

Dear editor,

thank you for the referee reports.

Apart from addressing the reviewer comments, I have also given the paper a complete make-over, streamlining and clarifying. To the extent possible, I have updated the data. --

I am happy to see that Reviewer 1 thinks the paper warrants publication in Emerging Markets Finance and Trade (without asking for any revisions).

Below I respond how I am addressing Reviewer 2's comments.

1. Theoretical framework

Reviewer 2 asks writes: "the study should be placed within a theoretical framework to make implications for academia and practitioners, such as economic growth, economic development, transitional economics, etc."

The guest editor adds: "The paper is a descriptive study. While it is valuable, I agree with the referee that there should be a theoretical context to the explanation. It would be helpful to frame questions and arguments."

Theoretical framework is a difficult term for me (and neither reviewer nor editor say what kind of theory they have in mind). If I look up the definition of theory, I find (at <https://www.merriam-webster.com/dictionary/theory>):

1 : a [plausible](#) or scientifically acceptable general principle or body of principles offered to explain phenomena *the wave theory of light*

2a : a belief, policy, or procedure proposed or followed as the basis of action *her method is based on the theory that all children want to learn* *b* : an ideal or hypothetical set of facts, principles, or circumstances —often used in the phrase *in theory in theory, we have always advocated freedom for all*

3a : a hypothesis assumed for the sake of argument or investigation *b* : an unproved assumption : [conjecture](#) *c* : a body of [theorems](#) presenting a concise systematic view of a subject *theory of equations*

[and further, less relevant items]

From philosophy of science I am also familiar with theory as a body of *several* hypotheses (related to the third item in the Merriam-Webster definition).

As far as I can tell, the paper in its current version has plenty of theory – namely all the theory that underlies national income accounting (our particular definition of GDP, our particular breakdown of aggregate expenditures). So "theory" cannot be missing.

Yet Reviewer 2 and guest editor both feel that something is missing. Is it a math model that is missing? (Yes, I don't write out the aggregate expenditure growth equation, and I don't write out the Solow growth model that underlies the calculation of TFP growth. I don't write them out because I assume that economists are very familiar with these theories, and personally

find it exceedingly boring to repeat them. I am interested in the results, and the interpretation of the results, which I provide.)

The guest editor provides a further hint in that s/he finds the study “descriptive.” But that, I think, can also not be the issue, because every math model (and every theory) is descriptive: These are description of assumptions and relationships, sometimes using English language and sometimes using math language. (The only difference between English and math is the degree of abstraction, in that the math language, explicitly or implicitly, tends to assume away more of the real world.)

What I suspect is, in the end, at issue, is that the paper is inductive more than deductive. (The growth-related part up front is deductive.) Deduction reduces the world to something very simple, along the lines of if X then Y. The relationship from X to Y can either be presented as the truth (I observe that X is in China, and conclude that therefore Y is), or it can be presented as a hypothesis, a relationship that I check with my research, and conclude that it holds at a Z% significance level.

* It is correct that in this paper I do not test one narrow hypothesis. The paper is by design an overview paper, trying to make sense of a big picture of what is happening on the ground.

* It is also correct that in this paper I do not start from some particular belief system (one or more “accepted” hypotheses (hypotheses that have been found by others to hold at some particular significance level in some particular circumstance)). – Keeping in mind that the beginning of the paper does work with a theory (belief system) of what is of interest to economists (for example, working at home is not of interest for GDP) and how economists want to look at the world (for example, aggregate expenditures are not being broken down into expenditures on socks vs. expenditures on everything else, but primarily into expenditures on consumption vs. on investment).

Having said all that, if Reviewer 2 and/or the guest editor wish to impose some particular belief system (theory) or to test some particular, narrow hypothesis, then they would need to specify what specifically they need me to buy into. -- Otherwise, I am quite happy with the approach of the paper as it stands. This particular approach, for this paper, is intentional. It is an overview paper. It’s intended to make broad, reliable arguments on trends into the future. It’s intended to be readable by many (while likely only economists understand all of the details). It’s also intended to be quotable by many since it offers a starting point for a multitude of narrower, technical studies.

