CENTRE FOR APPLIED MACROECONOMIC ANALYSIS

The Australian National University

CAMA Working Paper Series **August, 2005**

AGGREGATE INVESTMENT IN THE PEOPLE'S REPUBLIC OF CHINA: A COMMENT

Jesus Felipe
Asian Development Bank

CAMA Working Paper 17/2005
http://cama.anu.edu.au

Aggregate Investment in the People's Republic of China: A Comment

JESUS FELIPE

Macroeconomics and Finance Research Division
Economics and Research Department
Asian Development Bank.
Manila, Philippines
e-mail:jfelipe@adb.org

The author is thankful to Dave Dole, Akiko Hagiwara, Carsten Holz, Bin Nguyen, Fan Zhai and Julián Pérez for their useful comments and suggestions. The usual disclaimer applies. This paper does not represent the views of the Asian Development Bank, its Executive Directors or those of the countries that they represent.

The determination of investment decisions by, broadly speaking, the level and rate of change of economic activity […] remains the pièce de résistence of economics

Kalecki (1971, p.165)

Macroeconomics consists of identities and opinions

Orley Ashenfelter[1]

This comment raises three main issues about He and Qin's (2004) attempt at modeling investment in the PRC. The first is this author's skepticism about the general applicability of the neoclassical model of investment to the PRC. Second, that their model for business investment, based on the neoclassical theory of investment, can be viewed as an *approximation* to an accounting identity derived by manipulating two other identities, namely, that of the capital share in output, and that of the motion of the capital stock. It is shown that the difference between He and Qin's equation and the identity is simply that they use the rental price of capital, while the identity relies on the profit rate. *At best*, all their analysis would indicate is that rental price of capital and profit rate are different. It is also argued that the empirical results are not clearly related to the supposed theoretical model. Based on this, the conclusion is that the policy implications of He and Qin's alleged model are somewhat dubious. Third, He and Qin's equation for government investment introduces the deviations of output from the long-run trend as an explanatory variable, estimated using an *aggregate* production function. The problems underlying this latter concept make the estimation of the output trend using this method a questionable exercise. Also, the empirical results suffer from serious problems of interpretation.

I. INTRODUCTION

In a recent paper, He and Qin (2004) have made a worthy attempt at answering the important and difficult question of what are the driving forces behind the People's Republic of China (PRC) fast-growing domestic investment. This is a very complex task given that modeling investment has always been a very hard undertaking, which in the case of the PRC is compounded by the fact that the country has been in a state of transition for about 25 years. Hence it is not clear which theoretical approach one should follow.

For modeling purposes, the authors split domestic investment into that of government (I_G) and that of the business sector (I_B), where the latter is composed of state-owned enterprises, collective-owned enterprises, and private enterprises. Following Song et al. 2001), He and Qin argue that in the light of the fact that the PRC's investment system has changed profoundly since the transition process began, and that the business motives of most PRC firms have become more market oriented since the 1990s, it is appropriate to model I_B by the "orthodox factor-input demand model" (He and Qin 2004, p.103). By this, the authors mean the neoclassical model of investment. From an econometric point of view, they regard this model as a long-run equilibrating force embedded in a dynamic model, which is to be identified by the data. On the other hand, I_G is modeled as a mixture of policy targets and supply-side constraints.

In essence, this is a comment about economic methodology. The purpose is to raise and discuss what in this author's view are some potential problems with He and Qin's (2004) specifications of the investment equations for both the business and government sectors and

[1] Cited by Lewis (2004, p.7).

the conclusions derived from their estimation. After all, as Blanchard once commented: "the discrepancy between theory and empirical work is perhaps nowhere in macroeconomics so obvious as in the case of the aggregate investment function" (Blanchard 1986).

In the case of the business sector equation, the problem is that an almost identical equation to that derived by He and Qin can be obtained by simply manipulating two accounting identities. This derived equation is, therefore, also an identity. By comparing the latter with the equation He and Qin estimate it is easy to ascertain what the estimation results would look like. The most important conclusion is that He and Qin's exercise does not entail the policy implications that the authors infer. It is important to state that the purpose of comment is not a critique of the neoclassical theory of investment; rather, it is a discussion of some problems regarding He and Qin's (2004) empirical implementation, and this author's personal reflections on the possibility of testing and potentially refuting this model.[2] In this sense, my comment conveys an open skepticism about the usefulness of the neoclassical model in explaining investment, in particular in the PRC.

In the case of the government sector equation, there is a problem relating to the introduction of the trend of output, estimated through an aggregate production function, into the specification. I conclude that, in my view, the empirical results obtained are questionable and the policy implications derived do not follow. I elaborate upon these issues in detail (hence the length of the comment) in the next two sections. The final section offers some suggestions for modeling investment in the PRC.

II. BUSINESS SECTOR INVESTMENT

To model the business sector investment, He and Qin (2004) follow the neoclassical investment theory (Jorgenson 1963). This theory indicates output and the cost of capital as the variables determining the optimal capital stock. Investment is the adjustment to the optimal capital stock. The theory assumes that firms choose their long-run desired capital stock (K_B^*) by optimizing their factor demand subject to a constant returns-to-scale Cobb-Douglas production function. This leads to the first-order condition $K_B^* = \gamma(Y/c)$, where γ is the elasticity of output (Y) with respect to capital, and c is the user cost of capital. Desired investment (I_B^*) follows $I_{Bt}^* = (K_t^* - K_{t-1}^*) + \delta K_{t-1}^*$, where δ is the constant depreciation rate, assumed to be 5%. From the first-order condition and the motion of investment it is easy to derive the basic equation used to model business investment, namely:

$$\Delta I_{Bt}^* = \gamma \Delta \left(\frac{Y}{c}\right)_t - \left[I_B^* - \gamma\delta\left(\frac{Y}{c}\right)\right]_{t-1} \qquad (1)$$

where the symbol Δ denotes the difference in the values of a variable between two consecutive periods. The key parameter to estimate is γ. However this equation is estimated, it must yield an estimate of γ. He and Qin (2004) argue that this equation resembles a standard error correction model, where the term in squared brackets implies the long-run relationship $I_B^* = \gamma\delta\left(\frac{Y}{c}\right)$. Moreover, the authors argue that "considering the possibility that

[2] I acknowledge that there might be some alternative ways to test and potentially refute the neoclassical model of investment.

the Chinese business-sector investment may also be affected by the government policies, we extend that equilibrium relation by adding government direct investment as a new explanatory variable" (He and Qin 2004, p.104). Thus the hypothesized long-run relationship becomes $I_B^* = \gamma\delta\left(\frac{Y}{c}\right)(I_G^*)^{\alpha_3}$, which in logarithms is $ln\, I_B^* = \alpha_0 + \alpha_1 ln Y + \alpha_2 ln\, c + \alpha_3 ln\, I_G^*$, with expected estimates $\alpha_0 = ln(\gamma\delta)$, $\alpha_1 = 1$, and $\alpha_2 = -1$.[3] For econometric purposes, this long-run relationship is embedded in a dynamic specification of equation (1), to which an error term with the standard assumptions is added (This will be discussed below). The equation is estimated with quarterly data using the actual values of investment (that is, for practical purposes, all variables used are the actual values, without the asterisk) for the period 1994Q4-2001Q4. However, the empirical implementation is not exempt of problems, as we shall see below.

