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FROM PARETO TO BRIDGMAN: THE OPERATIONAL TURN OF SAMUELSON, SRAFFA AND LEONTIEF

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ABSTRACT

This paper reconstructs the attempts to adopt an operational methodology by Samuelson, Sraffa and Leontief as a reaction against the perceived limits of the experimentalism of the Paretian school. While appreciating Pareto's anti-deductivist position and his critique of any substantialist theory of value, these three authors are persuaded that his theory still relies upon the unverifiable assumptions of Jevonian and Walrasian theory. Despite the striking differences in their programs, it is shown that Samuelson, Sraffa and Leontief share the belief that only by recasting fundamental concepts of economics in an operational way, it becomes possible to ground theory on experience, and that their theories represent parallel outcomes of the same trend in the history of general equilibrium economics. Their attempt at redefining, respectively, behaviour, value and equilibrium are analyzed by focusing on their common source in the Paretian framework, on their shared criticism of unoperational notions and on their peculiarities. The feasibility of the operationalist program in economics, as well as its compatibility with general equilibrium is then assessed, also by bearing in mind its reception in physics and epistemology.

Keywords: P. Samuelson, P. Sraffa, W. Leontief, Economic Methodology, Operationalism.
JEL Codes: B31, B41.

Introduction

The aim of this contribution is to sketch the parallel developments of Paul Samuelson's, Piero Sraffa's and Wassily Leontief's economic theories

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in their search for an operational redefinition of economic concepts. The paper shows 1) why these three authors perceived general equilibrium theory to be lacking an adequate grounding in experience; 2) how they tried to reformulate some of core concepts in general equilibrium theory in order to make them rely on actual measurement and no longer on *a priori* assumptions concerning behaviour and production, seeing in Bridgman's epistemological work a useful reference in their criticism of Pareto; and 3) how their attempts ultimately proved unsuccessful, since they failed to discard concepts which couldn't be grounded on measurement procedures. This was partly due to the limits of operationalism itself, and partly because of the deductivist approach which, despite Samuelson's, Sraffa's and Leontief's methodological commitment, remains inherent to the general equilibrium framework.

Operational methodology was publicly endorsed by Samuelson (1938c: 344; 1947: 3; 1948: 251; 1963: 232) and Leontief (1947: 371-372; 1952a: 1; 1952b: 169; 1954: 224; 1958: 104), and finds appreciation in Sraffa's private notes.[1] Its tenets where exposed for the first time in 1927 by the physicist Percy Bridgman in *The Logic of Modern Physics*. According to Bridgman, scientific theorizing, by pretending to describe the properties and essential qualities of the objects it inquired, had fallen prey to unverifiable metaphysical assumptions which ended up hampering the adherence of theory to actual experience, and precluding any understanding of new kinds of experience which do not agree with the *a priori* assumptions on nature held by the researcher. Such was the case, for instance, of the Newtonian understanding of time as an absolute succession of instants, and of the consequent definition of simultaneity as the property of two events taking place in the same instant: "When the range of experience was broadened, as by going to high velocities, it was found that the concept no longer applied, because there was no counterpart in experience for this absolute relation between two events" [Bridgman 1958 (1927): 8].

In order to cope with these previously unknown phenomena, Einstein had to redefine, in his special relativity theory, the meaning of time and simultaneity, which could be preserved only by making reference to the position of an observer. According to Bridgman, such a revolution called for a radical rethinking of the function of scientific concepts in order to make them more suitable for dealing with yet undiscovered new orders of

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[1] Bridgman is explicitly referred to in D3/12/16: 30A. For a reconstruction of Sraffa's epistemological and scientific readings, see Kurz and Salvadori (2005) and Sinha (2016: 72-82). While according to Kurz and Salvadori Sraffa's mature position rejected the aspects he had earlier appreciated in the 'modern scientific outlook', Sinha correctly shows the persistence of Sraffa's epistemological tenets.

experience which may not agree with our presuppositions: "The attitude of the physicists must therefore be one of pure empiricism. He recognizes no *a priori* principles which determine or limit the possibility of new experience. Experience is determined only by experience" (*ibid.*: 3).

The essentialist perspective, according to which concepts describe the supposed inherent properties of an object, had to be given up in favour of a new framework according to which scientific concepts are meaningful only in so far as they can be defined by the operations performed in measuring the object to which they refer. Science had to renounce the claim of providing a systematic and unitary explanation of nature and to content itself with the registration of observable regularities between measurable magnitudes [Bridgman 1958 (1927): 47]. Concepts are meaningful only in the restricted experimental domain where the operations of measurement through which they are defined are performed, and must therefore give away to any pretense of absoluteness. Questions concerning the ultimate nature of matter, since they could not be posed in an operational way, are therefore to be dismissed as meaningless [Bridgman 1958 (1927): 28-31].

Operational methodology expressed a common mindset in the scientific community of that time, which had been partly anticipated in the works of Poincaré, Hertz and Mach. In 1925, for instance, Heisenberg claimed that quantum mechanics should be "founded exclusively upon relationships between quantities which in principle are observable" [Heisenberg 1967 (1925): 261]. In his 1927 highly influential Gifford lectures (published the following year with the title *The Nature of the Physical World*) astrophysicist Arthur Eddington stated that

> the essential point is that, although we seem to have very definite conceptions of objects in the external world, those conceptions do not enter into exact science and are not in any way confirmed by it. Before exact science can handle the problem they must be replaced by quantities representing the result of physical measurement (Eddington 1928: 253).

