

A. G. PAPANDREOU'S ACADEMIC ECONOMIC THOUGHT 1943–1963

BY

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The aim of this article is to make an overall assessment of Andreas George Papandreou's theoretical contributions during his American academic career, from the perspective of the history of economic thought. Papandreou contributed to the postwar development of economic thought in competition theory and experimental testing of consumer theory. In developing competition theory, he introduced a new method of evaluating the monopolistic power of a firm through a coefficient measuring the firm's penetration in the market. Furthermore, he suggested a way of experimentally testing whether individual preferences satisfy the axiom of transitivity. He actively participated in the methodological controversies on the realism of economic assumptions that took place between 1946 and 1953, and on the empirical meaning of economics in 1963, between Milton Friedman, Paul Samuelson, Fritz Machlup, Herbert Simon, and others.

I. INTRODUCTION

Andreas George Papandreou (1919–1996) is known worldwide as a socialist political leader of Greece who served three times as prime minister from 1981 to 1996. He is less known as an economist who had a successful academic career in the United States between 1947 and 1963. Yet, there is no critical evaluation of his theoretical contributions, except two hagiographical assessments (Kariotis 1997; N. Papandreou 2018), a partial account (Moscati 2007), and a short general appraisal (Psolidopoulos 2010). The aim of this article is to make an overall assessment of Papandreou's theoretical contributions during his US academic career, from the perspective of the history of economic thought. He was also the discussant of important American economists such as Paul

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Samuelson, Fritz Machlup, Richard Heflebower, John Kenneth Galbraith, Herbert Simon, Leonid Hurwicz, and Kenneth Arrow, and was much appreciated by his younger peers such as Dale Jorgenson and Roy Radner.¹

The two postwar decades were extremely productive for economic theory. Paul Samuelson's *Foundations* (1947) marked the beginning of a new era, called "neoclassical synthesis." Still, in many subfields of economic analysis there were innovative contributions defying the boundaries of neoclassical synthesis, in general equilibrium theory (Kenneth Arrow-Gerard Debreu), in game theory (John von Neumann and Oskar Morgenstern, John Nash, Leonard Savage), in organization theory (Simon, Galbraith), in monetary theory (Milton Friedman), in macroeconomic theory (John Muth, James Tobin, Robert Solow), and in welfare economics (Arrow, Amartya Sen, Tibor Scitovsky). How did Papandreou participate in all that? In which fields? What was the impact of his contributions? After his arrival in New York in May 1940, escaping from Greece's dictatorial regime, he aimed to continue his law studies in Columbia.² He changed his mind and moved to Boston to enroll as a graduate student of economics at Harvard in September 1940 (Makrakis 2000; Draenos 2012). After getting his PhD in 1943, he earned US citizenship after serving in the navy for thirty months. His first academic position was at the University of Minnesota from 1947 to 1950. Then he moved to Illinois (Northwestern) for one year before returning to Minnesota as a professor from 1951 to 1955. Next he moved to California, to hold the Chair of Economics at Berkeley, from 1955 until 1963. He returned to Greece twice, in September 1959 and in January 1961, on academic leave, to manage the newly created Center of Economic Research. Two years later, he took his first political steps as an MP and then as a minister in his father's government until July 1965. After the military coup in April 1967, and his release from prison, he had a second academic career in Stockholm and Toronto during his new political exile (1968 to 1974), when he produced heterodox political economy texts, beyond the scope of this article.

Papandreou deserves to be mentioned in the postwar history of economic thought for his contributions in organization and competition theory and his experimental work on consumer theory. In competition theory, he suggested a new method for evaluating monopolistic power of a firm through a coefficient measuring its penetration in the market. In consumer theory, he proposed a way of testing experimentally whether individual preferences satisfy the axiom of transitivity. Moreover, he participated in the great postwar methodological controversies on the realisticness of economic assumptions (1946 to 1953) and on the empirical meaning of economics (1963) in the debate between Friedman, Samuelson, Machlup, Simon, and others. From 1964, his alternative mainstream views evolved dramatically towards radical political economy and neocolonial dependency theories once he was engaged in Greek politics, especially after his second political persecution.

¹ While serving as a chair of the economics department in Berkeley (1956 to 1959), Papandreou recruited many promising economists such as Gerard Debreu and Roy Radner (N. Papandreou 2018), Tibor Scitovsky (in 1958), Dale Jorgenson and Daniel McFadden (Stearns 2021), and Oliver Williamson (Mahoney and Nickerson 2021).

² Young Andreas was arrested and tortured by the police in April 1939 as a member of a Trotskyist political group, and after his release he decided to leave Greece (Stearns 2021, pp.16–18; Papandreou 1970).

A brief account of the state of neoclassical economics is given in the next section of this article in order to contextualize Papandreou's theoretical debuts. His effort to study the evolution and the power of the firm is analyzed in [section III](#). The next section evaluates his involvement in the experimental research stream of neoclassical consumer demand theory in the 1950s. His constant interest in the empirical relevance of economics is presented in [section V](#). The last section considers his transformation after his strong political engagement and concludes by assessing Papandreou's intellectual journey.