II.1 The accounting identity problem

To see the problem with He and Qin's (2004) equation (1), consider first the definition of the capital share (s_t^K) in output, namely, $s_t^K \equiv \Pi_t / Y_t$, where Π_t denotes total profits and Y_t is GDP. Total profits can be written as the product of the average *ex-post* profit rate (ρ_t) times the stock of capital (K_t), that is, $\Pi_t \equiv \rho_t K_t$. From here it follows that:

$$s_t^K \equiv \frac{\rho_t K_t}{Y_t} \tag{2}$$

The symbol $\equiv$ denotes that expression (2) is true by definition, in the sense that it is an accounting identity, not a behavioral relationship. Likewise, the labor share in output can be written as $s_t^L \equiv 1 - s_t^K \equiv W_t / Y_t$, where W_t denotes the total wage bill, and the latter can be written as the product of the average wage (w_t) rate times employment (L_t), that is, $W_t \equiv w_t L_t$. It should be clear that any system of consistent accounts (e.g., the National Income and Product Accounts –NIPA– of any country, including those of the PRC, from which the factor shares can be inferred) provides output (*Y*) as the sum of the total wage bill (*W*) plus total profits (Π), that is $Y_t \equiv W_t + \Pi_t$. Hence it is theoretically possible to express output as $Y_t \equiv w_t L_t + \rho_t K_t$.[4] The expressions for the labor and capital factor shares do not depend on any model or theory and must hold always, both at the aggregate and sectoral levels (e.g., business and government sectors).

Likewise, define the law of motion of the stock of capital as:

$$K_t \equiv I_t + (1 - \delta)K_{t-1} \tag{3}$$

which obviously is also an accounting identity.

---

[3] Given that the variables in equation (1) are the actual levels, the introduction of the relationship in logarithms is somewhat *ad-hoc*.

[4] That some countries do not collect data that allow the construction of $Y_t \equiv W_t + \Pi_t$ or $Y_t \equiv w_t L_t + \rho_t K_t$ does not undermine the theoretical argument.

By rewriting equation (3) for investment, substituting for the stock of capital from equation (2), and *assuming only* that the capital share is constant (i.e., $s_t^K = s^K$), we obtain

$$I_t \equiv s^K \left[\frac{Y_t}{\rho_t} - \frac{Y_{t-1}}{\rho_{t-1}} \right] + s^K \delta \frac{Y_{t-1}}{\rho_{t-1}} \tag{4}$$

Finally, subtracting I_{t-1} from both sides yields

$$I_t - I_{t-1} \equiv \Delta I_t \equiv s^K \Delta \left[\frac{Y}{\rho} \right]_t - \left[I - s^K \delta \frac{Y}{\rho} \right]_{t-1} \tag{5}$$

The obvious point behind equation (5) is that it is *almost* identical to equation (1), the one specified by He and Qin (2004). It must be stressed that equation (5) is an accounting identity that has been derived as a transformation of two accounting identities. The difference is simply that while He and Qin's model uses the user cost of capital (c), the identity (5) uses the profit rate (ρ). The implications of this difference are discussed in the next subsection. The only assumption made to derive equation (5) is that the capital share is constant ($s_t^K = s^K$), something that can be verified or refuted very easily. As such, equation (5) *does not* provide a behavioral explanation of (the change of) investment; and as a consequence, it carries no policy implications whatsoever.

Provided the capital share is (sufficiently) constant, econometric estimation of equation (5) as

$$\Delta I_t \equiv \gamma_1 \Delta \left[\frac{Y}{\rho} \right]_t + \tau_1 \left[I + \zeta_1 \frac{Y}{\rho} \right]_{t-1} \tag{6}$$

where $\zeta_1 = -s^K \delta$, will be a pointless exercise since one knows, *ex-ante*, that the results will be: $\gamma_1 = s^K$, $\tau_1 = -1$ (the supposed speed of adjustment to the long-run equilibrium in an error correction model !), and a perfect statistical fit (there is no error term of any kind).[5] Certainly, if the assumption about the constancy of the capital share were incorrect, then estimation of equation (6) using standard regression methods (e.g., OLS, IV) would not yield a perfect fit, and the estimated parameters would diverge from the theoretical values. But such result will only mean that capital's share is not sufficiently constant.[6] A solution would be to use a time-varying parameter model. This would take us back to the identity. The conclusion is that the hypothesis that equation (6) is a good explanation of investment in the PRC (or in any other place) can never be rejected statistically; hence it cannot be postulated as a model to explain investment behavior because it is not falsifiable.

---

[5] Of course, no actual data set will display a perfect constancy of the factor shares. For econometric purposes, this condition must be understood as "roughly" constant.

[6] The general expression without assuming that capital share is constant is: $I_t \equiv s^K \left[\frac{Y_t}{\rho_t} - \frac{Y_{t-1}}{\rho_{t-1}} \right] + (s_t^K - s_{t-1}^K) \frac{Y_{t-1}}{\rho_{t-1}} + s_{t-1}^K \delta \frac{Y_{t-1}}{\rho_{t-1}}$.

II.2 He and Qin's equation as an *approximation* to the accounting identity

He and Qin, however, did not obtain a perfect fit when they estimated their model. This is because they did not estimate equation (6). In fact, the equation they estimated differs substantially from (6). In this and the next subsections I discuss the differences between the equation they estimated and the identity, and how these differences led to the results the authors reported. All this is due to the interaction of five reasons. The question to discuss is the following: does the fact that in their empirical exercise He and Qin did not obtain the results that follow from (6) imply that what they did entails the policy implications they derived? I argue that, on the principle of *parsimony*, He and Qin's (2004) results are mainly driven by the accounting identity equation (6). In what follows I discuss the five reasons:

(i) There is an issue about equation (1) that I believe is incorrect, and may render the exercise flawed from the outset. This is that the authors use *Y* to denote output, defined in their paper (Appendix) as "real GDP." However, the way it is defined seems to refer to output of the total economy. The paper does not define separately output of the business sector and output of the government sector. Likewise, the authors also use the same notation for output, *Y*, in the equation for government investment, discussed in section III. If this were the case, then, even in their terms, and without considering any of the issues I raise in the rest of the comment, the business sector investment model would be more than questionable. Output in equation (1) above (equations (2) and (5) in He and Qin's paper) *should* refer to that of the business sector. The "theoretical" model relates the business sector investment to output of the business sector, not to that of the total economy, unless the authors have in mind some sort of relationship not clearly spelled out. If in the identity (2) profits (numerator) and output (denominator) refer to different economic units, certainly the notion of labor share disappears and so does the rest of the argument.[7]

(ii) Second, there is a conceptual difference between equations (1) and (6). From equation (2), it can be seen that the capital stock can be defined as $K_t \equiv s_t^K\left(\frac{Y_t}{\rho_t}\right)$. This expression resembles the first-order condition that He and Qin derived, namely, $K = \gamma\left(\frac{Y}{c}\right)$, but with the difference that the denominator of the latter, c, is the user cost of capital, not the ex-post profit rate ρ. The profit rate is a measure of the return on the capital invested, and, simultaneously, it is also a measure of the *total* capital cost to the firm (the same as the wage rate w is, simultaneously, the return to labor and the labor cost to the firm.). Empirically, the profit rate is estimated from the NIPA as $\rho_t \equiv \frac{\Pi_t}{K_t}$ so as to preserve the capital share identity.[8]

---

[7] Probably it can be argued that the authors' model implicitly assumes that that business sector output (Y_B) and total output (Y), with $Y = Y_B + Y_G$, where Y_G denotes government sector output, are related through a simple linear relationship such as $Y_B = \lambda Y$. If this is the case, then the derivation of the model should have incorporated this assumption explicitly and then the authors should have explained how λ and the other coefficients are estimated. This is because equation (1) above contains total output Y twice, so λ would appear twice, first in $\Delta\left(\frac{\lambda Y}{c}\right)_t$ and a second time in $\left[I_B^* - \gamma\delta\left(\frac{\lambda Y}{c}\right)\right]_{t-1}$.

