Discussion: The Reallocation Myth*

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This is a very interesting and provocative paper on an important topic. Growth in advanced economies of the world have settled into a tepid pace of 1%-2% a year associated with a decline in total factor and labor productivity over the past decade. It is imperative to understand the sources of low productivity growth and to evaluate what policies can rejuvenate growth. The potential growth rate of the economy has implications for measures of output gaps and real rates and therefore has implications for monetary policy. Chang-Tai and Pete in their ambitious paper take up this challenging task for the U.S. economy.

Before proceeding to describe their contributions it is useful to take a quick look at the trends in U.S. productivity over the last 30 plus years. Figure 1 from Fernald (2015) highlights three important features: First, the recent decline in TFP that starts in 2003 precedes the great recession. Second, the low TFP growth in the recent decade (2003-2013) of 0.6% per annum is a decline only relative to the previous decade of 1995-2003 when TFP grew at a 1.7% annual rate, and in fact is normal when compared to the two decades preceding the technology boom when TFP grew at 0.7%. Consequently, the interesting question could just as well be what drove the extraordinary growth in productivity from 1995-2003, as opposed to what explains its decline. As emphasized by Fernald (2015) the high growth in the middle years is driven by information technology (IT) intensive sectors, both IT using and IT producing sectors.

The potential culprits for slowing TFP growth in the recent decade relative to the previous decade include 1) worsening measurement of TFP: Recent work by Syverson (2017) and Aghion, Bergeaud, Boppart, Klenow, and Li (2017) do not find evidence that worsening measurement is a significant factor in explaining slowing TFP growth; 2) lack of ideas: this line of argument has been pursued by Gordon (2016), Bloom, Jones, Van Reenen, and Webb (2017), among others; 3) loss of dynamism, associated with rising frictions to creative destruction and reallocation of

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resources from less innovative incumbents to more innovative incumbents and entrants, a line of argument pursued by Decker, Haltiwanger, Jarmin, and Miranda (2017b) and Decker, Haltiwanger, Jarmin, and Miranda (2017a), among others.

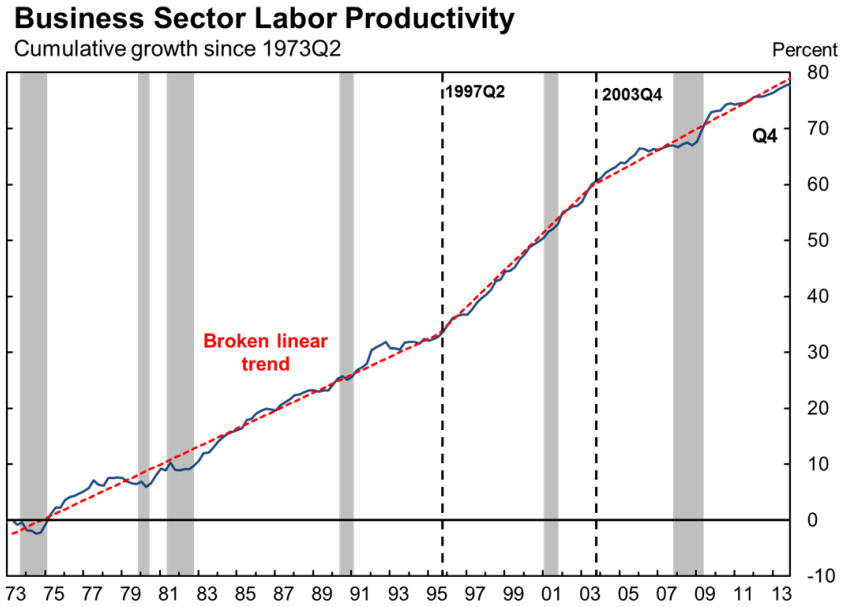

Figure 1: Trends in U.S. Labor Productivity, Source: Fernald (2015)

In this paper, Chang-Tai and Pete argue that the third line of argument, namely the lack of dynamism, has a limited role in explaining U.S. TFP growth. Specifically, they do not dispute the following fact:

Fact: Measures of reallocation have declined over time

They agree that there has been a secular decline in firm entry rates and in job creation and destruction rates from the seventies to now. Decker, Haltiwanger, Jarmin, and Miranda (2017a) estimate a decline in job reallocation rates from 36% in 1979 to 28% in 2011. What Chang-Tai and Pete dispute is the relative importance of this business dynamism and its decline over time in accounting for growth in the U.S. They call this the 'reallocation myth'. I will split their "myth" into two:

Myth 1: Lower reallocation implies greater misallocation
Myth 2: Lower reallocation implies lower innovation

Myth 1 refers to a literature that associates measures of shifting resources from less to more productive firms as evidence of improved allocative efficiency. According to this literature declining measures of reallocation are therefore bad for productivity from an allocative efficiency

perspective. Myth 2 is about the idea that even if there are no distortions in resource allocation across existing firms, the majority of innovation necessarily takes place through a process of creative destruction. Consequently a lower reallocation rate must mean lower innovation.

Chang-Tai and Pete argue that neither of these channels have been essential for growth in the U.S. and are therefore myths. They claim that as regards 'Myth 1': there is no evidence of improving allocative efficiency at any point in the last 30 odd years in the U.S., unlike what the previous literature has asserted. As for 'Myth 2', they argue using the census's longitudinal business data base for all non-farm enterprises that over five year periods most innovation is done by slow growing incumbents, not entrants or 'gazelles' defined as firms that grow at an annual rate of over 20%. They attribute at most 25% of U.S. productivity growth to the forces of creative destruction and reallocation and conclude that only 10 basis points of the 177 basis point decline in U.S. growth from 1995-2004 to 2005-2016 arises from weaker creative destruction.

I will make two comments in praise of the paper before raising concerns.

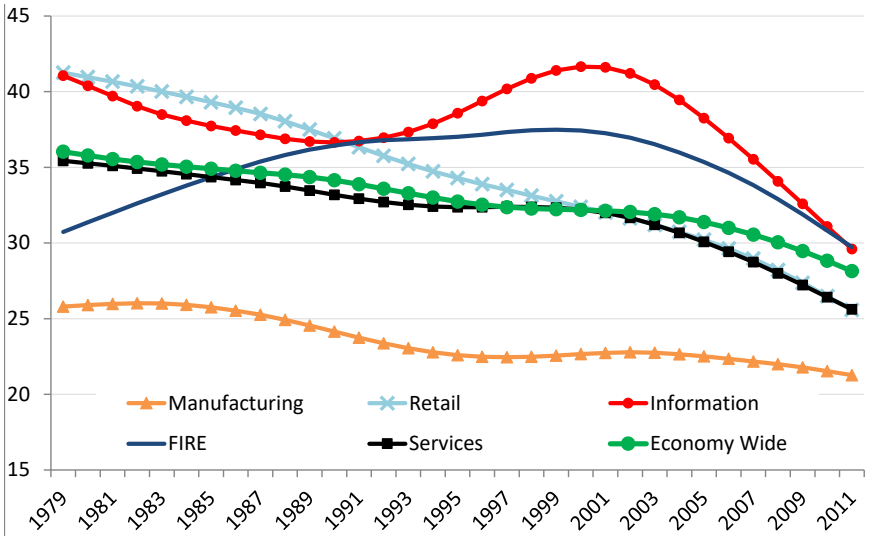

Figure 2: Job Reallocation Rates: Source: Decker, Haltiwanger, Jarmin, Miranda (2017)

The first has to do with Myth1. The critique against Myth 1 in the paper is about the *measurement* of misallocation. There is simply no fool proof measure of misallocation. A point they make and I agree with is that it is incorrect to simply attribute a shift in resources towards more productive firms as an improvement in allocative efficiency. When production is subject to diminishing returns there is an optimal firm size. When there is demand for variety in products then again there is an optimal firm size. Consequently just documenting that resources have shifted towards more productive firms is not necessarily evidence of improving allocative efficiency.