II. THE STATE OF NEOCLASSICAL ECONOMICS IN THE EARLY 1940s

In 1940, when the twenty-one-year-old law student arrived in New York, mainstream economics was in disarray and in the making. From the early 1930s to the late 1940s, neoclassical microeconomics was contested theoretically, methodologically, and factually. In 1933, two outstanding young scholars—Edward Chamberlin and Joan Robinson—tried (independently) to build a theoretical alternative to the economics of full competition based on product differentiation and price discrimination (Robinson 1932; Schumpeter 1950, p. 1150; Tsoulfidis 2010, pp. 227–233). Furthermore, neoclassical microeconomics was challenged from both the demand and the supply sides. Between 1932 and 1938 Harold Hotelling and Henry Schultz unsuccessfully tried to derive empirically a consumer demand curve on the grounds of utility maximization theory. Using different techniques (see Mirowski and Hands 1998, pp. 262–266), Hotelling and Schultz aimed to strengthen the empirical foundations of the theory of demand, in order to overcome its disclaimer from the dramatic experiences during the Great Depression. From the side of the theory of supply, Ronald Coase criticized the lack of a theory of the firm in the neoclassical model of zero transaction costs. Because these costs form an important part of the real business world, durable profit-oriented organizations exist to minimize their burden. As he wrote, “The main reason why it is profitable to establish a firm would seem to be that there is a cost of using the price mechanism” (Coase 1937, p. 390). As well, the strong influence of the original American institutionalism (Veblen, Commons, Mitchell, and Ayres) in the US academia in that period defied the supremacy of neoclassicism as the mainstream approach (see Bateman 1998; Backhouse 1998; Biddle 1998).

Methodologically, in the late 1930s the fundamental behavioral assumption of economics was severely disapproved of for its poor empirical relevance. In 1937, Friedrich von Hayek (1937) raised the epistemic “problem of knowledge,” criticizing the idea of the “quasi-omniscient individual” who knows everything that is needed to reach equilibrium. John Maynard Keynes, as well, opposed the idea of certain knowledge, in his response to the reviewers of his *General Theory* in the *Quarterly Journal of Economics*. In 1938, Terence Hutchison (1960) launched his full-scale attack against the methodological foundations of neoclassical theory and introduced the Popperian criterion of testability. He qualified the assumption of maximization under constraint as unworkable without the secondary, quite restrictive, assumption of “perfect expectation” that guarantees that this kind of behavior will indeed result in a situation whereby the economic agent would attain a maximum. Robert Hall and Charles Hitch (1939) showed that entrepreneurs do not care to maximize profits, as the marginalist

theory claimed. In their empirical survey, Hall and Hitch confirmed that entrepreneurs set their prices not by equating the marginal cost to the marginal revenue but simply by adding a markup to the average cost of production at full capacity. Seven years later, Richard Lester (1946) empirically tested the marginalist principle and proved that business executives tend to adapt their rate of output based on the cost of production and the aspired level of profit. Lester's paper initiated a new round of the marginalist controversy, in which Papandreou took part, as seen below.

The rise of the US corporations in the 1920s marked another major challenge of the neoclassical model. New theoretical insights from the organizational point of view were provided by Adolf A. Berle and Gardiner C. Means (1932) and by Chester Barnard (1938) in order to capture a phenomenon still invisible to the mainstream: the idea of internal hierarchy inside the firms (Ménard 2005). From an evolutionary point of view, Joseph Schumpeter (1942, ch. 8) recognized that monopolistic practices, despite their negative effects upon consumer prices, promote stability and increase efficiency. The rise of big firms transforms the entrepreneurial function and revolutionizes production by exploiting innovation that guarantees their profitability in the long run. Unfortunately, held Schumpeter, the success of capitalism undermines the social conditions that sustain it and grows the seeds of its final destruction. In this particular intellectual milieu, Papandreou made his first academic steps.

III. APPRAISING THE EVOLUTION AND THE POWER OF FIRMS

Papandreou's PhD thesis at Harvard (October 1943),³ under the supervision of the statistician William L. Crum, was an attempt to study the progress of the entrepreneurial function from a historical and organizational point of view. In a period when a theory of the firm was still lacking, since the mainstream economists paid no attention to its internal organization, Papandreou studied the evolution of entrepreneurship from the medieval European guilds to the US corporations of the 1940s. What is more, he openly challenged the established view of decision making under certainty and perfect knowledge by focusing on the conditions of uncertainty that prevail in entrepreneurial decisions. As he emphasized, "Uncertainty is the essence of the definition of the entrepreneur" (1943, p. 15), preceding Herbert Simon's view (1947) four years later. After discussing at length Frank Knight's idea of uncertainty and Schumpeter's concept of the innovative entrepreneur, he defined the entrepreneurial function "as the making of profit-maximizing decisions, whose outcome is dependent on the unregulated forces of the economy" (1943, p. 37), especially under conditions of "uncertainty and unpredictability" (1943, p. 23; Hébert and Link 2009, p. 5).⁴ His doctoral thesis aimed to divulge and explain the shift of the entrepreneurial function from the owners of the firm to its directors. This "process of displacement," as he named it (1943, p. 72), relates to the original idea of Berle and Means, who developed the Veblenian view of the

³ While at Harvard, he attended Chamberlin's graduate course on economic theory, in 1940–41. He also followed the courses of Alvin Hansen, A. P. Usher, and Gottfried Haberler but not Schumpeter (Makrakis 2000, pp. 76–77; Psalidopoulos 2010).