[8] Likewise, from the National Income and Product Accounts, one can obtain the labor share as $s_t^L \equiv (W_t / Y_t)$. Now suppose one obtains independent data on the average wage rate (*w*) and

On the other hand, the notion of the user cost (or rental price) of capital derives from Jorgenson's (1963) neoclassical model of capital and investment. It is a theory-dependent concept (Felipe and McCombie 2004). There is nothing wrong with this as long as it is clear what it refers to and means. This must be stressed because substitution is the crucial mechanism in neoclassical theory. The model that underlies the concept of the user cost of capital makes a distinction between the cost of capital (under competitive conditions) and pure profits. The model *assumes* that the wage rate and the user cost of capital reflect the corresponding marginal productivities, i.e., $(\partial Y/\partial L)=w$ and $(\partial Y/\partial K)=c$; in other words, that factor markets are competitive. While there are statistics on wage rates, in general, there are no statistics on the implicit price that the firm charges itself for the assets that it owns, the user cost of capital, which is equal to the price that it would have to pay to rent an equivalent asset in a competitive market. Except for a few markets (such as for aircraft), there are no data on rental costs. To infer them, Jorgenson (1963) assumed the existence of a perfect market for secondhand goods, as well as perfect markets for all inputs and output. The former implies that firms would not need to worry about locking themselves in by purchasing long-lived investment goods, as such goods could be sold on the secondhand market at a price equal to the present value of their expected services over their expected remaining lifetimes. This way, firms are seen as renting capital goods to themselves during each time period and charging themselves an implicit cost, namely, the rental price of capital. Jorgenson's derivation leads to the well-known formula that the rental price of capital is $c_t = q_t(r_t + \delta - \dot{q}_t/q_t)$, where q_t is the price of the capital goods, r_t is the nominal interest rate, and $\dot{q}_t/q_t$ is the capital gain-loss.[9] In this framework, monopolistic profits are not an economic cost to the firm.

What this implies in terms of the framework above is that the income accounting identity that is implicit in He and Qin's (2004) analysis is $Y_t^{mc} \equiv w_t L_t + c_t K_t$, where Y_t^{mc} denotes output valued at marginal cost, while the identity consistent with the NIPA is $Y_t \equiv w_t L_t + \rho_t K_t$. But this means that $Y_t \equiv Y_t^{mc} + \pi$, or $Y_t \equiv w_t L_t + c_t K_t + \pi_t$, where π_t are monopolistic profits. That is, total profits are decomposed into $\Pi_t \equiv \rho_t K_t \equiv c_t K_t + \pi_t$. But monopolistic profits can be also written as the product of some cost of (or return to) capital times the capital stock, that is, $\pi_t \equiv \tilde{c}_t K_t$. This implies that $\Pi_t \equiv c_t K_t + \tilde{c}_t K_t \equiv (c_t + \tilde{c}_t)K_t \equiv \rho_t K_t$. In other words, what I define as the profit rate (ρ) can be viewed as the sum of the neoclassical cost of capital under competitive conditions (c) plus the cost of the so-called pure profit ($\tilde{c}$), i.e., $\rho_t \equiv c_t + \tilde{c}_t$. It would be interesting to see the three series c_t, $\tilde{c}_t$ and ρ_t.[10]

The important question to ask is whether (market) conditions in the PRC's business sector, despite the reforms implemented and the introduction of some market features, are such that they warrant the use of this framework. Moreover, empirically, it is impossible to know whether the user cost of capital calculated as above is a meaningful estimate of the

---

employment (L), and finds that $s_t^L \equiv (w_t L_t / Y_t)$. This would pose problems as there would be a statistical measurement error, similar to what happens with the measurement of GDP by the expenditure, output and income methods. It is obvious that residual adjustments would have to be made to preserve the labor-share identity.

[9] The expression He and Qin (2004) use is slightly different as it adds the effects of taxes. This does not affect the argument.

[10] It is worth noting that neoclassical economists refer to the profit rate as the marginal product of capital, e.g., Mankiw et al. (1992, p.431).

capital cost, in the sense of the neoclassical model, i.e., the marginal productivity of capital under competitive capital markets, in the PRC. One has to take it at face value.[11]

Given the above, are He and Qin estimating the accounting identity? Not strictly speaking. In general $c_t = q_t(r_t + \delta - \hat{q}_t) \neq \frac{\Pi_t}{K_t} = \rho_t$ and this is why estimation of equation (1) will not yield the results mentioned above for equation (6). Many economists would argue that the fact that in general $c_t \neq \rho_t$ is enough to invalidate the identity resemblance problem. In fact, empirically, user cost and profit rate behave, in most cases, very differently. The user cost (c_t) tends to be countercyclical since it contains the interest rate (r_t), while the profit rate (ρ_t) tends to be procyclical. However, I argue that estimation of equation (1) would not be more than a (rather complicated) "test" for whether the user cost of capital (c_t) and profit rate (ρ_t) differ. In fact, it would be interesting to see the estimation results of equation (1). Given that $\rho_t = c_t + \tilde{c}_t$ (given that series of ρ_t and c_t can be constructed as indicated above, $\tilde{c}_t$ can be obtained residually), equation (1) can be rewritten as:

$$\Delta I^*_{Bt} = \gamma \Delta\left[\frac{Y}{\rho - \tilde{c}}\right]_t + \tau\left[I^*_B + \gamma\delta\frac{Y}{\rho - \tilde{c}}\right]_{t-1} \qquad (7)$$

which shows clearly the relationship between equations (1) and (6). The difference is that the former (or (7)) includes $\tilde{c}_{t-1}$. This is why the estimates of γ and τ in equation (7) will differ, in general, from the estimates of $\gamma_1 = s^K$ and $\tau_1 = -1$ in (6). When will equations (6) and (7) yield identical results? Obviously when $\tilde{c}_t$=0, i.e., when $\rho_t = c_t$. Again, it must be stressed that the only thing regression (7) will tell us is whether c_t and ρ_t are different or not. But for this, a simple t-test for the difference between both (if not just the visual inspection of the series), will suffice; and in any case, given the resemblance between equations (1) and (6), it is an almost foregone conclusion that equation (1) should provide "good" results.[12]

(iii) Thirdly, and probably a minor issue is that, as indicated above, equation (1) assumes that the elasticity of output with respect to capital (γ) is constant. As argued above, the capital share s^K-the parameter being approximated in the identity, may not be sufficiently constant in the data set the authors use for OLS and IV to yield sensible results. My conjecture is that most likely this is not the source of a serious problem for econometric estimation purposes in the case at hand. Young (2000, Table XXIII) provides data for the PRC's labor share (total economy). This (and obviously the capital share) has been fairly constant for a long time (i.e., $s^K_t \cong s^K$), at around 0.6. This suggests that regression results should be very accurate due to parameter constancy.

