production are labor, education-skills, machines, and buildings (including residences). Variations in factor supplies should show themselves in factor returns. Likewise, variation in income inequality is hard to attribute to wealth ownership, or human capital investment or to differential shifts in rewards to factors like raw labor, experience-skills, education-skills, and machines. Rognlie thus concludes that "concern about inequality should be shifted away from the overall split between capital and labor and toward other aspects of distribution, such as the within-labor distribution of income." The only dissent I wish to make is this: Rognlie is correct, today, but if Piketty is right he may no longer be correct in 50 years.

Matthew Rognlie's conclusion is bad news for us economists. It leaves us in the same position as those trying to explain an earlier large puzzle in the production function, the twentieth-century retardation of the British economy. It was Robert Solow (1970) who said: "Every discussion among economists of the relatively slow growth of the British economy compared with the Continental economies ends up in a blaze of amateur sociology" (pp. 102–3). But this time, I really would like us to be able to do better than we did then.

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COMMENT BY

ROBERT SOLOW Matthew Rognlie's excellent paper circles around a fundamental question in medium-run macroeconomics: how strongly, if at all, does the rate of return on capital fall as capital intensity increases? I describe it as fundamental because it lies at the heart of at least two important and contentious current issues. Capital intensity may be increasing for some time in developed economies if only because the growth of population will slow with no commensurate reduction in saving. Then the behavior of the return on investment will certainly affect the demand for investment and thus the plausibility of secular stagnation. In addition, the response of the rate of return will affect the functional distribution of income between compensation and profits and thus, eventually, the degree of income inequality, which is already a political issue, at least rhetorically. (It is interesting, although not directly relevant, that another imponderable, the likely future of total factor productivity, connects both these issues: rapid technological progress could sustain the return on investment as it has in the past, but that may not happen again.)

This question of diminishing returns to capital intensity has preoccupied economists for a long time, from Ricardo and Mill to Keynes and Schumpeter. As an indication of how little was ever settled, it is not so long ago that growth theory was littered with so-called "AK models" that were founded on little more than the assumed absence of diminishing returns to capital intensity. Those models are not so fashionable now. So at last I find myself with the delightful task of discussing a paper—by someone younger than several of my grandchildren—that makes a serious and intelligent effort to see what we know or what we might be able to find out about diminishing returns to capital intensity. No doubt this effort was stimulated by the Piketty phenomenon, but it is of more general interest.

CAPITAL SHARE AND RETURNS TO CAPITAL The paper does a useful service by documenting in some detail that a substantial fraction of recent real capital accumulation in the United States took the form of land and buildings, including housing. How should we think about this fact? For some purposes we can say (and do say) that houses just represent a very capital-intensive form of production: they produce housing services, measured by market and imputed rents, with very little labor input. That is okay for national income and product accounting, but it misses the deeper point: we are really interested in the intensity of diminishing returns to capital.

For estimating an economywide elasticity of substitution, it would be better to eliminate the housing stock and associated land on the capital-input side and the rents on the output side, recognizing that the motives underlying behavior are slightly different from those whose effects we are trying to isolate. I would also favor eliminating some other sectors: financial services, because it is so unclear what one means by output; unincorporated enterprises, because it is impossible to separate labor income from return to capital; and general government, because the accounting conventions make no sense. The usual calculations of the elasticity of substitution should probably be confined to the inputs into and the value-added produced by nonfinancial corporations, just under half of gross domestic product. The paper does, very sensibly, omit unincorporated enterprises and general government, but it includes financial corporations along with nonfinancial. I would recommend excluding them as well. In the 1960s and 70s, the profits of financial corporations were about 15 percent of all corporate profits; just before the financial crisis they were up to nearly 40 percent of the total (and are rather less now). I cannot believe that this has anything to do with the marginal product of capital, as we understand that notion, or with the substitutability of capital for labor.

The paper spends more time and effort than I would have done on the consequences of the growth of housing for the economywide share of capital. This is not to say that the accumulation of capital in the form of housing is not important for the understanding of capital accumulation and the functional distribution of income. But one has to recognize that much of that capital is acquired as a store of value (and perhaps a vehicle for speculation) rather than as a productive input. This is certainly true of the 20-million-dollar condominiums bought by crooks from Russia, Latin America, and elsewhere, and their offspring. It probably also played a substantial part in the housing boom and bubble of the previous decade, although that may of course change. Whether willingness to invest in housing can provide an offset to otherwise excess saving and might thus be a factor in warding off secular stagnation is a possibility; but it seems like a weak reed. If diminishing returns should drive down the return on industrial capital, would that increase the demand for housing? There is not much evidence.

