development required an adequate renewal of highly competent teachers and also of a large contingent of well-trained mathematicians that could appreciate the frontier of research (605–606).

32. Morse's theory, called "the calculus of variations in the large", analyzed qualitative properties of all of the extremals of a variational problem. It studied global properties of stationary points and gave more attention to the topological properties of the entire space of curves (functions, surfaces, etc.). In contrast, the classical calculus of variations considered small variations of a functional, around a neighborhood, and was mainly interested in extremals associated with maximal/minimal values (Springer Verlag GmbH, European Mathematical Society)

The survey in itself focused mainly on recent works employing the second variation in specific contexts that Bliss considered to be unexplored. Following the work of Weierstrass and Hilbert, the sufficient conditions for an extremum were typically examined with the Weierstrass E-function and not so much with the second variation as Bliss was suggesting. Bliss's suggested approach would later culminate in his magnum opus *Lectures on the Calculus of Variations*, published in 1947, and would greatly simplify the exposition of sufficiency results from Jacobi, Legendre, and Weierstrass. It is considered to be the culmination of the "classical" calculus of variations begun by Weierstrass and Bolza. A noteworthy remark, from this same survey, that hints to the changing views on mathematics of the time, was his "feeling" that "mathematical research in America is drawing farther and farther away from the forms of mathematics most immediately useful in related subjects [sciences]" (343)³³.

While Bliss and his students kept refining previously established results, two major developments in dynamic optimization appeared in the following years. In the 1950s, American mathematician Richard Bellman devised a sequential approach to complex intertemporal optimization problems. Rather than tackling the problem in its entirety, using the classical calculus of variations, he proposed to write the functional in terms of what he defined as "state variables". These were a finite set of variables that could characterize the particular system at any given point t . Bellman aimed to reduce the optimization process to a series of n steps: at each step one had only to find the optimal path forwards. So the problem of finding the extremal of say $J(z) = \int_0^T F(x, z)dt$ became that of finding the extremal of

$$J(z) = f(c, T) \tag{1.5}$$

where c is a set of variables that describes the functional at each time period. More generally, Bellman proposes to breakdown a maximization problem in several N steps. In each step a certain quantity is divided into two parts c and $c - x$ and one obtains $g(c)$ a $f(x - c)$ from these quantities. Moreover, these processes can have different costs associated to them. In the following step, the original quantities are reduced following some other function and the process repeats. More generally, a discrete maximization problem can be expressed as

33. A similar sentiment is shared by mathematician Edward J. McShane (1904-1989) one of Bliss's PhD advisees. McShane, reflecting upon the history of the calculus of variations and optimal control says that by the 1930s, American mathematicians were answering questions that no one was asking: "We who were working in it were striving to advance the theory of the calculus of variations as an end in itself, without attention to its relation with other fields of activity" (McShane 1989, 921).

$$f_N(c) = \text{Max}_{\{x_i\}} F(x_1, x_2, \dots, x_N) \quad (1.6)$$

for $c \geq 0$ and $N = 1, 2, \dots$ steps. The recursive formulation requires solving the following equations

$$f_N(c) = \text{Max}_{0 \leq x \leq c} [g_N(x) + f_{N-1}(c - x)] \quad (1.7)$$

for $N = 2, 3, \dots$ and where $f_1(c) = g_1(c)$. One can show that a typical problem in the calculus of variations can also be expressed in this fashion³⁴. The problem of finding the extremal of

$$J(y) = \int_0^\infty F(x, y) dt \quad (1.8)$$

subject to $G(x, y) = \frac{dx}{dt}$ and $x(0) = c$. The first step is to write the functional in terms of only the initial value c

$$\text{Max}_y J(y) = f(c) \quad (1.9)$$

and then split the problem arbitrarily at a point S . So

$$f(c) = \int_0^S F(x, y) dt + \int_S^\infty F(x, y) dt \quad (1.10)$$

Denote $c(S)$ the value of c at time S . Then one can write

$$f(c) = \text{Max}_{y \in [0, S]} \left[\int_0^S F(x, y) dt + f(c(S)) \right] \quad (1.11)$$

Bellman reported his findings as a generalization of the calculus of variations³⁵. Bellman's 1954 *Dynamic Programming and a New Formalism in the Calculus of Variations*, more specifically, shows how one can translate a typical problem of finding the extremal of $\int_0^t F(x, z) du$ subject to the constraints $dx/du = G(x, z)$, $x(0) = c$ to a sequential problem in dynamic programming. His 1957 book on dynamic programming has a chapter devoted to exploring these relations. He shows how many problems in the calculus of variations can be solved with his novel formalism. He argues that his approach has two key advantages: it deals much better at accommodating restrictions to the optimization problem; and it also allows for a much simpler

34. For a more complete discussion refer to Bellman (1957, 4-15, 245-251).

35. Since the calculus of variations can be so neatly restated either as an optimal control problem or as a multistage problem in dynamic programming, some authors have attempted to bring these together in a unifying history of optimal control. Sussmann and Willems (1997) is an example in this direction. The authors merge the calculus of variations with optimal control theory by expressing both within the same mathematical formalism and apply both mathematical methods to similar problems.

computational implementation³⁶. As noted above, the solution to a problem in the calculus of variations necessarily passes through the Euler-Lagrange equation, a second or first order partial differential equation. Since there is no general theory to solve these equations, many problems may be unsolvable. Overall, he argues that “the determination of the next move in terms of the current state of the process is in many ways a simpler, more natural and even more important piece of information than the determination of the entire sequence of moves in an optimal policy to be followed from some fixed initial position” (Bellman 1957, 248).

Some authors, working in the calculus of variations, were initially resistant to accept optimal control or dynamic programming as significant advancements. Edward McShane (1904-1989), a former student of Bliss and Lawrence M. Graves (1892-1973) — also a former PhD student of Bliss —, argues that L. M. Graves had previously transformed the problem of Lagrange into the control formulation and even established analogs of the Lagrange multiplier rule and the Weierstrass condition; these results together were equivalent to Pontryagin’s maximum principle, although only for specific cases (McShane 1989, 922). McShane does concede that research in the calculus of variations, for the better part of the 20th century, had become too self-centered as mathematicians “were striving to advance the theory of the calculus of variations as an end in itself, without attention to its relation with other fields of activity” (921). Pontryagin’s greatest merit was to have paid attention to problems in economics and engineering that urgently asked for answers. In solving these questions, they introduced new and important ideas into the calculus of variations.

Before entering the discussion of how the calculus of variations was first applied to problems in economic theory a small addendum is in order. In the briefly sketched history presented above, save for the consideration of Hamilton and the overpassing mention of Euler and Lagrange’s contributions to mechanics, very few has been said about the intimate relations of the calculus of variations with physics. A more complete consideration of these topics would far exceed the scope of the present work. Below I sketch some observations on how the calculus of variations provided a very convenient mathematical framework for mechanics.

This can be seen more clearly in the coordinate-free framework that the law of nature assumes under a variational formalism (Kreyszig 1994, 676). In the preface to *The Variational Principal of Mechanics*, Hungarian mathematician and physicist Cornelius Lanczos (1893-1974) endeavors into a philosophical digression on this matter. Lanczos argues that complex problems in mechanics attest the advantages of the variational approach over the alternative vectorial treatment of motion. He states that the main advantage of the calculus of variations is its “invariant” prop-

36. For a more complete analysis see Bellman (1957, 245-282).

erty in relation to coordinates (frames of reference). This “invariant” property also fits neatly within A. Einstein’s theory of general relativity, in which the laws of nature ought to be independent of the coordinates being considered. In this sense, Lanczos argues that the variational principles of mechanics are not merely an alternative mathematical formulation of the fundamental laws of Newton: the calculus of variations automatically satisfied central assumptions of the theory of general relativity³⁷.

Metaphysical underpinnings follow the calculus of variations since the theological implications put forward by Maupertis following his statement of the principle of least action. The variational principle, indeed, allows for a unification of disparate phenomena under the same mathematical structure. The principle of least action developed an aura of mystique for it suggests that the entire trajectory is “calculated” at the initial state, that is,

[...] it has been claimed that the motion of the system during the whole of the time interval is predetermined at the beginning, and thus teleological reflections have intruded into the subject matter. To illustrate this attitude: if a particle moves from one point to another, it must, so to speak, ‘consider’ all the possible paths between the two points and ‘select’ that which satisfies the action condition (Yourgrau and Mandelstam quoted in Ferguson 2004, 23).

While the principle of least action has gathered numerous theological and metaphysical analogies these were mostly ignored when framing problems in economic theory. Likewise, there was scant interest in developing the conservation implications implicit in the Hamiltonian. The canonical equations, derived by Hamilton and Jacobi, imply either the conservation of energy or of momentum. Economists seemed to not worry about the “disanalogies” carried over from the use of conservative systems — while in physics the conservation of energy has a clear meaning, the conservation of capital or utility does not (Wulwick 1995).

37. For more details, see Lanczos (1970, i-xxv).

Chapter 2

The Calculus of Variations in Economic Theory

2.1 Griffith C. Evans

Griffith C. Evans was a Harvard trained mathematician. His PhD, obtained in 1910 under Maxime Bôcher, was on Volterra's Integral Equations¹. Bôcher, as mentioned previously, had also traveled abroad to Göttingen to pursue his doctoral studies. During this time, he studied chief branches of mathematical physics under Felix Klein and was stimulated by him to focus his study on potential theory². Bôcher had a thorough background in mathematical physics, and his studies at Göttingen helped solidify his interests in potential theory, differential equations, pure geometry, and algebra. He is hired, in the fall of 1891, at Harvard, being appointed to an instructorship in the mathematics department. He was notorious for the clarity in his mathematical work³; a significant part of his career, indeed, was devoted to polishing previously stated results and theorems. While Evans's academic career was permeated by the many intricacies of potential theory, his earlier work followed his PhD thesis in the field of integral equations. This research led him to pursue a post-doctorate at the University of Rome, under Vito Volterra (1860-1940), a leading mathematician in integral and integro-differential equations, whose scientific work spanned fields in biology, population dynamics, physics, and pure mathematics. Evans' acquaintance and later friendship with Volterra had a lasting impact on himself. Weintraub (1998) argues that Volterra was Evans's "intellectual model" (229) and that the "interests he developed in Volterra's Rome were to be the defining intellectual themes of his mathematical life" (228).

Evans began his teaching career in 1912 as an assistant professor at the newly founded Rice Institute (currently Rice University) in Houston, Texas. He garnered some notable institutional recognition for his work during this time, being elected vice president of the AMS in 1924, and vice president of the American Association for the Advancement of Science (AAAS) in 1931. In 1934 he was also elected to the National Academy of Sciences. During his time as a professor, he managed to attract — as visiting professors — significant mathematicians such as Polish-French Szolem Mandelbrojt (1889-1983), from the University of Paris, and Austrian

1. The study of integral equations began in 1792 with Pierre-Simon Laplace (1749–1827). In 1884 Vito Volterra demonstrated that a specific class of integral equations could be solved using variational methods. In the early 20th century, Hilbert and some of his students at Göttingen used integral equations to demonstrate a more rigorous version of the Euler-Lagrange equation. For more on the history of integral equations, see Bateman (1911).

2. Potential theory is a branch of mathematical physics that sprouted almost in parallel with the calculus of variations and that has many applications in electrostatics, gravitational theory, and magnetism. Some authors such as J. L. Lagrange and A. Legendre, for instance, wrote extensively on both topics. Its modern developments, in the 20th century, were due to linkages with topology, functional analysis, and stochastic processes such as Markov chains.

3. Julian Coolidge (1873 - 1954), head of the mathematics department at Harvard said of Bôcher that he "never tried to be clear, because his constitution was such that he did not know how to express his thoughts in any but the clearest form." (quoted in Osgood 1919, 343).

Karl Menger (1902-1985), son of the marginalist economist Carl Menger (1840-1921), from the University of Vienna. During his stay in America in 1930-1931, Menger also visited Chicago, where he met - among others - E. H. Moore and Veblen. He also spent some time at Harvard where he became acquainted with M. Morse and Joseph Schumpeter (1883-1950), his fellow compatriot. In his reminiscences, Menger notes that despite the prejudice towards mathematics by Austrian marginalists, Schumpeter displayed an increasing interest in mathematical analysis and was keen to understand the young economists in the 1920s that applied more sophisticated mathematics (Menger 1994, 168-170). Schumpeter invited Menger to the charter meeting of the Econometric Society to be held in Cleveland in connection with the Christmas meeting of the American Association of the Advancement of Science (AAAS). Charles Roos, one of Evans pupils, at the time was serving as secretary of Section K (social and economic sciences) of the AAAS and had previously been in conversations with Norwegian economist Ragnar Frisch (1895-1973) and Irving Fisher to bring together a society for the advancement of research in economic theory in its relation to statistics and mathematics⁴. At that same AAAS meeting, the Econometric Society was founded, and Frisch was elected its president. Among the first fellows were Evans, Hotelling, Menger, H. Schultz, E. B. Wilson, and the other authors mentioned above. In the following years, the Society's journal *Econometrica* would host a variety of articles on mathematical economics. Evans himself contributed with a paper in 1934 on a theoretical model of a full production economy.

In 1934 Evans accepted a generous offer from the University of California at Berkeley not only to teach but also to build their mathematical department. Historical contingency was in his favor as at that time both the economic recovery and the mass immigration of European scholars fomented "an opportune time, when there were more fine mathematicians available than there were places for them to go" (Rider 1988, 295). Evans was very successful in bringing Berkeley's mathematical department to the highest tier. Some notable hirings during his tenure include German mathematician Hans Lewy (1904-1988), Polish mathematical statistician Jerzy Neyman (1894-1981), and Polish logicist Alfred Tarski (1901-1983). In 1939 Evans was elected president of the AMS. He chaired Berkeley's department until 1949 and retired in 1955 - a homage to his legacy stands in Evans Hall, the building that currently houses the departments of statistics, mathematics, and economics at

4. The idea of the Society was first visualized by Frisch in 1926. In the following years, during his travels to America, he became acquainted with Irving Fisher at Yale, Charles Roos at both Princeton and Cornell, and J. Schumpeter at Harvard who helped rally others to join the project. In 1930 Fisher sent out letters of invitation to potential members for the foundational meeting of the Econometric Society that was to be held in December of that same year. For more on the foundation of the Econometric Society, see Louça 2007, 25-48.

Berkeley.

Evans's works in mathematical economics date mostly from the 1920s to the early 1930s up until his departure to Berkeley. Following the poor reception of his 1930 book *Mathematical Introduction to Economics*, Evans became less active in economic theory. He still exerted an influence on economics, however, through the works of his PhD students at Berkeley. His advisees follow some lines of his work, most notably his previously mentioned 1934 *Maximum Production Studied in a Simplified Economic System*. Evans also continued to attend some meetings organized by the Econometric Society and was also invited by Roos - who recently had been hired as director of research at the Cowles Commission - to give lectures at some of the Cowles' seminars in 1937. Ronald Shephard (1912-1982) is perhaps Evans' most notable student during his Berkeley years. Shephard wrote his PhD on taxation and production theory and further pursued this work which culminated in his 1953 *Cost and Production Functions* where he derived the now famous *Shephard lemma*. In the book's preface, Shephard stated that Evans had given in terms of a simple total cost function a treatment that is both general and analytically convenient⁵. His work intended to extend this analysis to encompass production functions and explore the relations between cost and production functions in different competitive contexts. The last chapter of his book is a restatement of Evans's monopolist problem in terms of a cost function which better incorporates expectations, by allowing the firm owner to anticipate cost changes associated with prospective variations in the prices of factors of production.