---

[11] Song et al (2001, p.232) argue that the calculation of the user cost of capital using the standard formula "can be very difficult in the case of China due to data limitations." They obtain it from the production function. He and Qin (2004, Figure 1) show that the estimated user cost of capital has oscillated around 3% during the period considered. On the other hand, they do not calculate the average profit rate. In an economy like the PRC, with so much investment, it probably displays a declining trend, although the period is relatively short.

[12] With respect to the user cost of capital, it is also a bit surprising that the authors find it significant variable. Ford and Poret (1991) and Chirinko (1993), for example, conclude that this variable has only a small effect. Output is a much more important variable.

(iv) Fourthly, He and Qin did not estimate equation (1) as it is. They introduced lags of the variables to estimate an error correction model. This led to the introduction important "distortions" with respect to the original model, equation (1). I elaborate upon this issue in the next subsection.

(v) Finally, as indicated above, He and Qin introduced government investment (I_G) into the specification. However, neither the neoclassical model (their own model), nor the identity (6) indicates that such a variable should be there. I also elaborate upon this issue in the next subsection.

II.3 Some issues about econometric estimation

The reader will appreciate that arguments (*iv)* and *(v)* in the previous subsection are both reasons why the equation estimated by He and Qin is not a tautology, and arguments why the theoretical foundations of their empirical implementation are shaky.[13] In this subsection I discuss in some detail the implications of these two arguments. It is important to emphasize that the purpose of this subsection is not to criticize the econometric work *per se*. This is nuance rather than substance. The reader will appreciate that my main objection here continues being the accounting identity.

How did He and Qin estimate their model, equation (1)? Instead of estimating the equation as it is, *it seems* that they postulated the general relationship $I = I(Y, c, I_G)$, with $(\partial I / \partial Y) > 0$, $(\partial I / \partial c) < 0$ and $(\partial I / \partial I_G) > 0$, and constructed an autoregressive distributive lag (ADL) model with the four variables. The problem with their approach is that in proceeding this way, the precise correspondence between the coefficients in their theoretical model (e.g., the elasticity of output with respect to capital, γ in equation (1)), the structural equation (1) above (equation (5) in their paper), which certainly can be estimated as pointed out above, the general ADL (equation (9) in their paper, which includes government investment), and the equation finally estimated (equation (9') in their paper) is not established.[14] This would have facilitated the evaluation of their work as well as the interpretation of the results.[15] There is no contradiction whatsoever between this argument and Hendry's (1995) modeling philosophy, which is not the object of my critique. One thing is to posit, for example, a consumption function such as $C_t = \beta_0 + \beta_1 Y_t$ (where β_1 is the relevant parameter) and to argue that it does not have dynamics, hence Hendry's proposal to estimate it in error correction form, and use the general-to-specific methodology, as long as one is be able to recover from the estimated equation the fundamental parameter β_1; and another thing, quite different, is to argue that the model is $C = f(Y)$ and then estimate an equation from which β_1 cannot be recovered. What this simply does is to corroborate Blanchard's lament quoted above.

---

[13] I am thankful to Carsten Holz for making this point very clear.

[14] Equation (9) in their paper is their starting general error correction model:

$$\Delta \ln I_t = \alpha_0 + \sum_{i=1}^{d} \alpha_{0i} \Delta \ln I_{t-i} + \sum_{j=0}^{d} \alpha_{1j} \Delta \ln Y_{t-j} + \sum_{j=0}^{d} \alpha_{2j} \Delta \ln c_{t-j} + \sum_{j=0}^{d} \alpha_{3j} \Delta \ln I_{Gt-j} + \lambda E_{t-1} .$$

Equation (9') is the specific estimated equation after the process of model reduction and simplification.

[15] Of course, the authors may claim that all they care about is the reduced form $I = I(Y, c, I_G)$. But if this is the case, then it is difficult to understand why emphasizing the relationship with the neoclassical model of investment.

He and Qin's ADL (equation (9) in their paper) is supposedly derived from a multiplicative form of $I = I(Y, c, I_G)$, which when expressed in logarithms becomes:[16]

$$\ln I_t = \alpha_0 + \psi_1 \ln I_{t-1} + \psi_2 \ln I_{t-2} + \gamma_1 \ln Y_t + \gamma_2 \ln c_t + \gamma_3 \ln I_{Gt} + \gamma_4 \ln Y_{t-1} + \gamma_5 \ln c_{t-1} + \\ + \gamma_6 \ln I_{Gt-1} + \gamma_7 \ln Y_{t-2} + \gamma_8 \ln c_{t-2} + \gamma_9 \ln I_{Gt-2} \qquad (8)$$

where, for stability, $|\psi_1 + \psi_2| < 1$ (perhaps with more lags, although this does not affect the argument). Subtracting $\ln I_{t-1}$ from both sides and performing a series of simple algebraic transformations (Banerjee et al. 1993, pp.50-55), equation (8) becomes:

$$\Delta I_t = \alpha_0 + \gamma_1 \Delta Y_t + \gamma_2 \Delta c_t + \gamma_3 \Delta I_{Gt} - \psi_2 \Delta I_{t-1} - \gamma_7 \Delta Y_{t-1} - \gamma_8 \Delta c_{t-1} - \gamma_9 \Delta I_{Gt-1} + \\ + (\psi_1 + \psi_2 - 1) \ln I_{t-1} + (\gamma_1 + \gamma_4 + \gamma_7) \ln Y_{t-1} + (\gamma_2 + \gamma_5 + \gamma_8) \ln c_{t-1} + \\ + (\gamma_3 + \gamma_6 + \gamma_9) \ln I_{Gt-1} \qquad (9)$$

and in error correction form:

$$\Delta I_t = \alpha_0 + \gamma_1 \Delta Y_t + \gamma_2 \Delta c_t + \gamma_3 \Delta I_{Gt} - \psi_2 \Delta I_{t-1} - \gamma_7 \Delta Y_{t-1} - \gamma_8 \Delta c_{t-1} - \gamma_9 \Delta I_{Gt-1} - \\ - (1 - \psi_1 - \psi_2) \left[\ln I_{t-1} - \frac{(\gamma_1 + \gamma_4 + \gamma_7)}{(1 - \psi_1 - \psi_2)} \ln Y_{t-1} - \frac{(\gamma_2 + \gamma_5 + \gamma_8)}{(1 - \psi_1 - \psi_2)} \ln c_{t-1} - \frac{(\gamma_3 + \gamma_6 + \gamma_9)}{(1 - \psi_1 - \psi_2)} \ln I_{Gt-1} \right] \qquad (10)$$

where equations (8), (9) and (10) are identical. The long-run solution is given by the terms in bracket in (10), that is, $\ln I = \alpha_1 \ln Y + \alpha_2 \ln c + \alpha_3 \ln I_G$ where the long-run elasticities are $\alpha_1 = \frac{(\gamma_1 + \gamma_4 + \gamma_7)}{(1 - \psi_1 - \psi_2)}$, $\alpha_2 = \frac{(\gamma_2 + \gamma_5 + \gamma_8)}{(1 - \psi_1 - \psi_2)}$ and $\alpha_3 = \frac{(\gamma_3 + \gamma_6 + \gamma_9)}{(1 - \psi_1 - \psi_2)}$. One can see the correspondence between the coefficients in equation (8) and in equations (9)-(10) –the latter is in the form of equation (9) in He and Qin.

