mations in most circumstances. Moreover, such empirical findings as the fact that an aggregate Cobb-Douglas production function (sometimes) appears to give a good explanation of wages cannot be used to justify the existence of aggregate production functions.

Interestingly, these problems do not merely apply to the meaning of aggregate capital. They extend to the meaning of aggregate output and aggregate labor (a fact only partially recognized by Cohen and Harcourt). Aggregate production functions and aggregate capital do not generally exist—although such nonexistence has little to do with the arguments advanced by the Cambridge, U.K., school, and, indeed, in most cases, the difficulties stem not from any special properties of capital but from the necessity of aggregating over firms. Further, any proposition about the behavior of (supposed) aggregates that stems from treating them as though they actually were related through a production function is extremely dubious. It is therefore not surprising to find such paradoxes as the Sraffian ones of "reswitching" or "reverse capital deepening."

Note, however, that these results have no implications for neoclassical (or any other) microeconomic analysis. They are important only as regards macroeconomics.

Although the starting point of both the Cambridge, U.K., view of capital and that of the aggregation literature is radically different, the conclusions, for the purposes of applied economists, seem to converge: The existence—and hence the use—of aggregate production functions is very problematic. The conditions for successful aggregation are so stringent that one can hardly believe that actual economies satisfy them. If no optimization condition is imposed on the problem, aggregation over sectors is possible if and only if micro production functions are additively separable in capital and labor. Even after imposing sensible efficiency conditions, the requirements for aggregation remain extremely restrictive. The existence of a labor aggregate, for example, requires that there be no specialization in employment with all firms hiring the same vector of labor types except for scale. Similarly, where there are many outputs, an output aggregate will exist if and only if all firms produce all outputs in the same proportions. This requires the absence of specialization in production, that is, all firms must produce the same market basket of outputs differing only in their scale. Without such conditions (and equally stringent ones for capital), the aggregates cannot be generated, and the aggregate production function will not exist.

In view of all this, one would have supposed that the use of aggregate production functions would have ceased long ago, but this is not the case. Such use is alive (if not well) in growth and trade theory, with the results listed above generally ignored, if, indeed, they are known at all. And a subject that ought to have died long since is still with us.

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In their review of the Cambridge capital theory controversies, Avi Cohen and Geoff Harcourt (Winter 2003, pp. 199–214) show how damaging the conclusions of this debate were for the theoretical foundations of the aggregate production function. So why is the aggregate production function still widely used in neoclassical macroeconomics, even after the legitimacy of the Cambridge, U.K.'s, critique was explicitly acknowledged by Samuelson (1966)? In our view, the most important answer is that the statistical estimation of aggregate production functions has often, although not always, provided good fits with plausible parameter values. As Cohen and Harcourt (2003, p. 209) note, the orthodox response is that "these 'low brow' [one-sector] models remain heuristically important for the intuition they provide, as well as the basis for empirical work that can be tractable, fruitful and policy-relevant."

The neoclassical aggregate production function is not only a construct with questionable theoretical foundations, but its use cannot be justified on the grounds that when estimated empirically it gives good statistical fits. While this critique can be found obliquely in a number of articles in the 1930s and 1940s, its first coherent statement was put forward by Phelps Brown

(1957) and was formalized later by Shaikh (1974, 1980) and Simon (1979). As Herbert Simon said in his Nobel Memorial lecture, the good statistical fits to the Cobb-Douglas production function "cannot be taken as strong evidence for the [neo]-classical theory, for the identical results can readily be produced by mistakenly fitting a Cobb-Douglas function to data that were in fact generated by a linear accounting identity (value of output equals labour costs plus capital cost)."

The problem with the instrumentalist defense of the neoclassical production function is that it may be derived from an income identity that makes no assumptions at all about technology of an underlying aggregate production function, which in all probability does not exist. The critique is straightforward. Define value added for an industry as the sum of the wage bill, W, and total profits, P, so that $V_t \equiv W_t + P_t \equiv w_t L_t + r_t K_t$, where V, w, L, r and K are constant-price value added, the real wage rate, total employment, the observed rate of return or rate of profit (not the user cost of capital) and the constant price value of the capital stock. Assume each firm sets prices by a constant markup on unit labor costs so that factor shares are constant. Totally differentiating the identity with respect to time and then integrating and taking antilogarithms gives $V_t = Bw_t^a r_t^{1-a} L_t^a K_t^{1-1}$, where B is the exponential of the constant of integration and a is the share of labor in value added (i.e., $a = w_t L_t / V_t$).

