

164 *History of Political Economy*

this is of interest primarily if one then counterpoises a supply schedule of labor to the "demand relationship."

To recapitulate, Wells is certainly correct in observing that Keynes, in *The General Theory*, never wrote about, or thought in terms of, an aggregate production function $q = f(N)$ whose derivative with respect to labor services could be set equal to the real wage to give a demand curve for labor like $w/p = f'(N) = MP_L$.

Wells' equations, his embodiment of his observation, permit just that interpretation.

KEYNES' EMPLOYMENT FUNCTION

Comment by Axel Leijonhufvud

I fully agree with E. R. Weintraub on the two main points: (i) Wells is correct in substance in rejecting those interpretations of the *General Theory* that attribute to it a "labor demand" function identical to the marginal product of labor schedule;⁴ (ii) but Wells' equations permit the interpretation that he wants to exclude, and his accompanying verbal argument is not likely to convince the authors that he criticizes.

The problems are semantic and conceptual and hence cannot be settled by recourse to algebra or geometry until we have a prior consensus on the use of terms and on the *conceptual experiments* underlying the "schedules" given algebraic representation. The first obstacle to coming to grips with the issue is the notion that a well-defined and unified body of "neoclassical" (or "Classical") theory exists, for it is mainly neglect of the differences between Marshallian and Walrasian habits of thought that becloud our understanding of it. Most of the recent writings on Keynes' theory, including my own, insist on examining it in a Walrasian perspective. For various reasons that will here have to be put aside, it is useful and instructive to do so. But Keynes was, of course, a price-theoretical Marshallian, and in the present context,

4. The value of the marginal product and the marginal value product is kept out of the discussion below. Note, however, that Keynes' First Classical Postulate does *not* restrict us to constructions yielding the marginal physical product schedule. First, he follows the postulate (p. 5) with the clarification: "That is to say, the wage of an employed person is equal to the *value* which would be lost if employment was reduced by one unit. . ." (italics here). Second, Joan Robinson's *The Economics of Imperfect Competition*, 2d ed. (London, 1969), p. 237, defines *marginal productivity* as "the marginal physical productivity multiplied by the marginal revenue"—undoubtedly indicative of accepted Cambridge terminology of the 1930's.

ignoring this fact will simply not do. This is Weintraub's reason for emphasizing the *supply-price*, rather than *supply*, of output as the counterpart to the amount of employment offered; yet, some further remarks may be found helpful.

"Advanced microtheory," today, is all Walrasian. Suppose we read Wells' paper from that standpoint. We arrive at his equation (1)—introduced as a "microeconomic equilibrium condition." Dividing through by w —not prohibited, is it?—it states that the real wage will equal the marginal (physical) product. That settles it. Why read further? The first commandment of Walrasian analysis is always to distinguish clearly between individual and market experiments. Market excess demand functions are built up by, first, conducting individual experiments whereby the individual transactor's net demand (or net supply) is determined by invoking the appropriate optimality condition for various prices and, second, aggregating the resulting endowment-constant individual schedules. Only at the third, distinct, stage do we get to the question of what the equilibrium price will be—the market experiment.

If, in this fashion, we interpret Wells' equation (1) as an *optimality* condition which, given the production function, defines the supply of output and corresponding demand for labor for the individual *price-taking* firm and for the aggregate of such firms, there is no need to proceed. The "labor demand function" has been unambiguously defined. Equation (5)—Keynes' "Employment function"—can be dismissed as a reduced form, mingling demand, supply, and market equilibrium conditions, and thus irrelevant to the question of what the demand function is. Similarly, Wells' verbal statement that "the real wage does not determine the level of employment. Rather, it is the level of employment that determines the real wage rate" will be regarded as obfuscation. What sense is there to such statements in the context of a simultaneous equation system?

As always in Walrasian analysis, the conceptual experiment defining this labor demand curve has "planned" (optimal) quantity expressed as a function of independently given price. The Marshallian counterpart has demand-price as function of quantity. In the usual competitive equilibrium contexts, we are so used to taking the inverse of the Walrasian function to get the Marshallian, or *vice versa*, with impunity that the operation hardly gives us pause. For the equilibrium analysis of non-competitive industries, of course, it won't do, but that is not the problem here. What about the *disequilibrium* analysis of industries composed of "atomistic" firms? Can we still "invert" back and forth between Walrasian and Marshallian functions—assuming, of course, that they are algebraically equivalent?

For disequilibrium contexts, to do so invites some not-too-subtle confusion. Recall that the Walrasian demand curve is obtained through aggregating the results of well-defined individual experiments *antecedent* to the imposition of any market equilibrium condition. This demand function we will obtain whether or not we then go on to consider the consequences of the interaction of transactors in the market.