A comparison of equation (10) with the identity equation (6) –and also with He and Qin's model in the form of equations (1) or (7) above- indicates clearly why He and Qin obtained coefficients that deviated from the values in equation (6). Surely equation (10) *is not* the identity derived in subsection II.1. But this does not mean that what the authors do is sound, in the sense that now it is not clear what true theoretical or structural model underlies this regression, in particular the correspondence between the parameter γ equation (1) and the coefficients in (10).[17]

---

[16] The reader will notice that now, in the empirical implementation of the model, all variables are expressed in logarithms, and therefore, first differences (Δ) are the growth rates. This means that there is not a one-to-one correspondence between the theoretical model in equation (1) and its empirical implementation in the form of an ADL. This implies that in equation (1) $\Delta\left(\frac{Y}{c}\right)_t \neq \Delta(Y)_t - \Delta(c)_t$. However, left and right-hand sides are equal when the variables are logarithms. I am thankful to Bin Nguyen for pointing it out.

[17] He and Qin's estimation results seem to imply that the short-run elasticity of investment with respect to the user cost takes on a value of around –0.30 (and a long-run elasticity of -1). This parameter is, supposedly, the elasticity of output with respect to capital, denoted γ in the theoretical model (equation (1) above), *if the estimated equation has any relationship with the theoretical model.* Here I am guessing since,

There are two additional issues regarding the long-run relationship that must be mentioned. First, recall that the long-run relationship is hypothesized to be (equation (6) in He and Qin 2004) $ln I_B = \alpha_0 + \alpha_1 ln Y + \alpha_2 ln c + \alpha_3 ln I_G$ with expected estimates $\alpha_0 = ln(\gamma\delta)$, $\alpha_1 = 1$, and $\alpha_2 = -1$. However, government investment is introduced completely *ad-hoc* in the model since there is no explanation as to how and why it appears in equation (1) (other than the naive justification given by the authors and cited at the beginning of my comment), derived from the firm's optimization problem. On the other hand, the variable seems to be deleted also *ad-hoc* from the long-run solution. It is easy to show that if the *true model* (i.e., the theory that indicates that I_G is a determinant of I_B) is given by equation (10) above, then elimination (in the process of model reduction) of any of the variables in growth rates (the short-term dynamics), e.g., ΔI_{Gt}, does not affect the ADL, in the sense that one can return to a meaningful ADL.[18] However, one cannot eliminate the variables in levels (long-run dynamics) as in this case it would not be possible to return to a meaningful ADL. For example, if one eliminates I_{Gt-1} in the term in brackets in equation (10) and one tries to reconstruct the corresponding ADL, one will return to a form where the coefficients are incorrect.[19] But this is what He and Qin seem to have done when they eliminated I_{Gt-1}.

Secondly, in the empirical exercise, He and Qin argue that they cannot reject the null hypothesis that the estimates take on the hypothesized values, which are imposed when the error correction term is constructed. However, note that the identity equation (4) above can be rewritten as $I_t \equiv s^K\left(\frac{Y_t}{\rho_t}\right) - (1-\delta)\left[s^K \frac{Y_{t-1}}{\rho_{t-1}}\right]$, which intuitively indicates that it will be very difficult for the putative long-run relationship to yield the hypothesized two estimates $\alpha_1 = 1$, and $\alpha_2 = -1$. In fact, no wonder He and Qin (2004, p.111) indicate that government investment is absent from the long-run solution in the estimated equation, supposedly because it is not significant. The result is that the authors end up with an estimated regression where government investment is a short-run determinant of the business sector investment, but not a long-run determinant of the business sector investment. It is possible, nevertheless,

---

as indicated in the main text, the correspondence between the estimated coefficients and the parameters of the theoretical model is not well established. The value of –0.30 is given by the estimate of the growth rate of the user cost of capital (in equation (9') in He and Qin (2004). I am speculating that this corresponds to γ in their equation (5) (equation (1) here). But this is not entirely clear due to the issue of the levels and logs mentioned above, and to the fact that the long-run relationship also determines γ (see equation (1) above). This is part of the constant term $\alpha_0 = ln(\gamma\delta)$. However, when the long-run relationship is estimated, it does not contain the constant (see equation (9') in He and Qin 2004). If this is that case, I take that its value is zero. But this implies 0 = $ln(\gamma\delta)$ and given that δ =0.05, it follows that γ =20, which is an absurd result.

[18] Indeed, the resulting ADL is:

$$ln I_t = \alpha_0 + \psi_1 ln I_{t-1} + \psi_2 ln I_{t-2} + \gamma_1 ln Y_t + \gamma_2 ln c_t + \gamma_4 ln Y_{t-1} + \gamma_5 ln c_{t-1} +$$
$$+ (\gamma_3 + \gamma_6) ln I_{Gt-1} + + \gamma_7 ln Y_{t-2} + \gamma_8 ln c_{t-2} + \gamma_9 ln I_{Gt-2}.$$

[19] Indeed, the resulting form is:

$$ln I_t = \alpha_0 + \psi_1 ln I_{t-1} + \psi_2 ln I_{t-2} + \gamma_1 ln Y_t + \gamma_2 ln c_t + \gamma_3 ln I_{Gt} + \gamma_4 ln Y_{t-1} + \gamma_5 ln c_{t-1} -$$
$$- \gamma_3 ln I_{Gt-1} - \gamma_9 ln I_{Gt-1} + \gamma_7 ln Y_{t-2} + \gamma_8 ln c_{t-2} + \gamma_9 ln I_{Gt-2}$$. It can be see that $ln I_{Gt-1}$ appears twice and with different coefficients; and that the coefficient γ_3 appears twice. This is not and ADL.

that, statistically, $H_0 : \alpha_1 = 1; \alpha_2 = -1$ may not be rejected, as the authors indicate (He and Qin 2004, p.109), but in my view this is just sheer "coincidence", or due to the fact that the estimates of output and the user cost of capital have very large standard deviations. In fact, the regression is not shown.[20]

Summing up, in this author's opinion, He and Qin's (2004, p.110) conclusion that the long-run solution derived from their alleged model suggests "strongly that aggregate business investment demand is now largely market-driven in the PRC" is unwarranted. The conclusion of this subsection is that the ADL the authors estimate is only vaguely related to the theoretical framework that they argue underlies it, namely, the neoclassical model of investment. While the authors start by assuming a relationship derived from this theory, the equation they end up estimating is so different that virtually has no relationship with the model. Hence, it is impossible to interpret it.[21]

III. GOVERNMENT SECTOR INVESTMENT

Given the lack of a sound theory to model PRC's government sector investment, the authors take a very simple approach. They propose the long-run relationship:

$$ln\, I_G^* = \beta_0 + \beta_1\, ln\, G_R + \beta_2\, ln(Y / Y^T) + \beta_3 u \qquad (11)$$

where I_G^* is the desired level of government investment, G_R is government revenue, Y^T is the desired long-run trend of output (Y) (thus (Y / Y^T) measures the deviations of output from its long-run trend), and u is the unemployment rate, with expected signs $\beta_1 > 0$, $\beta_2 < 0$, and $\beta_3 > 0$.[22] As in the case of the business-sector investment, this hypothesized long-run relationship is inserted as part of a dynamic equation in growth rates containing lags. Again, empirically, actual values (without asterisk) were used.