However, despite being explicitly referred to as a model both by Bridgman and Eddington, Einstein explicitly rejected the operational point of view. In a discussion with Heisenberg, Einstein claimed that observation is always theory-laden [Heisenberg 1971 (1969): 63-64]. In opposition to Bridgman, who had accused Einstein of carrying into general relativity theory "precisely that uncritical, pre-Einsteinian point of view which he has so convincingly shown us, in his special theory, conceals the possibility of disaster" (Bridgman 1949: 337), Einstein reproached that theories did not need to be operational in order to produce testable statements (Einstein 1949: 679).

Observational methodology also came under the harsh scrutiny of Rudolf Carnap (1936), who claimed that its program was unfeasible because science could not do away with dispositional terms like "soluble". These therms, since they referred to the properties possessed by objects independently of their manifestation in an actual experience (for example, a match, which has been destroyed before that his solubility could be ascertained by putting it into water, can nonetheless be described as soluble), could not be operationally redefined.[2] Popper (1959) made remarks similar to those made by Einstein on the relationship between theory and observation, while Carnap's argument was reinstated by Goodman (1953) and Hempel (1954).

Despite these criticisms, operationalism nonetheless was met with interest in contemporary and post-war economic thought.[3] Samuelson's claim of reliance on an operational methodology has been widely discussed,[4] and more recently contributions exploring the issue in Sraffa and Leontief have appeared.[5] Still, the reason why antagonistic positions like those of Sraffa and Samuelson referred to operationalism lacks adequate clarification. A comparative account of their positions can be drawn by tracing their interest in an operational perspective back to their shared adherence to the task, initiated, although in a contradictory fashion, by Fisher and Pareto, to redefine the aims and method of economics by reflecting upon the theoretical consequences brought upon marginal utility theory by the adoption of a general equilibrium framework and of an anti-deductivist stance.[6] Fisher and Pareto asserted that the task of general equilibrium theory was not to explain the nature of some hidden force behind prices, but simply to determine equilibrium prices starting from data which had to be, at least virtually, observable. Economics had to become, in Pareto's program, a truly experimental science.[7]

This program, however, was perceived to be pursued only halfway. Theory still relied upon unverifiable psychological assumptions in order to account for the achievement of equilibrium and to determine its po-

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2 See also Akhabbar (2007).

3 See Cohen (1995).

4 See Gordon (1955), Machlup (1964), Wong [2006 (1978)], Cohen (1995), Hands (2004), and Carvajalino (2018).

5 For Sraffa, see Kurz and Salvadori (2005), Sinha (2016), and Carabelli (2018). For Leontief, see Akhabbar (2007, 2019).

6 On the anti-walrasian nature of Fisher's and Pareto's program see Marchionatti and Gambino (2007) and Marchionatti (2009; 2020).

7 As remarked by Marchionatti and Gambino (1997: 1323), according to Pareto experimental method requires theories to be empirically testable.

sition. Moreover, the magnitudes which had to be taken as given, since they depended on counterfactual and mutually excluding situations which could not be recreated experimentally, could not be ascertained through actual measurement. The claim that theory and experience had not yet been bridged produced different, but parallel, outcomes. Samuelson tried to recast neoclassical economics in a new fashion by redefining its basic concept in a way that could effectively do away with any "vestigial traces of marginal utility" (Samuelson 1938a: 71), while Sraffa attempted a refoundation of economic theory which, while rejecting subjectivism, aimed at avoiding the perceived contradictions of the classical and Marxian theory of value. Finally, if concepts which did not refer to observable (and, therefore, measurable) magnitudes, were to be acknowledged as meaningless, Leontief tried to contest the separation between the elaboration of theories and their econometrical testing. It is therefore no surprise that Bridgman's philosophy provided the three of them with useful support in their attempt to severe economics from a foundational approach which they perceived to be remote from experience and prey to metaphysical prejudices. As remarked by Napoleoni (1956: 1710-1711), in the development of economic theory after Pareto, we can observe the deconstruction, both in consumption theory and in production theory, of the marginal concepts upon which the subjective theory of value was grounded.

As we shall see, this does not mean, however, that Samuelson, Sraffa and Leontief actually put Bridgman's methodology into practice. On the one hand, this was actually infeasible because operational constraints, as already shown by Carnap and Popper, were too narrow for formulating any scientific theory. On the other hand, these three authors, despite sharing Bridgman's concerns with the issues of measurement and interdependence as well as his criticism of any essentialist understanding of scientific concepts, were actually imbued with a deductivist approach inherited from the Walrasian and Paretian framework they wished to reinvigorate.[8]

Moreover, it needs to be acknowledged that Samuelson and Sraffa were exclusively concerned with ideal measurements[9] in order to pursue a purely theoretical goal (respectively, a refoundation of consumer theory and a proof that profit could not be interpreted as a price phenomenon), while Leontief actually devised measurement procedures in order to develop an alternative to econometrics in the study of concrete economic systems.

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[8] For Samuelson, see Cohen (1995). For Leontief, see Akhabbar (2019). For Sraffa, see Blaug (1997).

[9] See Samuelson (1938a). For Sraffa, see Lutz and Hague (1961: 305).

1. From deductivism to experimentalism

The core tenet of early marginalism was that satisfaction existed as a magnitude whose variation could be described as a function of the quantity of the good enjoyed. Pleasure increased with every new unit of a desired good being supplied. However, as the desire for a particular good was getting satiated, each new unit of that good brought a lesser degree of satisfaction than the former, i.e. total utility was said to increase, while marginal utility was said to decrease as more of the same good was enjoyed.[10] Given the optimizing behaviour of the agents, who tried to attain the maximum possible degree of satisfaction, the point of maximal satisfaction the agents were able to attain could be defined as the one where the last unit of money spent on each good yields the same degree of utility (i. e. the so-called principle of equimarginality). Once equilibrium had been achieved, differences in relative prices could then be explained as reflecting differences in the marginal utility of each good. Production theory was included by assuming a negative utility connected with the employment of productive factors and an increasing marginal disutility in their employment. Equilibrium was thus the result of psychological forces (the strive for the maximum level of pleasure), which, through the application of differential calculus, could finally be treated in a mathematical fashion. Marginal utility was considered by these authors to be the ultimate determinant of exchange value.