⁴ Papandreou also discussed Maurice Dobb's *Capitalist Enterprise and Social Progress* (1926) and rejected it as being too universalistic.

problems arising from the separation of ownership and control of the firm (see Galbraith 1952; Williamson 1985, p. 299).⁵ Papandreou suggested an evolutionary explanation to deal with “the phenomenon of the delegation of the entrepreneurial function” by the owners to the managers (1943, p. 128). What he called “indirect entrepreneurship” follows the evolution of the economic system to the stage of “monopolistic-competitive capitalism” when the “very large corporations with a more or less widely dispersed stock-ownership and intricate problems of administration of a significant volume of wealth” do prevail (1943, p. 149). Like Berle and Means (1932, p. 139), he also highlighted the meaning of the independence of the board of directors vis-à-vis the shareholders of a firm. But unlike Berle and Means, he believed that “it is the appearance of the large-scale corporate unit that finally precipitated the split of the property atom that separated ownership and entrepreneurship” (1943, p. 160. See Williamson 2008). From the moment that large firms are financed through the public’s money through the stock market, the real power of ownership depends on the concentration of securities in the hands of a single entity or a group of holders.

As Papandreou wrote, “The extent to which ownership exercises the indirect entrepreneurial function is in direct proportion to the number and size of blocks of stock that are held by individuals, again in the case of a corporation whose stock has a large public market” (1943, p. 171). This is why he considered in detail the 200 largest non-financial corporations, in order to discover the various types of corporate control (1943, pp. 257–286). He even attempted to measure statistically the degree of separation of ownership from the entrepreneurship in these corporations (1943, p. 303), concluding that in 94 out of 200 firms, ownership was fully or almost fully separated. While his descriptive empirical study confirmed the main findings of Berle and Means, Papandreou opposed their negative appraisal of “concentration of economic power,” as this historical phenomenon “is paving the way to the emergence of another status quo in the evolution of the function” of entrepreneurship, the one that “will be exercised by the state itself” (1943, p. 325). Papandreou predicted that in the future, state entrepreneurship will increase significantly, as it is hoped that the entrepreneurial function would move “from the private to the public sphere” (Psalidopoulos 2010, p. 346).

This evolutionary view of the firm co-existed in Papandreou’s thought with his theoretical view of the structure of the market. This is more obvious in his literature review paper of 1952, “Some Basic Problems in the Theory of the Firm,” where the latest developments in organization theory are examined in conjunction with the studies on the external environment of the firm; i.e., the social frame of the market.⁶ He adopted Neil Chamberlain’s idea on “the degree to which labor is in a position to exercise influence over the executive organization of the firm” (Papandreou 1952, p. 39). His main concern was to apprehend the structure of the external influence in

⁵ In fact, Veblen (1904) rediscovered the classical view of the principal-agent problem. Adam Smith ([1776] 1976, vol II, p. 264) wrote about the directors of joint stock companies who are “the managers rather of other people’s money than of their own” and for that reason do not have “the same anxious vigilance with which the partners in a private copartnery frequently watch over their own.” Similarly, J. S. Mill ([1848] 1973, p. 139) has pointed out that the “quality of the hired servants” is inferior to “the ministrations of those personally interested in the work.” See Zouboulakis (2015) for a brief account of the classical theory of the firm. Papandreou quoted Veblen in his bibliography.

⁶ Concerning organization theory, Papandreou focuses on the ideas of Barnard (1938) and Simon (1947). Coase’s 1937 paper, missed in his PhD thesis, is cited once in a footnote.

dealing with the different aspects of the operation of the firm, in a way broader than the structure-conduct-performance approach, popular in the 1950s.⁷ The firm was considered both as a hierarchy coordinated through a “decentralized executive process” and as an organization interacting with an uncertain external environment that includes the “societal influences”: the households (buyers), the creditors, the labor, and the government. For that purpose, he introduced the novel term “peak-coordinator” to personify the authoritative head decision maker, a term praised later by Fritz Machlup (Machlup 1967, p. 21). In his comment, Richard B. Heflebower pointed out that Papandreou’s idea rightly concentrates on the firm as a “system of communication and coordination under authority” and offers “a new look at the alleged diseconomies of large-scale management and at the enlarged scope of coordination under authority in vertical integration or even socialism” (in Papandreou 1993, p. 58). The second commentator, Edward Mason, was more critical of Papandreou’s “exceptionally comprehensive survey,” being more skeptical about the claims made by his young colleague concerning the recent accomplishments in the theory of the firm (in Papandreou 1993, p. 62).