III.1 The Long-Run Trend, the Production Function and some theoretical problems

This subsection raises four issues relating to the long-run trend of output:

(i) A key aspect of equation (11) is that it contains the long-run trend Y^T, which is unobservable and hence has to be estimated. The authors argue that they define Y^T as the "*symbolically market-driven* Cobb-Douglas production function with constant returns to scale to reflect the long-run prospect $Y^T = AK^{\theta}L^{1-\theta}$" (He and Qin 2004, p.105; italics

---

[20] Of course, one could always insist that the expression $ln\, I_B = \alpha_0 + \alpha_1\, ln Y + \alpha_2\, ln\, c + \alpha_3\, ln\, I_G$ 'embodies' a long-run relationship, and that, indeed, that is what is being estimated and tested. But then it could also be argued that the term in brackets in the accounting identity implies $ln\, I = \alpha_0 + \alpha_1\, ln Y + \alpha_2\, ln\, \rho$, or $ln\, I = \alpha_0 + \alpha_1\, ln Y + \alpha_2\, ln(c + \tilde{c})$ with $\alpha_1 = 1$ and $\alpha_2 = -1$.

[21] I am thankful to Julián Pérez for making a subtle comment in discussing the paper. This is that if my argument here is correct, then probably much of applied macroeconometrics surely suffers from a similar disease, namely, that many equations that researchers estimate are close approximations to accounting identities. Personally, I believe this is the case. Is this a storm in a teapot? At this point I am willing to argue (perhaps concede) that we are in a knife-edge situation, and I am happy to let the reader decide.

[22] As indicated in a footnote above, output is denoted *Y* in the paper. In the case of the equation for government investment, it is correct to argue that the latter is positively related to the deviations of the total economy's output from its trend. This is different from what He and Qin do in the equation for the business sector investment.

added). This is certainly a most unusual definition of a production function and there is no explanation of what it means. The procedure followed to estimate Y^T is the standard one of fitting the Cobb-Douglas production function $ln(Y/L)_t = ln\,A + \theta\, ln(K/L)_t$ and then using the fitted value to approximate the long-run trend Y^T. For empirical purposes, He and Qin used also an autoregressive distributed lag specification with a view to splitting short and long-run dynamics. What is interesting is the result obtained and reported for the elasticity of output with respect to capital: θ=0.95. Since the authors thought that there was something wrong with this result, they experimented until they settled for a not much different value of θ=0.85.

Given the authors' comment that "this parameter is normally found to be well below 0.5 in most market economies" (He and Qin 2004, p.112), it seems that what they mean by a "symbolically market-driven" production function is simply a production function such that when estimated the results are consistent with competitive markets.[23] This means that the estimated elasticities should be approximately equal to the factor shares. The NIPA of the advanced countries report a capital share in the neighborhood of 0.25-0.30.

(ii) He and Qin, however, justify their findings as follows: "Such a large capital elasticity reflects the persistent problem of *over employment* in the PRC, which makes capital the dominant input constraint on the whole" (He and Qin 2004, p.112; italics added). It is not clear either what the term "over employment" means. My conjecture is that He and Qin mean that the PRC can be characterized as a *surplus labor* economy. The standard definition of this concept is that it is possible to remove a fraction of the labor force without causing any reduction in output. It has often been claimed that surplus labor is pervasive in overpopulated backward economies, where, the argument goes, the marginal productivity of labor is zero over certain ranges. As a result, the withdrawal of part of the labor force has no effect on output.[24] Interestingly, if He and Qin were thinking of the PRC as a surplus labor economy, then the Cobb-Douglas production function rules out, *ab initio*, this possibility. This is because the marginal product of labor is given by $\partial Y/\partial L = (1-\theta)(Y/L)$. Under neoclassical assumptions, the elasticity of output with respect to labor $(1-\theta)$ equals the share of labor in output, i.e., $(1-\theta) = s^L$>0, and (*Y/L*)>0, which implies that the estimate of marginal productivity of labor ($\partial Y/\partial L$) must be positive (Felipe and McCombie 2002).

(iii) It seems odd that the postulated function does not include any variable to account for technological progress (not even the standard exponential time trend, i.e., $Y^T = A\,exp(\lambda t)K^{\theta}L^{1-\theta}$), presumably a very important factor in the PRC during the period of estimation. It would be interesting to see the results of this regression or of any other production function that incorporates a proxy for technological progress.[25]

(iv) When one puts together the estimates of the aggregate production function with those of the user cost of capital, something seems to be wide of the mark. As indicated above, the elasticity of output with respect to capital is estimated at θ=0.85, and the user cost of capital is around 3% (He and Qin 2004, Figure 1). These two values imply

---

[23] The empirical evidence, however, does not corroborate this statement (Sylos-Labini 1995). This is a well-known problem to those who estimate production functions, namely, the implausible results that often appear with time-series data estimating the simple Cobb-Douglas with a time trend.

[24] This approach, however, raises questions about the rationality of workers and employers. Sen (1975) showed that zero marginal productivity of labor is neither necessary nor sufficient for the existence of surplus labor.

[25] My guess is that this regression will suffer from the same problems as those in Chow (1993), also for the PRC, discussed by Felipe and McCombie (2002), and this is why it is not shown.

$0.85=\frac{\partial Y}{\partial K}\frac{K}{Y}=c\frac{K}{Y}=0.03\frac{K}{Y}$, which means that the implied capital-output ratio (*K/Y*) by the model is around 28.[26] Surely there is something wrong here.[27]

Overall, it seems that He and Qin's explanation for their results is *ad-hoc* and inconsistent with any existing theory.[28] First, the authors' description of the aggregate production function and the justification of the empirical results are rather questionable. Secondly, the lack of a variable proxying technological progress in the production function is an important deficiency of the model. And finally, the estimation results imply an unbelievable capital-output ratio. In my view, their arguments are no more than an *ex-post* rationalization of a series of dubious empirical results.

III.2 The Aggregate production Function and the Accounting Identity

Explaining why He and Qin (2004) obtained a poor result in their estimation of the Cobb-Douglas production function is relatively simple. To begin with, the notion of an aggregate production function has been questioned for a long time due its weak theoretical foundations (Cohen and Harcourt 2003; Felipe and Fisher 2003; Felipe and McCombie 2003a). Second, it has been well-known for many years (Simon and Levy 1963, Simon 1979) that the NIPA accounting identity $Y_t \equiv W_t + \Pi_t \equiv w_t L_t + \rho_t K_t$, which is not derived from Euler's theorem, for it simply shows how data are collected, can be easily rewritten as a form that resembles a production function $Y_t \equiv A_t F(K_t, L_t)$, where the function $F(\bullet)$ can take any of the standard forms, such as the Cobb-Douglas, CES or translog (See also Felipe and McCombie (2003b) and Felipe and Adams (2005) for recent extensions of this argument).[29]

---

[26] Theory does not provide any guidance as to the value of the capital-output ratio (*K/Y*). However, estimated capital-output ratios are values that oscillate between 2 and 4. See the discussion in Mankiw et al. (1992, pp.430-432). A value of 28 is so counter-intuitive that it cannot be true. An alternative way of calculating the user cost of capital is through the estimated elasticity and the actual output-capital ratio, that is $c_t = (\partial Y / \partial K)$ = 0.85 (Y/K). It would be interesting to compare this value with the one computed in the paper. Of course, it may be argued that the user cost of capital calculated is a quarterly value (the model uses quarterly data). The annual value, about 0.12, is what should be used. But this still yields an implausible capital-output ratio of about 7. Moreover, if the previous adjustment were the correct procedure, shouldn't the estimate of the elasticity of capital, θ =0.85, also be "adjusted", since it was also estimated with quarterly data?

