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Commentary

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Basu and Fernald (2009) describe and evaluate alternative theoretical models of potential output to provide a frame of reference for policy analysis. They also discuss what is (and what is not) known about potential output and illustrate their approach by estimating a two-sector model with price rigidities.

I find the overall theme—that models ought to be used to guide policy choices—important and a welcome reminder of the value of using a consistent framework for policy evaluation. I wholeheartedly agree with the approach. When it comes to specifics, they conclude that to capture some essential features of the U.S. economy, the standard one-sector model should be abandoned in favor of a two-sector model with differential technological change. Here, I am not totally convinced by their arguments. The second major point that they argue—and I fully agree with them here—is that any useful notion of potential output cannot be assumed to be properly described by a smooth trend, and it is likely to fluctuate even in the short run. As before, their choice of model and the empirical strategy they use are subject to debate.

THE LONG RUN: WHAT SIMPLE MODEL MATCHES THE DATA?

Basu and Fernald argue that the appropriate notion of potential output is the steady state of an economy with no distortions. They consider two models: a standard one-sector model and a two-sector model with differential technological change across sectors. They derive the steady-state predictions in each case and confront the theoretical predictions about capital deepening—defined as the contribution of the increase in capital per worker to output—with the data. They conclude that the two-sector model, which allows for a change in the price of capital, outperforms the simple one-sector model.

At this level of abstraction, it is not easy to pick a winner. Basu and Fernald base their preference for the two-sector model on two different arguments. First, they show that in the data the relative price of capital has decreased substantially, which is inconsistent with the one-sector model. Second, they highlight the ability of the two-sector model to account for the low contribution of capital in the period of productivity slowdown.

Basu and Fernald's first argument—the change in the price of capital—is not completely persuasive. There is no discussion that capital has become cheaper, but this does not automatically imply that this fact is of crucial importance. Of necessity, models are abstractions of reality and, by their very nature, will miss some dimension of the data. To be precise, models that account for everything are so complex that they cannot be useful. Thus, adding a sector—which can only improve the ability of the model—cannot determine a winner. It is easy enough to find other

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Table 1

Capital's Contribution: Model Prediction/Data

Period	One-sector model	Two-sector model
1948-73	1.2	1.05
1973-95	0.4	1.06
1995-2000	0.6	1.75
2000-07	0.9	1.54
1948-2007	0.9	1.18

Table 2

Price of Capital (1948 = 1)

Period	Model	Data
1973	1.05	1.09
1995	0.90	0.91
2000	0.82	0.87
2007	0.77	0.86

changes in relative prices (e.g., some professional services) that would necessitate a third sector to accommodate them, and this approach would logically lead to a complex and useless model.

Basu and Fernald's primary reason for choosing a two-sector model rests in its ability to explain capital deepening. Table 1 presents the two models' predictions for capital's contribution to growth relative to the data for various time periods. Considering the longest available horizon (1948-2007), it is difficult to choose a winner. The one-sector model underpredicts the contribution of capital by 15 percent, while the two-sector model overpredicts it by 18 percent. Depending on the period, one model clearly dominates the other, but I see no reason to emphasize the 1973-95 period (where the two-sector model is a clear winner) over the 2000-07 period (in which the one-sector model dominates).

Basu and Fernald's preferred model is a two-sector version of the Solow growth model. Using data on the relative price of capital, they estimate the productivity growth rates in the general goods and investment goods sectors. Their estimate hinges on the assumption that the technologies in these two sectors are similar. In particular, letting $\alpha_c = \alpha_i$ be the growth rate of total factor productivity (TFP) in sector j, the specification implies that

$$\hat{P}_i - \hat{P}_c = \hat{z}_c - \hat{z}_i.$$

In a version of the model in which the capital shares are allowed to differ across sectors, the relative price of consumption satisfies

$$\hat{P}_i - \hat{P}_c = \hat{z}_c - \frac{1-\alpha_c}{1-\alpha_i}\hat{z}_i.$$

Valentinyi and Herrendorf (2008) estimate $\alpha_c = \alpha_i$ and $\alpha_i = 0.28$, which implies that, relative to Basu and Fernald's estimate, the productivity growth rate of the investment sector was about 9 percent higher. This implies that their estimates of the contribution of capital deepening must be increased by almost 10 percent, which exaggerates even more the overprediction of the two-sector model relative to the data in the recent past.

Even accepting as a reasonable approximation that $\alpha_c = \alpha_i$, there are two measures of the relative price of investment goods ($p_i = P_i/P_c$) that, according to the model, should coincide. One is given by

$$p_i = M_k\left(\frac{Y/L}{K/L}\right),$$

where $y = Y/K(k = K/L)$ is output (capital) per hour and M_k is a constant under the balanced growth assumption. Thus, the growth rate of the relative price of capital is

$$\hat{p}_i = \hat{y} - \hat{k}. \tag{1}$$

As above, the model implies that

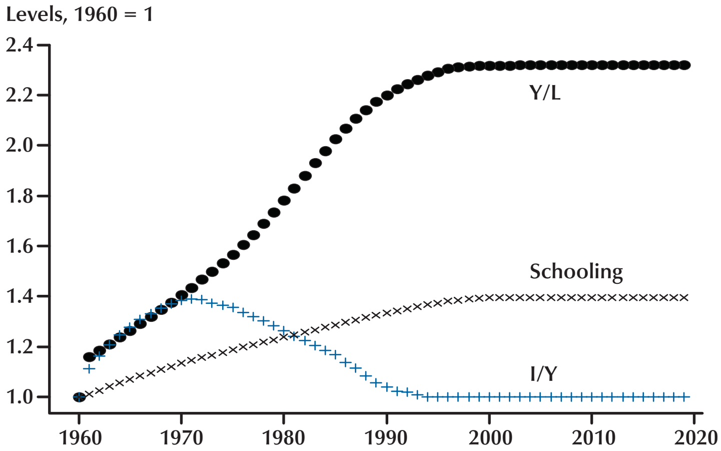
$$\hat{p}_i = \hat{z}_c - \hat{z}_i. \tag{2}$$

The estimates based on equation (1)—using Bureau of Labor Statistics data on output per hour and capital per hour—are presented in the column labeled "Data" in Table 2, while the values from equation (2)—based on model-produced estimates of productivity growth—are labeled "Model."