The tension between market efficiency and power of large firms led Papandreou to the measurement of the firm’s power within a branch or industry through a new coefficient. After the Great Depression, many economists questioned the role of powerful firms in imposing “sticky prices” in complete disequilibrium with the collapsed purchasing power of consumption (Tsoulfidis 2010, p. 234). Many efforts were made to measure the market power of big businesses in order to substantiate government intervention. Abba Lerner’s index (1934) measured the influence of a firm on market prices: $L = (P - MC) / P$. Since competitive firms follow the perfect competition rule $p = AR = MC$, the firm has no power over prices and L index equals to zero. The bigger the power of the firm, the higher the difference between p and MC and the “monopoly revenue,” and the L index tends to be equal to unity. Similarly, Joe Bain (1941) grounded his measure on the discrepancy between price and average cost. High differences between them indicate large “supernormal profits” and, as a result, certain power of the firm over the market.⁸ As we know from standard microeconomics, $|e| = AR / (AR - MR)$, and in equilibrium $MR = MC$. So, Lerner (1934) expressed his index as: $L = 1/|e|$. Therefore, $|e| = AR / (AR - MC) = 1/L$, meaning that the degree of monopoly power is inversely proportional to the demand elasticity, which maximizes profit. The lower the elasticity, the higher the margins between MR and MC and hence the monopolistic power of the firm.

The problem with the above indices was that they were appropriate to describe monopoly but missed the whole idea of competition among firms. While, in monopoly, the firm acts freely on prices, in a perfectly competitive market, if a firm cuts prices to attract demand, it simply disappears from the market, as it has no power to sell below its MC . On the contrary, Nicholas Kaldor (1935) described monopolistic power in imperfect competition conditions as part of the relation between prices, marginal revenue, and

⁷ Papandreou was a student of Chamberlin and knew the work of Edward Mason (mentioned in Papandreou 1952, n26). He was also closely related to Joe Bain, who developed further the Harvard structure-conduct-performance approach. I thank an anonymous referee for this suggestion.

⁸ Papandreou also briefly presented two more measures of monopoly power, published by Kurt W. Rothchild and Theodore Morgan in 1942 and 1946 respectively.

demand elasticity. Kaldor called this “Pseudo-monopolistic power” since the cross-elasticities of demand for their product are large but not infinite (1935, p. 38). Based on that idea, Papandreou (1949, p. 889) presented a new index to “take into account the capacity of the firm which attracts demand units by cutting price to match them with supply units.” Therefore, instead of using the demand elasticity of one product, he put forward the idea of measuring monopolistic power through the cross-elasticity between two or more products offered by different firms in an industry. In a monopolistic-competitive market, the cross-elasticity of demand of firm *i* in terms of the price of the product of firm *j* differs from the cross-elasticity of demand of firm *j* in terms of the price of the product of firm *i*.

The cross-elasticity is: $N_{qipj} = p_j/q_i \cdot \partial/\partial p_j f_i(p_i, p_j, P_{N''})$.

Papandreou called his monopolistic power index the “coefficient of penetration of firm *j* into the market of firm *i*,” defined as: $R_{qipj} = K_j \{ p_j/q_i \cdot \partial/\partial p_j f_i(p_i, p_j, P_{N''}) \} = K_j N_{qipj}$.

K_j is a factor taking any value between zero and unity, including both. Hence, K_j indicates “the capacity of firm *j* to match with supply units the demand units which stand ready to shift to it following its price change” (Papandreou 1949, p. 890). The higher the coefficient R_{qipj} the higher is the monopolistic power of a firm, and the easier it is for the firm *j* to penetrate the market of antagonist firms. Papandreou assumed that the prices of other products $P_{N''}$ will remain unchanged and that firm *j* has the capacity to produce any quantity demanded at any time.

So, besides the difficulty of calculating elasticities and marginal costs already present with the much simpler Lerner index, Papandreou moved forward, asking the analyst to calculate cross-elasticities of two perfectly substitutable products, assuming that firms have the capacity “to match” the “abrupt shifts” of demand immediately and endlessly with more supply units. As he fairly recognized in a footnote, “this discrepancy between the time-dimension of the demand functions and the time-dimension of the output-outlay function is not damaging, however, to this purely abstract formulation” (1949, n33). It is then not surprising that Papandreou explored no further his original idea of coefficient of penetration of the firm and abandoned his project to classify different market forms accordingly.⁹ In his publications after 1949, monopolistic power was associated to other things like horizontal and vertical coalitions between firms, or trademarks. Thus, the central issue of the textbook co-written with John T. Wheeler was John Maurice Clark’s concept of “workable competition,” that is, free competition regulated by government authorities (Papandreou and Wheeler 1954). Likewise, the influential use of trademarks, because it affects the decisions of the consumers, constitutes a source of monopoly power that needs to be somehow regulated (Papandreou 1956b). Eventually, the literature on monopolistic power—and competition authorities—focused on less apocryphal and more manageable measures, like the Herfindhal-Hirschman Index to measure market concentration in order to detect monopolistic elements.