Early marginalists acknowledged the difficulties in measuring utility,[11] but claimed such a measurement was unnecessary from a theoretical perspective. So did Edgeworth (1881: 83-93), who argued in the first appendix of his *Mathematical Psychics* that even physics made use of mathematical deductions without providing an exact measurement of some of the quantities involved, and Walras, who in his private notes stated that, regardless of the objections he received from Poincaré,[12] it was admissible to suppose utility to be quantifiable in economics from a theoretical perspective "in the same fashion that we suppose mass to be quantifiable in mechanics when we define it as the number of molecules or as the quantity of matter contained in a body" (Jaffé, vol. 3: 171).

Despite skepticism from mathematicians, no revision of the theory was made until Fisher's *Mathematical Investigations in the Theory of Value and Price* (1892). Fisher was the first to point out that, since the satisfaction provided by one kind of good is rarely independent of the satisfaction

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10 See Kauder (1965) and Blaug (1997).

11 See Stigler (1950: 316-318), Kauder (1965), and Blaug (1997).

12 See Marchionatti (2020: 285-288).

provided by its complementary goods or by its potential substitutes, utility could not be conceived as a magnitude which could be the object of even an ideal measurement: it becomes impossible to choose a unit to measure utility which remains immutable as the utility of the good we want to measure varies (1892: 64-65). Fisher proposed an experimental approach which stood against Walras' and Edgeworth's mathematical-deductive method, reproaching his predecessors for assuming a psychological doctrine which was both uncertain and unnecessary for the economist's task to determine prices and distribution (*ibid.*: 11). Rather than indulging in considerations on the nature of pleasure, economists could content themselves with the observations of choices made by buyers and sellers according to their desires. Tastes, which could be ascertained in the agents' choices, were sufficient to determine the equilibrium point, without needing to know the actual difference in satisfaction between two options where one is chosen as better than the other. However, in order to assert the existence of a single equilibrium point, Fisher could not do away with making assumptions concerning the nature of desire, which he nevertheless abstained from discussing, and which were reflected in the convex-shaped indifference curves he made use of in order to represent the sets of goods which granted the costumer the same satisfaction (*ibid.*: 71). While Edgeworth, the previous year, had (wrongly) invoked the principle of diminishing marginal utility in order to account for the shape of indifference curves (Edgeworth 1881: 34-36), Fisher did not provide any reason for such convexity, and simply assumed their shape to be in an intermediary position between the cases he described of perfect substitute and perfect complementary goods (Lenfant 2012: 118-119).

We can find the same ambiguity in Pareto, who, after having at first endorsed the idea of a quantifiable utility, started to question the legitimacy of employing such a concept (1898; 1900). Like Fisher, he defined the equilibrium position as the one which the agents preferred the most to the possible alternatives (1900: 220-221), proclaiming the uselessness of hedonistic calculus, and made use of convex-shaped indifference curves, whose properties were assumed in his *Manual* to be derived from "every day experience" [1971 (1909): 330].

Pareto vigorously repudiated any interpretation of utility as the cause of value as a metaphysical residue, and stated that in general equilibrium theory prices depended on the whole of the conditions taken as given – preferences and obstacles (1901: 247-248).

Nevertheless, Pareto held that the notion of a quantifiable utility could still be retained, at least as a hypothesis, just like ether was employed in optics without any reassurance concerning its actual existence, and thus insisted that his redefinition of equilibrium was equivalent in content to the former one [2012 (1900): 14]. In his *Manual*, where he first employed at length his new conception, the old notion of utility was widely employed as a viable approximation.[13]

As suggested by Bruni (2010), the reason for this apparent contradiction could be located in the fact that, despite advocating that economics could be exposed without any reference to concepts borrowed from psychology, Pareto still believed, at the time when he composed his *Manual* (1907), that social science still lacked a definitive foundation which could ultimately come only from psychology [Pareto 1971 (1909): 20]. Experimental proceedings were not categorically opposed to a deductive explanation which had yet to be found.

The attempt to provide a new theory of consumer demand that could be completely independent of psychological motives was carried on by the works of Johnson (1913), Slutsky (1915), Hicks and Allen (1934) and Hicks (1939). First Johnson (1913: 490) and then Hicks observed that if the notion of utility as a magnitude is given up, this must also be the case with marginal utility, which Hicks proposed to displace by introducing the concept of marginal rate of substitution (Hicks and Allen 1934: 55), i.e. the quantity of a good which the owner is willing to give up to get an additional unity of another good.[14] The condition of equimarginality was recast so that the ratio between two prices in equilibrium had to be equal to the marginal rate of substitution between these goods (*ibid.*: 56). Hicks also explicitly rejected diminishing marginal utility and, in order to secure that a position of equilibrium could ultimately be achieved, he stated that the marginal rate of substitution had to be diminishing. Hicks never provided the grounds for such a statement, whose content actually depended upon the psychological assumption of a preference for variety already employed, for example, by Senior [1965 (1836): 11].

While initially presenting his reformulation of marginal theory as opposed to the cardinal doctrine of utility, in later works Hicks modified his attitude, first by presenting the two conceptions as non-antagonistic in *Value and Capital* (1939: 17-18), and then by proclaiming that the diminishing marginal rate of substitution differed from the diminishing marginal utility only because of the higher degree of generality of the first (1956: 153-154). Any revision of utility theory still relied on a psychological presupposition [Wong 2006 (1978): 17-33].