Not so with the corresponding Marshallian experiment. Here we start with a given quantity of output to be produced and a corresponding quantity of the variable factor, labor, to be employed. What now is the "optimal" labor demand price for the individual firm? Not the real wage corresponding to the marginal product of that volume of employment, certainly—that is simply the *maximum* that the firm would pay voluntarily. If it can get away with paying less, it will be better off. In order to get the individual firm "up onto" the marginal product schedule, we have to add in a market experiment of sorts—i.e., assume that competition among firms will force w/p up until it corresponds to the marginal product. This same competitive process would also force labor into its highest valued use so that all firms pay the same real wage and evidence the same marginal product of labor. Note that without invoking this assumption about market interactions among transactors, we could not aggregate over individual firms to obtain a market labor demand schedule.

In short, switching from the Walrasian $N^d = f(w/p)$ to the Marshallian $(w/p)d = f^{-1}(N)$, we sneak a market equilibrium condition into the latter that was not present in the former.⁵ The two geometrically congruent schedules are conceptually distinct. We can then realize the ambiguity of Wells' characterization of his equation (1) as a "micro-economic equilibrium condition," since currently popular usage allows using the term "equilibrium" interchangeably with "optimal decision" in reference to individual experiments. Walrasians are hence free to read equation (1) as deriving simply from the optimality condition applying to each individual firm. For Marshallians, it needs to be phrased as a condition applying to firms when the *market* is in equilibrium.

The above sketch of the Marshallian analytical procedure is, of course, incomplete. It starts by taking a quantity of output and corresponding quantity of labor input as "given"—somehow. But how? Again, we are faced with the impossibility of drawing a clear line between individual and market experiments as would be the Walrasian fashion. To be able to say anything definite about what the individual

5. The two varieties of neoclassical analysis differ also in their definitions of the "equilibrium" concept. In brief, the Marshallian concept denotes "constancy of behavior," the Walrasian "consistency of plans."

“representative firm” will be doing, we must *backtrack* from the Marshallian market equilibrium condition of equality between demand price and supply price. (The *ex ante/ex post* distinction goes the same way as the individual/market experiment distinction in Marshallian constructions.) With regard to the case at hand, nothing comes out on the left side of the Employment function—equation (5)—unless the value for *effective demand* is put in on the right side.

If the Marshallian experiment is *indeterminate* when effective demand is *not* introduced, the Walrasian becomes *overdetermined* if it is.⁶ The Walrasian firm equates the announced real wage with its marginal physical product of labor to calculate its demand for labor and supply of output; planning in this way implies the belief that this volume of output can be sold at the announced price. If one were now to add an independently specified “sales-expectation” to this conceptual experiment, it falls apart—the firm is supposed to have two different beliefs about sales at the same time. In the discussion of recent years,⁷ the usual resolution of this inconsistency has been to replace the independently specified sales-expectation with a constraint on *realized* sales in the face of which (it is reasonable to suppose) the firm gives up the notion that

6. Patinkin's chapter 13, “Involuntary Unemployment,” has always struck this reader as a strange interlude in a justly celebrated masterpiece; its “off-curve analysis” seems tortured, obscure, and *ad hoc* in curious contrast to the smoothly developed, supremely lucid, and painstakingly rigorous development of the book's main themes. Patinkin seems not to perceive that the way in which the problem has been set up makes it overdeterminate. His discussion suggests that he hopes one “can get around it” somehow, although the way out is not yet clear. Cf., e.g., p. 323 n.: “There is, nevertheless, a basic analytical problem here whose full solution is still not clear to me.”

Cf. also the critique by Professor Davidson in “A Keynesian View of Patinkin's Theory of Employment,” *Economic Journal*, Sept. 1967.

7. Following Clower's “dual decision hypothesis.” The Walrasian general equilibrium model deals exclusively with (*ex ante*) “consistency of plans” problems; it does not even ask any questions about the execution of plans or about realized results. Keynes' Marshallian construction, in contrast, describes actual (*ex post*) behavior, but leaves “notional” magnitudes undefined. The dual decision hypothesis forces realized sales—a Marshallian element—into the frame of the Walrasian notional calculus. The result is an apparatus that enables us to keep track of “effective” and “notional” solutions at the same time. By highlighting the tension between where the system is and where it *could be* in this manner, it directs analytical attention to *communication* (effective demand) *failures* as possible sources of system malfunctioning. Yet, the dual decision hypothesis is a not altogether clear mix of Marshallian oil and Walrasian water. While it has served admirably as a vehicle for escaping from the cul-de-sac in which Keynesian theory had landed ten years ago, it is unlikely that it would hold up as the linchpin joining disequilibrium macrotheory to explicit and consistent microfoundations. Clower himself chose to abandon it even before the spate of attempts by others to develop it got under way. Cf. his “A Reconsideration of the Microfoundations of Monetary Theory,” *Western Economic Journal*, March 1968.

it could sell its optimal output. Naturally, this necessarily implies that it also gives up the corresponding "notional" input demand. Whatever labor demand function might be defined for this experiment,⁸ one thing is clear—it cannot be a labor-demand schedule coincident with the marginal physical product schedule. *A fortiori*, Keynes cannot be saddled with an MPPL labor demand construction, for his discussion of entrepreneurial behavior takes as its point of departure an independently specified short-term sales-expectation which he then—in characteristic Marshallian fashion—proceeds to merge with realized sales proceeds (Keynes, chap. 5).