⁹ Against Robert Triffin’s (1940) classification, Papandreou distinguished five possible market forms according to his penetration/isolation index: atomistic homeopoly, atomistic heteropoly, circular homeopoly, circular heteropoly, and pure monopoly (Psalidopoulos 2010, p. 349). Triffin’s aim was to push the theory of monopolistic competition away from partial equilibrium toward general equilibrium analysis (Tsoulfidis 2010, p. 236).

IV. EXPERIMENTAL TESTING OF CONSUMERS' RATIONAL CHOICES

In the immediate postwar years, many economists in the US attempted to suggest alternative solutions so as to ground empirically the neoclassical demand theory (see Mirowski and Hands 1998). Samuelson's revealed preferences theory (RPT) was introduced with the explicit intention to eliminate any utilitarian residue left in John Hicks's and Roy G. Allen's consumer choice theory, and move away from any discussion about utility and associated intentional concepts (Hands 2014). Samuelson (1938, 1947) wanted to ground his theory only to observable data related to consumers' behavior, such as their choices revealing their preferences about a different bundle of goods before price changes. In that sense, his theory aimed to be a direct application of *Operationalism*, determining a functional relation between two independent, observable variables: market prices and demand quantities. Although Samuelson proudly announced that his "fundamental theorem of consumption" contained almost all the meaningful empirical implications of the entire theory of consumer choice and was "expressed in the form which is most suitable for empirical verification" (Samuelson 1947, p. 111), his theory was hardly testable. As Mark Blaug remarked (1992, p. 143), unless we know already the demand elasticity of each commodity involved, it is impossible to predict what a consumer will do according to Samuelson's RPT theory and therefore it is impossible to construct properly his/her indifference curve. This fact undermines the feasibility of empirical control of the revealed preferences theory. Moreover, his general request for "hypotheses about empirical data which could conceivably be refuted if only under ideal conditions" refers also to the key element of the internal consistency of consumer preferences (Samuelson 1947, p. 4). Its control requires strict laboratory conditions, since, by definition, "consistency" excludes the tendency of diversification of choices and also qualifies as inconsistent any behavior that reflects some changes in the consumers' tastes.

Papandreou took this challenge seriously and actively participated in the experimental research stream of neoclassical consumer demand theory in the 1950s (Moscati 2007). In a series of laboratory experiments (Papandreou 1953, 1954; Papandreou et al. 1955, 1957), he tested through a questionnaire the hypothesis that individual preferences satisfy the axiom of transitivity. He actually tested a probabilistic version of the transitivity axiom: "if the probability that an individual will choose bundle A over B is equal or greater than .5, and the probability that he will choose bundle B over C is equal or greater than .5, then the probability that he will choose A over C is equal or greater than the probability that he will choose B over C" (Papandreou et al. 1955, p. 333; 1957, p. 8). Eighteen individuals (Minnesota university students) were asked "to make pairwise choices between the elements of each triple" six times.¹⁰ The author decided that experimental results "overwhelmingly supported" the hypothesis that each individual "had a transitive ordering relation" (Papandreou et al. 1955, p. 334). However, as the test was a statistical one, Papandreou and his colleagues made the heroic assumption that the

¹⁰ Choices consisted of attending ten different recreational activities costing \$3.60: five athletic (basketball, baseball, football, boxing, tennis) and five "aesthetic activities" (stage play, music, ballet, opera, and symphony).

null hypothesis corresponded to choices generated by stochastically transitive preferences. As it was correctly argued, “Since the statistical properties of the test were unknown, it was impossible to define precisely the appropriate critical region for the null hypothesis” (Moscati 2007, p. 381). Furthermore, the discussant of his first paper in the Chicago meeting of the Econometric Society,¹¹ the psychologist Ward Edwards, observed, “Many of the choices in both his experiences can be accounted for on the basis of simple preferences of activities involved. A direct count of intransitivities can be made instead of fitting a model.” Instead, Edwards observed, Papandreou seems to have “forced subjects to choose between alternatives to which they were indifferent” (in Papandreou 1953, p. 477).

Papandreou’s experimental proofs of the transitivity hypothesis had some immediate impact as Debreu, Arrow, and John Chipman referred to his laboratory tests during the Econometric Society meetings. Nevertheless, the entire discussion of experimental testing of consumer demand theory was eclipsed by the realization that any empirical disconfirming evidence always can be imputed to the failure of some auxiliary assumption, saving the theory from refutation (Moscati 2007, p. 393). This explains why Papandreou was no longer interested in testing hypotheses and started questioning the methodological foundations of the whole enterprise of axiomatic modeling.