Instead of this accounting measure, Chang-Tai and Pete prefer a measure based on dispersion

in marginal revenue products (Hsieh and Klenow (2009)). The idea is that at the margin the return to factors of production should be equalized across firms for efficiency. Consequently, dispersion in marginal returns is evidence of misallocation. When the marginal return is measured as the ratio of the nominal value added to the quantity of the input used (often referred to as 'revenue' based measure of productivity, as opposed to a 'quantity' based measure of productivity) they find no evidence of a decline in dispersion over time over the last few decades and accordingly no evidence of an improvement in allocative efficiency at any point of time.

While their measure of dispersion is driven by first principles and therefore a useful benchmark, the actual measurement of returns to factors can get quite complicated when one departs from the stark assumptions that motivate their empirical measure. For example when input decisions are made not on a static basis but are dynamic because there are adjustment costs then measures of changing dispersion do not speak to allocative efficiency. The simple argument is that with lags between decisions and outcomes firms equate *expected* returns. Consequently, *realized* returns may not be equalized even if there is no misallocation. These concerns have been flagged in the literature by, Asker, Collard-Wexler, and De Loecker (2014), Gopinath, Kalemli-Ozcan, Karabarbounis, and Villegas-Sanchez (forthcoming), among others.

That said, and setting aside measurement issues, just comparing trends in productivity and reallocation suggests that there may be more going on than declines in reallocation. Figure 2 taken from Decker, Haltiwanger, Jarmin, and Miranda (2017a) documents that the economy wide decline in reallocation has been secular from 1979 to 2011. This however covers the period when TFP was high in the 1990s alongside the period when it has been low. Further, it is useful to compare trends in two sectors, the 'information' sector and the 'retail' sector. These were both sectors that experienced increases in productivity during the 1990s but as can be seen in Figure 2 in the case of the 'information' sector there was an increase in reallocation while in the 'retail' sector there was a decline in reallocation with the advent of companies like Walmart. Therefore the link between TFP growth and measures of reallocation is far from straightforward.

The second word of praise has to do with Myth 2. It is a major contribution of the paper that this is an analysis for *all non-farm private enterprises* and therefore speaks to aggregate economic activity. Using a model of growth they provide a decomposition of growth arising from creative destruction/reallocation and from incumbents improving own variety. They conclude that in terms of explaining sources of growth: own variety improvement is responsible for 65% growth in the 1976-1987 period and 77% in 2003-2013. Therefore creative destruction/reallocation has a minority role.

I will now make three comments that are of a more critical nature and that suggests need for

	TFP Growth	Job Destruction	Job Creation
1976-1986	1.03%	32%	45%
2003-2013	1.44%	28%	33%

Table 1: TFP Growth and Reallocation: Source Garcia-Macia, Hsieh, and Klenow (2016)

further work:

Comment # 1: What about the years when TFP growth was high?

The growth decomposition provided in the paper covers the decades 1976-1986 and 2003-2013, missing the intermediate years including the 1990s when growth was high and relative to which there is a decline in productivity in recent years. For completeness it would be useful to cover these intermediate years. Also, unlike the comparison between the 1976-1986 and 2003-2013 decades when TFP growth *increased* alongside a *decrease* in reallocation as reported in Table 1, if the comparison was of the high growth years to the most recent decade we would observe a *decrease* in TFP growth alongside a decrease in reallocation.

Secondly, as I described previously, different sectors had different experiences with reallocation and TFP. It will be valuable if the analysis incorporates this sectoral heterogeneity, besides speaking to economy wide trends. Because the IT producing and using sectors were the ones that experienced the sharpest productivity increases in the 1990s focusing on these sectors can further our understanding of what drives productivity growth.

Comment # 2: Is it the right model?