This question of the relation between housing and the relative share of capital reminds me of a complaint that I have been nursing. It is directed not at this excellent paper but at the literature. The 19th century German mathematician Leopold Kronecker—he of the Kronecker delta—is supposed to have said: "God created the integers; everything else is the work of man." There is a strong implication that God knew what She was doing, but mankind has made a mess of the rest. If Kronecker had been an economist he might have said that God created prices and quantities, and all the rest is a manmade mess. The real subject of Rognlie's paper is the effect of increasing capital intensity on the rate of return. To put it in terms of a relative share—a ratio of prices times a ratio of quantities—is to add unnecessary complication to an already complicated question. Rognlie does a nice, clearheaded job, and he has some very interesting things to say. It is the literature that creates a detour.

I do not want to spend much time on the net-gross distinction. Once one focuses on the rate of return, it becomes obvious that the return net of depreciation is what matters, both for distribution and with respect to investment demand (and hence secular stagnation). Nevertheless, it is worth remembering that the only reason research has devoted so much effort to gross concepts was the sense that measured depreciation might verge on the meaningless because it reflected accounting conventions and tax incentives that had little or nothing to do with the changing productive capacity of existing plants and equipment. The conceptual basis of the data might be much better nowadays. One further reminder: modelers now universally assume, without comment, that depreciation is proportional to the stock of capital. This is an overwhelmingly convenient assumption: it is the only assumption that makes depreciation independent of the history of gross investment. Convenience may be its only advantage. Back in the early years of my research, when I used to see an occasional survival table for some class of capital goods, what I saw did not look much like declining exponentials. Maybe this does not matter, but how do we know?

ROGNLIE'S "PURE PROFIT"—AND ITS IMPLAUSIBLE VALUE The most exciting result in Rognlie's paper is his finding that, during the postwar period, most of the action in the distribution of corporate value-added (after taxes on production) comes not in the compensation of labor nor in the market return to capital but in a *residual.* He calls it "pure profit," but I like to think of it as monopoly rent, broadly conceived. This is a big deal, because it can help to explain many things, but it is also a big annoyance because it makes for very difficult analytical-empirical problems.

The easy way to solve them is to just assume that value-added is divided between labor and capital roughly in accord with marginal products; this is of course the competitive allocation. But I suspect we do not believe it is true. A corporation facing a demand curve with elasticity ε (a sort of "as if" elasticity reflecting many things) will choose inputs and output so that each real factor price is $(\varepsilon - 1)/\varepsilon$ times its marginal product. The result will be a monopoly rent equal to a fraction $1/\varepsilon$ of value-added. Looked at differently, $1/\varepsilon$ is equivalent to (price − marginal cost)/price. It is what Abba Lerner long ago defined as "the degree of monopoly" for that firm or for the representative firm. According to Rognlie's calculations, that is what has been rising for U.S. corporations since about 1980. So, how big is it?

According to Rognlie's calculations, $1/\varepsilon$ averages to about zero, and it manages to grow only by going from negative to positive. This strikes me as wholly implausible. It worries Rognlie, too. He appeals to the idea of Chamberlinian large-group monopolistic competition: free entry over-crowds the market and drives pure profit to zero. But this way out seems just as implausible: it is precisely barriers to entry, of which there are many, that create monopoly rents in the first place. The full calculation leads to the further conclusion that the market return on capital was about 13 percent a year between 1950 and 2010, if it is assumed to have been constant, and to have fallen from above 16 percent in 1950 to below 12 percent in 2010 if it is allowed to have a linear trend. A quadratic trend does no better in the author's figure 7. It is hard to believe that the discount rate was this high from 1950 to 2010. (Household saving was available at an interest cost of 4 to 5 percent; one would have expected more investment to have taken place.) If the market rate of return were assigned a lower value, presumably the estimated monopoly rent would be a larger fraction of value-added.

All of this provokes an interesting question, to which I do not have an answer: Why do Rognlie's sensible calculations conclude that pure profit or monopoly rent was negative nearly all the time between 1950 and 2010? (or, almost equivalently, Why was his version of Tobin-Brainard's q less than one most of the time?) Equation 5 in the paper looks very busy, but the basic idea is simple and smart: the difference between the stock market value of a corporation and the "book value" of its assets is interpreted as the present discounted value of the anticipated stream of rents. Maybe the version of book value that he uses, in which physical capital appears not as reproduction cost but at historical value (or something else), is peculiar, especially when there is inflation. Maybe stock market valuations are equally garbage-ridden. Rognlie needs to use the difference between these numbers, which must certainly have a lot of noise, and not necessarily white noise.