V. THE METHODOLOGICAL CONTROVERSY OVER THE EMPIRICAL MEANING OF ECONOMIC THEORIES

As mentioned above, Richard Lester initiated the famous second round of marginalist controversy—or the “War of 1946,” according to Machlup (1967)—with his groundbreaking paper in the *American Economic Review* (*AER*). Lester recognized that the previous empirical tests were not conclusive in assessing the relevance of the marginalist theory of the firm. So, he undertook a case study to verify the existence of a marginalist behavior, taking the opportunity from the newly legislated minimum wage in many states of the American South from 1937 to 1941. As a result of the minimum wage, the cost of labor grew dramatically, eliminating the wage differential with the same branches of industry in the North. To test directly the maximizing pattern of behavior of southern entrepreneurs, he asked them a series of questions with pre-selected answers.¹² Lester (1946, p. 77) asked the entrepreneurs, “How would your firm be likely to adjust to such a permanent 50% reduction in the North-South wage differential?” The executives were requested to give percentage ratings for the relative importance of six alternative adjustments to this wage differential. The results showed that only four of them placed the marginalist type of adjustment first—i.e., to “reduce production by deliberate curtailment of output”—in order to minimize higher production costs (Lester 1946, p. 79). Instead, thirty-five out of fifty-eight executives preferred to “improve efficiency through better production methods, organization, supervision, [work] incentives, etc.,”

¹¹ This was the historical meeting where Arrow and Debreu first presented their proof on the existence of a general equilibrium.

¹² Lester used the method of mailed questionnaires to study the behavior of a non-representative sample of fifty-eight southern US business executives. He sent 430 questionnaires and received sixty-eight replies, ten of them blank.

meaning that their plant was producing below its maximum capacity. Rather than reducing their production to face increased variable costs, business executives preferred to increase production and “keep sales up when profits begin to be squeezed.” On that basis, Lester (1946, p. 81) deduced, “Unlike economists, business executives tend to think of costs and profits as dependent upon the rate of output, rather than the reverse.”

Machlup responded directly to Lester's challenge by arguing that the “alleged ‘inapplicability’ of marginal analysis is often due to a failure to understand it, to faulty techniques, or to mistaken interpretations of ‘findings’” ([1946] 1967, p. 148). Not only had he objected to Lester's method of mailed questionnaires without supporting interviews as being “hopelessly inadequate for empirical studies of business conduct,” but he also believed that Lester failed to interpret correctly his poor findings ([1946] 1967, p. 189). Nonetheless, instead of providing more empirical evidence to support the assumption of maximization under constraint, Machlup slipped to trivial arguments:

The business man who equates marginal net revenue productivity and marginal factor cost when he decides how many to employ need not to engage in higher mathematics, geometry, or clairvoyance.... On the basis of hundreds of previous experiences of a similar nature the business man would ‘just know’ in a vague and rough way, whether or not it would pay him to hire more men. (Machlup [1946] 1967, pp. 167–168; 1967, p. 17)

In conclusion, no drastic modification was needed to face the empirical challenge of Lester.

Nevertheless, the so-called marginalist controversy endured for the next seven years, involving numerous leading American economists.¹³ This debate ended only in 1953, surmounted by Friedman's milestone paper and the methodological escape he suggested through the unrealisticness of the economic assumptions thesis. According to Friedman (1953, p. 9), “Factual evidence can never ‘prove’ a hypothesis.” Truly significant hypotheses do not describe the reality as it is. Consequently, it is useless to criticize the unrealistic nature of the maximization assumption, because the aim of any assumption is only to provide the basis for successful predictions. And since all theories are “wildly inaccurate descriptive representations of reality” (Friedman 1953, p. 14), the best thing to do is to consider them temporarily *as if* they were true, and choose the one that predicts better.¹⁴

Papandreou had also dealt with the problem of empirical relevance of economic assumptions since his first methodological paper, published in the *Economic Journal* in 1950. Explaining that orthodox economics is not a “pure science” (like mathematics), as its hypotheses should be “tested against the facts” (1950, p. 716), he endorsed Samuelson's idea of operationally meaningful theorems, as against Ludwig von Mises's aprioristic Praxeology and Lionel Robbins's introspectively based maximization principle. Willing to face the immense problems of the testability of the maximization

¹³ From 1946 to 1953, twenty-two articles were published in the *American Economic Review* written on this subject by Machlup, Stigler, Blum, Eiteman, Oliver, Apel, Bishop, R. A. Gordon, Haines, Reynolds, Bronfenbrenner, Kaplan, and Ritter. Friedman (1953, p. 15) responds directly to some of them. See the full list of articles in Mongin (1986). See also Walker (2018, pp. 72–73).

¹⁴ For a more detailed presentation of the controversy raised by Friedman see, among the enormous secondary literature, Blaug (1992), Boland (1982), and Caldwell (1982).

hypothesis, he observed: “The only basis, in fact, for rejecting the postulate would consist in the discovery that human behavior is: a) strictly or primarily random; b) ‘irrational’ in the sense that is inconsistent; c) instinctive; d) strictly traditional. In so far as the presence of ‘deliberative process’ can be confirmed, the essence of the rationality of the maximization postulate must be accepted” (1950, p. 718).