To briefly mention other students, I remark first Francis Dresch (1913-2011) who worked his PhD in 1937 on index numbers and aggregation. Evans drew on this work and presented a paper on this topic, *Indices and the simplified system*, at the Third Annual Research Conference organized by the Cowles Commission. Dresch then expanded this research and presented his developments at two Cowles conferences, in 1939 and 1940, namely *A Simplified Economic System with Dynamic Elements* and *The Study of Business Fluctuations by Means of Economic Models*. Evans also attended the Santa Monica meeting of the Econometric Society in 1951 where he presented *Note on the Velocity of Circulation of Money* and his PhD Student Edward A. Davis presented *A Dynamic Model for an Economy*, a three sector model of a dynamic economy using similar aggregating ideas of indices that had been borrowed from François Divisia (1889-1964) by Evans and Dresch. Kenneth O. May (1915-1977) also worked on production theory and aggregation; he was also critical of utility, but not for the same reasons as Evans - his arguments follow the more axiomatic flavor of the 1950s.

Before analyzing Evans's works in greater depth, I make a quick detour to sum-

5. Specifically he mentions the quadratic equation presented in Evans (1922, 371).

marize the importance of Vito Volterra as an intellectual mentor for Evans. Volterra had a profound impact on Evans's methodological views and his thoughts on mathematical models and their relations to scientific theories (Weintraub 2002, 42-43). Volterra and Evans remained close and exchanged correspondence throughout the years in which they discussed mathematical physics, functional equations, and, occasionally, mathematical economics⁶. It should be noted that Volterra's interest in mathematical economics was quite brief. In this sense, Pomini (2018) argues that Volterra's influence on Evans's work in economics is mainly methodological (62-63).

Volterra was a notorious mathematician and a central figure in the uprising of Italian mathematics. He spread his influence not only to other scientific disciplines but also to Italy's political and cultural landscapes. He was convinced in the virtues of scientific and technological progress for the development of civilization though, ironically, his own scientific credences also halted progress in many ways Israel 2008, 75-76⁷. As a proponent of science, he played a pivotal institutional role, founding, among others, the Italian Society for the Progress of the Sciences in 1908 and Italy's National Research Council in 1919. Volterra displayed his most significant work in applied fields, notably those of mathematical physics and biomathematics; his work in pure mathematics, particularly in functional analysis and integration, was also very significant, but his interest in solving particular problems strayed him away from the level of abstraction and generality that was becoming the standard for mathematics⁸. His work on population dynamics, summarized in the Lotka–Volterra equations contrast growing differences in the scientific world of the time: while his aim was to ground biology on classical mechanics, searching for “mechanical analogies” to explain physical phenomena, Lotka achieved similar results using a more

6. Pomini (2018) finds only two letters on mathematical economics in their correspondence — both written by Evans. The first is from 1920 in which Evans announces that he was working in mathematical economics and preparing a presentation to be delivered in the AMS conference of that year. This work would later become Evans's 1922 article. In the second letter, Evans mentions that he is beginning a project to write a mathematical textbook similar to Amoroso's 1921 book *Lezione di Economia Matematica*.

7. Israel (2008) mentions as examples of this Volterra's distrust in probability calculus that handicapped his own research in biomathematics; his anti-German sentiment that led him to boycott an Italo-German applied mathematics congress; he was also against the creation of theoretical physics chair for Italian physicist Enrico Fermi (1901-1954).

8. Indeed, some historians contest Volterra's role as the founder of modern functional analysis as his “concept of ‘function of lines’ is restrictive and insufficient as far as developing a general foundation of the theory is concerned.” Volterra himself stated that a general, purely mathematical theory of functionals, never was his interest. This reveals how Volterra's view on applications and mathematics “placed him outside the ongoing early axiomatic developments.” (Israel 2008, 76). Volterra's work in pure mathematics, in fact, date from his earlier days when he was still a student at the Scuola Normale, in Pisa. These works were published in the *Giornale di Matematiche* in 1880 and 1881. In 1882, however, he became closer to physical-mathematician Enrico Betti (1823-1892) with whom he worked his thesis on hydrodynamics that same year. Following this, Volterra was hired at Pisa for the chair of rational mechanics and his interests remained in applied mathematics throughout his lifetime. For more on Volterra's academic formation, see Paoloni (2013, 5-30).

open-minded framework — better attuned to the recent innovations in the physical science. Although he played a key role in uplifting Italian mathematics to international standards, his views on culture, science, and mathematics were backward looking. By the end of his life, the axiomatic approach to mathematics would have overrun the more physics inspired math, that he held as an ideal (Israel 1988).

Volterra's mathematical convictions, closer to the physically grounded mathematics of the 19th century were put in check during the debates around the Foundational Crisis in the 20th century. Mathematical rigor for Volterra was firmly associated with the conceptual connections established with physical, observable reality. That is, a rigorous mathematical model should be grounded in quantifiable concepts that could be observed. Volterra sought to ground science, and, by extension, mathematics, on physical reality: a scientific theory should always make reference to empirical and observable phenomena, and the accompanying mathematical formalization should accurately represent this underlying reality. Mathematics, in this sense, should not develop independently, seeking generalizations for the sake of greater abstraction. To be sure, Volterra does not exclude the usefulness of research that aims to better clarify or extend a specific topic. This is seen more clearly in his pioneering development of functional analysis. He argues that his new theory can serve to extend previous results on functions of complex variables presented by Bernhard Riemann (1826-1866) and that it also arises from a recent work by Henri Poincaré (1854-1912). More importantly, however, Volterra finds that these functionals appear extensively in physics. Thus, this “external” motivation (the applications to quantifiable and observable phenomena in physics) justifies the study of functionals - the “internal” motivations are also important, although not essential.

This view of mathematics, also shared by other notable mathematicians such as Henri Poincaré, was questioned in the early 20th century. Set-theoretical developments and paradoxes brought forward by German mathematician Georg Cantor (1845-1918), in the late 19th century, forced mathematicians to rethink the foundations of mathematics. Various mathematicians and philosophers tried to propose a solid foundation for mathematics in response to Cantor's findings. Hilbert devised the most influential of these attempts, which he announced at the International Congress of Mathematics in 1900. His project was to establish the axioms needed to derive the entire body of mathematics. Mathematical rigor was recast as a matter of logical internal consistency: as long as the mathematical statements did not lead to a contradiction, they were considered valid rigorous affirmations. Although Hilbert's ambitions were proven to be unattainable, his axiomatic vision of mathematics was still very influential in the 20th century.

In economics, the aftershocks of the Foundational Crisis was evident in the axiomatic mathematics that became dominant in the postwar period. Physical reality

no longer constrained mathematical methods. Evans, however, held that observable concepts should serve as the basis for economic theory. In this sense, Weintraub (1998) argues that postwar mathematical economics, “increasingly connected to the new (very un-Volterra-like) ideas of mathematical rigor in both mathematics and applied mathematical science, moved away from Evans” (256).

For Volterra, a scientific theory (aided by a mathematical model) should ultimately be verified against empirical data. If the model is not in accord with the statistical evidence then it should be reconsidered. To develop a mathematical model without regards to physical reality was to construct a nonrigorous theory⁹. Since the mathematical events that lead to the turn of the century Foundational Crisis, a formalist or axiomatic posture was adopted by many of the leading mathematicians of the time. Contrary, to Volterra’s empirically grounded mathematics, the formalist position was to free mathematics from any physical constraint: theories should be developed so long as they remain logically consistent with the previous body of mathematical knowledge. Mathematical rigor would be related to a proposition’s internal consistency within a system of axioms; a proposition would no longer be “true” because it was grounded in a “real phenomena” (Weintraub 1998, 237). This shift in mathematics, at a time when economics itself was becoming increasingly more mathematical, heavily influenced the “style” of mathematics that came to be prevalent in economic theory¹⁰.

Another central aspect of Volterra’s work is his emphasis on finding mechanical analogies: new scientific fields should try to emulate mechanics in their methods and seek to explain fundamental phenomena in this fashion¹¹. It becomes clear as to Volterra’s initial enthusiasm with marginalist theories of economics: the mathematical formalization of marginal utilities and costs that equilibrated the market owed much to mechanics¹².

9. On this specific point, Weintraub (1998) states that: “For Volterra, to be rigorous in modeling a phenomenon was to base the modeling directly and unambiguously on the experimental substrate of concrete results ... To provide a nonrigorous explanation or model in biology, economics, physics, or chemistry was to provide a model unconstrained by experimental data or by interpersonally confirmable observations” (235).

10. Morgan and Rutherford (1998) argue that: “By the mid-twentieth century, economists were more likely to be enthralled by a turn in mathematics itself that viewed mathematics as a way of writing down consistent theories. Mathematics became the language for the expression of abstract and general theory rather than a tool for uncovering and writing down true descriptions of the economic world. Thus at around the time the main body of American economists began to prefer the language of mathematics on defensive ground, the scientific role of mathematics was itself coming free of its connections to the scientific (economic) world” (19).

11. Volterra had a strong background in physics and especially mechanics. He graduated in physics, in 1882, at the Scuola Normale in Pisa. The following year, he became the chair of rational mechanics at Pisa and remained there for the next 10 years. In 1892 Volterra became a professor of mechanics at the University of Turin, and eight years later he accepted the chair of mathematical physics at the University of Rome.

12. Volterra maintains amicable relations with Irving Fisher who was closely aligned to his mechanical ideals. Fisher, indeed, sought to make explicit mechanical analogies in his work: proposing

Evans work in mathematical economics, as I shall present in greater detail, reflects many of these methodological considerations. While he was not as adamant as Volterra in seeking for mechanical analogies to economics, he does insist that economic theory should be built upon quantifiable and observable concepts. Evans will show a blatant animosity with the long-held subjective theory of value, more specifically, to the widespread use of utility in economics. He argued that more “concrete” concepts such as profits, prices, and production rates would provide a firmer basis for economic analysis¹³. Evans does, however, partake in the modern understanding that mathematics should be unconstrained: one had complete freedom in choosing assumptions; the mathematical method allowed for logical deductions that followed from any set of hypothesis. Evans’s criterium is that the choice of assumptions be guided by the goal of better describing reality (Evans 1930, 110-115).

to find the analogues of energy, momentum, etc. in economic theory. Scott (2018), in particular, explores their relationship through letters and notes that Fisher developed a coherent and systemic mechanical analogy in his economic theory (Scott 2018, 529-533).

13. On this he comments: “Would it not be better than to abandon the use of the utility function, and investigate situations more directly in terms of concrete concepts, like profit and money value of production, in order to take advantage of the fact that money is fundamental in most modern economics and to use the numbers which it assigns to objects? Concrete concepts suggest concrete hypotheses.” (Evans 1932, 324).

2.2 Evans's Dynamic Economics

Evans's first publication in mathematical economics is a reformulation of Cournot's monopoly problem¹⁴. It appeared in 1922 in the *American Mathematical Monthly* under the title *A Simple Theory of Competition*. Evans considered the problem of finding the price that maximizes the monopolist's profits given a static demand curve and then showed how to extend this to a more general case of an arbitrary number of competing firms. His following article, *The Dynamics of Monopoly*, published in 1924 in the same journal, used the calculus of variations to bring his previous analysis of monopoly to an intertemporal framework. Evans's major innovation here was to allow for demand to vary with changes in prices: now the monopolist must choose his prices optimally in various instants of time. This essentially brought Cournot to a dynamic scheme in which the solution is no longer an optimal price but an optimal *price curve*.

When read side by side, Evans's first two articles can be seen as somewhat of a *manifesto*. In the concluding section of his 1922 article, Evans explored some of the limitations of a static approach to the problem of competition. Specifically, he showed how a sudden change in the firm's cost function could lead to them going bankrupt since - hypothetically - the firms could not take into account the dynamic nature of prices. This kind of anomaly did not arise if one formalized the monopoly problem using the calculus of variations. Using this dynamic approach, one could allow for all price factors to change through time and find the solution that makes profits a maximum in an interval of time. In this sense, Evans's first article set the stage for his second one by highlighting some of the potential pitfalls of economic theories that fail to account for dynamics.

Evans (1922) considered what one currently would define as the "firm's problem" of profit maximization under different assumptions of competition: monopoly, duopoly, and the case of an arbitrary number of equal competing firms. The article displayed not only Evans's methodological approach to economics but also his mathematical style: contrary to other mathematical economists of his time, Evans explicitly defined his functions in order to give the problem more concreteness and to arrive at analytical solutions. That is, instead of working, say, with a continuous, convex and increasing cost function $C(x)$ that mapped factors of production x into the firm's cost, Evans (1922) writes simply a quadratic function $C(x) = Ax^2 + Bx + C$, where A , B , and C are all positive arbitrary constants. This function is then minimized given the linear demand expression $y(p) = ap + b$ where $a < 0$ and $b > 0$ that

14. Evans held Cournot in high regards as can be seen in his 1929 critical appraisal of the republication of Cournot's *Researches into the Mathematical Principles of the Theory of Wealth* (originally published in 1838). Evans says that he regards Cournot's book as "the best elementary treatise on the theory of economics".

represents the “obviously typical case” (374) in which demand correlates negatively with prices. Evans closely follows four postulates, two of which he borrows directly from Cournot.

The first of these, as mentioned, is a review of the works of Cournot in competition.

- (a) Each competitor assumes that the production of the other or others is independent of his, and tries to make his profit a maximum.
- (b) Each competitor regards the price as fixed and tries to make his profit a maximum.
- (c) Each producer tries to determine the amount x_i of his production per unit time so as to make the total profit a maximum.
- (m) The producer determines his production per unit time so as to make profit a maximum. (monopoly)

He succinctly derives the implications of each of these assumptions and explores some properties of these results such as that the price under postulate (m) – monopoly – is always higher than under the three alternative hypothesis. In the following sections of the paper, he entertains several hypothetical scenarios and, following suit, considers them under each of the pertinent postulates listed above. He discusses, for instance, conditions of failure, that is, under which circumstances will a firm cease production, and also considers if it is possible for a tax to be imposed on the monopolist as to keep the level of production unchanged. His method is very clear: both cost and demand functions are stated in an explicit fashion, and the underlying hypotheses are likewise clearly ordered; the mathematical model is then explored under each combination of hypothesis and the results are understood and compared given that they derive from these chosen hypotheses.

Evans is cautious when talking about generalizing these findings. He states that conclusions should always be interpreted in relation to the assumptions that generated them. He argues that economics can gain much by formulating propositions in a way as “make evident the limitations of the theory itself, and thus indirectly suggest the devising of other [mathematical] formulations” (Evans 1930, 111)¹⁵. As Evans argues in his 1932 methodological paper *The Role of Hypothesis in Economic Theory* the main workhorse of economics should be that of making different hypothesis, finding their implications (mathematically), and finally testing these consequences against the existing facts of real-world economic systems. In practice, Evans seems

15. Evans is not against the search for general theories in economics. His emphasis is that bringing out clearly the assumptions on which theories rest would allow one to understand to what extent these theories are specialized (Evans 1930, 115).

to have implemented this method as that of proposing different functional specifications for his cost and demand functions (reflecting different sets of assumptions). These, in turn, imply different objective (profit) functions. The implications are then compared to real-world economic “facts”¹⁶. This methodological approach permeates all of Evans’s works on mathematical economics and even those by his doctoral advisee Charles F. Roos. Roos, indeed, in a later article on the determination of demand curves, summarized the procedure of evaluating if a chosen functional form is adequate. Note how the choice of the specific functional form is arbitrary but it must eventually be “tested” against statistical evidence.