[27] I wonder if there is any relationship between the elasticity of output with respect to capital here and in the business sector model. Since the authors also used an ADL to estimate the production function, it would have been interesting to see the full equation with both long and short-run elasticities and see, for comparison purposes, their magnitudes.

[28] $\theta = 1$ would be consistent with the *Y=AK* production function used in some endogenous growth models (Romer 1987; Rebelo 1991). This production function, unlike the production function in the original Solow's growth model (where capital has decreasing returns), generates growth that depends on the investment rate. He and Qin, however, do not give any indication that this is what they have in mind. Interestingly, a key result of the *AK* growth model is that the growth rate of the economy is an increasing function of the investment rate. Therefore, government polices that increase the investment rate of this economy will increase the growth rate of the economy permanently.

[29] For example, by differentiating the identity totally with respect to time, assuming that factor shares are constant (i.e., $s_t^L = s^L$ and $s_t^K = s^K$) and then integrating, one obtains $Y_t \equiv A w_t^{s^L} r_t^{s^K} L_t^{s^L} K_t^{s^K}$, or $Y_t \equiv A B_t L_t^{s^L} K_t^{s^K}$, where $B_t \equiv w_t^{s^L} r_t^{s^K}$ (the requirement that factor shares be constant is only for purposes of this theoretical derivation. For econometric purposes, all that is needed is that they be "roughly" constant. In their simulations, Felipe and Holz (2001) show that the Cobb-Douglas specification provides plausible results despite being subjected to substantial variations in the factor shares). This

This argument explains why, if the "correct aggregate production" function is estimated, the statistical fit obtained has to be very high (potentially unity), and the estimates of the coefficients have to be the factor shares (this, however, does not imply that factor markets are competitive); and simultaneously, why implausible results can also appear. He and Qin's problems and irreconcilable results is that they chose an incorrect $Y_t \equiv A_t F(K_t, L_t)$ to approximate their data set.[30]

For purposes of He and Qin's work, this argument implies that the fitted value of output from the production function ($\hat{Y}$) has to be (almost) identical to the actual value of output, that is, $\hat{Y} = Y^T = Y$. Hence the deviations from trend ($Y - Y^T$) should be zero. Felipe and McCombie (2002) show how this argument can be applied to the estimation of a production function for the PRC and obtain plausible estimates of the coefficients (i.e., close to the factor shares).

IV. CONCLUSIONS AND SUGGESTIONS

I believe that He and Qin (2004) have made a commendable effort at tackling a most difficult issue, namely, that of modeling investment in the PRC. However, for the reasons explained in this comment, their arguments and empirical results, in the opinion of this author, are dubious and carry no policy implications whatsoever. I certainly agree that there is always a gap between theoretical models and their empirical implementation for testing purposes. However, one gets the impression that using He and Qin's specification of the business sector investment, it is virtually impossible to reject statistically the neoclassical model of investment, even in the case of the PRC.

This line of work calls for further research. In what follows I make some suggestions, although surely there are other possibilities. A first possibility is to try to work out a different empirical specification of the neoclassical model of investment. A second one, perhaps more fruitful, is to draw on some elements of development theory (e.g., Khayun 1991, Sun 1998), including some specifics about the PRC. Likewise, drawing on some ideas from a variety of heterodox models of investment, rather than mechanically applying the orthodox neoclassical model, might yield more useful insights towards successfully modeling and understanding the determinants of the business sector investment in the PRC. The neoclassical model of investment assumes that investment responds to fluctuations in output and to changes in the rental price of capital. While this may be considered an important theoretical contribution (an

---

expression resembles a Cobb-Douglas production function. However, it is the identity rewritten under the assumption that factor shares are constant. The particular form resembling a production function in each case will depend on the properties of the data set $Y_t \equiv w_t L_t + \rho_t K_t$.

[30] He and Qin's (2004) Cobb-Douglas production function, i.e., $Y = AK^{\theta}L^{1-\theta}$ will provide an excellent approximation to the accounting identity *if and only if* it were estimated with a data set where factor shares are constant, and wage and profit rates are also constant. Certainly the latter is not true for the PRC, hence the poor results they obtained. My conjecture about the estimation results of a Cobb-Douglas with a time trend is that they would be very bad. The regression $Y = Aexp(\lambda t)K^{\theta}L^{1-\theta}$ will work *if and only if* factor shares are constant, and wage and profit rates grow at constant rates. This is also most likely not true in the PRC. Assuming factor shares have been roughly constant, Felipe and McCombie's (2002) conjecture is that the approximation to the identity in the form $Y = B_t K_t^{\theta} L_t^{1-\theta}$, where B_t is a trigonometric function, should work well and provide a good approximation to the accounting identity. As seen above, the function B_t, whatever it is, will have to approximate well $w_t^{s^L} r_t^{s^K}$.

aspect not denied in this comment), this model suffers from a critical shortcoming, namely an inability to properly capture the expectational components necessary for any realistic model of capital accumulation.

For example, Kalecki (1936-7, 1971) emphasized the importance of reinvested profits as a source of investment, profit rates (in particular the difference between the expected rate of profitability and the interest rate) and capacity utilization. See, for example, the classical treatment in Allen (1968, chapter 4). Also, Blecker (1989) and the recent work of Hein and Ochsen (2003) can shed light. On the impact of profitability on capital accumulation, see the evidence provided by Glyn (1997).[31]

The idea that investment depends upon profits is amongst the oldest of macroeconomic relations. If indeed the private sector of the PRC is behaving more like a market economy, then surely a proxy for the profitability of investment (i.e., the average profit rate) and profits themselves will play an important role in modeling investment. This brings to the fore ideas that were put forward by the Classical Economists. For these, accumulation and productive investment of a part of the social product was the main driving force behind economic growth. In a market economy, this takes the form of the reinvestment of profits. For the Classical Economists, profits were assumed to be largely saved and invested. Moreover, the share of profits in income also determined the share of investment in output, and the rate of accumulation.

In market economies, the growth of profits produced by recent investments is the bellwether factor in shaping business future profit expectations.[32] Thus, planned investment growth will be strongly influenced by the evolution of profits relative to recent investments. This way, the average profit rate ρ_t probably plays a role in shaping investment decisions (the equilibrium relationship), while the incremental profit rate affects changes in investment (short-run dynamics). The incremental profit rate is the change in profits net of depreciation relative to investment lagged one period, i.e., $\rho_t^{inc} = (\Pi_t^n - \Pi_{t-1}^n)/I_{t-1}$, where Π_{t-1}^n denotes profits net of depreciation. Expectations bring about, if fulfilled, the necessary justification for past capital expenditures and lend confidence in the anticipation of future results for planned spending. Such prospects of future yields are strongly influenced by current profit levels derived from recent investments. Expectations of future returns will be especially sensitive to the evolution of profits related to past recent investments. Therefore, the movement of the investment growth path should be well explained by a lagged function of the incremental profit rate.

