

Comment on “Special Deals with Chinese Characteristics” by Chong-en Bai, Chang-Tai Hsieh, and Zheng Song

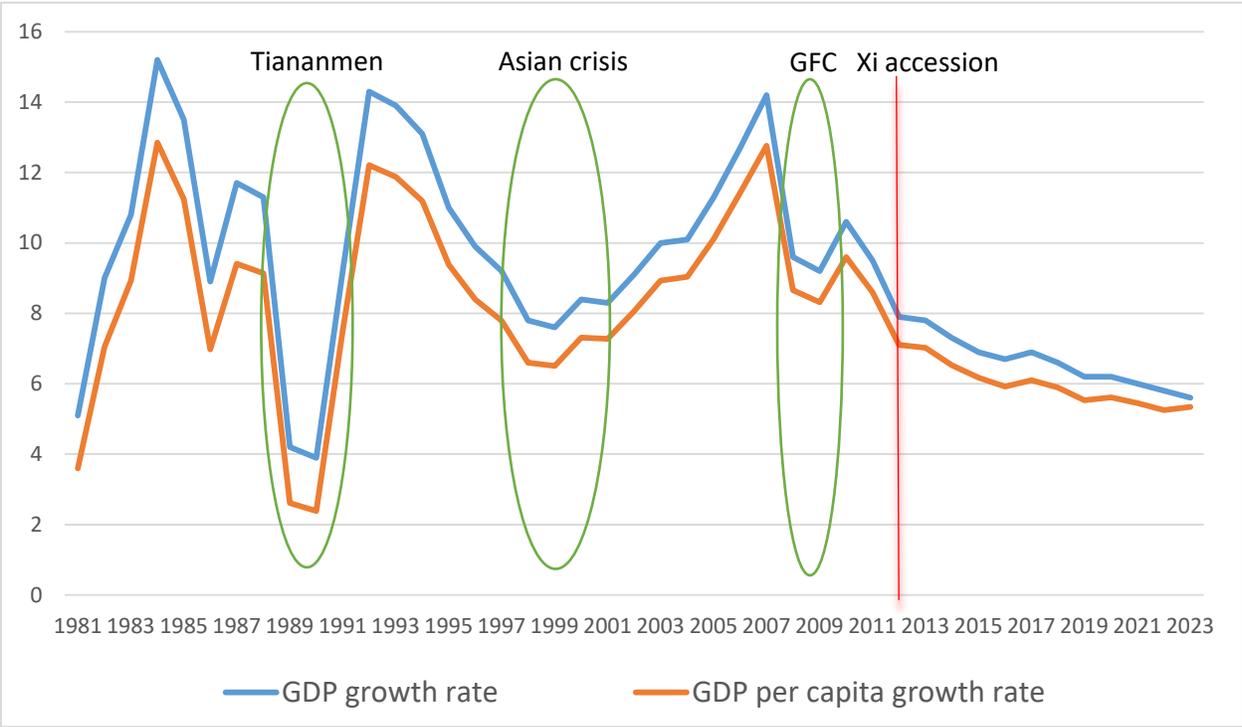
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December 18, 2018 marked the 40th anniversary of the Third Plenary Session of the 11th Central Committee of the Chinese Communist Party (CCP), at which Deng Xiaoping launched China on its remarkable recent trajectory of economic growth. By giving greater play over time to market forces, Deng’s reform and opening initiative spurred rapid income convergence. But it did so while preserving the CCP’s political monopoly.

As Figure 1 shows, China’s real GDP growth (insofar as the official data are accurate) has been

Figure 1: China’s growth and its deceleration since 2007
(percent per year)



high on average, but anything but smooth. Moreover, it has declined markedly since the last year of double-digit growth in 2010, and even more so since the heady pre-crisis peak.

China's politics have not evolved monotonically, either. This year, 2019, marks the 30th anniversary of the Tiananmen protests, sparked by the (vain) hope that China's economic evolution would encourage a commensurate political evolution. Chinese politics – and I will argue, economics – have only regressed under the more authoritarian rule of President Xiaoping Xi. In this paper, Chong-en Bai, Chang-Tai Hsieh, and Zheng Song likewise link China's falling growth rate over the past decade to political factors, but I will emphasize a set of factors different from the one that they highlight.