In order to determine an appropriate form for the dynamic demand function, the economist ought to find it worthwhile to compare the resulting mathematical solutions of price and rate of production of the foregoing problem for various arbitrarily selected functions of demand with the statistical solutions of the problem offered by business. The general type of demand equation which yields a price and rate of production solution agreeing with statistical data should be considered as a possible form of the dynamic demand equation. (Roos 1927, 640)

In Evans’s 1922 paper, the working hypotheses are mostly borrowed from Cournot and define both the number of firms and their objectives, while the expressions for cost and demand change accordingly. He considers how his theory of competition fares under hypothetical scenarios, such as a wartime price fixing, and what taxes it suggests to contour the inefficiencies of monopolies. More interestingly he investigates how his mathematical model deals with a situation where a group of producers wish to expand its production without properly taking into account their own influence on market prices. This leads to an unusual situation where bankruptcy becomes a normal event in this system of competition. This anomaly suggests a revision of the mathematical method employed and is tackled in the closing section of his paper.

This final case which Evans considers is that in which the cost function changes. He assumes that a given number of producers under the assumption (b) wish to “make a larger profit by increasing their overhead expenses, decreasing their other expenses, and producing larger amounts.” (379). This implies that the *entire* cost function is changed, as it is not solely a change in either the intercept or the slope of

16. Evans is not specific as to how one should compare theoretical results to real-world facts. In some passages (Evans (1922), Evans (1932)) he says that statistical methods could be used to estimate the theoretical relations postulated although he does not carry out these procedures. His doctoral advisee, Charles F. Roos, however, does produce statistical analysis based on mathematical formulations akin to those used by Evans. This is especially developed in Roos (1934). See also Dimand and Veloce (2007, 521–530).

the curve¹⁷. Given the new cost function, the producer then chooses optimally the output that maximizes profits. Since supply is a function of price this expansion in production implies a reduction in price¹⁸. There is a possibility that price decreases in such a way that production is no longer lucrative under the new cost curve. If such a change is irreversible (i.e. the firm cannot costlessly descale its production) then it is now out of business. This example is not gratuitous: by considering a dynamic process in which both price and the functional form of the cost function change through time, Evans' disguisedly showed the limitations of Cornout's statical framework. The profit maximization choice under Cornout's postulate (b) not only can fail (in the sense that if a firm were to choose its level of production following this model it would go bankrupt) but it would also not describe the changes from one state to the other. Evans suggests a modification that changes the problem fundamentally: to consider that the firm makes his profit a maximum over a period of time, with reference to cost functions that change themselves through time. He then makes the matter clearer as to what such a change implies for the mathematical model one has to consider: "[t]he mathematical discipline which enables us to find functionals which make a maximum or a minimum quantities which depend upon them throughout periods of time is the calculus of functionals, or in special cases the calculus of variations" (Evans 1922, 379).

The follow-up article, *The Dynamics of Monopoly*, begins where the previous paper ended. The familiar quadratic cost function is there, but the demand curve is now a function of the change in price $\frac{dp}{dt}$, and profit has now to be made a maximum in a given time interval $[t_1, t_2]$ ¹⁹. Formally, the problem is to maximize

$$\Pi = \int_{t_1}^{t_2} \pi(p, p', t) dt \quad (2.1)$$

where Π stands for profits, p for the price of the commodity, p' the change in the price, and t denotes time. Evans also sets the boundary conditions $p(t_1) = p_1$ e $p(t_2) = p_2$. As mentioned above the cost function remains the same: $C(x) = Ax^2 + Bx + C$. Demand is now given by the first order differential equation:

$$y(p) = ap + b + h \frac{dp}{dt} \quad (2.2)$$

where $\frac{dp}{dt}$ denotes the derivative of prices with respect to time, $a < 0$, $b > 0$, and

17. Evans carries this out only graphically, but the change would be of increasing C (overhead cost) and decreasing B and/or A such that the new cost function is associated with lower costs for higher levels of production.

18. Recall that although under the assumption (b) producers take the price as fixed (in their profit maximization) the market price is still a function of supply.

19. I make small modifications in the article's original notation. For instance, Evans defines the relevant time interval to be $[0, t_2]$.

the sign of h is undefined a priori. The negative sign of a implies that demand for a commodity is negatively related to its given price as is usually assumed (Evans refers to this as the “law of demand”). Evans’s major innovation is that he now considers how changes in prices can affect the demand for a good (the $h\frac{dp}{dt}$ term in the equation above). Suppose, for instance, that h is found to be positive. Then increases in the price of commodity raises its demand in the very short term. His intention is not only to overcome some shortcomings of statical optimization but to also incorporate some “anomalous” cases where he argues that the “law of demand” is known to not hold²⁰. Profit is defined to be receipts py minus costs $C(x)$. Finally, Evans assumes that demand equals supply, that is, $x = y(p)$. Using this fact one can rewrite (2.1) (to keep the notation simpler I omit the (t) and denote derivatives using primes):

$$\Pi = \int_{t_1}^{t_2} \{p(ap + b + hp') - A(ap + b + hp')^2 - B(ap + b + hp') - C\} dt \quad (2.3)$$

Finding a solution for the above equation is a fairly straightforward problem in the calculus of variations²¹. Using the Euler-Lagrange equation one finds the following second order linear differential equation:

$$p'' + \frac{a(a - Aa)}{Ah^2}p + \frac{b - 2Aab - Ba}{2Ah^2} = 0 \quad (2.4)$$

The solution to this equation is given by Evans as:

$$p(t) = p_0 + C_1e^{mt} + C_2e^{-mt} \quad (2.5)$$

where C_1 and C_2 are constants – to be defined by boundary conditions –, p_0 is a constant, and m is the positive characteristic root found from the solution of the homogeneous equation²². The following steps would be to verify second order conditions to check if the solution found is indeed a maximum. This would be, of course, the most straightforward way to tackle this problem for someone already familiarized in the calculus of variations. Evans, however, opts for an arguably more didactic presentation in his article. He begins by assuming that the problem has

20. He so describes this law of demand: “An exaggerated instance of this law of demand, perhaps an undue generalization from the last crisis, is what the wholesale lumber dealers tell us is characteristic of their sales, namely, that when prices are going up the demand is insatiable, but when prices go down it is nil until the price movement stops” (Evans 1924, 72).

21. A very similar problem is solved as an example in Chiang (1999, 49-52)

22. Recall that the homogeneous equation of a second order linear differential equation (with constant coefficients) is of the form: $Ay'' + By' + Cy = 0$. The solution is found by guessing that $y = e^{rt}$ so that $y' = re^{rt}$ and $y'' = r^2e^{rt}$. So one can write $e^{rt}(Ar^2 + Br + C) = 0$. Since e^{rt} can never be zero this means the expression inside the parenthesis must equal zero. This merely implies solving the second order polynomial in r which can be achieved using the quadratic formula.

a solution, say $p^*(t)$, and then causes a small “perturbation” on $p^*(t)$ through an auxiliary function. By then rewriting the problem (equation 2.3) in terms of this auxiliary function and using integration by parts one arrives at the Euler-Lagrange equation (2.4). This approach resembles the one used to derive the necessary conditions for a maximum/minimum value in ordinary differential calculus and is also very common in mathematical textbooks on the calculus of variations²³. The article discusses possible extensions such as when the derivative of the demand functions have a finite number of discontinuities, and also speculates on further extensions as to consider how the solutions would change if both constants a and h were to be functions of p, p' , that is, if the demand curves sensibility to price also fluctuated according to the price level and the changes in prices.

Evans has thus shown how a familiar problem in economics, that of competition and monopoly, can be further explored, in a dynamical fashion, with the calculus of variations. In his 1922 article, he states Cournot’s theory and explores both its implications and limitations given a set of hypothesis. To overcome these, and to also offer a more realistic account of how business operated, Evans argued that one had to turn to the calculus of variations. His choice of assumptions, that the monopolist wishes to make his profits a maximum over a period of time, thus lead to a particular mathematical formalization; in a sense, the theory Evans was proposing suggested the mathematics necessary to develop it. In another paper, published shortly after, in 1925, he states further that the linkage between economic theory and the calculus of variations “is not accidental, nor the result of a generalization from previously found differential equations, since it is in the nature of an economic system that there should be a striving for a maximum of some sort” (Evans 1925a, 95). As he argued, such a choice then opens new avenues for research that he deems to be very promising. At this stage, Evans’s research in mathematical economics becomes intertwined with that of Charles F. Roos. Roos, indeed, follows closely not only Evans’s research topics but also his methodology and presentation. His first article is an extension of Evans’ 1924 paper, where he generalizes the mathematical problem of the monopolist first to the case of duopoly, and then to that of an arbitrary number of producers. The expressions for the cost curve and for the market demand are borrowed from Evans (1924) and the assumptions are clearly stated first verbally and then mathematically. In the closing section of his article, Roos proposes a change in the functional form of the demand function, so as to make the current price of a commodity depend on its entire past history of prices. That is, the integral in (2.1) is now defined from $-\infty$ to t_2 . This assumption requires

23. Both modern (see, for instance, Chiang (1999), Kamien and Schwartz (1981), Liberzon (2012)) and classical (see, for instance, Bolza (1904) Bliss (1925), Forsyth (1927)) textbooks use this presentation.

him to add a weighing function $\Phi(t, \tau)$, that returns very small values when the distance between t (present) and τ (past) is large which he relates to the typical mathematical restriction to treat hysteresis. One can interpret this as a prototypical discount factor only stated in an implicit rather than explicit fashion²⁴.

$$y(t) = ap(t) + b + \int_{-\infty}^{t_2} \Phi(t-\tau)p(\tau)d\tau \quad (2.6)$$

Not only do important problems in economics lend themselves to mathematical treatment, but the results to be found from such a mathematical theory of economics are precise in a way in which verbal theories cannot be. The assumptions and hypothesis postulated can also be verified to withhold in actual, real-world, societies and can even be calculated in a definite fashion. This is not merely the “typical” defense of mathematical economics, arguing that mathematics can bring a special kind of precision or rigor that eludes theories formulated in a purely verbal medium. Notice how the assumptions are to be verified; this turns out to be an important aspect of Evans’s mathematical modeling. This reveals his shared mathematical background with Volterra. Evans, however, is not a 19th century mathematician. A scientist has free reign over the hypothesis he chooses; in fact, this freedom in choosing the most convenient hypothesis and definitions is what characterizes a theoretical science. Mathematics is to be successfully employed in domains in which different hypothesis are linked by deductive chains of reasoning; in this sense, clarity is paramount, as one is interested in verifying the logical implications of a certain set of assumptions, and also of comparing these with those to be derived from a different set of assumptions. As does Evans in his 1922 article, each of the postulates takes on a distinct mathematical formulation, leading to a set of equations that arrives at conclusions that are particular to those postulates.

24. In later works, such as Roos (1930), Roos uses the familiar exponential function to give a higher weight to more contemporaneous prices. Evans had avoided this complication by always framing his objective function in a finite interval of time and maximizing the undiscounted stream of profits. Setting either the upper or the lower bound of the integral in the objective function to infinity, however, usually required some discount function to guarantee that the integral will assume a finite value. This will become a hotly contested point amongst the authors that employ this mathematical formalism (see Duarte (2016)).

2.3 Evan's 1930 Mathematical Introduction to Economics

Evans's most lasting contribution to economic theory was his 1930 *Mathematical Introduction to Economics*. Published by Mc Graw Hill, the book was intended for advanced undergraduate and graduate students in economics. The book carried Evans's characteristic style and condensed much of his previous works, in a systematized and didactic manner²⁵. The first nine chapters dealt with topics such as monopoly, cost, indexes, measurement units, and taxation, in mostly a static optimization framework. In chapter IV, Evans considered a dynamic formulation for the supply and demand functions using differential equations. He argued that allowing for prices to change through time provided a more realistic representation of economics. Specifically, his equations were the following:

$$y = ap + b + h \frac{dp}{dt} \quad (2.7)$$

$$u = \alpha p + \beta + \gamma \frac{dp}{dt} \quad (2.8)$$

$$y = u \quad (2.9)$$

where $a < 0$, $b > 0$ and $h > 0$ were constants reflecting the behavior of the demand curve; $\alpha > 0$, $\beta > 0$, and $\gamma < 0$ are likewise constants that govern the supply curve. Equilibrium was found by imposing $u = y$ and solving for prices. The solution was of the form:

$$p(t) = p_1 + C e^{-\frac{\alpha-a}{\gamma-h}t} \quad (2.10)$$

where C is an arbitrary constant that can be solved using boundary conditions. Evans analyzed the above equation using a phase diagram and explored the effects of different assumptions for the magnitudes of the constants. Since a differential equation governed the behavior of prices, there were multiple solutions to consider given the different values of the supply and demand parameters a , α , b , and β . If $(\alpha - a)/(\gamma - h) > 0$, for example, then regardless of initial conditions all solutions should converge to p_1 as $t \rightarrow \infty$. Evans also gave some attention to the question of the stability of the system. This kind of analysis, based on the limiting properties of a system of linear differential equations, was similar to what Samuelson would present many years later in his 1947 *Foundations* — though Samuelson most commonly used discrete rather than continuous time.

A full treatment of Evans economic theory, using the calculus of variations, was

25. The book reflects his lectures on mathematical economics not only at Rice University but also from courses at Chicago (summer quarter of 1925), and the University of California (summer sessions in 1921 and 1928)

presented in the book's two final chapters and appendix. He first introduced his mathematical model of a monopolist firm (following the formalization of his 1924 article) and then discussed various applications of the calculus of variations to economics. He showed how one could model production lags or deal with discontinuities in production schemes, and also how one could express a demand function that related current prices to their past values²⁶. The mathematical appendix considered a wider analysis of supply and demand equations in a dynamic scheme similar to Roos (1927). It also suggested more general applications of the calculus of variations, mentioning works on depreciation and replacement of machines - citing Hotelling (1925), Roos (1927). Evans's bibliographical references throughout the text suggest his familiarity with the contemporary literature in mathematical economics²⁷.

Mathematical Introduction to Economics also had three chapters that are worthwhile mentioning from a methodological point of view. Chapters 10 through 12 presented both (a) Evans's views on the mathematical method and how it should be applied to economics; and (b) Evans's critical remarks to the subjective theory of value and utility functions. Evans stated, broadly, that he wanted to emancipate economic theory from its murky definitions, that is, in a crude sense, to investigate those theoretical relations that are "independent of sentiment" (Evans 1930, 110). He defended that mathematics could help economics by providing both clarity of definitions and of hypotheses. As previously mentioned, the mathematical method, in his view, allowed for freedom of hypotheses: the researcher could postulate any set of assumptions and arrive at the logical implications that followed from these assumptions using mathematics. He still maintained, however, that mathematics ought to pertain itself to more empirically based definitions for these would naturally lead to more useful theories. In broad lines, Evans argued that utility was an ineffective concept because it was neither observable nor quantifiable²⁸. He had already argued

26. The same equation appeared in Evans (1925b) as equation (7) and uses an integral to account for the past history of prices. Specifically the equation for the demand $y(t)$ is $y(t) = ap(t) + \int_{-\infty}^t \phi(t-T)p(T)dT$ where a is a constant and $\phi(t)$ acts as "weighing" function that gives different weights for past prices of the commodity y .

27. Apart from Cournot, Jevons, Marshall, and Edgeworth, Evans mentions Pigou's 1924 "The Economics of Welfare", A. L. Bowley's 1924 "Mathematical Groundwork of Economics", and many of Fisher's works. He also mentions some books separately in his preface: (1) Pareto's *Manuel d'Economie Politique* (1927); (2) Amoroso's *Lezioni di Economia Matematica* (1921); (3) Divisia's *Economique Rationnelle* (1928); (4) Tonelli's *Traité d'Economie Rationnelle* (1927); and (5) H. L. Moore's *Synthetic Economics* (1929).