To strengthen the empirical content of economic theories, Papandreou suggested, following Samuelson, to restrict the number of variables and focus on those variables that imply empirically observable quantities. He realized that the consequence of this procedure leads to less universal and “more useful” theoretical constructions. His basic conclusion was that economics should import the empirical findings of sociology and psychology concerning “the ultimate ends, the value-system, of the society in which behavior is studied” (1950, p. 722), as Talcott Parsons professed. Only a closer collaboration with other social sciences would “[i]ncrease significantly the number of operationally meaningful hypotheses in economics, albeit it will reduce the universality of our science” (1950, p. 723). In that sense, in his chapter on the theory of the firm (1952), he urged economists to replace profit maximization with a broader preference-function maximization, in order to include non-pecuniary motives, such as power or prestige.

Six years later, in his book *Economics as a Science*, Papandreou wished to reinstate the empirical character of economics, once the *as if* methodology became popular among economists. He appears then to be far less enthusiastic about Samuelson’s “empirical meaningfulness,” believing that his operationalist methodology “constitutes a program of research rather than an accomplished fact” (1958, p. 11). To face the difficulties of empirical testing required by both Samuelson’s methodology and Friedman’s “predictive power” criterion, Papandreou placed himself in the trend of logical positivism. Starting from a short note published in *Econometrica* (Papandreou 1956a) and more extensively in his 1958 book, he distinguished between theories and models. The difference between them lies in the fact that “[i]n models the class of phenomena whose explanation we seek—the relevant social space—is not adequately characterized” (Papandreou 1958, p. 9). As economists deal mostly with models rather than with theories, their explanations can’t be tested empirically.¹⁵ Noticeably, Papandreou mentioned twice the criterion of refutability (1958, pp. 7, 140), without ever mentioning Karl Popper.¹⁶ On the contrary, he adopted a non-critical view of the living legends of logical positivism: Alfred Whitehead, Rudolf Carnap, Alfred Tarski, Carl Hempel, together with Frank Ramsey and Bertrand Russell. This view goes hand in hand with his methodological practice of empirical testing of consumer choice theory, as seen above. Using the arsenal of Carnap’s symbolic logic and the trendy set theory (after John von Neumann), he aimed to reinforce the relation of economic theory with reality. In fact, he used a Duhemian argument, concluding, “In order to refute the basic theory, we

¹⁵ Papandreou (1959a, p. 1096) gives an example of a model, the Keynesian model of national income determination: $Y = C + I + G$. An empirically meaningful theory would be then a statement that contains specific values of the variables and additional “specifications” concerning the “social space” (or context) in which this theory applies.

¹⁶ Karl Popper’s masterpiece *The Logic of Scientific Discovery* was translated into English only in 1959. Since Papandreou did not speak German, his knowledge of Popper must have come from Friedman’s paper of 1953, widely diffused and discussed. Hutchison’s innovative contribution is never mentioned.

must refute each and every augmented theory that corresponds to it" (Papandreou 1958, pp. 10, 135; 1959a, p. 1099). As he explained, a "basic theory" is an incomplete theory (in the sense that the "social space" it refers to is not complete), to which "correspond many augmented theories" containing all the specifications needed and which could therefore be genuinely refutable.

Knowingly, Blaug (1992, p. 106) characterized Papandreou's methodology as "strident positivism." Papandreou's last methodological contribution, "Theory Construction and Empirical Meaning in Economics" (1963), also concerned the problem of testability of economic theories, in the special issue of the *American Economic Review* on the realism of assumptions, along with Machlup, Ernest Nagel, Sherman Krupp, George C. Archibald, Simon, and Samuelson. Willing to defend—once again—Samuelson's methodology, which believed that the aim of science is to search for relations between observable variables and to draw from them testable conclusions, Papandreou tried to specify the conditions under which economic models—qualified as untestable in his 1958 book—may indeed be tested: "When anchored to some particular space-time segment and thus become descriptions of that segment, [the models] may be tested by reference to data drawn by that segment" (Papandreou 1963, p. 205). In the discussion that followed in the *AER*, Archibald observed that as far as this local anchorage to a given historical space-time is not somehow measurable, it remains impossible to decide "how much error" economic theories contain, leaving thus "[t]he basic structure ... irrefutable" (Archibald, Simon, and Samuelson 1963, p. 228).

Papandreou did recognize that economic models qualified as "taxonomic" are mostly confirmable, not refutable.¹⁷ As he said, "If tested and confirmed, they constitute an explanation. If untested, they constitute a prediction," meaning that these models may still provide testable hypotheses (1963, p. 206). Herbert Simon responded to Papandreou, stating that his method is not workable as he fails to incorporate any criterion of truthfulness. Simon believed that "[u]nreality of premises is not a virtue in scientific theory," and it is always important to know how close to reality are economic theories (Archibald, Simon, and Samuelson 1963, pp. 230–231). Unexpectedly, and despite his plan to define the class of empirically meaningful theorems, Papandreou abruptly ended his endeavor there.