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[13] See Lange (1934: 218) and Stigler (1950: 327).

[14] This is the definition employed by Hicks in *Value and Capital* (1939), and which is currently adopted. In Hicks and Allen (1934), it referred to the quantity of a good someone was willing to receive in order to give up an additional unity of another good (and hence the rate was assumed to be increasing).

2. Operationalising behaviour: Paul Samuelson

The reliance of the assumption of a decreasing marginal rate of substitution upon a psychological statement for which no proof had ever been provided was underlined by Paul Samuelson in his 1938 paper *A Note on the Pure Theory of Consumer's Behaviour*. He clearly recognized how "the modern criticism" of early marginalism "turns back on itself and cuts deeply" (Samuelson 1938a: 61):

> For just as we do not claim to know by introspection the behaviour of utility, many will argue we cannot know the behaviour of ratios of marginal utilities or of indifference directions. Why should one believe in the increasing rate of marginal substitution, except in so far as it leads to the type of demand functions in the market which seem plausible? (*ibid.*: 61).

Consequently, Samuelson asked himself whether utility analysis had become "meaningless in the operational sense of modern science" (1938b: 344). However, Samuelson's methodological stance was explicitly discussed by the author only in the 1950's and 1960s. In 1952 he observed that the revolution in economics in the 1870s "had little really to do with either subjective value and utility or with marginalism; rather it consisted of the perfecting of the general relations of supply and demand. It culminated in Walrasian general equilibrium" (Samuelson 1952: 61). Moreover, he blamed Menger for having taken concern with "pseudo problems of qualitative essence" (*ibid.*: 63). In 1963, while engaging in a discussion concerning Milton Friedman's stance on methodology, Samuelson mentioned approvingly Hertz, who "said that a belief in Maxwell's theory of light meant nothing more and nothing less than that the observable measurements agreed with the partial differential equations of Maxwell" and Poincaré, according to whom "the whole content of classical dynamics was summed up in the hypothesis that certain sets of second-order differential equations exhibited solutions that to a good approximation duplicated the behavior of celestial bodies and terrestrial particles" (Samuelson 1963: 232). In a successive reply to that discussion, he condemned the abuse of *a priori* assumptions made by economics, with references to Menger and Robbins (1964: 736), and stated that description, not explanation, to be the true task of the scientist, and praised Newton's lack of concern for "the fruitless question of why" (*ibid.*: 737):

> Scientists never 'explain' any behavior, by theory or by any other hook. Every description that is superseded by a 'deeper explanation' turns out upon careful examination to have been replaced by still another description, albeit possibly a more useful description that covers and illuminates a wider area (*ibid.*: 737).

Therefore, there is no actual distinction between a theory and the set of consequences it predicts.

Going back to Samuelson's 1938 paper, once considerations about utility are ruled out, it was still possible to formulate hypotheses about behaviour which, by making reference only to measurable magnitudes, i.e. prices, quantities and income, could be eventually empirically disproved, and at the same time derive the same results of utility theory without any reference to non-operational notions. The hypothesis Samuelson proposed to employ instead of the diminishing marginal rate of substitution, was that of postulating the consistent behaviour of the agents, i.e. that in the choose between two batches of goods "if an individual selects batch one over batch two, he does not at the same time select two over one" (1938a: 65). Devoid of any reference to mental states, consistency was defined only through actions performed by the agents and could be ascertained by presenting him different bundles of goods for different prices. In this sense, it could claim to be an operational concept as defined by Bridgman, despite Samuelson's emphasis on the verification of an assertion, rather than on the assessment of the meaning of a scientific concept,[15] which was quite alien to the *Logic of Modern Physics*. However, the procedure devised by Samuelson to verify consistency, since it requires confronting the agent with an infinite number of market-choices and believing that the agent will always behave in the same way also in the future, cannot be put into practice: consistency maintains the same *a priori* status of previous assumptions concerning desire and behaviour [Wong 2006 (1978): 42; Cohen 1995: 66-69].

Ten years later, Samuelson offered another interpretation of his program, stating that his purpose had always been to construct a procedure through which the customer could "reveal his preference pattern – if there is such a consistent pattern" (Samuelson 1948: 243). Despite his methodological commitment, he was now accepting non-operational entities – preferences – which had to be empirically ascertained through a procedure which was employed to determine whether the consumer was indifferent to the choice between two bundles of goods. This was a considerable shift from his initial aim to free consumer theory of any concept which could not be grounded in observation [Wong 2006 (1978): 73-74]. Moreover, the procedure proposed by Samuelson had to assume that preferences did not change while the operations constructing the curve are performed, which was equally unjustified from an operational point of view (Robinson 1962: 50).

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[15] See Cohen (1995).

After this paper and the employment of ordinal utility theory in his 1947 book, Samuelson was perceived to have renounced his claim to build an alternative theory, as was suggested for example by Houthakker in 1950. Houthakker (1950: 173) suggested that, if his aim was to yield the same results as utility theory, Samuelson had to restrict his postulate of consistency so that preferences could be integrable, i.e. so they could always be expressed in the form of a utility function. Samuelson confirmed Houthakker's claim and, stating that he aimed at "arriving at the full empirical implications for demand behaviour of the most general ordinal utility analysis" (1950: 369), accepted his suggestion, declaring that he hadn't been previously able to solve the issue. This harshly clashes with Samuelson's claim in 1938, where he rejected the problem as not relevant, "particularly if we are willing to dispense with the utility concept and its vestigial remnants" (1938a: 68). Samuelson definitively admitted the two theories to be coextensive, retracting the anti-psychological claims he had made earlier.