28. On this matter he states that: "The demand that any quantity must be measurable in terms of units seems at first glance to be trivial. For what can economists talk about which is not measurable? It is precisely this tendency to talk about not measurable things as if they were measurable which drives a fair amount of economics on the reefs, and we shall find the criterion useful. It is common for example, to define 'wealth' as the total of material things owned by human beings, and to talk about making 'pleasure' a maximum; but neither of these things has any significance in the sense of measure, and we cannot make either a maximum. They are not quantities in the sense of Sec. 9; they do not have dimensions. Hence they cannot be terms of

similarly in a previous article on mathematical economics that “[t]here is no such measurable quantity as ‘value’ or ‘utility’ (with all due respect to Jevons, Walras and others) and there is no evaluation of ‘the greatest happiness for the greatest number’; or, more flatly,— there is no such thing” (Evans 1925b, 110). This same stance was again presented in his 1929 appraisal of Cournot’s *Researches into the Mathematical Principles of the Theory of Wealth*, “Cournot is almost alone in holding to a clear realization of the difference between measurable and non-measurable quantities, pointing out in Chapter 1 that many of the controversies in the subject arise from ambiguities of this sort” (Evans 1929, 269). In his more methodological 1932 article *The Role of Hypothesis in Economic Theory* he demonstrated how integrability conditions forced arbitrary mathematical constraints to demand theory. Moreover, he likewise argued that the aggregation of individual preferences is yet another major complication for the subjective theory of value - as the social optimum will rarely equate the sum of individual optima.

Reactions to Evans’s textbook varied substantially. It is safe to say that the criticism dissatisfied him, and he felt that his work was misunderstood. It is important to note that Evans held the mathematical works in economics, up until the 1920s, in low esteem. This was especially evident in his 1929 critique of Cournot, where he explicitly critiques Arthur L. Bowley, an important English mathematical economist²⁹. This animosity might explain why Bowley wrote a rather dry and critical appraisal of Evans’s book, stating that it was unfit for both the mathematician and the economist. His critique did not focus, however, on Evans’s view on utility, but rather on his decision to work with explicit functional forms. Similarly to his articles, Evans’s book was permeated by the quadratic cost function and the linear demand function. Bowley thought that explicit functions should be reserved for examples and exercises. In this sense, Bowley criticized Evans’s book for not presenting a general theory of economics, which left the mathematical reader without a thorough understanding of economics; he also argued that economists would find the mathematical treatment too advanced and would be bothered by the “unelucidated mathematical character of the solution[s]” (Bowley 1932[94]). He also stated that “... the appendix to Marshall’s *Principles of Economics* is far more useful to the student of economics, apart from more recent studies on mathematical economics” (Bowley 1932[94]). His only positive remark was on the last chapters of the book (the ones on the calculus of variations) for they introduced novel topics for advanced

relations in theoretical economic” (Evans 1930, 19-20).

29. Weintraub (1998) writes: “[...] he is on record in print as believing that Jevons and his school, which of course means Marshall, and Walras and his school, which of course means Pareto, H. L. Moore, and virtually everyone else writing in mathematical terms, are entirely misguided for basing analysis on a nonquantifiable theory of value. Moreover, Evans has sneered in public at the mathematical competence of the author in England, A. L. Bowley, who had written the basic text in mathematics for economics” (248).

students.

Bowley's complaints are very akin to those presented by his colleague at the London School of Economics, the mathematical economist R. G. D. Allen. Allen, importantly, would also be the author of the influential textbook *Mathematical Analysis for Economists* published in 1939. Allen argued that one could not expect to approximate cost and demand functions by *any* numerical form; thus, one should always leave these implicit. Evans's results, in his perception, seemed to be presented in a "somewhat clumsy algebraic form" that were not helpful unless as examples to illustrate the general theory. Both Bowley and Allen figured that using "arbitrary functions", that is, leaving the cost/demand function implicit was the appropriate formulation for a mathematical economics textbook; specific functional forms should be used only to illustrate the theory³⁰. Hotelling wrote a generally favorable review but also harped on these points, suggesting that a better presentation for the book would be to give "perfectly general proofs" and to "incorporate the special cases in examples and exercises" (Hotelling 1931b, 108). E.B. Wilson voiced a similar impression arguing that Evans's book seemed "wrongly conceived". He took issue with Evans's mathematical presentation and the absence of an overarching economic theory: specifically, he says that Evans "seems to treat the whole subject as a series of rather minor problems" (quoted in Weintraub 1991, 158).

Mathematician and statistician Philip G. Wright (1861–1934), and Schultz were the only ones sympathetic with Evans's approach. Wright relates Evans's method with theoretical sciences in general: one assumes a certain set of axioms and postulates a priori and then mathematically deduces conclusions from these. He perceives this as a straightforward application of mathematics to any science; in this he seems to share Evans's views on the mathematical method in general as he states that: "By varying the assumptions, different conclusions are derived. If the conclusions in any case seem at some removes from reality, that is not the fault of the 'logical mill' that grinds them out, it is the fault of the grist that is put into the hopper" (Wright 1931, 94). Wright argues briefly describes Evans's book and argues that mathematics open opportunity to arrive at conclusions that would escape mere logical syllogisms. He states, however, that "[t]he only regret that may be expressed in regard to Professor Evans' admirable book is that it is likely to have so few readers. He is a professor of pure mathematics; and symbols and processes which appear simple to him, will strike terror into the hearts of most economists (Wright 1931[94])".

Schultz gave the book a mostly positive review but took issue with Evans's critical view of utility. Evans, as mentioned above, stated that economics ought to restrict its scope to the study of the relations of quantifiable entities. Schultz, on the

30. Allen goes as far to say that the specific functional form of either demand or cost could be even "unknowable" (Allen 1931, 94).

other hand, argued that most economists would disagree with Evans: even though a direct measurement of individual pleasure was not possible, one still could devise an index of pleasure, i.e., a utility function. On the matter of integrability, Schultz conceded that Evans demonstrated that there was no unequivocal correspondence between preferences and demand functions in specific contexts. However, he argued that similar contentions had been dismissed by E. B. Wilson, in his 1909 review of Pareto's *Manuale di Economia Politica* (Manual of Political Economy). Specifically, Wilson had stated that integrability problems - that come naturally to the mathematician - could be ignored without much cost by the economist³¹. Schultz and Hotelling, in fact, faced many issues involving integrability conditions in their work on demand curve estimation. A further discussion will be postponed to the final part of section 2.5.

It is not easy to grasp the impact of Evans's book though Simon (2008) affirms it — alongside Evans's articles — was important for the mathematically inclined students of economics. He states that “Evans's books and his articles were an important resource for early American students with an appetite for mathematical economics, who prior to their publication found an extremely sparse literature on which to graze” (Simon 2008, 493). As I will present in the following section, almost all authors who applied the calculus of variations to economic problems mentioned Evans and his most commonly cited work is his book. Evans also wrote a more detailed presentation of his dynamic economics (including even a proper discussion on first order and second order conditions) in the *Mémorial des Sciences Mathématiques* edited by Henri Villat in 1932. It is unclear what sort of impact this work had on American economists but the lack of references to it suggest it was minor.

In 1938, Allen published the textbook *Mathematical Analysis for Economists*. The book was much more comprehensive and broader in its scope than Evans's *Mathematical Introduction*. Allen's book began from mathematical fundamentals (functions, elementary topics in geometry, and limits) and eventually made its way to the calculus of variations. Chapter 20, the last chapter of Allen's book, presented the basic theory of the calculus of variations. It discussed functionals and presented the necessary solution (the Euler-Lagrange equation) to the typical problem in the calculus of variations³². It also used the works of Evans (1924) on monopoly and Ramsey (1928) on optimal savings as examples of this literature³³. Allen, when

31. Wilson states that: “the whole discussion of the order of integration, a point that naturally occurs to the mathematician, can be thrown out of court by the economist” (quoted in Schultz 1931, 488). For Wilson's complete text refer to his 1909 review of Pareto's *Manuale* (Wilson 1909). A discussion on the integrability conditions will be postponed to a further section.

32. The formulation is presented 523-533

33. Gerhard Tintner's review of the book in the *Journal of Political Economy* is positive and

discussing general theoretical statements, does use functions in implicit form, that is, he writes profits as a function of prices and the change in prices without specifying the precise functional form that relates these variables. This same presentation is found in Bowley's *Mathematical Groundwork of Economics*³⁴.

As for these books impact on graduate education, the matter is unclear. Consulting available records online³⁵, it seems that Evans's book was used in mathematical economics courses in the 1930s and 1940s, but often only its first three chapters were required readings. These chapters are on monopoly (the Cournot case), change of units, and competition (partial equilibrium). These courses were lectured by E. B. Wilson, J. Schumpeter, Russian-American economist W. Leontief (1905-1999), and American economist Richard Goodwin (1913-1996) at Harvard; Hotelling at Chapel Hill; and Ukrainian-American economist Jacob Marschak (1898-1977) at the New School for Social Research. Allen's book is not mentioned as often as Evan's and is used as a general reference book. In these records, none of these authors make reference to Evans nor Allen when speaking about dynamics, that is, it seems that these textbooks were more commonly used for their analysis of static equilibrium. As early as the beginning of the 1950s, Samuelson's *Foundations* was already the main in economic dynamics. It also should be noted that Samuelson, despite knowing Evans's book, did not cite Evans. As I will show in the following chapter, the mathematical textbooks of the 1950s present dynamics in a Keynesian framework using differential or difference equations, similarly to Samuelson. Most notably, Allen published a second mathematical textbook in 1956 that omitted the calculus of variations and presented dynamics using aggregate variables and difference equations. The calculus of variations, in a mathematical model of economic dynamics, reappeared in textbooks only after the popularization of optimal control and dynamic programming from the 1970s onwards.

also makes a nod to Evans's textbook. Tintner says that Allen's book would be appropriate for graduate studies in economics but claims that its presentation of the calculus of variations is too short: Evans's textbook could be used to supplement this gap (Tintner 1939, 272).

34. Despite being similar to Allen in using implicit functions, Bowley's *Groundwork* differs from Allen's by presenting a mathematized economic theory beginning with a simple exchange economy and culminating a general equilibrium scheme, followed by discussion on welfare and taxation. Mathematics is presented more systematically only in an appendix. Allen (1938) on the other hand is structured around the mathematical topics it presents, i.e., derivatives, exponential functions, partial derivatives, differential equations, etc.

35. Irwing Collier makes available a rich collection of class materials (syllabi, lectures notes, bibliographical references, etc.) on his website <http://www.irwincollier.com/>. I consulted all related topics on mathematical economics related to Evans, R G D Allen, and mathematical economics that included a reading list for either a graduate or undergraduate class in economics. A more complete research, unfortunately, would require direct access to archives which were not accessible to the author.

2.4 Charles Roos

Charles Roos studied at the Rice Institute and received his PhD in 1926. He was supervised by Evans and his main interests at the time were in integral equations, the calculus of variations, and the applications of these to economic theory. His early research was deeply influenced by the previous work of his advisor; indeed, his 1925 paper, as noted above, is a direct extension of Evans's 1924 *Dynamics of Monopoly*. Roos also followed suit in Evans's contempt for utility theory. In the next couple of years, Roos continued his academic studies as a National Research Fellow at the Universities of Chicago, Princeton, and Cornell.

Roos's most notable work in the 1920s is *A Dynamical Theory of Economics*, published in 1927 in the *Journal of Political Economy*³⁶. It attempts to generalize Evans's and his own past work to a general competitive setting, that is, he puts forwards a dynamical theory of economic equilibrium. His approach is to expand Walras's general equilibrium scheme with dynamical supply and demand functions³⁷, that depended not only on prices and levels of production, but on the change of prices, past prices, and current and past production rates. He devised an innovative set of profit maximization equations that allowed him to succeed where Walras and Pareto had fallen short; these additional restrictions provided him the necessary conditions to identify all of the unknowns given the available equations in his theoretical scheme. Thus he showed that there existed a choice of prices and rates of production, in a fixed time interval, that satisfied the dynamical equations of demand, and guaranteed that the profit integral for every commodity market would attain a maximum – and that also admitted these variables to change through time. His conclusion, however, is more restrained. He concedes that the resulting prices and rates of production don't necessarily make *all* the profit integrals a maximum, that is, competitive equilibrium may drive out firms or lead some to bankruptcy. As mentioned above, he is convinced that his work not only could be empirically verified but that it could also facilitate real-world decision making. He argues that the theory exposed in his paper could be verified quantitatively in special cases, although he admits one should not expect a close correspondence between theory

36. Roos had some difficulty finding a journal willing to publish his work. His comment reads: "I was at a loss to know where to send the paper for publication. Finally, I sent it to a mathematical journal. The editor wrote me that he would be glad to publish the mathematical parts of the paper if I would rewrite it and delete the economics. Instead of following this suggestions I sent the paper to a statistical journal and was told the journal would publish the parts dealing with statistics and economics if I deleted the mathematics! ... I therefore sent the manuscript to the *Journal of Political Economy*. It was accepted but publication was delayed" (Roos 1934, xiii).

37. Roos does acknowledge Pareto (1901) — Henry Schultz had brought it to his attention — as an attempt to overcome the statical limitations of Walras's general equilibrium theory by using time derivatives in the equilibrium equations. It is unclear however if Roos was aware of the difficulties that Pareto faced that eventually led him to abandon this project.

and observation; he also argues that his findings could guide the choice of how to vary prices and production so as to attain the maximum profit as possible.

Roos would later repeatedly reference his 1927 paper – interchangeably with Roos (1925) and Evans (1924) – as the work presenting the “new dynamical theory of economics”³⁸. Both Evans and Roos were attempting to structure a theory that overcame the limitations of static economic analysis. Roos used the same variational method to tackle depreciation (Roos 1928b), business forecasting (Roos 1929), and business cycles (Roos 1930). His method is similar across all these papers. He begins by proposing a mathematical model with a dynamic equation for demand (along the lines of Evans’s equation ((2.2))) and a cost function that relates prices, productions rates, and their changes (i.e., their first derivatives). Other assumptions were added according to the problem at hand, but in the end Roos built an intertemporal profit function that was to be maximized. He solved this problem and explored the dynamics of its solutions. This method occasionally led him to quite complex dynamics. His 1930 article, *A Mathematical Theory of Price and Production Fluctuations and Economic Crises*, published in the *Journal of Political Economy*, for instance, ends up in 12 different solutions for the optimal trajectory of prices with various transversality and boundary conditions.

Following his National Research Fellowship, Roos was hired as assistant professor at Cornell University in 1928. He resigned this position in 1931 to assume one as permanent secretary at the AAAS. As already mentioned, this allowed Roos to organize symposiums on topics of his interest that were attended by members of the Econometric Society. In midst of America’s Great Depression, he arranged a symposium on unemployment; the papers presented at this meeting were later published by the AAAS in the book *Stabilization of Employment*. In 1933 Roos decides to leave the AAAS to study mathematical economics in England under a Guggenheim fellowship. He quickly declines this opportunity, however, as he was called to act as principal economist and director of research of the newly created National Recovery Administration (NRA). The NRA was part of the New Deal effort to assuage the pernicious effects of the Great Depression.