What mechanism linking politics to growth do the authors emphasize? In his speech commemorating the 40th anniversary of Deng's reforms, President Xi stated that "We will resolutely reform what should and can be reformed, and make no change where there should not and cannot be any reform."¹ An alternative title for this paper could have been "Chinese Cronyism: Can It Be Changed? Should It Be Changed?" The authors' main message is that cronyism is deeply ingrained in China's mode of doing business, but it has had beneficial growth effects that Xi's drive to consolidate power is stunting.

What the paper does

This paper assembles a fascinating body of evidence on how business is done in China. It makes the overarching claim that since the early 1990s, China's primary growth driver has been an increasing availability of "special deals" that competing local governments offer to private firms. To support this account, the paper offers three lines of argument.

- Explanation of the "special deals" regime and anecdotes on how it can work to drive growth.
- A Dornbusch-Fischer-Samuelson (1977) model (DFS) of deals' allocation effects, for both (1) a two-city economy with labor mobility and (2) two cities with export markets. The underlying DFS model is essentially the two-country version of Eaton and Kortum (2002), parameterized as in the dynamic extension by Ken Rogoff and me (Obstfeld and Rogoff 1996).
- Four categories of supportive empirical argumentation:
 1. Data on the growth of large firms and conglomerates.
 2. Evidence that bigger firms generate faster employment growth.

¹ Speech transcript at Transcend Media Service, December 24, 2018, <https://www.transcend.org/tms/2018/12/xi-jinpings-speech-on-40th-anniversary-of-chinas-reforms-opening-up-full-text/>, accessed June 6, 2019.

3. Evidence that firms that are more successful also are better connected politically.
4. Evidence that the system encourages smaller firms to export.

Stepping back before diving in

Even before reviewing the latter categories of evidence, I want to lay out some reasons I am skeptical of the paper's basic thesis.

The paper lays out convincing evidence of the prevalence of cronyism – for example, the webs of hidden ownership and the preferential access to credit of firms owned by CCP-connected officials. Where it is much less convincing is in establishing that cronyism spurred growth. I did not see anything in the paper that one could construe as direct evidence.

The paper's DFS model establishes, at most, that special deals can have a *level* effect on productivity, not an effect on steady-state growth. Of course, given an increase in cronyism, the model might predict a transitional growth increase as the economy attains a new, higher level of TFP. However, the paper leaves open if such effects can explain China's growth experience.

Related, one would think a regime of special deals would soon hit diminishing returns at the national level. In particular, as competing sub-national governments promote duplicative capacity bolstered by protective trade restrictions, distortions rise and the gains from specialization (including possible increasing returns) rapidly erode. True, exporting is an option, but export capacity has limits.

Even at the local level, I wonder if the multitudinous officials hustling non-cooperatively for local deals, without necessarily internalizing the possibility of driving workers away through lower real wages, might push a city far past the optimum in their Figure 2.

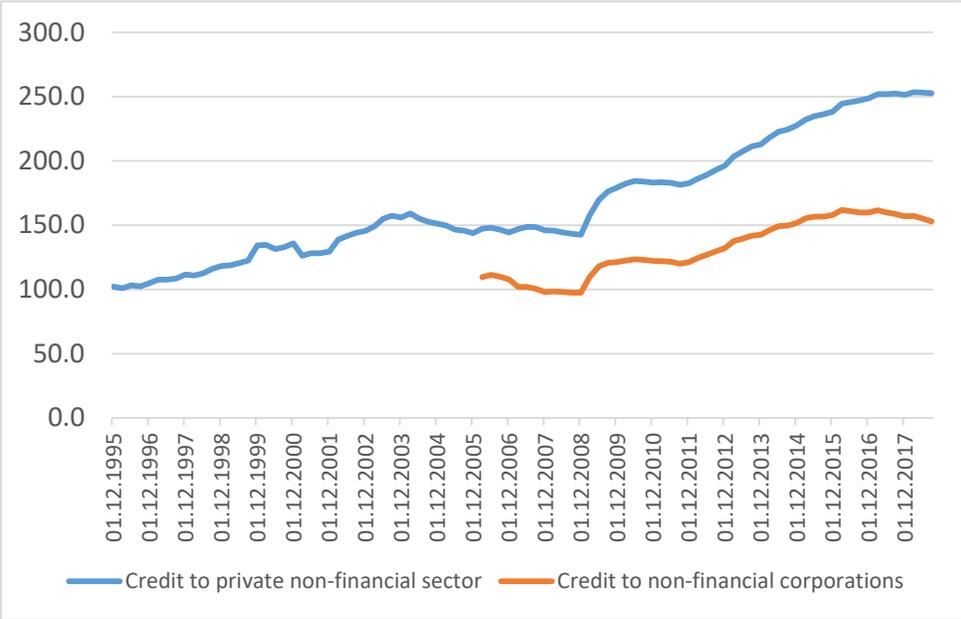
One avenue for cronyism to result in an (eventually) growth-limiting coordination failure is through excessive credit expansion. Indeed, the authors (Bai, Hsieh, and Song 2016) argued in an earlier paper that, starting with the 2008 crisis, uncoordinated local-government credit extension for favored industries, through the mechanism of off-balance sheet vehicles, "potentially worsens the overall efficiency of capital allocation. The long-run effect of off balance-sheet spending by local governments may be a permanent decline in the growth rate of aggregate productivity and GDP." I am not sure how to reconcile that finding with the claims in the present paper: according to the authors' evidence, firms controlled by Party members had preferential access to bank loans even in the early 2000s, and they find no evidence that access had increased by 2012.