The best suggestions I can manage are a couple of almost-constructive suggestions for further work. First, I think it is essential to get the financial services industry out of the calculation. The profits of financial firms, mostly from trading and mostly from asymmetric information, are not to the point here. Second, a clearer picture would allow for the fact that recorded wages include a certain amount of monopoly rent. This is obviously true of executive compensation, but even garden-variety compensation has a nontrivial rent component.

THE PRICE-TO-MARGINAL-COST RATIO The real issue here is the ratio of price to marginal cost in American industry (or nonfinancial industry, as I would prefer). There is a large literature on average mark-ups of price over cost, mostly concerned with cyclical behavior. Much of it is summarized and discussed in the article by Julio Rotemberg and Michael Woodford (1999), cited in Rognlie's paper. But I am more interested in work that aims explicitly at the ratio of price to marginal cost ($\varepsilon/(\varepsilon - 1)$ in that notation). Robert Hall (1986) estimates that ratio to be between 2 and 3, which would imply that monopoly rents amount to between 1/2 and 2/3 of value-added. That seems shockingly high. Mark Bils (1989) has an ingenious method that puts rent at about 30 percent of value-added. Both of those papers go back to the 1980s; if Rognlie is correct, as I think he is, the right number, whatever it is, would be higher now.

At the BPEA conference where Rognlie presented this paper, Robert Hall remarked that his *current* estimate of the ratio of price to marginal cost is about 1.2, which would make rent about 16 to 17 percent of value-added. He suggested that this might just about cover fixed costs, leaving net rent at zero. My conclusion is that the degree of monopoly in U.S. industry remains an open question and needs more research, both micro-economic and macroeconomic. The matter of fixed costs strikes me as more complicated. In the short run, one imagines fixed costs to be mainly capital costs. In the medium to long run, as in Rognlie's paper, capital costs are modeled explicitly and treated as variable. Remaining fixed costs are a little hazy.

All of this work makes a tacit assumption which, as I have already suggested, may be in error, namely that all of the rent accrues to the capital-income part of value-added. It seems likely that, at least in many industries, the reported compensation of labor includes some rent, either in the form of wages or benefits or working conditions. I have always taken it for granted that the division of rent was what collective bargaining was all about, back when there actually was collective bargaining. Even without formal bargaining, I would imagine that accepted business practices, social norms, and even public opinion, all have an influence on the division of rents within a firm and thus in the aggregate. It may not be mere coincidence that the share of rents accruing to the capital side began to rise about when Ronald Reagan was elected president.

Imagination is one thing; measuring what has happened will be very difficult. I would like to see Rognlie stay with this aspect of the problem. It has both analytical and policy implications. For instance, when it comes to estimating the elasticity of substitution, the presence of a significant amount of rent means that reported input prices (and relative shares) are a bad basis for inference. Unless factor prices can be purified of the rent element, the best (or only) bet would seem to be estimating production functions directly from data on inputs and output.

FINAL THOUGHTS This brings me to a final comment. Rognlie makes a valuable contribution by organizing a multisector model as a vehicle for some inferences about what matters most for movements in relative shares. There he simply assigns values of the elasticity of substitution to different sectors in accordance with the literature. That is a useful step. I want to suggest that a further extension in the direction of general equilibrium might even change the picture.

The fundamental question of interest is this: How far would the rate of return have to fall for the economy to absorb a likely increase in capital intensity? One way the economy does that is by substituting capital for labor in the production of final output. That is why that elusive elasticity of substitution enters the story. But there is another route by which the economy can absorb capital. When the return on capital falls, capital-intensive goods should become cheaper relative to labor-intensive goods. (Housing is one example, of course.) If these cost changes are passed into prices, consumers may shift toward more capital-intensive goods. The same process may affect producers' choices among alternative intermediate inputs.

The economy can become more capital-intensive even apart from shifts within production processes. I have no idea about the likely quantitative importance of this kind of adjustment, but there is no theoretical reason why it should be negligible.

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GENERAL DISCUSSION Robert Hall opened the discussion by observing that much of the literature, including Thomas Piketty's work, treats capital as a primary factor, whereas in his view capital is an intermediate factor. Following an Arrow-Debreu view of intertemporal economics, he said, people who own capital can be understood as having chosen to defer consumption. Agreeing with a point discussant Robert Solow had made in his comment, he said the purchase of land is an exception and must be considered a