In 1934, Roos is approached by Alfred Cowles to work as a professor at the Colorado College and to act as director of research at the newly instituted Cowles Commission for Economic Research based in Colorado Springs. Cowles was no stranger to Roos. A year prior, Cowles had to finance the operations of the Econometric Society, and made the regular publication of *Econometrica* economically feasible. Cowles himself was interested in business cycles, financial markets, stock market forecasts, and demand estimation. The Cowles Commission, in latter years, would be a leading center for the research in mathematical economics, specially that linked

38. See, for instance, Roos (1928a, 156) or Roos (1930, 501).

with Walrasian general equilibrium theory. Roos penned its first monograph entitled *Dynamic Economics: Theoretical and Statistical Studies of Demand, Production and Prices*. This work summarized much of his previous articles and explored business applications to his functional estimations in the demand for gasoline, agricultural products, and residential building. Cowles's second monograph was also written by Roos and was based on his work at NRA. The Cowles Monographs and *Stabilization of Employment* were published by the Principia Press, a publisher owned and operated by mathematician and statistician Harold T. Davis (1892-1974). Davis had become acquainted with Roos and Evans following the latter's review of his PhD thesis on Volterra's equations in 1928. Davis's own work was focused in time series analysis. Similarly to Evans and Roos, he was influenced by Henry Moore's harmonic analysis of business cycles and statistical estimation of demand curves. Davis would succeed Roos as research director of the Cowles Commission in 1937 when Roos leaves the organization.

Roos's experience with the government and with big corporations led him to believe that there was a great interest for demand studies and business forecasting³⁹. In 1937 Roos moves to New York and begins a consulting firm later named as the *Econometric Institute Inc.* Roos was immediately successful in this venture and some of his first clients include General Motors, Armco Steel, and Gulf Oil. His work with GM, in particular, would result in the publication of *The Dynamics of Automobile Demand* and a couple of papers in collaboration with his colleague Victor von Szeliski in 1939. Roos and von Szeliski had met in meetings of the Econometric Society and the AAAS; von Szeliski had published work in stock price forecasting in the *Journal of the American Statistical Association* and in *Econometrica*.

The transition to the business world nonetheless signified Ross's departure from academic affairs. His work in the Econometric Institute was focused on forecasting time-series and his methods relied mostly on curve-fitting using least squares and decomposing series (time trend, seasonal components, etc.). He continued to publish sparsely, throughout the years, writing mostly on business forecasting; his presentation also became much lighter on the mathematics: his published articles in the 1940s and 1950s rarely feature a single equation (this is true even of his 1955 survey on forecasting techniques published in *Econometrica*⁴⁰). In this sense, Ross

39. H. Davis notes that Roos's time at the NRA made it evident that there existed a gap between theoretical economics and the observed behavior of time series data. In this sense, Roos found that more *ad hoc* formulations, based on the observable features of the data, delivered better fits and forecasts than theoretical models based upon profit maximization or marginal cost minimization (Davis 1958, 582-584).

40. Roos's survey reveals his critical opinion of the current state of econometrics in the 1950s. He argues that successful econometric models had to be based on a "truly" dynamic theory of economics and not the "static theories of Walras, Pareto, and Marshall or the quasi-dynamic theory of Keynes". He also adds that he will omit from his survey the "Keynesian models that failed so

became estranged to both mathematical economics and econometrics: the former followed Samuelson's lead in macrodynamics using differential equations; general equilibrium theory, in the renewed Cowles Commission, was now studied using differential topology and convex analysis that were deemed more general and flexible than the calculus (Fox 1988, 27-28). Econometrics was now more influenced by the probabilistic approach of Norwegian economist Trygve M. Haavelmo (1911-1990) and preferred structural estimation to the curve-fitting methods of Roos and H. Davis (Dimand and Veloce 2007, 537-538). Roos would still be involved with the Econometric Society and the AAAS up until his death in 1958.

The other important American figure to use the calculus of variations in economics is the previously mentioned Harold Hotelling. He was the first author to take notice of the work being done by Evans and Roos - and also the first to cite Ramsey's paper on optimal savings, that used variational calculus. Hotelling himself will contribute with two articles into this literature one of which would become very influential in environmental economics many years later.

utterly to indicate the post-war economic condition" (Roos 1955, 365).

2.5 Harold Hotelling

Harold Hotelling (1895-1973) was an influential figure in both statistics and mathematical economics. His most productive period in the latter field was from the 1920s up to the 1940s. Hotelling's research is characterized by the application of sophisticated mathematical techniques to analyze social problems and to better understand policy implications. His lifelong concern for social issues is the unifying thread that brings together his work on mathematical economics and is perhaps most clearly exhibited in his papers on consumer welfare and taxation. Since the beginning of his academic studies, Hotelling was convinced that a rigorous theoretical framework for economics required a thorough domain of mathematics. In this sense, he was often critical of the mathematical ignorance within the economic profession. On many occasions he expressed this frustration⁴¹ (see, for instance, Hotelling (1931b, 108) or "The Mathematical Training of Social Scientists, Report of the Boulder Symposium" (1950, 198-199)) and he repeatedly argued that his work, alongside that of Evans and Roos, was significantly overlooked due to its mathematical complexity. Most notably, he found the American Mathematical Society (AMS) more amicable to his work than the American Economic Association (AEA), whom he believed did not support mathematical economics. Hotelling, in fact, only joined the AEA in 1965 when he was elected its first Distinguished Fellow for his contributions in welfare analysis.

As a statistician, he made both important theoretical contributions to the field and to the teaching of statistics more broadly. Throughout most of his lifetime, and during most of his teaching career at both Stanford and Columbia, statistics was lumped into neighboring disciplines and was deemed to not merit its own research. In this regard, Hotelling found himself frustrated in his repeated attempts to persuade the directorship at Columbia for the creation of a statistics department, that would centralize statistical education at the University level. This intransigence led Hotelling to accept an offer from the University of North Carolina at Chapel Hill where he helped to build their pioneering statistics department. At Chapel Hill, Hotelling devoted most of his efforts to teaching and while he remained interested in economic theory and mathematical economics, his research was directed to multivariate statistical analysis. Kenneth Arrow (1921-2017), Hotelling's PhD advisee, notes of this period that "It is no exaggeration to state that during the 1930s and early 1940s, Hotelling nearly single-handedly brought American statistics into the

41. See, for instance, Hotelling (1931b, 108). In a report on The Mathematical Training of Social Scientists, a joint effort of the Econometric Society, the Institute of Mathematical Statistics, and the Mathematical Association of America, Hotelling claims that the "social sciences will not be in a satisfactory state until the specialists in them know a good deal more mathematics than at present" ("The Mathematical Training of Social Scientists, Report of the Boulder Symposium" 1950, 198)

modern age and laid the foundation for the extraordinary development of the subject after the Second World War” (Arrow and Lehmann 2005, 11).

Hotelling wrote two papers using the calculus of variations. The first one, published in 1925, was on depreciation: attempting to improve upon previous work in this literature, Hotelling framed the problem as one of finding the optimal duration of a machine’s lifetime along with the associated price trajectory that would make net receipts a maximum for its owner. His second work came out only in 1931, though he already had presented similar versions of the finalized paper in annual meetings of the AMS in 1925. Generalizing his work in depreciation to that of any asset, Hotelling aimed to find the optimal extraction rate of an exhaustible resource. Though his research never ventured back into the variational realm, Hotelling held both of these papers in high regards. He described the latter one, in particular, as one of the major triumphs of his career as he had “found effective use in it for the calculus of variations” (Hotelling 1965 quoted in Darnell 1990, 14). Indeed, his enthusiasm with the calculus of variation is also noted by a review of his career composed by Samuelson who even judges it to be exaggerated. Commenting on the 1925 article, Samuelson says that there is “perhaps an overemphasis upon the calculus of variations and maximizing continuous functionals here, due no doubt to the coeval work of Evans and Roos and the many students of G. A. Bliss; and also to Hotelling’s own developing interest in the problem of mining that was to be the subject of his third major paper, that of 1931 [...]” (Samuelson 1960, 22). He further notes that: “For the most part economists have found it just as convenient to work with a finite number of variables defined at many discrete periods of time as to deal directly with functionals. And except in connection with capital theory there have been rather few important applications of the calculus of variations in the the thirty years that have since elapsed” (22).

The unifying thread throughout Hotelling’s extensive career was his lifelong concern with social welfare and public issues in general. Although his graduate studies were in pure mathematics, his academic career began in journalism at the University of Washington. Recollecting his thoughts, many years later in his unpublished autobiography, Hotelling says he believed that journalism would both provide him a livelihood and equip him with tools to enact social change⁴². He received his bachelors degree in journalism at Washington University in 1919 and a year later, by the invitation of his former teacher, the mathematician Eric Bell Temple (1883-1960), he returned to pursue a graduate education in mathematics, attaining his masters

42. In the manuscripts of his unpublished autobiography (quoted in Darnell 1990, 3) Hotelling attributes his sympathy for social justice and race equality to his Methodist upbringing. He explains his decision to pursue a degree in journalism believing that it “seemed to offer both a means of livelihood and an opportunity to stimulate proper action on public matters” though he later deemed that he had overrated both of these aspects in it.

degree in 1921. Hotelling's interest at this time was mathematical economics: his masters studies were a stepping stone in this direction as he wished to accrue what he deemed to be the necessary mathematical foundation to research economics. He followed to his PhD at Princeton University expecting to study both mathematical economics and statistics but found that no professor researched either specialty. Having settled on a thesis in topology, written under the guidance of Oswald Veblen⁴³ Hotelling earned his PhD degree in 1924. Veblen at the time was the president of the AMS and had introduced Hotelling to the society a year prior. In the periodic meetings of the AMS, Hotelling alas encountered other active researchers in mathematical economics, including most notably Evans and Roos.

Hotelling's first work in using the calculus of variations, *A statistical theory of depreciation based on unit cost*, was presented at an AMS meeting in December of 1924 and published in the *Journal of the American Statistical Association* in the following year. It was initially motivated by a paper presented by mathematician James B. Taylor on one of the first AMS meetings Hotelling had attended. His formulation of the problem assumes that the owner of a machine wishes to maximize its value, that is, the present value of its output minus operational costs. He then presents the following specification:

$$V(t) = \int_t^n [pY(\tau) - O(\tau)] e^{-\int_t^\tau \delta(v)dv} d\tau + S(T) e^{-\int_t^T \delta(v)dv} \quad (2.11)$$

where p is the price of a unit of output, $Y(\tau)$ gives the units of output per year at time τ , $O(\tau)$ stands for the yearly operational costs, and $S(n)$ is the machine's salvage value at n , the moment the owner disposes of the machine. Here the exponential term gives the familiar discount factor in a perhaps unfamiliar form (note that if $\delta(v)$, the "force of interest" is constant, say r , the exponential reduces to $e^{r(t-\tau)}$). Hotelling further assumes that the machine is always operated at full capacity, that is, $Y(t)$ is always a maximum. The most interesting case he considers is that in which both x (price) and n (machine's lifetime) are to be determined and the operating cost, $O(\tau)$ depends on the machine's value, $V(\tau)$. He thus rewrites the operational cost as $O(\tau) = A(\tau) + B(\tau)V(\tau)$, so the operational cost of the machine is related linearly to a known function $A(\tau)$ and to the value of the machine $V(\tau)$ by another known function $B(\tau)$ ⁴⁴. For ease of notation I depart from Hotelling's presentation and assume that $\delta(\tau)$, the interest rate, is equal to a positive constant r . Now

43. Veblen, as mentioned previously, had studied mathematics in Chicago and worked his PhD under E. H. Moore. At this time his work revolved mainly around the foundations geometry and in finding the linkages between differential geometry and topology raised by both mathematicians and physicists. Incidentally, He was also the nephew of economist Thorstein Veblen (1857-1929).

44. These functions, alongside $Y(\tau)$, the machine's production rate, and $\delta(\tau)$, the interest rate, are to be determined (or at least estimated) according to experience (observation) (Hotelling 1925, 346).

plugging the new expression for the operational cost into (2.11):

$$V(t) = e^{rt} \int_t^n e^{-r\tau} [pY(\tau) - A(\tau)] d\tau + S(n)e^{r(t-\tau)} - e^{rt} \int_t^n e^{-r\tau} B(\tau)V(\tau)d\tau \quad (2.12)$$

Taking the derivative, with respect to time, on both sides results in the following first order linear differential equation:

$$V'(t) - [r + B(t)] V(t) = -pY(t) + A(t) \quad (2.13)$$

Again to ease notation, let $r + B(t) = \gamma$, where γ is a constant. Then the solution is of the form:

$$V(t) = e^{\gamma t} \int_t^n e^{-\gamma\tau} [pY(\tau) - A(\tau)] d\tau + K \quad (2.14)$$

where K is a constant of integration than can be determined either by setting a terminal value for $S(n)$ or an initial value for the cost of the machine $V(0) = c$ (where c is a positive constant the represents the machine's cost). Using this equations, one can find the terminal time n , the optimal price p and the trajectory of $V(t)$. To make this exercise more concrete Hotelling inputs values for the constants and specific functional forms for the known functions⁴⁵. The depreciation rate is defined as the rate of decrease of value, that is, $D(t) = V(t-1) - V(t)$. Hotelling shows that his solution better resembles empirical evidence: his depreciation curve, he argues, better estimates depreciation, specially in its earlier periods, when compared to other contemporary methods. He also says that his formulation of the problem is an improvement not only because it allows these variables to change through time but also because it assumes – more correctly – that the firm wishes to maximize the present value of the profits. To be sure, Hotelling does not solve a problem in the calculus of variations *per se* but of an integral equation, or more specifically, a Volterra equation. He does comment, however that extensions to his theory would lead into the “dawning economic theory” that employed the calculus of variations which he describes to bear “to the older [statical] theories the relations which the Hamiltonian dynamics and the thermodynamics of entropy bear to their predecessors” (Hotelling 1925, 352).

Roos's two papers on the problem of depreciation, in particular, show his inclination for seeking business applications to his theoretical work. Though his work exhibits considerable mathematical sophistication and abstraction he consistently argues how a firm owner could, given the necessary information, use these mathematical formulae to optimally chose production rates. Both papers cite Hotelling's 1925 *A General Mathematical Theory of Depreciation*. Another article that stems

45. The precise specification would be: $Y(\tau) = ae^{-\lambda\tau}$, $A(\tau) = be^{\mu\tau}$, $S(n) = 0$, with constants $\gamma = .09$, $\lambda = .06$, $\mu = .14$, and $c/b = 20$

from this discussion on depreciation is that of Pixley (1930). Henry H. Pixley was a Chicago mathematician that studied the calculus of variations under Bliss, and worked his Ph.D under Lawrence M. Graves, a former Ph.D student of Bliss. His work is based on a suggested extension in Roos (1928b). He considers how to chose production rates optimally when the firm has the power to change its machine to a supposedly more productive one after an interval of time. This adds considerable complexity to the problem as there are now multiple discontinuities to account for. The general formulation of this problem, indeed, is no longer in the class of “classical” calculus of variations. Nonetheless, Roos himself had worked out most of the properties necessary to solve this problem in a previous 1928 article.

Hotelling’s 1931 article on exhaustible resources was in the makings of his depreciation model. Instead of a machine that depreciates its value, Hotelling now considered the case in which the use of an active - say, a natural resource - would eventually lead to its depletion of any value. He argued that problems of this kind must employ the calculus of variations: static theories would lead one astray as maintaining a constant production rate is feasibly impossible - physical production is bound to decline at some point. The problem, once again, is to maximize the present value of the profits: these are now associated with the exploitation of a natural resource, a mine in Hotelling’s analysis. The quantity extracted $q = f(p, t)$ is a function of prices p and time t . Hotelling argues that the owner of the mine is indifferent in receiving a value p_0 for his product in the present or a price $p_0 e^{\gamma t}$ in the future, where γ is the interest rate (that is assumed to be constant). So price is given by $p = p_0 e^{\gamma t}$. Denoting the total supply of the resource by S we have the following two restrictions:

$$\int_0^T q dt = \int_0^T f(p_0 e^{\gamma t}, t) dt = S \quad (2.15)$$

$$f(p_0 e^{\gamma t}, T) = 0 \quad (2.16)$$

The first of these equations states that total supply must equal total demand and the second one that the resource will be depleted at time T ⁴⁶. With a functional form for q this problem should be solvable for T . To find the optimal extraction rate, Hotelling assumes that one wishes to make the social value of the resource a maximum, that is, that one wishes to maximize the aggregate utility derived from the consumption of the resource.

$$V = \int_0^T u(q(t)) e^{-\gamma t} dt \quad (2.17)$$

46. The latter is actually the transversality condition for a problem with variable terminal time.

Where $u(q(t))$ is the utility at time t as a function of the quantity extracted. The solution turns out to be precisely $p_t = p_0 e^{\gamma t}$ or, alternatively, $\frac{dp}{dt}/p = \gamma$, that is, the optimal growth rate of prices should follow the market interest rate.