If we further assume that the real wage rate grows at a constant trend rate, denoted by λ per annum, and the rate of return is constant, then the approximation to the identity will be given by $V_t = Be^{a\lambda t} L_t^a K_t^{1-a}$. In other words, it gives a form that resembles the Cobb-Douglas "production function," where the output elasticities are *definitionally* equal to the factor shares. Remember that in deriving this expression, no assumption has been made about the underlying technology or the state of competition. Therefore, this expression is not a technological relationship, but the identity rewritten under these assumptions.

If the assumptions of constant markups, constant growth in real wages and constant rate of return, which are not solely dependent on neoclassical production theory, happen to be correct, estimation of the putative aggregate production function $V_t = Be^{\gamma t} L_t^\alpha K_t^\beta$ will give a misleadingly close statistical fit (potentially one) because it is an approximation to the underlying income identity. It will also erroneously suggest that the output elasticities are equal to the relevant factor shares, hence "confirming" the neoclassical theory of factor pricing, and that constant returns to scale prevail. If, on the other hand, the assumptions do not hold exactly, then the estimates could suggest increasing returns, but only because of the biases involved.

We have derived from the identity more general "production functions," such as the CES or translog (Felipe and McCombie, 2001, 2003), and have also evaluated some of the current discussions in macroeconomics, such as the endogenous growth models (e.g., the existence of increasing returns), the procyclicality of productivity or the existence of market power (Felipe, 2001; Felipe and McCombie, 2002, 2003; McCombie, 2000-2001), in the light of our arguments. As Joan Robinson (1970) pointed out, the aggregate production function "must have needed an even tougher hide to survive Phelps-Brown's (1957) article on 'The Meaning of the Fitted Cobb-Douglas Function' than to ward off Cambridge Criticism of the marginal productivity theory of distribution."

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Avi Cohen and Geoffrey Harcourt (Winter 2003, 17:1, pp. 199–214) ask, "Whatever Happened to the Cambridge Capital Controversies?" For me, they ended when Leland Yeager wrote "Toward Understanding Some Paradoxes in Capital Theory," showing that the paradoxes traced to measuring capital in purely physical terms and thus were not paradoxical at all. The article took *Economic Inquiry* article-of-the-year honors for 1976, yet Cohen and Harcourt neglect it altogether.

Yeager deploys Samuelson's champagne example, in which either of two techniques will produce one bottle of the beverage. Technique A requires 7 units of labor two periods before the champagne is ready and no more labor. Technique B requires 2 units of labor three periods before and then 6 units of labor one period before the champagne is ready. Simple financial arithmetic makes Technique A the more profitable at interest rates above 100 percent, Technique B the more profitable at rates between 100 and 50 percent and Technique A the more profitable, again, at rates beneath 50 percent.

Yeager shows and perhaps even intuition suggests that, in purely physical terms, Technique A is the more capital-intensive technique. As he says, however, the critical point is that the physically more capital-intensive technique, whichever it is, remains so, at every interest rate—tons of steel are tons of steel. If the capital-intensity award had gone to Technique B, then the interest rate's declining through 50 percent would have brought capital reversal, a switch to the less capital-intensive technique. With Technique A as the physically more capital-intensive, the rate's declining through 100 percent brings capital reversal, and its further declining through 50 percent, reswitching.

This sort of example gave the English Cambridgeans a chance to cry "paradox," but there really was none. The interest rate, fully in accordance with the logic of the price system, rations the scarce factor of production that Yeager, following Gustav Cassel and before him Robert Turgot, calls "waiting for value through time," or "capital disposal." The amount of that factor required to produce a bottle of champagne depends partly on the factor's own price, the interest rate. A lower interest rate reduces the required value amount of waiting. Owing to the difference in inflow-outflow patterns, however, the required value amount of waiting doesn't decline uniformly across the two techniques. When the lower interest rate brings a switch to the other technique, the new technique really *is* the more profitable, at the lower interest rate. Why, Yeager asks, should it count as an indictment of economic theory that, judged by a fundamentally noneconomic (i.e., physical rather than value-based) criterion, the switch seems perverse?

Yeager disposes of the reversal-and-reswitching paradox, but he leaves another capital paradox to puzzle over. Why, I wonder, did the English Cambridgeans, or neo-Ricardians, fret about paradoxes that arise as a result of measuring capital physically? Legend has it that Joan Robinson, a leading neo-Ricardian, would write a production function that included as an input "Mrs. Robinson's house" and then invite any student who found the marginal productivity theory of interest plausible to step forward and differentiate the production function with respect to her house. But for neo-Ricardians, doesn't capital resolve itself, ultimately, into labor? Shouldn't "Mrs. Robinson's house" have vanished before the student had reached the chalkboard?

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