Samuelson's actual methodology was at best a return to the mix of deduction and experimentalism avowed by Fisher, Pareto and Hicks, especially in his determination to devise tests for theories previously established as hypotheses, with some sentences leaning more to a purely deductivist stance.[16] What Samuelson pretended to observe, as pointed out by Machlup, was "merely the logical consequence of a set of assumptions" (1964: 735).

The attempt to emancipate demand theory from assumptions not grounded in experience proved unsuccessful:

> Consequently, the attendant philosophical and psychological controversies of utility theory, which Samuelson hoped to evade with his observational theory, are not exorcised from the corpus of economic theory and, therefore, still await resolution or further elaboration [Wong 2006 (1978): 55].

3. Operationalising value: Sraffa

From 1927 onwards, we can find passages in Sraffa's notebooks which show the relevance he attributed to the issue of measurement in order to assess the validity of a concept. This is a point he will fully reinstate in the intervention he made at the Corfu conference on capital theory (1958): "The theoretical measures required absolute precision.[17] Any imperfections in these the theoretical measures were not merely upsetting, but knocked

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[16] See also the discussion in Cohen (1995).

[17] On Sraffa's methodology, see also Carabelli (2018).

down the whole theoretical basis" (Lutz and Hague 1961: 305). According to Sraffa, the "chief failing" of the marginal theory of capital lied in "an inability to define measures for capital accurately" (*ibid.*: 305). In a note predating 1928, we can read of "the necessity of unity of measurement, not for measurement, but for conception" (D1/20: 5). In fact, in a series of notes written in the summer of 1927, Sraffa shared Fisher's point that the existence of substitute goods was sufficient to prove the impossibility of establishing utility as the ultimate explanation for value: "if we accepted this sort of utility as an ultimate standard we would reason in a circle, explaining the utility of A with the utility of B, the utility of B with the utility of C, etc." (D3/12/3: 19). In the same notes, he sketched a history of economic thought where he described political economy as gradually emancipating from the task of finding a prime cause of value. This evolution was partly due to the emergence of economics as a specific science distinguished from philosophy and to its adoption of an avalutative and theoretical perspective in spite of its origin as a practical form of knowledge:

> But it is a fact that while classical economists were inquiring into the 'prime cause' and the 'ultimate standard' of value, the modern attitude is largely to ignore those questions […] Two sets of causes have contributed to bring about this change. In the first place the general progress of economics as a science, with its consequent shifting from the consideration of broad philosophical questions to the technical analysis of the mechanism through which economic equilibrium is reached. In the second place, the change in the practical issues which have confronted the economists; the influence of the latter on theories which are supposed to be abstract and without any practical application is interesting (D3/12/3: 13-14).

Sraffa praised "the introduction of the concept of equilibrium, which wiped out the primitive notion that there had to be somewhere or other one single ultimate cause of value" (D3/12/3: 8). Despite Pareto's nominal adherence to utility theory (D3/12/3: 69), his work could be employed without any substantial theory of value (D3/12/3: 25).[18] Sraffa carefully annotated some of Pareto's writings (*Anwendungen der Mathematik auf Nationalökonomie*, *Le nuove teorie economiche*, *Les systèmes socialistes* and the *Manual*),[19] where he had introduced the notion of interdependence and had counterposed the employment of mathematics in order to treat such interdependence to any monocausal conception of value. If Pareto had full right to employ "the commodities themselves" (Pareto 1902: 1111) as the

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18 On Sraffa's debt to Pareto, see SINHA (2016: 48-49) and KURZ (2020).

19 See KURZ and SALVADORI (2005) and KURZ (2020).

only measurable magnitudes, his experimental method was still not practicable, because it required ascertaining every possible point of the demand and supply curves by letting just one variable change, hypothesizing that the others were left unchanged in the attempt – a point which actually contradicted with the notion of interdependence. Since counterfactual states of the system could never become actual objects of observation and measurement, they had to be left out of theory:

> Finally, there is the class of quantities, which form the basis of Marshall's theory (or, rather, of Pareto's), such as demand and supply curves, marginal productivities (i.e. rate of growth of total), indifference curves, etc. Here the constant quantities have no names – they are the parameters of curves. The several quantities represented by these curves do not exist at any one moment, nor during any period of the recurrent steady process of production or consumption. They are alternatives, only *one* of which can exist in any one position of equilibrium, all the others being thereby excluded (even the one does not really exist if there is no change, since it is the rate of growth of a quantity, i.e. marginal product: it can be inferred from price, but so can marginal utility, which under (1) we have agreed does not exist). Therefore, they cannot be found by merely observing the process or state of things, and measuring the quantities seen. They can only be found by means of *experiments* – and these quantities in effect are always defined in terms of such experiments (successive doses applied to land; alternatives offered to the consumer; etc.) These experiments cannot be carried out (and never have been, as a matter of fact) for various reasons: 1) the practical difficulties, 2) the lack of definition of the conditions to be required, which are always summed up in the absurd 'other things being equal'. But even apart from these difficulties, which might conceivably be overcome, there remains something about these experiments which is very curious: they are generally regarded as acceptable, as if they were calculated to reproduce under controlled conditions, so as to be able to measure them, facts which actually happen 'in nature' all the time but cannot directly be pinned down for observation. But the experiments have an entirely different significance: they actually *produce* facts which would otherwise not happen at all; if the experimenter did not step in first to produce them, and then to ascertain them, they would remain in the state of 'unknown possibilities', which amounts to the deepest inexistence (D3/12/13: 3, 5).