As has been hinted to before, the optimal growth literature of the 1960s is responsible for a renewed interest in the calculus of variations. Even so, these growth models didn't account for exhaustible resources and Hotelling's paper would only become widely known in the following decade, when environmental concerns became more prevalent in the scientific debate⁴⁷.

Hotelling's work in consumer theory is not properly in the calculus of variations but brings up worthwhile discussions that relate to some issues on the integrability conditions of demand functions brought up by Evans in another context. As mentioned previously, one of Evans main critiques of utility functions is that they impose the so called integrability conditions: these guarantee that a particular demand function was "derived" from a specific utility function and that one can obtain this primitive form. It is important to note that Evans' discussion of integrability conditions doesn't resemble modern Slutsky symmetry conditions⁴⁸. He frames the issue as that of finding an integrating factor for a Pfaffian differential equation, representing an arbitrary indifference curve map. Problems of integrability arise when one considers a three good case, say x , y , and z , in which marginal utilities $U_x(x, y, z)$, $U_y(x, y, z)$, and $U_z(x, y, z)$ are functions of all three goods. The equation below gives an indifference plane, where small variations in the consumption of the goods leave the consumer indifferent.

$$U_x(x, y, z)dx + U_y(x, y, z)dy + U_z(x, y, z)dz = 0 \quad (2.18)$$

The problem of integrability is to find a function $R(x, y, z)$ such that we can integrate the above equation, that is, the equation becomes the complete differential of some function $\phi(x, y, z)$

$$d\phi = R(x, y, z)U_x(x, y, z)dx + R(x, y, z)U_y(x, y, z)dy + R(x, y, z)U_z(x, y, z)dz \quad (2.19)$$

47. Erreygers (2009) comments that Hotelling's work went relatively unnoticed until the 1970s when environmental issues came to the forefront of the growth theory literature. To be sure, the article does not focus on the how or why of this nearly four decade omission, but some contextual arguments are put forward to explain the general disinterest for a growth theory with exhaustible resources until this period. Specifically, the scientific and public debate that followed the publication of the Club of Rome's *Limits to Growth* and John Rawls' (1921-2002) *Theory of Justice* (see Erreygers 2009, 264-269).

48. Integrability is usually posed as a problem of verifying if the Slutsky substitution matrix is symmetric negative semi-definite (see for example Mas-Colell, Whinston and Greene, 1995, 45-50).

The resulting integrability condition is

$$X \left(\frac{\partial Z}{\partial y} - \frac{\partial Y}{\partial z} \right) + Y \left(\frac{\partial X}{\partial z} - \frac{\partial Z}{\partial x} \right) + Z \left(\frac{\partial Y}{\partial x} - \frac{\partial X}{\partial y} \right) = 0 \quad (2.20)$$

This equations Evans deemed to lack any economic significance. It also is limited to the three good case, an entirely new condition needing to be derived for those with four or more goods. A special case that is always solvable is when one assumes that the marginal utility for a given good is a function of that good alone, i.e. all demands are independent amongst themselves. It is unlikely that Evans had access to Slutsky's original contribution as they were scarcely known during this period and he makes no mention whatsoever of them. The first author to point out this issue was, in fact, Vito Volterra in a review of Pareto's *Manuale*. Volterra argued that Pareto's analysis of indifference curves became marred when dealing with three or more goods.

Henry Schultz, following the work of his PhD advisor, Henry L. Moore, encounters the integrability problem in his work on demand estimation. Hotelling works together with Schultz but their efforts arrived at mixed results. Hotelling's 1932 article was more specifically on the integrability conditions of the producer not subject to a budget constraint. Thus his analysis applied to the case of unconstrained consumer maximization; he then inadvertently provided Schultz with these "wrong" integrability conditions. Indeed, the empirical investigation undergone by Schultz revealed striking contradictions from Hotelling's theory. In 1935 Hotelling tackled the consumer problem when faced with a budgetary constraint and derived the appropriate integrability conditions for this case. In *The Theory and Measurement of Demand*, Schultz also faced the problem of the order of integration, that is, whether the order in which one sums the marginal utilities affects the final result. This issue was also pointed in Volterra's critique of Pareto and even previously by Fisher in 1892 monograph. Schultz argues that while the order of consumption of the commodities may have an influence on the utility derived from them, the order of purchase need not have such an influence (Schultz 1938, 15-17). He then assumes that the order of consumption is always known and, consequently, that the utility function always exists. Schultz had previously stated a similar argument in his review of Evans's 1930 textbook⁴⁹. In both occasions Schultz retorts to an argument of authority, stating that E. B. Wilson, in his own review of Pareto's *Manuale*, had asserted that integrability conditions, "a point that naturally occurs to the mathematician, can be thrown out of court by the economist" Wilson 1909, 469. Wilson argued that rigor in a physical science is different than rigor in mathematics and argues by analogy that discarding good physical theories, that perhaps lacked mathematical rigor,

49. See Schultz (1931, 486-489)

would be equivalent of doing good mathematics but bad physics. Evans responds to this by noting that Schultz had merely assumed away the problem of integrability Evans 1932, 324.

Both R. G. D. Allen and John Hicks (1904-1989) also dealt with these issues in a series of works published in the 1930s. Allen's 1938 *Mathematical Analysis for Economists*, in particular, deals with integrability from the standpoint of finding integrating factors for differential equations. He thus arrived at the same conclusions that Evans had but, unlike Evans, he found an economic interpretation for the integrability conditions in terms of marginal rates of substitution. He presents his analysis for the three good case as an example for the n-good case. He does not that to generalize these results to more complicated cases with complimentary goods. Simultaneously, Samuelson was developing his revealed preference theory. Under this new light, non-integrability could be ruled out as a consequence of the strong axiom. In the 1950 article *The Problem of Integrability in Utility Theory*, Samuelson surveys the history of this integrability debacle. He shows how Evans formulation of the problem was equivalent to the marginal rate of substitution approach that verified second order conditions (the Slutsky substitution matrix). This result, in turn, was equivalent to the one that had previously been derived by Russian economist Eugen Slutsky (1880-1948) and published in 1915.

2.6 Further Developments using the Calculus of Variations

Evans and Roos were not alone in their dynamic theories. At this point, many authors, in a somewhat dispersed manner, are publishing papers on theoretical economics employing the calculus of variations. In Italy, their work had a direct impact on mathematical economists Luigi Amoroso and Giulio La Volpe. The work of Vilfredo Pareto also influenced both of these authors. Pareto, in the early 1900s, tried to devise a dynamic theory of economics within the Walrasian general equilibrium framework. In his first attempt he followed d'Alembert's principle of "inertia":

$$(\phi_a - \frac{\delta x_a}{\delta r_a})\delta r_a + (\phi_b - \frac{\delta x_b}{\delta r_b})\delta r_b + \dots = 0$$

where ϕ_i represents the marginal utility of good i and $\frac{\delta x_i}{\delta r_i}$ is a supposed "counterforce" or "inertia", forces that counterbalance the marginal utility provided by good i . Pareto does not endeavor further down this analytical path for it lacks empirical foundation. That is, to know the functional form of these differential equations would require measuring that Pareto believed were not possible at his time. His second attempt comes in *Le Nuove Teorie Economiche* (1901). His approach was to extend the Walrasian general equilibrium a dynamic environment postulating that transactions (consumption and production) were realized in an arbitrary interval of time dt . The resulting equilibrium, however, exhibited very few departures from the statical case: the only dynamical variable in his scheme were individual savings and dynamics was mostly exogenous. Other authors didn't follow this second route, but Amoroso tried to develop Pareto's rational mechanics model further. In *Atti della Accademia Reale dei Lincei*, 1912, he proposed a new set of equations, following those of d'Alembert. While he was successful in this regard, he admitted that the economic interpretation of his results was problematic. Not only did his equations imply a cardinal measure of utility, but he would also still face the trouble of finding the exact functional forms of these equations from empirical observations.

Amoroso and La Volpe found in Evans's use of the calculus of variations a new form to model dynamics. La Volpe, in particular, uses the calculus of variations to solve the consumer problem in an intertemporal framework. Similarly to Pareto, his final goal was to establish a dynamic general equilibrium. In his 1936 book *Studies on the Theory of General Dynamic Economic Equilibrium* he proposed that the maximization behavior of consumers and firms could sustain, at each point in time, a changing equilibrium. His mathematical formalization included the individual's expectations, and an intertemporal budget constraint that contained the individual's

expenses on goods and services, his accumulated savings, the variation of his savings (in time), and a proportion of income that came from shareholding (Pomini 2018, 70)⁵⁰. The optimal path of consumption required consumers to match their marginal utility rates through time given their future expectations.

La Volpe also used the transversality conditions of the calculus of variations in his work. In particular, he assumes that at the terminal time (the last period considered) the consumer should not hold any savings. Using this condition, he changed his analysis to consider the entire life-cycle of the consumer. He concluded that individuals

Independently in England, the young mathematician Frank Ramsey published a seminal paper in 1928 in which he tried to determine the optimal saving rate that society should choose to maximize its overall welfare. Ramsey defined an abstract level of maximum enjoyment that he denoted “bliss”. His problem was to minimize, at each point in time, the gap between bliss and the current pleasure, defined as the difference between the utility gained by consumption and the disutility imparted by labor. Ramsey’s problem, unlike previous applications of the calculus of variations, involved an improper integral and had no time discounting: specifically, Ramsey set its upper bound to infinity and did not include any weighing function to avoid explosive trajectories. He was careful, though, to guarantee that his problem had a solution⁵¹. Ramsey found that the optimal rate of savings, multiplied by the marginal utility of consumption, should always equal the difference between “bliss” and the actual rate of utility enjoyed (Ramsey 1928, 547). Ramsey’s work gained considerable notoriety in the 1960s when it became incorporated in the optimal growth literature.

Also independently, the Dutch economist Jan Tinbergen (1907-1988) used the calculus of variations to model business cycles. Tinbergen had a solid background in mathematics and physics which he studied at the University of Leiden from 1921 to 1925. His doctoral thesis, published in 1929, used the calculus of variations to model oscillations around a trend (Boumans 1993, 142-147). The dynamics in his mathematical model, as did in Evans’s 1924 article, came from the optimizing

50. Pomini (2018) notes that in solving this problem with the calculus of variations, La Volpe was aided by two Neapolitan mathematicians (Pomini 2018, 70). He also notes that La Volpe’s decision to include expectations “reveals the influence of the Austrian school for which dynamic analysis was a multi-period analysis influenced by the expectations of economic agents” (69).

51. As mentioned previously, exponential discount rates were introduced in part to guarantee that the improper integral converged to a finite value. Ramsey avoided this by stating his problem as one of minimizing the distance between the current value of the function and its theoretical maximum value (“bliss”). As shown in Duarte (2016), exponential discounting became the standard procedure in infinite-horizon utility maximization problems. This required the introduction of transversality conditions to guarantee the existence of well-behaved solutions. As an aside, Ekeland (2010, 1-4) argues that the transversality conditions commonly used by economists are not sufficient conditions for a maximum, and that, to his knowledge, the only mathematically correct version of this problem is the one proposed by Ramsey.

behavior of a monopolist firm.

Tinbergen was also interested in the question of the relevant time horizon for individual decision making, and in a way to formalize the uncertainty implicit in this decision. This was a question that enticed many authors of this period, including Roos, La Volpe, and especially Albert Gailord Hart (1909-1997). Hart's *Anticipations, Business Planning, and the Cycle* was a theoretical paper on the effect of expectations on the business cycle, published in *Econometrica* in 1937. He reckoned that the firm's planning should be modeled as that of the maximization of present discounted value of profits; he mentions Hotelling's 1931 paper as an example of this, but also mentions that Evans, in his 1930, had dealt with a similar issue but opted to maximize the undiscounted stream of profits.

Gerhard Tintner (1907-1983) was a profuse author in the calculus of variations literature. Tintner was born in Vienna, Austria and studied economics, statistics, and law at the University of Vienna. He was familiar with the Vienna Circle and was particularly influenced by philosophers Karl Popper (1902-1994) and Rudolph Carnap (1891-1970). In this sense, Tintner was interested in unveiling economics from ideological biases and was attracted to logical-deductive reasoning (Deistler and Deutsch 1984). In 1937 Tintner moves to America and joins the Cowles Commission as a Research Fellow; he then is hired as professor of economics, mathematics, and statistics at the Iowa State University. In 1963 he joins the University of Southern California and in 1973 he returns to Vienna. Tintner's major contributions were spread through various topics: operational analysis, stochastic processes, econometrics, and dynamic theories of demand. He wrote 10 articles using the calculus of variations early in his career from 1936 to 1942 and later worked extensively with optimal control in stochastic environments (Fox 1988, 24).

In one of Tintner's first articles to use the calculus of variations, there was already an indication as to the potential limitations of this method when one was interested in economic interpretations of the results. In *Monopoly Over Time*, published in 1937 in *Econometrica*, Tintner derived the implications of the monopoly models of Evans and Cournot in terms of elasticities. More specifically, he presented the elasticity with respect to prices (p) and price changes ($\frac{dp}{dt}$) when considering profits, revenues and demand. He defined these as "economic parameters", that is, "magnitudes that have either a definite economic meaning attached to them from previous economic discussions or are formed analogously to them" (Tintner 1937, 170). From the standpoint of profits, for instance, Tintner remarked that Cournot's formulation implied that

$$\frac{d \log \pi}{d \log p} = 0 \quad (2.21)$$

so that the elasticity of profits, in relation to prices, had to be zero. In Evans's case

this expression became:

$$\frac{d\log\pi}{d\log p'} = \frac{\pi - \pi_0}{\pi} \quad (2.22)$$

where π_0 was an arbitrary constant. This implied that the elasticity of profits with respect to changes in price was proportional to the level of prices. Tintner further remarked that if profits were independent of price changes, then one returned to the Cournot case. Tintner proposed to generalize this to a situation where demand also depended on derivatives of higher orders, that is,

$$y = f(p, p', p'', p''', \dots) \quad (2.23)$$

This general formulation implied a much more complex expression for the elasticity of profits in relation to price changes:

$$\begin{aligned} & \frac{\partial \log \pi}{\partial \log p'} + \frac{\partial \log \pi}{\partial \log p''} + \frac{\partial \log \pi}{\partial \log p'''} \\ & + \frac{p'}{\pi} \left[-\frac{d}{dt} \frac{\partial \pi}{\partial p''} + \frac{d^2}{dt^2} \frac{\partial \pi}{\partial p'''} - \dots \right] \\ & + \frac{p''}{\pi} \left[-\frac{d}{dt} \frac{\partial \pi}{\partial p'''} + \dots \right] + \dots = \frac{\pi - \pi_0}{\pi} \end{aligned}$$

While the first terms do not present major difficulties, Tintner argued that the expressions inside the brackets are very difficult to interpret (Tintner 1937, 166). He proceeded analogously for the elasticity of revenues and of demand with respect to price changes and arrived at similar results. He concluded that it would be unlikely that one could attach any economic meaning whatsoever for the expressions that result from the more general formulation. He notes that the analogies that appeared between Cournot's model and Evans's dynamic monopoly formulation gave him "some hope that, at least in the less complicated cases of monopoly over time, the treatment in economically significant terms may not be impossible" (170). Again the problem of interpreting results in terms of economic entities appeared. While Amoroso and La Volpe tried to explain their results through mechanical or physical analogies — that themselves carried several other problems —, Evans and Roos had difficulty translating their results in a form that was accepted by other economists. Tintner's later works that used the calculus of variations were on the theoretical derivation of dynamic demand curves and the theory of production under technological risk and uncertainty (Fox 2017).