Because the model-based measure predicts a higher decrease in the price of capital, it is not

Figure 1

Transitional Dynamics: TFP Shock



surprising that the theoretical model tends to overpredict the contribution of capital to output. At this level of abstraction, it is not possible to identify the source of the problem. However, if the effective cost of capital is changing—a violation of the balanced growth assumption—then the "Data" estimate is biased. In any case, the difference should make us cautious about the appropriateness of the model.

Is it clear that balanced growth is a reasonable approximation in the long run, given the length of the horizon covered in the article? It is consistent with the findings of King and Rebelo (1993), who showed that for reasonable parameterizations, the standard growth model converges rather rapidly to its balanced growth path. However, recent work that retains the dynastic specification of preferences but specifies that individual human capital completely depreciates at the time of death (see Manuelli and Seshadri, 2008) has shown that even one-sector models can display very long transitions. Figure 1 presents the impact of a once-and-for-all permanent increase in the level of productivity. From the point of view of this discussion, the interesting result is how long it takes for the model to reach steady state: approximately 30 years. Thus, if human capital that "disappears" when an individual dies (even though dynasties have infinite horizons) is a realistic feature to incorporate in a model, the balanced growth assumption is difficult to justify unless the horizon is very long.

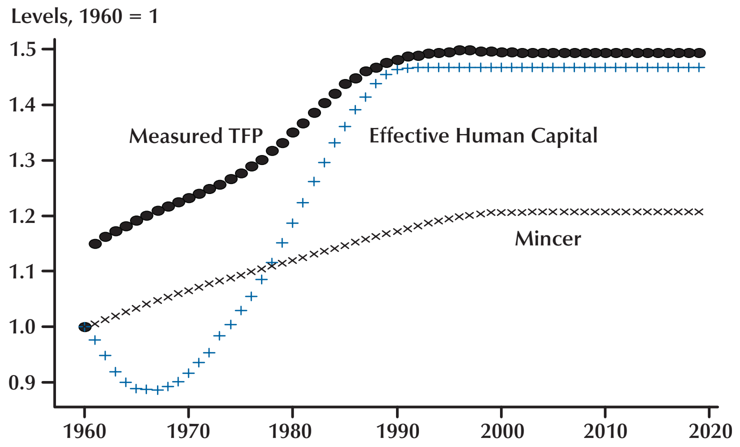
In this case, a second difficulty is associated with the measurement of productivity. In the model analyzed by Manuelli and Seshadri (2008), conventionally measured TFP and actual TFP do not coincide. The divergence is due to the endogenous response of the quality and availability of human capital after a shock. Figure 2 displays measured TFP (computed using the human-capital series labeled "Mincer"), which shows an upward trend—that is, one displaying growth—while "true" TFP jumps in the first period (labeled 1960 in the figure) and remains constant.

In this example, the series labeled "Effective Human Capital" moves in response to a productivity shock. Because measured TFP is simply $zq^{1-\alpha}$, where q is the ratio of Mincer and Effective Human Capital, it follows that measured TFP has a large endogenous component.

Basu and Fernald discuss a variety of scenarios about future productivity growth and trace the implications for output growth. The previous argument suggests that even simple shocks might have a large impact on conventionally measured TFP, which would not be captured in their calcu-

Figure 2

TFP Shock, Effective Human Capital, Mincerian Human Capital, and Measured TFP



lations. Moreover, given the model that they use—essentially one in which the only key decision, saving, is taken as exogenous—any reduced-form representation of the economic variables of interest is an appropriate model to forecast the future, with significantly less structure.

SHORT-RUN CONSIDERATIONS

In this section of their article, Basu and Fernald describe their estimates of technology shocks (i.e., TFP) in a two-sector model and define potential output as the output that would be obtained in the absence of frictions (e.g., price stickiness). Their major finding is that the variability of productivity shocks is high, even at the business cycle frequency, and hence that the prescription that in the short run government policy should try to stabilize output is suspect.

The key question is whether the technology shocks they identify are indeed "purified" of policy-induced fluctuations. I am not totally convinced that simple econometric procedures can effectively isolate TFP shocks, especially given the authors' strong assumption about orthogonality between measured TFP and policy shocks. In particular, it is relatively easy to introduce policies in the Manuelli and Seshadri (2008) model that endogenously change the rate of utilization of human capital (with no change in measured employment) that would appear as changes in technology. Whether these sources of misspecification are important is a question that is difficult to answer using Basu and Fernald's partial-specification approach. As they are aware, some sources of bias can be detected only when they are fully specified in the model.

CONCLUSION

In this discussion, I have taken issue with some of the specific choices made by Basu and Fernald and with their interpretation of the results. I would like to end on a more important note: This paper points policy-based economic research in the right direction because it emphasizes the necessity of being explicit about the assumptions underlying our models. Moreover, by making explicit the economies that are modeled, it is possible to subject the models to a variety of tests. On the other hand, reduced-form atheoretical approaches to policymaking must rely on (often implicit) assumptions to justify their recommendations, and intelligent evaluation of the results is often very difficult, if not outright impossible.

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