According to Sraffa, the only quantities allowed in economics were the actual magnitudes which could be measured in a given instant of time (D3/12/13: 2). It should not surprise us that Sraffa in that time was reading the works of Hertz, Bridgman and Eddington, as well as following the development in quantum theory introduced by Heisenberg (Sinha 2016: 72-82).

The system of equation Sraffa was devising, as he will outline in a note written in 1942 (D3/12/7: 65-67), would show that once the quantities employed by each method of production were known, given the assumptions (discussed here below) of a uniform rate of profits and of a given uniform real wage rate, it was possible to determine the ratio of exchange between the commodities which allowed the process to repeat itself. This would be possible even for a man who "fell from the moon on the earth", without having any actual knowledge of human psychology and the working of the earth's socio-economic system. Prices which secure the reproduction of the system could thus be known without any references to the forces operating in the system, which could not be ascertained through observation (D3/12/15: 2).

While holding modern economics' disillusionment with first and final causes to be fully justified, Sraffa thought that classical economics' concern with the question of value couldn't simply be dismissed, and that such a question could be recast from one of 'cause' to one of 'meaning' (D3/12/4: 5.1), i.e. of measuring the relationship between the inputs employed in production and the net product of a society and determining the relationship existing between the net product and its distributive shares. This relationship, which is immediately observable in the case of a system where a single commodity is produced, loses its evidence when a plurality of commodities is produced, due to the fact that, once a commodity is chosen as numéraire, its value changes with changes in the distribution of net product. However, Sraffa solved the problem by constructing a composited commodity as invariable standard. By demonstrating that the ratio between output and the means of production employed was independent of changes in prices and that, once the real wage rate was given, the profit rate could be determined without any reference to prices, he asserted, against marginal productivity theory, the independence of distribution from price determination.[20] Value, which pre-general equilibrium theory claimed to be a quantity of labour or utility, was thus operationally redefined as the measure of ratios between aggregates of physical heterogeneous commodities, a measurement which became possible through the construction of the standard commodity.

However, in order to determine the prices which ensure that the production process could be repeated with the same data, Sraffa had to assume a uniform rate of profits between industries, a statement which the classics justified by making reference to the intersectorial movements of capital in order to secure the highest return possible, which, in a mechanism described as analogous to gravitation, made the rate of profits of each sector fluctuate around an average. Having ruled out psychological inducement

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[20] See SINHA (2016).

as a force operating in the economy, Sraffa could not accept such an explanation, which relied on counter-factual reasoning, and struggled for a long time to justify such an assumption without making reference to gravitation: "The assumption (in the 2nd equations) that rate of interest (surplus) is equal in different industries is much too rationalistic: it assumes that the capitalists are 'perfect economic men', who *move* their capital accordingly" (D3/12/9: 9). Being just a tendency, such a uniformity cannot be conceived as observable in a given instant of time in the classical framework, unlike e.g. the tons of steel employed in a given year. Therefore, the argument provided in Sraffa's book remains cryptic, relying on an unexplained necessity ("the surplus or profit must be distributed in proportion to the means of production – or capital – advanced in each industry").[21] In order for prices to emerge only from measurable conditions of productions, a proposition which does not rely on experience had to be introduced. Sinha's argument (2009; 2016: 222-225) that, once a relationship between the maximum rate of profits and the real wage is established, the uniformity of the profit rate is "a consequence of 'given' and uniform wages from outside the system" (Sinha 2016: 204) omits that a uniform wage, as correctly stated by Blaug (1999: 225-230; 2009: 209), can be justified only as a consequence of the intersectorial movements of the workforce (driven by their expectation to maximize their wage for a given effort), which tendentially level it around an average value.[22] In other words, if we measure the quantity of labour

[21] Sraffa 1960: 6.

[22] Sraffa himself made such a condition explicit in D3/12/52: 6-7, where he stated that homogeneous labour is brought about by the "higgling and bargaining of the market" (see Levrero 2019). A different interpretation of the homogenisation process is advanced by Sinha (2020: 447). According to Sinha, "Sraffa is translating heterogeneous labour to homogeneous labour by using the empirical wage differentials as the multiplication factor. The procedure simply amounts to converting the empirically given proportion of industrial wage bills to total wage bill into the proportion of industrial labour input to total labour input". It seems to me that Sinha misses the point that wage differentials aren't simply taken as empirically given: since the wage has to be uniform, they are assumed to reflect differences (in intensity – both physical and intellectual, in the costs of formation, in the risks and in the reputation) between the different kinds of labours, which are to be compensated with different wages. In order for wages to tendentially reflect these differences (accounted for by Smith [1976 (1776): 116-135], Ricardo [1951 (1817): 20-22] and Marx [1996 (1867): 54, 208-209]), intersectorial movements of workers have to be assumed. This is the process of "higgling and bargaining of the market", which therefore cannot simply be treated as a "catch-all expression for the existence of unequal wages" (Sinha 2020: 448), being employed by Smith to describe the process according to which wages are proportional not only to the time expended in labouring, but also to "the different degrees of hardship endured and of ingenuity exercised" [Smith 1976 (1776): 48-49]: "It is often difficult to ascertain the proportion between two different quantities of labour. The time spent in two different sorts of work will not always alone determine this proportion. The different degrees of hardship endured, and of ingenuity exercised, must likewise be taken into account. There may be more labour in an hour's hard work than in two hours' easy business; or in an

employed and the wage in each sector in a given instant of time, there is no warranty for us to see the condition, assumed, but unjustified by Sraffa, "that each unity of labour receives the same wage" (1960: 10). Just like a uniform rate of profits, and unlike for instance the tons of steel employed in a given year, a uniform wage cannot be taken as given in consequence of direct measurement: if a 'subsistence system' can exhibit only measurable data as given (Sraffa 1960: 3-5), this is no longer the case once distribution is taken into account.