This could be eventually explained because Papandreou seemed to be more aware of the limits of testability than Samuelson himself.¹⁸ In a previous paper published in the eminent journal *Science* in 1959, he admitted that although economists wish to make "statements that are universal in character" having an empirical content, in fact they produce only "models [that] are strictly explanatory devices" (Papandreou 1959a, p. 1100). "Explanatory" means here that models can be confirmed only by the specific data used to build them. Only theories having general applicability possess empirical content and therefore are testable and refutable. And, since economists "are engaged in the construction of flexible, general explanatory schemata" (or models) rather than with

¹⁷ To clarify the word "taxonomic," Papandreou referred to Friedman's dichotomy of analytical/factual propositions (1953, p. 7), although Friedman does not use the term.

¹⁸ As it is well known, in his exchange of arguments with Machlup, Samuelson, while criticizing Friedman for "twisting" the criterion of testability, slipped to descriptivism (Caldwell 1982, pp. 193–195; Zouboulakis 2014, pp. 77–80).

the construction of meaningful theories, they are unable to make predictions.¹⁹ As he concluded, in a rather pessimistic tone, “The basic statements of economics may serve to explain the past but not to predict the future” (Papandreou 1959a, p. 1096; 1958, p. 145).

VI. PLACING POLITICS BEFORE ECONOMICS AFTER 1963

Papandreou evolved, both politically and theoretically, after his permanent return to Greece and his involvement in the political matters of his home country. He was elected MP in 1964 and served as minister for seventeen months. After the fall of his father’s government in July 1965, he remained a MP until the military coup of April 21, 1967. Arrested the very same night, he spent eight months in prison before being released, and moved with his family to Stockholm, in January 1968, to teach for one academic year. In the following year he moved to Canada, as a professor of economics at York University until the summer of 1974 (Kariotis 1997, p. 41). During his second academic career, Papandreou’s publications are within the field of political economy, far away from the alternative mainstream views of the first period from 1943 to 1963. Already in his Wicksell Lecture of 1966, “The Political Element in Economic Development,” his point of view was more socio-political, openly against the neoclassical theoretical framework.²⁰ Development was viewed as a structural change including not only economic but also social, political, and cultural dimensions. The same broad vision is also apparent in his paper “Multinational Corporations and Empire” and in his lecture “Economic Development Rhetoric and Reality,” both published in 1973 (see Papandreou 1993). The contrast with the publications of his first academic period until 1963 is astonishing not only regarding his microeconomic and methodological work but also his published material on macroeconomic planning (Papandreou 1962a, 1962b, 1962c). Characteristically, in the pre-1963 period (before leaving academia and the US citizenship), his planning of national development was mostly an optimization problem of resource allocation, including human capital. The main issue was then the “selection of strategy” that matches the “macro-pattern of investment” programmed by the state, with the “multitude of micro-investment decisions” (Papandreou 1962a, p. 51. See Papandreou 1960).²¹ In the post-1963 period, development becomes mainly a political process resulting from the international division of labor, the distribution of power within a nation, and the national socio-cultural characteristics. As Amartya Sen (2000, p. 14) remarked, “Andreas has argued that economic development cannot be understood without paying serious attention to the political processes that influence it and yield evaluation of its outcomes.”²²

¹⁹ In his conclusion to the *AER* discussion, he admitted that only “the empirically relevant aspects of welfare economics, of comparative economic systems analysis, of economic history, and of development policy” fall under the category of empirically meaningful statements (Papandreou 1963, p. 210).

²⁰ The title of the lecture is reminiscent of Gunnar Myrdal’s first book, *The Political Element in the Development of Economic Theory* (1929), but Myrdal is never mentioned (Papandreou 1993, pp. 85–116).

²¹ To evaluate the impact of the different growth strategies, Papandreou (1959b) used the techniques of linear programming further developed in his textbook *Project Selection for National Plans* (Praeger Publishers), co-written with Uri Zohar in 1974.

²² Sen (2000) maintained that it was Papandreou who introduced in his 1966 lecture the idea of “sustainable growth.”

In conclusion, Papandreou, during his US academic career, was eager to adapt his research agenda to the winds of change of the intellectual milieu of American economists. He began positioning himself in the field of the emerging managerial theories of the firm marked by the pioneer work of Berle, Means, and Barnard studying the evolution of the entrepreneurial function (Papandreou 1943). This led him to construct a measure of the monopolistic power of firms through a new coefficient, with little success (Papandreou 1949). He focused then on the interaction of the internal organization of the American corporations and their external environment, through the concepts of “peak coordinator” and of “workable competition” (Papandreou 1952, 1954, 1956b). Initially impressed by Samuelson’s *Foundations*, he endorsed his methodological instructions about testing hypotheses, inaugurating a short period of intense experimentation regarding the consistency of consumer preferences, between 1953 and 1957. Despite the acceptance from his peers, Papandreou terminated his effort, realizing the limits of empirical testing of mainstream economic theories (Papandreou 1959a, 1963), and turned to the problems of national planning and development (Papandreou 1962a, 1962b, 1962c) as a result of his involvement with the Center of Economic Research in Athens. The crucial jump from planning and policy suggestions to active political decision making came naturally when Papandreou engaged with the imperative political issues of his native country. So, political leadership came before academia.

COMPETING INTERESTS

The author declares no competing interests exist.

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