Thinking that the Solow growth model with its steady state idea might come close to what the reviewer and guest editor may have in mind, I now included a footnote (#8) saying why I don’t think it’s useful for the purpose at hand.

In reworking the paper, I have deleted some of the charts to make it more focused, and I have kept reviewer 2’s ‘implications’ in mind.

2. Clarification of empirical evidence.

Thank you. Yes, it’s confusing. I have now clarified.

One data point is the funding source for investment. This includes items such as foreign funds and own funds, where the own funds of a foreign enterprise in China are considered own funds and not foreign funds.

The other data point is the dominant ownership form of a firm (in an aggregate of firms, across a sector, or the economy) in which investment is happening.

3.1 I have also clarified in that the data that I originally cite are investment funding data and not debt data.

Debt numbers such as the 300% reflect debts of government, firms (financial and non-financial), and households and are balance sheet numbers. There is no immediate relationship to investment and investment financing. I elaborate in the paper.

3.2 More precise estimation of state-ownership

I agree that some shareholding companies are SOEs; others are HongKongMacauTaiwan-owned, “foreign-”owned (not Hong Kong / Macau / Taiwan), or privately owned. Shareholding companies comprise limited liability companies (the largest part) and stock companies. A subset of the stock companies is listed on stock markets.

I agree that one can examine the ownership structure of the stock companies listed on stock markets (and then potentially sort out which ones of these are state-owned and owned by which level of the state). I view this as a separate research paper. It would probably require a grant application to finance a research assistant to spend considerable time on this. The link to this current paper appears too weak to me to pursue, for this paper. The current paper in one section postulates potential dangers to continued investment and economic growth, with one of the dangers—as viewed in the literature—being the fact that some of this investment and growth is driven by state-owned enterprises. In the paper, I question the view that all investment by state-owned enterprises is problematic (and I would have extensive earlier, published research to back this up), and then I provide a few numbers to show that state-ownership isn’t even that dominant in investment. – Whether it’s 5% more or less is in my view not important enough for this paper to warrant the excursion into the only vaguely related stock market data – which *cannot* be explicitly related to the statistic of interest, the currently cited statistic of 32% share-holding companies.

4. Interpretation of capital-output ratio

The reviewer again refers to the “proper theoretical framework(s)” and “theories” without specifying what these are in the reviewer’s mind. Doing a quick search of Wikipedia and google scholar for capital-output ratio or incremental capital-output ratio provides me with no hint of what the reviewer may have in mind. That changes in capital-output ratios depend on the industry structure of the economy would seem obvious (without reference to some theory) and I already covered that territory in the paper as is.

The only thing that comes to my mind is the Solow growth model with an approach to a steady state, which to me, so obviously, is too far removed from reality to be useful. (See the trend of the capital-output ratio of the U.S., which one then might want to explain via a changing sectoral/industrial structure, at which point one keeps varying / expanding one’s

theory to match individual cases, and in the end does not really have one theory for all cases but one theory for each case.)

The reviewer points out that China's capital output ratio is relatively high, while the capital labor ratio is relatively low (and more), and wonders about the implications. These are great extensions and I am grateful for the reviewer pointing them out. I now expand on these issues.

Guest editor's comments

A. Theoretical framework – please see my response to reviewer 2, item 1.

B. Middle income trap

Thank you for pointing out the link to the middle income trap topic. (Personally, I think of a sequence of development stages and the idea that a typically undefined “middle-income” level is special, and constitutes a trap, does not fit with a sequence of stages. Therefore, the middle income trap topic was not on my mind when I wrote the paper.)

Google scholar reveals a lively discussion on the existence (or not) and the characteristics of the “middle income trap,” and I am not trying to turn this paper into a middle income trap paper.

I am now explicitly extending my findings to the topic of the middle income trap, in the conclusions.

-- I find working with the track changes function in Word quite cumbersome and have therefore revised the paper without using that function. I have then afterwards used the comparison function in Word to obtain a comparison of the revised paper and the previously submitted version of the paper, which shows all the changes made. I'll try to upload both files.