Finally, estimation of the output trend in the government investment equation through the production function is a framework that should be discarded, and other alternative methodologies should be explored. For example, a Harrodian warranted path with a Kaldorian classical savings function implies $I = s_c \Pi$, that is, investment equals the product of the capitalists' propensity to save out of profits (s_c) times aggregate profits (Π).[33] This implies that the warranted rate of accumulation of capital is given by $(I/K) = s_c(\Pi/K) = s_c \rho$. It therefore follows that the maximum warranted growth rate occurs when all profits are reinvested, i.e., s_c=1. This implies that the maximum growth rate

[31] For estimates of profitability in the PRC see Holz (2002).

[32] Although following a neoclassical approach, it is worth reading Gilchrist and Himmelberg (1999), who provide evidence on the determinants of investment and add support to the link between profitability and accumulation.

[33] See Pasinetti (1974), in particular essays IV, V and VI.

of an economy ($g^{Y\,max}$) will be given by the rate of profit (i.e., $g^{Y\,max} = \rho$). Therefore, one can interpret the ratio of the actual growth rate (g^{Y}) to this maximum growth rate, i.e., (g^{Y} / ρ), as an indicator of the degree to which the growth potential of the economy is being utilized. Moreover, the greater this ratio, the greater the likelihood that excess demand will end up accelerating inflation.

REFERENCES

Allen, R.G.D. 1968. *Macro-Economic Theory*. New York: St. Martin's Press.

Banerjee, Anindya, Juan J. Dolado, John W. Galbraith, and David F. Hendry. 1993. *Co-Integration, Error Correction, and the Econometric Analysis of Non-Stationary Data*. Oxford University Press.

Blanchard, Olivier J. 1986. ""Comment on Paper 'Investment, Output and the Cost of Capital' by Matthew Shapiro, in *Brookings Papers in Economic Activity* 0(1): 153-158.

Blecker, Robert A. 1989. "International competition, income distribution and economic growth." Cambridge Journal of Economics, 13: 395-412.

Chirinko, Robert. 1993. "Business fixed investment spending: modeling strategies, empirical results and policy implications." Journal of Economic Literature, 31: 1875-1911.

Chow, G.C., 1993. "Capital Formation and Economic Growth in China." Quarterly Journal of Economics, 108 (August): pp. 809-842.

Felipe, Jesus, and F. Gerard Adams. " 'A THEORY OF PRODUCTION'. The Estimation of the Cobb-Douglas Function: A Retrospective View." Eastern Economic Journal. 2005. Center for Applied Macroeconomic Analysis (CAMA), Australian National University, Working Paper 11/2004.
http://cama.anu.edu.au/Working%20Papers/Papers/Jesus%20Felipe112004.pdf.

Felipe, Jesus, and Carsten Holz. 2001. "Why Do Aggregate Production Functions Work? Fisher's Simulations, Shaikh's Identity, and Some New Results." International Review of Applied Economics, Vol.15, No.3 (July): 261-285.

Felipe, Jesus, and F.M. Fisher. 2003. "Aggregation in Production Functions: What Applied Economists Should Know." Metroeconomica, Vol.54, No.2-3 (May-September): 208-262.

Felipe, Jesus, and JSL McCombie. 2004. "On the Rental Price of Capital and the Profit Rate. The Perils and Pitfalls of Total Factor Productivity Growth." Center for Applied Macroeconomic Analysis (CAMA), Australian National University, Working Paper 10/2004.
http://cama.anu.edu.au/Working%20Papers/Papers/Jesus%20Felipe102004.pdf

Felipe, Jesus, and JSL McCombie. 2003a. "Whatever Happened to the Cambridge Capital Theory Controversies? A Comment." Journal of Economic Perspectives, Vol.17, No.4 (Fall): 229-231.

_____. 2003b. "Some Methodological Problems with the Neoclassical Analysis of the East Asian Miracle." Cambridge Journal of Economics, Vol.54, No.5 (August): 695-721.

_____. 2002. "Productivity in China before and after 1978 revisited." Zagreb International Review of Economics and Business, Vol.5, No.1 (May): 17-43.

Ford, R., and P. Poret. 1991. "Business Investment: Recent Performance and some implications for policy." OECD Economic Studies, 16: 79-131.

Gilchrist, S., and C. Himmelberg. 1999. "Investment: Fundamentals and Finance." In Ben Bernanke and Julio Rothemberg (eds), *NBER Macroeconomics Annual 1998*. Cambride, MA: The MIT Press.

Glyn, Andrew. 1997. "Does Aggregate Profitability *really* Matter?" Cambridge Journal of Economics, 21: 593-619.

He, Xinhua, and Duo Qin. 004. "Aggregate Investment in People's Republic of China: Some Empirical Evidence." Asian Development Review, 21, No.1: 99-117.

Hein, E., and C. Ochsen. 2003. "Regimes of Interest Rates, Income Shares, Savings and Investment: A Kaleckian Model and Empirical Estimations for some Advanced OECD Economies." Metroeconomica, Vol.54, No.4: 404-433.

Hendry, David F. 1995. *Dynamic Econometrics*. Oxford University Press.

Holz, Carsten. 2002. "Long Live China's state-owned enterprises: deflating the myth of poor financial performance." Journal of Asian Economics, 13: 493-529.

Jorgenson, D.W. 1963. "Capital Theory and Investment Behavior." American Economic Review Papers and Proceedings Vol.53, No.2: 247-259.

Kalecki, M. 1936-7. "A Theory of the Business Cycle." *Review of Economic Studies*, 4 (2): 77-97.

_____. 1971. *Selected Essays on the Dynamics of the Capitalist Economy, 1933-70*. Cambridge: Cambridge University Press.

Khayun, Mohammed. 1991. *Macroeconomic Modeling and Policy Analysis for Less Developed Countries*. Westview Press.

Lewis, William W. 2004. *The Power of Productivity*. Chicago: The University of Chicago Press.

Mankiw, N. Gregory, David Romer, and David N. Weil. 1992. "A Contribution to the Empirics of Economic Growth." Quarterly Journal of Economics, 107, 2 (May): 407-437.

Pasinetti. Luigi. 1974. *Growth and Income Distribution*. Cambridge: Cambridge University Press.

Rebelo, Sergio. 1991. "Long-Run Policy Analysis and Long-Run Growth" Journal of Political Economy, 96 (June): 500-521.

Romer. Paul. 1987. "Crazy Explanations for the Productivity Slowdown." *NBER Macroeconomics Annual 1987*. Cambridge, MA: MIT Press.

Sen, Amartya. 1975. *Employment, Technology and Development*. Oxford: Clarendon Press.

Simon, H. A. and Levy, F.K. 1963. "A Note on the Cobb-Douglas Function." Review of Economic Studies, (June): 93-94.

Simon, H. 1979. "On Parsimonious Explanations of Production Relations." The Scandinavian Journal of Economics, 81: 459-474.

Song, Haiyan, Zinan Liu, and Jiang Ping. 2001. "Analysing the determinants of China's aggregate investment in the reform period." China Economic Review, 12: 227-242.

Sun, Laixiang. 1998. "Estimating Investment Functions based on Cointegration: The Case of China." Journal of Comparative Economics, 26: 175-191.

Sylos Labini, Paolo. 1995. "Why the Interpretation of the Cobb-Douglas Production Function Must be Radically Changed." Structural Change and Economic Dynamics, 6: 485-504.

Young, Alwyn. 2000. "Gold into Base Metals: Productivity Growth in the People's Republic of China during the Reform Period." National Bureau of Economic Research, Working Paper 7856.