If, then, Keynes did not employ an MPPL labor demand schedule, readers may still wonder at Wells' denial that "Keynes specified the demand for labor to be a (i.e., some other) decreasing function of the real wage rate." The real wage rate, after all, appears on the right side of equation (5). Would not taking the partial derivative of N with respect to w , holding effective demand constant, have to show that such a decreasing function is implied? The answer is a familiar one: trying to deduce the consequences of "an arbitrary change in price" is impermissible here, since it violates the *mutatis mutandis* conditions built into the construction we are dealing with. The relevant experiment that is permissible is to let the aggregate demand function shift, tracing out successive effective demand points on the aggregate supply price schedule. In the original version of the Employment function, the variables would have been scaled in wage-units rather than the units of real output of Wells' equation (5). Conducting the experiment with the original form of the function, there is only one value of p that may be associated with each value of effective demand, namely, that price which will clear the market for output. That is the *mutatis mutandis* condition. Letting effective demand vary, while obeying this condition, we will obtain a locus of solution points for w/p and N in the input market that does trace the MPPL schedule.

One can then appreciate Keynes' careful use of words in the quotation given by Wells: "real wages and the volume of output (and employment) are *uniquely correlated* . . . an increase in employment can only occur *to the accompaniment of* a decline in the rate of real wages."

8. When the sales-expectations implied in the standard Walrasian conceptual experiments are overridden, the corresponding optimality condition is necessarily also abandoned. A demand-locus for labor corresponding to the independently specified sales-expectations requires some new set of optimality conditions to characterize it. The problem is in some respects analogous to that (mentioned in the text) of specifying what a Marshallian firm would choose to pay for inputs in a situation of market disequilibrium.

This would be odd language indeed for a Walrasian to use; it is incumbent upon a Marshallian to put it in this way.

In the review article of my book that Wells cites, Grossman is completely wrong on the pivotal point of his entire argument—which was that “the *General Theory* repeatedly assumes that firms refer only to the real wage in determining their demand for labor, and Keynes never even suggests the notion of a sales constraint” (Grossman, p. 29, n. 13). (Grossman argues this case without once making reference to either chapter 5 or chapter 20 of the *General Theory*!) Our analysis above also shows, however, that Grossman was right in his criticism of my book on another point: it is *not* correct to attribute to Keynes a general reversal of the Marshallian ranking of relative price and quantity adjustment velocities. In the “shortest run” for which system behavior can be defined in Keynes’ model, output-prices *must* be treated as perfectly flexible. The Marshallian ground rules of his analysis will not accommodate a still shorter Hicksian “fix-price market day.”

That the analysis of this issue in the *General Theory* was rigorously carried out in obedience to Cantabrigian tradition does not mean that it produced true results. The predicted “unique inverse correlation” was soon challenged, on empirical grounds, by Dunlop and Tarshis. Keynes’ lengthy reply was “pragmatic” in the extreme.⁹ Of the many possible reasons that he canvassed why actual observations apparently did not trace an MPPL locus, one is worth noting here—the paper speculated at some length on changes in the “degree of monopoly” as possibly responsible. The possibility of some “stickiness” in output prices was mentioned, but not seriously pursued in an analytical manner. Particularly striking by its absence is any reference to what would seem the most simple and obvious reason why observed points would lie below the Marshallian employment schedule during unemployment periods—namely, decumulation of inventories, opening up a gap between the rate of output and the rate of sales. This case, moreover, could well have been handled without another “long struggle to escape from habitual modes of [Marshallian] thought.”

In developing Keynes’ Marshallian construction of the labor demand-price schedule, we stressed two assumptions: (a) atomistic firms, and (b) a “very rapidly” converging competitive process forcing p to the level where w/p would correspond to the marginal product of the pre-

9. Cf. John T. Dunlop, “The Movement of Real and Money Wage Rates,” *Economic Journal*, Sept. 1938; and Lorie Tarshis, “Changes in Real and Money Wages,” and J. M. Keynes, “Relative Movement of Real Wages and Output,” both in *Economic Journal*, March 1939.

170 *History of Political Economy*

vailing level of employment. When the theory appeared endangered by empirical evidence, Keynes' suspicions focused on the first of these assumptions. The second was and is *at least* as suspicious.

A more complete explanation of the differences between the two modes of "neoclassical" analysis, and exploration of their wider ramifications, would require at least a full-length article. That task will have to be postponed until a later occasion.¹⁰

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10. Cf. Leijonhufvud, "The Varieties of Price Theory: What Microfoundations for Macrotheory?" forthcoming.