Another author who referred to Evans is the Australian economist Arthur Smithies (1907-1981). Smithies obtained his PhD from Harvard in 1934 and joined the faculty of the University of Michigan in 1938. He was an advocate of Keynesian theories and researched growth, cycles, and the theory of capital (Harcourt 2016). One of

his articles, published in *Econometrica* in 1939, Smithies analyzed the behavior of the firm under changing cost and demand functions. His problem, simply put, was stated as that of maximizing expected profits in a finite interval of time, given that all of the production during this period is sold. He provided economic intuition for his problem in terms of discounted marginal costs and revenues, that should be equal at every point in time⁵². He then formalizes and solves the problem using the calculus of variations. Smithies shows familiarity with all of the main authors who used the calculus of variations in economics⁵³.

Hungarian economist Edward Theiss presented *A Quantitative Theory of Industrial Fluctuations Caused by the Capitalistic Technique of Production* at a joint meeting of the Econometric Society and Section K of the AAAS in 1932, presided by Roos. This work was published a year later in the *Journal of Political Economy* under the same title. Theiss proposes to study business cycles fluctuations as results of production lags and to formalize this idea he uses Evans's dynamic equations of supply and demand presented in his 1924 article and his 1930 book. The resulting equation for the behavior of prices includes a cosine term that serves as an approximation for observed oscillations. Theiss would later publish *Dynamics of Saving and Investment*, a theoretical study of how savings and investment dynamics can lead to business fluctuations, in *Econometrica* where he again models aggregate demand following Evans's equation ((??)). In this article, fluctuations arise as a consequence only of misalignments between savings and investment as in the case of hoarding or if the demand function has a speculative or anticipatory term - so no fluctuations arise if one uses Evans's equation.

52. Specifically, he wrote that "an entrepreneur will so distribute his production and sales as to make the discounted marginal revenue from sales and the discounted marginal cost of goods produced at every point of time both constant and equal" (Smithies 1939, 313).

53. Specifically he cites Roos (1925), Evans (1930), Hotelling (1931c), and Tintner (1937). In the footnote of the introduction of his paper he also thanks Russian mathematician George Y. Rainich (1886-1968), who was a professor at the University of Michigan at the time, and to Evans's 1930 *Mathematical Introduction to Economics*.

Chapter 3

The Calculus of Variations and Beyond

Some accounts, in the wake of the propagation of optimal control theory in economics from the 1960s onwards, judged that authors like Evans, Roos, and others that employed the calculus of variations were either underappreciated or even forgotten. Graves and Tesler (1968), for instance, claim that Evans's 1930 *Mathematical Introduction to Economics* was "insufficiently appreciated" and also mention Pixley's 1930 article on depreciation¹ as a "little known work" (307) that showed a very sophisticated mathematical treatment². Nerlove (1972) argues that Evans and Roos were "largely neglected in the main body of economic thought" (228). Specifically, they note that:

Evans (1924) and Roos (1927a) attempted to develop dynamic optimization models of the firm very early. Perhaps because these attempts were rather mechanical applications of the calculus of variations, or perhaps because the more pressing economic problems of the thirties intervened, the papers of Evans and Roos and the direction of research they represent were largely neglected in the main body of economic thought until recently. The late fifties and early sixties, however, saw a significant resurgence of interest in dynamic optimization models as a basis for dynamic economic theory. (Nerlove 1972, 228)

1. Pixley's work built upon Roos's previous 1927 article on the same topic: he considers how to choose production rates optimally when the firm can change its machines to more productive ones after an interval of time. The difficulty now is to account for the multiple discontinuities that arise in the problem. Pixley notes that his solution uses methods that were similar to those presented in Roos's 1929 *General Problem of Minimizing an Integral with Discontinuous Integrand* that was an extension of Bliss's 1925 *Lectures on the Problem of Lagrange in the Calculus of Variations*.

2. In the original quote, Graves and Tesler thank professor L. M. Graves for calling their attention to Pixley's work. L. M. Graves obtained his PhD in mathematics under Bliss in 1924 and later became Pixley's PhD advisor in 1931 at Chicago. Incidentally, both R. L. Graves and L. G. Tesler were Chicago economists.

Fox (1998) makes the case that Tintner's theory, which heavily relied on the variational formalism, and also the contributions of Evans and Roos, were forgotten by the mainstream of economics. Their theory, he argues, had become unfashionable due to the mathematical direction that eventually consolidated in general equilibrium theory, especially in the works of authors associated with the Cowles Commission. Namely, differential topology and convex analysis took the place of the calculus in a now mostly statical analysis of general equilibrium in the late 1940s (Fox 1998, 28-30). Fox's 1998 article *Econometrics needs a history: two cases of conspicuous neglect*, it should be noted, is not a historical piece but rather a plea for one. He argues that, for their institutional importance alone, more attention should have been given to Evans and Roos. He also argues that influential authors that did not cite Tintner also did not cite Roos nor Evans. Specifically, Samuelson's 1947 *Foundations*, the leading book on economic dynamics, did not mention Tintner, Evans or Roos³.

The renewed Cowles approach, mentioned by Fox, resembled little of the theories of past research director Charles Roos. Roos, as discussed above, served as the first research director of the Cowles Commission from 1934 to 1937. He was also the author of the first two Cowles monographs: *Dynamic Economics* published in 1934 and *NRA Economic Planning* published in 1937. Tintner was a research fellow at Cowles in 1936 and became part of the staff the following year. He wrote a Cowles Monograph on time series analysis named *The Variate Difference Method* that was published in 1940⁴. The Cowles Commission - and postwar mathematical economics more broadly - favored a different kind of mathematical formalism, especially when dealing with general equilibrium theory, closer to convex analysis and set theory. Mirowski and Hands (1998) credit this inflection to the recruitment of Polish economist Oskar Lange (1904-1965) who hired Polish-American economist Leonid Hurwicz (1917-2008) in 1942 and Ukrainian-American economist Jacob Marschak (1898-1977) in 1943. Marschak served as research director at Cowles from 1943 until 1948 and had an immense impact on the Commission's research priorities: after his entrance, the Cowles Commission became first and foremost the center for

3. It should be recalled that Samuelson knew these works as did his intellectual mentor, E. B. Wilson. Samuelson mentions Evans as "one whose works he 'pored over' when working on the *Foundations of Economic Analysis*" (Simon 2008, 2, italics in original).

4. The "variate difference method", as explained by Tintner, assumes that any time series is an additive function of two components: the first is given by the mathematical expectation and reflects "permanent causes in economic life"; the second, is a purely random component that gives the "effect of the nonpermanent causes which work in economic life". The method consists of using finite differences of the original series to eliminate the mathematical expectation (the non-random component) and compares the variances between the original and the differenced series to find the correct order of finite difference. The permanent component is then approximated by a smooth function and the method, in general, can be used to estimate standard errors, and correlation coefficients between series. For more on the method, see Tintner (1940, 1-21).

Walrasian general-equilibrium formalized with differential topology rather than the calculus. Mirowski and Hands (1998) argue that both Roos and Davis, while supporters of mathematical economics and econometrics, were not “enamored of Walras or Pareto” and that previously to Marshack the Commission’s studies “tended toward practical studies of financial assets and government policy” (274).

Pomini (2018) raises a similar argument to explain the gap between the first works in dynamics in the 1920s and 1930s, using the calculus of variations, and the theories of the mid-1960s that centered on optimal control and Hamiltonians. The works of Evans, Roos, and Italian mathematical economists Giulio La Volpe, and Luigi Amoroso became out of sync with the mathematics that prevailed in postwar general equilibrium theory. The latter two were keen on developing a dynamic theory in the Walrasian general equilibrium scheme, which proved to be an intricate task that imposed interpretative limitations on their theory (76). Not only this, but these authors tried to establish mechanical analogies for their mathematical models that lacked economic meaning. At the same time, dynamical analysis was incorporated into the Keynesian tradition through the works of Hicks in 1939 and Samuelson in 1947. Their approach not only provided a more unambiguous economic interpretation for dynamics, in terms of marginal rates of substitution and balanced growth paths, but also avoided more sophisticated mathematics (Pomini 2018, 76-77). Samuelson’s dynamic theory used differential equations and analyzed stability conditions around steady states. This ironically led to a statical analysis of stable and unstable equilibrium⁵, where equilibrium was understood as the limiting point of the dynamic system (Weintraub 1991, 101-104). The neoclassical synthesis, in the 1950s and 1960s, incorporated this analysis in the Walrasian tradition and related equilibrium to competitive conditions (120-124). Economists by this time, thus interpreted dynamics differently from Evans and Roos.

Recall that the Euler-Lagrange equation provides the necessary solution to a properly defined problem in the calculus of variations. The solution to the Euler-Lagrange equation gives the variable of interest as a function of time and describes its entire trajectory. For concreteness, suppose it is of the form:

$$y(t) = y_0 + K_1 e^{r_1 t} + K_2 e^{r_2 t} \quad (3.1)$$

Using either boundary or transversality conditions one can determine all constants in the above equation and arrive at the optimal curve associated with the maximization/minimization problem at hand. In this sense, stability conditions aren’t

5. The absolute value of the eigenvalues associated with the dynamic system determined its stability in Samuelson’s analysis. A dynamic system was considered stable if all small perturbations around an equilibrium point (or steady state) was followed by a return to the same equilibrium (Boumans 2009, 130-143).

the focal point and differential equations are derived from the optimization problem as a necessary condition. Roos argued that a dynamic theory “must recognize that it is in the nature of economic phenomena to change” (Roos 1934, 7) and that static equilibrium conditions are never completely achieved. An accurate dynamic conception of economics, in his view, had to consider the process of change of economic variables and the interrelations of these changes through time⁶.

Samuelson’s analysis began with differential equations and explored its limiting properties in terms of stability. He also put greater emphasis on difference equations indexed by integer values (i.e., in discrete rather than continuous time) whereas Evans and Roos used only continuous time⁷. Pomini (2018) argues that Samuelson’s formulation of dynamics was not only more straightforward, in the mathematical sense, but also provided a more precise economic intuition. Wulwick (1995) notes that the calculus of variations brings several “disanalogies”: perhaps the most problematic is the conservation principle, which has a meaningful interpretation in mechanics but finds no clear metaphor in economics.

Polish mathematician Isreal Herstein (1923-1988) published a survey of mathematical economics in 1953. He mentions Samuelson’s dynamics as a prime example of the application of differential equations in economics. Of works using the calculus of variations he cites Roos, Hotelling, and Evans⁸. He references also the work of German economist Martin J. Beckmann (1924-2017) on transportation. Specifically, he mentions *A Continuous Model of Transportation*, published in 1952 in *Econometrica*. Beckmann states his problem as that of finding the optimal vector giving at each point the direction of shipment and the density of its flow (Beckmann 1952, 645). Thus, he has a two-dimensional objective functional and his problem is no longer in the “classical” calculus of variations. In the introduction to his article he thanks both Herstein and Tjalling Koopmans (1910-1985): he also explains that his article was developed while he was a fellow of the Economics Department at the University of Chicago and a staff member of the Cowles Commission.

There is, however, a gap between mathematical textbooks of the 1950s and 1960s and those from the 1970s and further on. Allen’s 1956 *Mathematical Eco-*

6. Roos also noted that the concept of equilibrium had to change in a dynamic theory. He gave a physical and biological analogy for this: “static equilibrium may be compared to the equilibrium of grains of sand on the seashore and dynamic equilibrium to that of gas molecules at constant temperature, pressure, and volume. More accurately, perhaps, dynamic equilibrium might be compared to that of the various parts of the human body: individual cells may live, grow, stagnate and then die, and yet the general appearance and condition of the body remain the same, at least over short periods of time ...” (Evans 1934, 8)

7. The theory of the calculus of variations was entirely built in continuous time. A discrete time version of the calculus of variations was developed only much later in Cadzow (1970).

8. Specifically, he cites Hotelling’s 1931 article on exhaustible resources, Evans’ 1924 article on the dynamic problem of the monopolist, and Roos’ 1925 *Mathematical Theory of Competition* that is a generalization of Evans’ 1924 paper.

nomics does not present the calculus of variations, unlike his 1938 textbook that included two chapters on dynamic optimization using variational techniques. To be sure, *Mathematical Economics*, as stated in its preface, is not intended to be an updated edition of *Mathematical Analysis for Economists* but a new book entirely. Allen argues that mathematical economics changed significantly in the period between the two books. He emphasizes the recent development of econometrics, the widespread usage of statistics, and the rise of Keynesianism. Indeed his presentation of economic dynamics is now formalized with differential equations and aggregate variables within a Keynesian framework: he presents the acceleration principle of Hicks and Samuelson, the cobweb model, the stock and flow model, etc. To mention another example, Bushaw and Clower (1957) also presents dynamics in a Keynesian framework using differential equations and *lagged* equations (difference equations). This provides some evidence that economic dynamics had now been framed in an entirely different manner following the very influential *Foundations*.

Some authors argue that Evans's legacy was hampered by his unwillingness to adopt utility functions. Dimand and Veloce (2007) argue that economic theorists preferred to avoid Evans's challenges to subjective utility theory, specifically his contention with the integrability conditions of demand functions⁹. Evans's critique of utility, as shown previously, was not only philosophical but also had a mathematical underpinning based on the stiff constraints forced by the integrability conditions of the demand function. Perhaps more importantly, Evans holds that utility is a stale concept because it is neither observable nor quantifiable: "[t]here is no such measurable quantity as 'value' or 'utility' (with all due respect to Jevons, Walras and others) and there is no evaluation of 'the greatest happiness for the greatest number'; or, more flatly, — there is no such thing" (Evans 1925b, 110). In this sense, Weintraub (1998) argues more broadly that Evans became a marginal figure in postwar economics due to the changing image of mathematics. Mathematical rigor was no longer an empirical question but a logical one. It was not so much that Evans abandoned mathematical economics "but that mathematical economics, increasingly connected to the new (very un-Volterra-like) ideas of mathematical rigor in both mathematics and applied mathematical science, moved away from Evans" (Weintraub 1998, 256)¹⁰.

9. The authors also explore the role of Roos and his colleague Harold Davis as research directors in the early years of the Cowles Commission, in the 1930s. They argue that their econometric approach, based on curve-fitting and harmonic analysis of time series, was plagued by identification and interpretation issues. The probabilistic approach, put forward by Norwegian economist Trygve Haavelmo (1911-1999) — also a member of the Cowles Commission — overtook these methods in the late 1940s.