Wage and profits cannot thus be treated by making abstractions from the social forces which establish their average value, and a tension emerges between a model which aims at taking only a 'snapshot' representing a given instant of time and an assumption which can only rely on movements of workers and capital which take place in time. By taking their uniformity as given, Sraffa had actually to introduce assumptions which cannot be operationally justified. Starting from Garegnani,[23] neo-Ricardian authors have often accepted the gravitation mechanism as an explanation for a uniform rate of profits, but in so doing they have adopted a position which Sraffa would have found incompatible with his standpoint.

4. Operationalising general equilibrium: Leontief

Since his first contributions to input-output analysis, Leontief showed his admiration of general equilibrium theory as a framework capable of

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hour's application to a trade which it costs ten years' labour to learn, than in a month's industry at an ordinary and obvious employment. But it is not easy to find any accurate measure either of hardship or ingenuity. In exchanging indeed the different productions of different sorts of labour for one another, some allowance is commonly made for both. It is adjusted, however, not by any accurate measure, but by the higgling and bargaining of the market, according to that sort of rough equality which, though not exact, is sufficient for carrying on the business of common life". In Smith [1976 (1776): 135-159], these wage differentials are to be distinguished from those originating in limits to the movements of workers, which, differently from the former, are progressively eroded by the generalisation of market relationships. It is only because wages are brought by the market to reflect qualitative differences between types of labour that "differences in quality" of labour can be "reduced to differences in quantity so that each unit of labour receives the same wage" (Sraffa 1960: 10).

As regards Sraffa's unexplained assumption either of a given uniform rate of profits or of a uniform wage, see also Sen (2004: 583-584), according to which Sraffa "did not go into the question as to why these characteristics (e.g., the same rate of profit in all enterprises) could be expected to hold, and it is possible to argue that such a justificatory inquiry would take one in the direction of equilibrium economics, involving the use of counterfactual considerations".

Sinha's argument also omits that the rate of profit is assumed to be uniform before introducing the possibility for workers to reclaim a share of the surplus (Sraffa 1960: 6).

[23] See Garegnani (1990).

coping with the complex interdependence of a modern economic system (1937a: 109-110). However, he complained that the theory had proved unable to offer any "detailed explanation, not to say prediction, of the specific states of the actually observed economic system" (Leontief 1954: 224):

> Seldom, in modern positive science, has so elaborate a theoretical structure been erected on so narrow and shallow a factual foundation. Traditionally – and that tradition still prevails among mathematical and non mathematical economists alike – 'pure' theory has not been implemented with empirical determination of any of the numerical parameters involved. As can be seen even from the sketchy outlines presented above, all empirical assumptions on which such theories are based are qualitative in character and, at that, they are quite vague and general (*ibid*. 1954: 224).

The concepts employed by general equilibrium theory in order to account for interdependence have remained "empty boxes", completely devoid of any empirical reference.

Leontief's papers on methodology written in the '50s (1954; 1958) reveal his deep dissatisfaction with his contemporaries' attempt to bridge the gap between theory and experience by simply devising tests for a theory which had already been developed as following from *a priori* assumptions. Such was the case of Koopmans-led Cowles Commission, according to which the main data of general equilibrium theory, demand and supply functions, had to be indirectly reconstructed through statistical inference.[24] According to Leontief, these attempts were unfeasible because of the complexity of the economic system they were trying to make inferences from. As he once stated, employment of indirect inference was akin to the task of reproducing

> the blueprint of a complicated motor on the basis of our knowledge of the general principles of operation of internal combustion engines and no other specific in-formation but that conveyed by the few dials located on the dash-board and possibly the noise coming from under the closed hood. And as if that were not difficult enough, the structural characteristics of the engine the economist is studying are known to change under the impact of its continual operation. The task as presented can hardly be accomplished (Leontief 1954: 228).

Statistical inquiry forced researchers to an oversimplifying employment of aggregation, and to make use of time-series which were either vitiated by auto-correlation or stretched too long over time, exposing the researcher "to the even more fundamental danger of assuming invariance in relationships which actually do change and even lose their identity over

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24 See Vining (1949).

time" (Leontief 1952: 3). Economists should give up "the point of view of laplacian superhuman intelligence, which would be able to see without the least mental friction all the infinite number of logical implications of any given system of assumptions" (Leontief 1937b: 338) and confine themselves to tasks which are possible for "a limited human intellect" (*ibid.*: 338). Empirical implementation had to be considered "as much a part of an economic argument as the consistent development of its logical consequences" (Leontief 1952: 4). The problem, therefore, must not be "of a tactical nature", but "would require redefinition of our general strategic objectives" (*ibid.*: 2): in other words, theory had to be reframed in order to become significant from an empirical prospect, therefore abolishing the division between pure and applied economics. Ruling out the possibility of experimental method in economics, as implied it was "possible to stop the motor, take it apart and subject each of its components to any desired tests and measurements" (1954: 228), Leontief stated that theory "is required to be operational" (1952: 1), i.e., to employ as its data only directly observable magnitudes and whose concepts are meaningful only if defined through reference to these primary data. In this way

> the statistical data collected fill the empty boxes of the theory of general equilibrium. Hypothetical production and consumption equation gain explicit meaning soon as the symbolic algebraic signs are replaced by observed numerical values. Once an empirical foundation is thus established, the vague generalities of abstract theoretical statements will acquire theoretical significance (1936: 116).