China’s authorities have (belatedly) recognized the perils of unfettered credit growth, as indicated by their recent attempt to slow it down (Figure 2) – an important reason for the recent decline in GDP growth.

As a general observation, I find it counter-intuitive to argue that simply by *easing* pre-existing distortions, cronyism unleashed growth rates as exceptional as those China has shown in recent

Figure 2: Reining in China’s credit growth

(percent of GDP)



decades, up until very recently. Distortions indeed were severe – but as the authors acknowledge, other forces were at play. A partial list would include trade reforms prior to WTO entry, the movement of labor from agriculture into the industrial sector with its higher average labor productivity, enhanced infrastructure and connectivity, and possibly even an easing of the urban housing constraints stressed by Hsieh and Moretti (2019) as a barrier to growth in the United States.

Are the empirical arguments convincing?

As noted, Bai, Hsieh, and Song present four main empirical arguments for their thesis. They assert that while each one, alone, might not be convincing, the four taken together make a compelling case. I did not come away convinced that the whole exceeds the sum of its parts.

Section 4.1 looks at the growth of firms, and argues that sponsored firms will grow relative to others due to the relaxation of constraining regulations, often through joint ventures with dispersed and non-transparent ownership. I wonder about the extent to which the duplicative efforts in localities could actually mitigate concentration at the national level. More importantly, I don't see why greater concentration should necessarily spur growth – for market economies, the theory and evidence on this is certainly mixed, and different effects might obtain for different concentration levels and at different phases of a firm or conglomerate's life cycle.

Section 4.2 documents a city's employment growth has tended to be higher over 1998-2007 when it contained more large firms. Again, the link to output growth is indirect, and it is unclear that the authors' favored mechanism is at work. Even if larger firms are favored and therefore hiring more, the productivity implications are unclear. Indeed, the paper's Figure 5 indicates that for a sample of privately owned firms, "capital productivity is decreasing in firm size." This seems to run counter to the idea that bigger necessarily implies more productive.

The suggested positive relationship between cronyism and city growth runs counter to other evidence. For example, Rodríguez-Pose and Zhang (2019) present evidence that better city institutions and measures to limit corruption promote urban growth. It would be helpful to know if the paper's Figure 4 displays a pattern peculiar to China, or if it would also characterize major market economies. The figure suggests that small cities tend to host smaller firms on average, and we also know that smaller cities may grow more slowly on average (including for Hsieh-Moretti reasons). The discussion calls out for a more thorough, multivariate analysis.

Section 4.3 explores firms' political ties. From the analysis in section 4.1, however, we know it may be hard to determine beneficial ownership. Thus, I would not view this evidence as dispositive either. Indeed, even if political connections contribute to firm growth, the authors also show in this section bigger privately owned firms tend to have lower capital productivity. To my mind, this finding makes the link from political connections to overall GDP growth more questionable.