10. Weintraub (1998) adds that "The point is that Evans's views on mathematical modeling are the views of an econometrician or applied economist today or one who insists that the assumptions and conclusions of an economic model, a model constructed and developed mathematically, must

Another argument put forward is that Evans's work was too advanced, mathematically, for the economists of the 1920s and 1930s. Hotelling argued that Evans's work (and his, by extent) was too mathematically advanced and thus inaccessible to the majority of the economists of the 1920s and 1930s¹¹. Contemporaries of Evans and Roos seem to concur that their work was mathematically challenging for economists. Wright (1931), in particular, corroborates Hotelling's reading stating that Evans's mathematical ideas would "strike terror into the hearts of most economists" (94).

The aforementioned Austrian mathematician Karl Menger surveyed applications of the calculus of variations in 1937 in the *The Scientific Monthly*. He argues that the calculus of variations allows one to model maximal/minimal principles and mentions specifically the works of Evans, Roos, and Hotelling. He also seemed to concur with Evans's sentiment that the linkage between the calculus of variations and economic theory was more than "accidental", that is, he states that:

If we speak of tendencies in nature or of economic principles of nature, then we do so in analogy to our human tendencies and economic principles. A producer most often will adopt a way of production which will require a minimum of cost, compared with other ways of equal results; or which, compared with other methods of equal cost, will promise a maximum return. It is obvious that for this reason the mathematical theory of economics is to a large extent application of calculus of variation. (Menger 1937, 251-252)

Evans's and Roos's works were also appreciated by other economists, mostly those affiliated with the Econometric Society or the Cowles Commission. Amongst these, one can mention Gerhard Tintner and A. Smithies. The main point I wish to emphasize is that Evans and Roos managed to find a public for their advanced mathematics. These economists and mathematicians understood the calculus of variations and could likewise apply it to other facets of economic theory. The apparent gap between this earlier literature and the optimal growth or dynamic programming literature from the 1960s is not as empty as one imagined it to be. It is also worthwhile to mention the authors that read Evans's dynamic theory and who decided to formalize dynamics in a different fashion¹².

be measurable or quantifiable. This distinction between 'modelers' (or 'applied economists') and 'theorists' divides modern departments of economics even as both groups consider themselves to be neoclassical economists" (256).

11. Hotelling (1925, 352), for instance, acknowledges the importance of Evans's work but also comments that a thorough understanding of his theory demanded a solid background in the calculus of variations. For this reason, Hotelling argued, that the topic had not developed further.

12. Also worth mentioning is a series of historical contingencies that curbed these authors abilities to follow this line of work. H. L. Moore retired from research after his 1929 *Synthetic Economics* due to ill health. Both Ramsey and Schultz died prematurely in 1930 and 1938 respectively.

A significant exception to the — for lack of a better word — “forgotten” narrative is the British mathematician Frank Ramsey. While he only published a single article using the calculus of variations in 1928, economists of the 1960s and 1970s contended that Ramsey was a “genius ahead of his time”. Ramsey’s article, they argued, had failed to impact the economic theory of his time due to its mathematical sophistication. Duarte (2009) details the intermission between Ramsey’s savings article and his “rediscovery” in the 1960s and argues that he was never forgotten. In fact, as mentioned previously, his work was acknowledged as early as 1931 by Hotelling¹³. To help explain Ramsey’s later canonization Duarte does contrast him with Evans emphasizing how Ramsey used a mathematical formalization that came to be greatly appreciated in the postwar period, namely a utility maximizing mathematical model with normative implication for welfare economics. As mentioned previously, Hotelling’s 1931 article on exhaustible resources, in similar fashion to Ramsey’s, became widely acknowledged only in the 1970s.

As argued above, Evans’s work was not “forgotten” nor misunderstood. Various contemporaries of his read his work and some even extended it further. As noted before, Evans’s work was influential to his PhD students at Berkeley in the 1930s through the early 1950s. His work had a substantial impact on dynamic economics. His work had a repercussion on Italian economists that followed Pareto’s attempts to embed Walras’s general equilibrium equations in a dynamic framework. As the optimal growth literature firmly established itself in mainstream economics standard textbooks on the calculus of variations and optimal control¹⁴ mention most of the pioneers in the calculus of variations. Chiang (1999) goes as far as to present the “classic Evans model of a monopolistic firm” (49) that follows Evans (1924) closely. The entry on “calculus of variations” by Morton Kamien (2008) in the *New Palgrave Dictionary of Economics* mentions the works of Ramsey, Evans, Roos, and Hotelling. Postwar mathematical economics, unlike Evans and Roos, used utility functions, but so did Ramsey, Hotelling, and other authors that used the calculus of variations in economics.

The calculus of variations had mathematical limitations that made it difficult to model some economic phenomena. Thompson and Proctor (1969) and Thompson et al. (1971) discuss some of these issues. The latter was published in *Econometrica* under the title *Optimal Production, Investment, and Output Price Controls for a*

Fascism in Italy also diverged the attention of Amoroso to problems of corporatist economies and their contradictory relation with general equilibrium schemes of competition (Keppler 1994).

13. The connection between Ramsey and Hotelling is often seen as very close (see for instances of this Gaspard and Missimer (2019, 1-2) though Gaspard and Missimer (2019) find little evidence for this. They argue, however, that the authors shared an “intellectual project” and a “vision of pure mathematics for research and policy” (22).

14. I considered as representative examples: Kemp and Hadley (1971), Kamien and Schwartz (1981), Chiang (1999), and Intriligator (2002)

Monopoly Firm of the Evans' Type. The article begins with Evans's formulation of the monopoly problem to show the difficulties one faces when formalizing a dynamic optimization problem using the calculus of variations. Inequality constraints, discontinuities and more general discrete problems become very cumbersome or intractable. In this sense, the authors translate Evans's monopoly model to both an optimal control and a dynamic programming framework to better accommodate these hypotheses. Economist Kenneth Arrow (1921-2017) had already voiced a similar comment on the shortcomings of the calculus of variations. In *Studies in the Mathematical Theory of Inventory and Production* he notes that the mathematical formulation he proposes is, essentially, equivalent to the calculus of variations. He notes that "the tools of the discipline [calculus of variations], however, are not directly applicable, because of inequality constraints. It is therefore necessary to build up the solution by composing it partly of the solution provided by the calculus of variations when ignoring the inequality restrictions, and elsewhere fitting the solution to the constraints." (Arrow, Scarf, and Karlin 1958, 38).

Graves and Telser (1968) is a paper on the differences between discrete and continuous time approaches to optimization problems. They use the calculus of variations to solve a monopolist problem akin to Evans's. Their main argument is that the monopolist problem changes fundamentally when framed in a discrete time manner. In particular, the continuous approach is incompatible with uncertainty as the smoothness requirements (namely, that the first derivative be continuous) imply that the slope of curve is the same when examining it from the right or the left. That is, in the neighborhood of a point one can always know the past and future values of the function. This need not be true in the discrete time-case; the random walk model, for instance, traces a continuous curve that doesn't have a derivative at any point so one can never tell the direction the function is heading towards. This also implies a symmetry between past and future that makes them virtually indistinguishable. A discrete model, on the other hand, forces one to take into account the irreversibility of past decisions and the effects of uncertainty.

Another possible explanation for the success of optimal control, and moreover of dynamic programming, is that the latter were much more operational than the calculus of variations. Bellman (1957) argues this issue extensively. Deriving the Euler-Lagrange necessary conditions in a variational problem generally leads to a second order partial differential equation that lead to computational difficulties. As mentioned previously, the recursive formulation of dynamic programming makes for easier implementation and, in some cases, offers a more pertinent interpretation in the form of a policy rule. Indeed, in many articles and most notably in his 1957 book, Bellman shows how his novel formulation generalizes the optimization problem from the calculus of variations. He also argues extensively on how his approach is more

amicable to applications: it incorporates more easily important aspects to economic modeling such as uncertainty, discontinuities, linear and non-linear restrictions, and since it already is formulated as discrete time problem it eases computational implementation. In 1955, Bellman publishes an article in *Econometrica* showing how one could model “bottle-neck” problems (i.e. problems in which the factor in shortest supply determines production) first in continuous time, using the calculus of variations, and then using dynamic programming as an approximation to find a solution. His complete model is too big to reproduce here, but in broad terms, one wishes to find the optimal production schedule when industries are interrelated; in his example: steel, tools (made of steel), and automobiles (made of tools and steel). In the last section of his article, Bellman notes that a large variety of economic problems reduce to the maximization of integrals of the form:

$$\int_0^T F(x_1, x_2, \dots, x_n, z_1, z_2, \dots, z_n) dt \quad (3.2)$$

where $x_i(t)$ is the production of commodity i at time t and $z_i(t)$ is its rate of change (first derivative with respect to time)¹⁵. This functional faces constraints of the form:

$$R_k(x_1, x_2, \dots, x_n, z_1, z_2, \dots, z_n) \leq 0$$

This section is short and Bellman refers the reader to Evans’s 1930 mathematical textbook and to Roos’s 1934 *Dynamic Economics* for a complete discussion on the matter. This is perhaps the most clear linkage between the “old” literature, focused around classical problems in the calculus of variations, and the “modern” literature that follows Bellman’s formalization.

15. To be sure, Bellman does not make this notation completely clear but this interpretation is consistent with the notation used in previous sections of the article. Specifically, I followed the notation presented in section 4 of his article. For more details one should refer to Bellman (1955, 76-77).

Chapter 4

Concluding Remarks

The calculus of variations came to economic theory to formalize dynamics: it was used to model depreciation, intertemporal savings and consumption, competition, and also more broad general equilibrium. Most economists lacked the necessary mathematical instruction to comprehend these models so they passed the 1930s and 1940s in relative obscurity. As was shown, however, there were a handful of mathematicians and mathematically inclined economists who did appreciate these new dynamical theories. On the matter of economic dynamics, mathematical economics strayed away from the calculus of variations. Samuelson's formalization, in particular, was deemed simpler and provided sounder economic intuition in contrast to some of the physical "disanalogies" that came along with variational calculus. In the late 1960s dynamic optimization came back with the popularization of optimal control and dynamic programming. This sprouted a renewed interest in the calculus of variations but it was now seen as a stepping stone to these modern techniques. Dynamic programming, in particular, was better suited to deal with uncertainty, discontinuities, and restrictions to the optimization problem. Its iterative, step-by-step solution, also made its policy interpretation easier and facilitated its computational implementation.

The history of dynamic optimization in economics begins in the 1920s and seems to halt in the 1940s, returning only in the late 1960s. I've attempted to show that this did not necessarily result from mathematical ignorance: important economists such as Paul Samuelson were fully aware of this kind of work, but chose to formalize economic dynamics differently. Secondly, I've tried to suggest that the calculus of variations, as a mathematical technique to solve optimization problems, was limited in its potential applications. Dynamic programming, as a new formalism for the calculus of variation, made it more amicable to relevant applied problems in economics and engineering. Also, when the calculus of variations became fashionable, mathematical economics and even econometrics had both changed considerably from what they were in the 1930s when Roos set to create the Econometric Society. Economic

thought had now somewhat converged around the neoclassical framework of utility maximization. General equilibrium was now also formalized with a different kind of mathematics altogether. Despite this,

I've shown how Evans and Roos brought the calculus of variations to economics in an attempt to build a dynamic economic theory. Evans began with a simple monopoly model, borrowed from A. Cournot, and following his own methodological standard developed a mathematical model of competition. Ross, following this lead, generalized these results and arrived at the theoretical conditions to sustain a general equilibrium in a variational framework. At their time, non many economists were mathematically equipped to understand their work; the few who were realized its significance, though they clearly ascribed differing levels of importance to it. In Italy, L. Amoroso e G. LaVolpe followed Evans's project to bring dynamics into economic theory. They did so more in line with the prevailing subjective theory of value, that is, they assumed rational individuals that maximized utility over time. Most notably, Samuelson chose a different route entirely to bring dynamics to economics. General equilibrium theory also followed a different path. Mathematics in this sense, did not bring unity, as Evans is an example of a sophisticated mathematical model that didn't follow the other rules of neoclassical economics.

Chapter 5

Appendix

5.1 List of Ph.D dissertations in mathematical economics Evans at UC Berkeley

1. John Murray Thompson. *Mathematical theory of production states in economies*. 1936
2. Francis William Dresch. *Applications of Index Numbers to the Study of General Economic Equilibria*. 1937
3. Ronald William Shephard. *I. Incidence of taxation studied in a simplified economic system. II. A note on the length of production*. 1940
4. Kenneth O. May. *On the Mathematical Theory of Employment*. 1946
5. Edward Allan Davis. *A Dynamic Economic Theory*. 1951

5.2 Irving Collier Notes Online

1. Harvard Intro to Mathematical Economics 1935-1942. Description: Graduate classes in Mathematical Economics (Econ 13b in 1934-35, Econ 104b in later years) were taught every second year by Edwin Biddle Wilson (1934-35, 1936-37, 1938-39, 1940-41, 1942-43). An introduction for undergraduates and graduates was offered by Joseph Schumpeter in 1934-35 (Econ 8a), but the course was taken over and offered for nearly a decade by Wassily Leontief (new course number beginning 1936-37, Econ 4a). In this posting you will find different scraps from the Schumpeter/Leontief course over the years. Literature: (1) Cournot - *Researches*; (2) Walrás - *Elements*; (3) Pareto - *Cours e Manuale*; (4) Fisher - *Mathematical Investigations*; (5) Edgeworth - ; (6) Marshall - appendix to *Principles*. Obs: under “no good textbook” Bowley and

- Evans. Source: Harvard University Archives. Wassily Leontief Papers. HUG 4517.30, Box 5, Folder "Introduction to Mathematical Economics (notes)"
2. Reading assignment: Evans chapter II ("On Change of Units"). Leontief 1938-1940. Source: Harvard University Archives. Syllabi, course outlines and reading lists in Economics, 1895-2003. HUC 8522.2.1, Box 2, Folders "1937-1938", "1938-39", "1939-40". Source: Harvard University Archives. Syllabi, course outlines and reading lists in Economics, 1895-2003. HUC 8522.2.1, Box 3, Folders "1941-1942". "1942-1943 (1 of 2)".
 3. Leontief 1941-1943. Evans, Allen, and Cournot.
 4. Reading assignment: chapters I-III Evans: Elementary theory of monopoly, On change of units, Competition and Cooperation. Source: Harvard University Archives. Syllabi, course outlines and reading lists in Economics, 1895-2003. HUC 8522.2.1, Box 3, Folder "1941-1942".
 5. Harvard graduate mathematical economics by R. Goodwin. Lectures on static market equilibrium. Dynamical aspects are framed as problems in linear differential equations: acceleration principle, cobweb model, etc. Readings: P. Frank, Foundations of Physics, Parts I, II and Section 14. P. A. Samuelson, Foundations of Economic Analysis, Chs. I, II, IX, and X, pp. 302-307 R. M. Goodwin, "The Multiplier," in The New Economics, ed. S. E. Harris Dynamics is presented as linear dynamic systems: J. Tinbergen, "Econometric Business Cycle Research," in Readings in Business Cycle Theory. P. A. Samuelson, "Interactions between the Multiplier Analysis and the Principle of Acceleration," in Readings. Ibid., "Dynamic Process Analysis," in A Survey of Contemporary Economics, ed. by H. S. Ellis. Ibid., Foundations of Economic Analysis, Chs. XI and XII.
 6. Introduction to the mathematical treatment of economic theory. References Evans in the readings list but unsure as to whether dynamical topics are viewed with the calculus of variations.
 7. Harvard undergraduate mathematical economics 1933-1950 <http://www.irwincollier.com/harvard-undergraduate-mathematical-economics-schumpeter-leontief-goodwin-1933-1950/>
 8. Hotelling. Mathematical economics at North Carolina. 1946-1950.
 9. Marschak. Mathematical introduction to economics. Cites Allen, Bowley, Evans. amongst others. Dynamics: Whitman, Kalecki, Tinbergen, Frisch.

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