The decomposition and conclusion that creative destruction is not a major driver of growth cannot be directly inferred from the data. As explained in Garcia-Macia, Hsieh, and Klenow (2016) one cannot directly identify which product replaces which and whether this is being done through creative destruction with a firm stealing another's business or with a firm replacing it's own product. The decomposition is therefore based on model based indirect inference using job flow data. The identifying assumption is that if creative destruction is the major driver of growth then it should show up as a thick tailed job flow distribution with a lot of mass associated with large percentage changes in job flows at the firm level, as firms shut down in response to business stealing or grow significantly as they steal others businesses. On the other hand if growth arises from firms improving their own variety then at the firm level at least we should not observe large changes in employment. Based on this assumption and other model features they arrive at a quantitative decomposition that supports the incumbent innovation channel because they find

the job flow distribution to be more continuous and less thick tailed relative to what is predicted by a creative destruction driven growth model.

While this is a clever identifying assumption it may not be sufficient to quantify the role of creative destruction and reallocation. Depending on the extent of heterogeneity allowed in the growth rates and innovation sizes at the firm level the predicted distribution of job flows may be quite continuous despite creative destruction being the main driver of growth. The model in this paper for example assumes that the step size of innovation is independent of firm size and firm age. This is counter to evidence documented by Akcigit and Kerr (forthcoming). Figure 3 taken from their paper shows that the number of patents per employee is much larger (and therefore the step size of innovation bigger) for smaller firms than for the larger firms. When this fact is incorporated into a growth model Akcigit and Kerr (forthcoming) conclude that only 25% of growth is own innovation while 75% is reallocation and creative destruction, in contrast to the findings in Chang-Tai and Pete's paper.

Another assumption made is that there are no adjustment frictions past five year horizons. That is entrants gain significant market share within five years, but in reality transition dynamics could be longer given the time it takes for entrants to build their customer base and recruit and train employees.

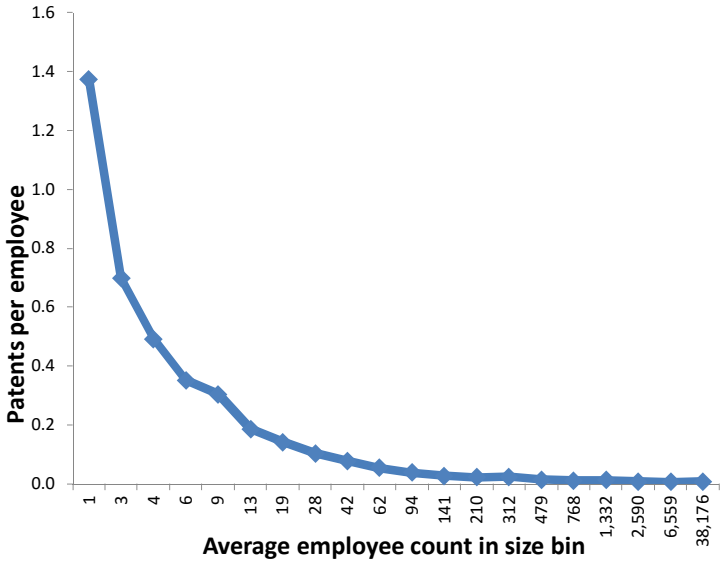

Figure 3: Innovation Intensity by firm size; Source: Akcigit and Kerr (forthcoming)

Comment # 3: About symptoms, not about cause

My third and last comment is that even if one accepts the assumptions in the model it falls short of telling us about what drives growth in a causal sense because the model is a purely

accounting model. Innovation is exogenous and no one makes any decisions about how much to innovate or whether to innovate or when to enter. So it is difficult to disentangle what needs to be fixed just from the symptoms. For example, suppose the main friction is too high costs to firm entry. This would then result in an economy where there is very little entry and exit, lower competition, which in turn lowers the incentives of incumbents to innovate. So even if we observe that declining growth in the U.S. is accompanied primarily by slowing innovation by incumbents the main source of the problem could be on the extensive margin and the lack of dynamism. De Loecker and Eeckhout (2017) provide evidence of rising market power of publicly listed firms. They estimate that starting 1980 average markups rise from 18% above marginal cost to 67% today, possibly suggestive of lower competitive pressure.

To conclude: Is the reallocation channel a myth? Chang-Tai and Pete certainly give us grounds for caution in their ambitious paper, but for the reasons I just listed I believe the jury is still out.

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