Finally, section 4.4's evidence on firm size and exporting status is fascinating – and does present a pattern different from what we see in market economies. It is not clear, however, that the specific model the authors present implies the distinctive spike of smaller exporting firms their Figure 8 shows (as opposed to the general point that more smaller firms will be induced to sell abroad owing to restrictions on sales in other localities). This pattern could be a product of the

trade diversion their model implies, but why it takes this particular form remains a mystery, and I would guess that additional factors are in play.

Other questions

Precisely because the ownership of firms is hard to determine, I would like to know more about the interactions between local private champions and state-owned enterprises (SOEs). Local governments also run many inefficient SOEs; they can support them in various ways, including through credit diversion, and it is hard to believe that there is a clear line between the large private firms and conglomerates that local governments have supported and the SOEs that they also support. Much of that interaction seems unlikely to benefit the efficiency of the private partners.

Clearly local governments have several incentives to support inefficient SOEs, including maintaining employment, maintaining influence, and tax revenues. These incentives differ across regions.

Figure 3: Relative profitability of Chinese private enterprises and SOEs
(rate of return on assets, percent per year)

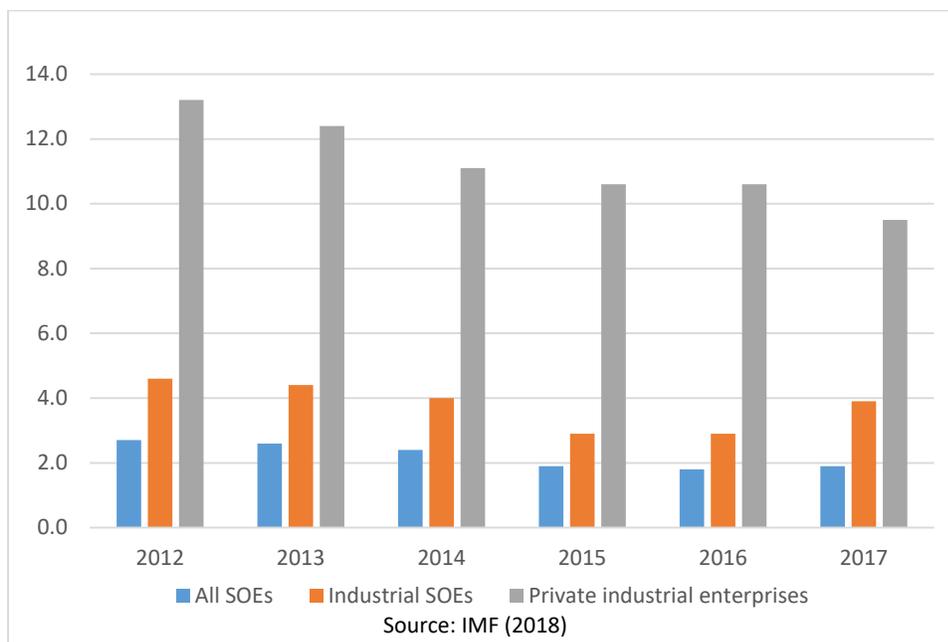


Figure 3 documents the relative profitability of three categories of firm. Industrial SOEs are more profitable than other SOEs, but all SOEs are far less profitable than private industrial enterprises. Reference to Figure 3 in my comment on Bai, Hsieh, and Song (2016) shows similar trends for the post-crisis period, but in addition, that locally operated SOEs have been the least profitable among SOEs, generating even lower returns than those that are centrally controlled. While private enterprise remains far more profitable in China, its profitability has declined since the crisis, in line with the authors' narrative, and more sharply than the relatively gentle decline in profitability of SOEs.

That profitability decline has coincided with a decline in overall GDP growth. The question is, why is this happening?

Forces behind China's growth slowdown

Many factors are contributing to China's falling rate of growth. Among these are population aging, the rebalancing of the economy toward consumption and services, and a diminished scope to move labor from agriculture to manufacturing. China's anti-pollution drive is also biting, due to a growing middle class demanding cleaner air and water. As Pritchett and Summers (2014) have argued, none of this is inconsistent with the historical tendency for growth rates to fall as poorer countries converge.