The gap between theory and experience could not be mended through the adoption of supposedly realistic postulates, since they presupposed "the existence of a uniquely described, or at least in principle unequivocally descrivable, reality which the model should be expected to fit" (1958: 105), a perspective which Bridgman had already criticized as metaphysical. Rather, it was necessary to devise procedures and definitions which ensured that concepts and models were built on actual experience, and not on superimposed frameworks:

> Actually, a typical abstract economic model can be related to so-called reality only through an intricate system of basic definitions, classifications, and rules of measurement which logically can be neither right nor wrong, but without which a most rigorously constructed model can have no empirical significance of any kind (*ibid*: 105).

Such a reframing implied taking the coefficients of production and consumption of industries and households as given, assuming them not to be dependent upon indifference curves and production functions:

> Theoretical economists deal with production functions in their quite general form. More specific characteristics, if introduced at all, take the form of hypothetical assumption rather than systematically observed and measured facts. The very nature of the present study necessitates the introduction of quite definite assumptions concerning the shape of our production functions; and at the same time it limits considerably the freedom of theoretical choice, because the numerical values of all the parameters must be ascertainable on the basis of available statistical information (Leontief 1937a: 111).

Interdependence could thus be no longer accounted "from above", "through observation of the dependent variables such as prices and total output", but "from below", deriving the coefficients of production not from an assumed production function, but from "engineering data" (1952: 7).

If given coefficients of production were to be taken as parameters of the equilibrium equations, they had to be assumed to be fixed. The merging of theory and measurement became possible only if an observational constraint was imposed upon theory (without any theoretical argument against substitution being advanced), and a theoretical constraint was imposed upon experience (Leontief acknowledged that technical coefficients actually change in time, but assumed them be fixed for the sake of making calculations in a general equilibrium framework).[25]

Leontief contented himself with the supposition that the impact of substitution effect could be assumed to be of little relevance:

> This theoretical proposition so clearly stated by Pareto in his criticism of Walrasian fixed coefficients of production is beyond dispute. It is, however, not the fundamental validity of the principle of substitution but its quantitative significance which is important from the point of view of empirical analysis (Leontief 1946: 38-39).

On the basis of his methodology, Leontief strongly opposed the attempts of the Cowles Commission, starting from Dantzig and Wood (1949a; 1949b) and culminating in Samuelson's non-substitution theorem (Samuelson 1951), to make of his own model just a particular case of a neoclassical production function. However, by not questioning the existence of substitution, at least from a theoretical point of view, Leontief had to acknowledge the distinction between theoretical and applied economics which he wanted to challenge.

Besides originating only from a mathematical exigence, the assumption of fixed coefficients (which in turns, implies constant returns to scale),[26]

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[25] See Akhabbar (2019: 155).

[26] See Casler (2004).

moreover, seems to involve a dispositional property which, since it is possessed by the system even in the absence of any change, cannot be formulated operationally (Akhabbar 2019: 153-155).

Substitution was not the only phenomenon which could not be accounted for: the adoption of fixed coefficients also implied that technological change was excluded from the model. While this is a problem which Leontief shares with general equilibrium theory, which assumes technological changes to be exogenous, it proves to be too strict an assumption if the goal is to study the development of an economic system over time.[27]

While input-output analysis can actually offer useful insights on the structure of a complex economic system by taking account of the actually employed technologies in a given instant of time, it still relies on an unoperational assertion which is superimposed upon experience. Confrontations between data collected in different years are nonetheless possible, and provide us with useful information in the changes in the structure of an economic system and in its tendencies of development, but do not provide us with a theory which can account for these changes.[28] Strange as it may be, such an analysis is at the time too grounded in experience to provide a theory,[29] and too grounded in theory to account for experience.

Conclusion

The paper has shown the common dependence of these three authors on: 1) the attempt to go further than Pareto in his anti-deductivist stance and in his criticism of the concept of force; 2) the appreciation for interdependence and the common employment of a general equilibrium framework; 3) the attempt to redefine economic concepts by operating on measurable data; 4) the permanence of non-operational propositions despite their stated methodology.

The desire of Sraffa, Samuelson and Leontief to respect the conditions of reproduction of general equilibrium clashes with their intent to remain faithful to observable experience and to rebuild theory by relying only on magnitudes which could be the object of direct measurement. The pretense to immediately bring together theory and experience results in a the-

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[27] A given linear technology which does not change over time is also presupposed in Leontief's dynamical model. See Kurz and Salvadori (2000).

[28] On Leontief's concern with the tendencies of capitalistic development, see Leontief (1938: 5), Leontief and Duchin (1986), and Akhabbar (2019).

[29] See Kurz and Salvadori (1995: 394).

ory which reasserts itself more dogmatically – since its assumptions aren't discussed – and in an impoverished understanding of experience, confined to unrelated instantaneous frames ('snapshots'). If these authors were right in criticising the introspection and the rigid deductivism employed by early marginal theorists, this deposes only against a particular theory, and not against the role of theorising itself in providing for a meaningful framework which enable us to read experience – as the debate had earlier acknowledged in epistemology after criticisms made by Carnap and Popper and in physics following Einstein's rebuke of Bridgman. Even before Sraffa's, Samuelson's and Leontief's attempts to recast general equilibrium theory in order to bring it nearer to experience, operationalism had already been pronounced as a dead-end because its requirements were too narrow for stating any general theory. It should therefore bear no surprise that the constitutive impossibility of implementing Bridgman's program became evident in the more or less explicit persistence in these authors of the aprioristic assumption of rationality (either as a vestigial remnant of utility theory, as in Samuelson's case, or as in the treatment of the choice of technique in the neo-Ricardian tradition). Moreover, no attempt was made to actually bring theory closer to experience by overcoming the inability of general equilibrium theory, rooted in the employment of simultaneous equation, to deal with historical time.[30]

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30 See Robinson (1974).

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