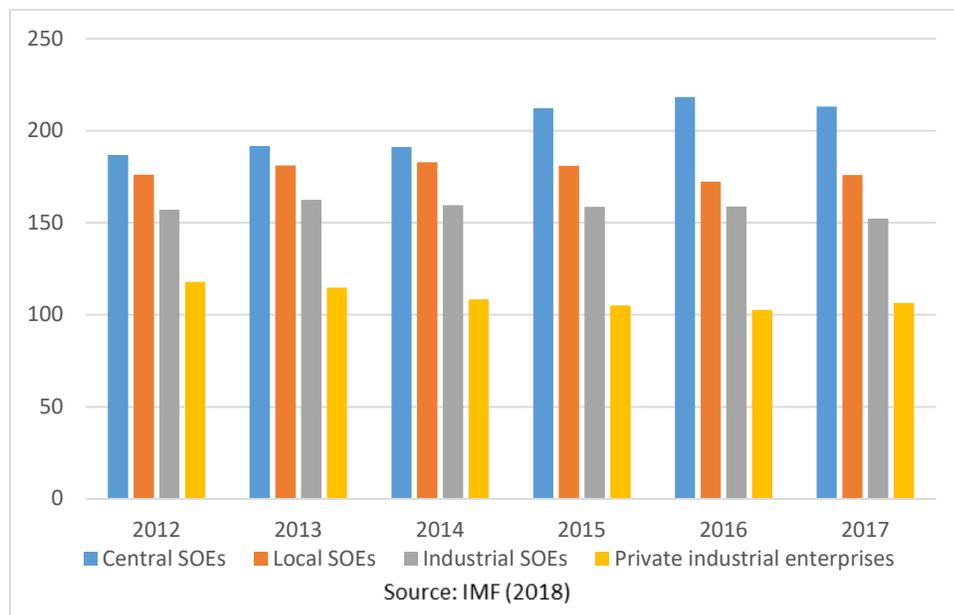
More specific to the Chinese case, one could argue, as does Jin (2019), that China's exceptional growth has owed to a government-directed model of forced saving and export intensity that was unsustainable and has become counter-productive. The authors also hint that a special deals regime could ultimately be counter-productive by creating strong vested interests inimical to necessary reforms.

Bai, Hsieh, and Song argue that President Xi's anti-corruption campaign has slowed China's growth by curtailing the scope for special deals. I agree that the anti-corruption campaign has been a factor, though not primarily for the reason the authors suggest. The Xi crackdown has certainly had a chilling effect on initiative and entrepreneurship. Some of this effect, no doubt, is to discourage special deals. However, the negative growth effects likely extend beyond that specific mechanism.

More importantly, the anti-corruption campaign has been a tool in President Xi's drive to consolidate political power in his own hands. Another facet of this drive, as Lardy (2019) documents, has been diversion of economic resources away from the private sector into the comparatively less productive state-owned sector (recall Figure 3). For example, China's credit

clampdown may be disproportionately affecting private firms. Lardy argues that this reallocation is robbing China of substantially higher potential growth.

Figure 4: Debt-to-equity ratios for SOEs and private enterprises
(percent)



Rough indicators of the centralizing trends in credit allocation come from firms' debt-to-equity ratios. Figure 4 shows that over the Xi era through 2017, central SOEs accumulated debt relative to equity, whereas local SOEs did not. Industrial SOEs as a group have seen a mild decline in debt-to-equity, but within private industry (where debt ratios are lower), the decline has been sharper in both percentage point and proportional terms.

A more recent factor slowing China's growth has been trade tensions with the United States – tensions that escalated sharply after the conference at which the authors presented this paper. Because China has moved promptly to deploy offsetting macroeconomic stimulus, the effect of trade conflict on China's growth is not immediately evident in data on aggregate GDP. But the underlying effects have worsened the tradeoffs that Chinese policymakers face.

Bai, Hsieh, and Song rightly question whether China's opaque economic arrangements are compatible with a rules-based global trading system. Indeed, some reformers within China welcome the external pressure on trade as a possible catalyst for economic liberalization and a turn back from Xi's increased support of the state-owned sector of the economy.

It may be overly optimistic to expect such a favorable resolution of current trade tensions. In his December 2018 speech commemorating Deng's reforms, President Xi also declared, "No one is in a position to dictate to the Chinese people what should or should not be done." At the same time, U.S. devotion to a rules-based global trading system (as opposed to choosing its own rules to suit the moment) is not obvious. The two countries are on a collision course that goes far beyond economics. How this conflict is resolved will be a prime factor in Chinese growth – and global growth – in coming years.

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