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Abstract

A recent policy brief from the Peterson Institute suggests that the "Too Much Finance" result may be an artifact of spurious attribution of causality. While more works needs do be done to understand the links between finance and growth and explore the drivers of possible non-monotonicities, this note shows that the too much finance result is robust.

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1 Introduction

William Cline recently wrote an interesting policy brief arguing that the "Too Much Finance" results of Arcand et al. (2011, 2015), Cecchetti and Kharroubi (2012), and Sahay et al. (2015) may be artifacts of spurious attributions of causality (Cline 2015).[1]

He provides a clever proof showing that quadratic effects in a typical finance-growth regression are subject to a negative bias and then argues that some of the studies in the too much finance literature have implausibly large negative effects.

This note addresses these criticisms. It first shows that there is a problem with Cline's proof (Section 3). Next, it shows that the too much finance result is robust to controlling for non-linearities in initial income (Section 4) and explains the magnitude of our parameters (Section 5). Finally, it shows that the quadratic relationship between growth and each of number of doctors and R&D is not robust to controlling for unobserved heterogeneity (Section 6). The quadratic relationship between finance and growth, instead, is robust to controlling for unobserved heterogeneity.

We conclude that the too much finance result is robust.

2 Spurious Negative Quadratic Influence

We will base our discussion on the formal proof provided in Appendix A of the policy brief. This section describes the proof. Section 3 shows that there is a flaw in the proof.[2]

Assume that, because of convergence, there is a negative linear relationship between growth (G) and initial income (Y):

$$G = \alpha - \beta Y, \tag{1}$$

(with $\alpha > 0$ and $\beta > 0$). Further assume that initial financial depth (F) is

[1] The list of papers that corroborate the finding that the marginal effect of financial depth becomes negative above a certain threshold includes Aizenman et al. (2015), Law and Singh (2014), and Pagano (2012).

[2] We follow Cline but rename the variables G (from y), Y (from x), and F (from z) to clarify how Cline's example relates to the finance and growth literature.

positively correlated with initial income:

$$F = \gamma + \delta Y, \tag{2}$$

(with $\delta > 0$). Suppose that we follow the too much finance literature and estimate the model:

$$G = \lambda + \eta Y + \pi F + \theta F^2. \tag{3}$$

Substituting (2) into (3) we get:

$$G = \lambda + \eta Y + \pi \left[\gamma + \delta Y\right] + \theta \left[\gamma + \delta Y\right]^2. \tag{4}$$

Now assume that η only captures part of the true effect of Y on G.[3] Specifically, let us set $\eta = -\sigma\beta$ (with $0 < \sigma \leq 1$, Cline chooses $\sigma = 0.5$). Then, the other coefficients need to capture the remaining effect of Y on G (i.e., $-(1-\sigma)$). Formally:

$$\frac{d\left\{\pi\left[\gamma + \delta Y\right]\right\}}{dY} + \frac{d\left\{\theta\left[\gamma + \delta Y\right]^2\right\}}{dY} = -(1-\sigma)\beta. \tag{5}$$

Taking the derivatives and solving for θ, we get:

$$\theta = \frac{(\sigma - 1)\beta - \pi\delta}{2\delta\left[\gamma + \delta Y\right]} \tag{6}$$

By assumption, $\beta > 0$ and $\delta > 0$. Cline correctly argues that it is reasonable to assume that γ and Y are also positive and concludes that regression (4) will yield a negative value for θ (because $\sigma \leq 1$; in Cline's example, $\sigma = 0.5$). This result, however, is spurious because F and F^2 do not belong in the equation for G.

3 Discussion

It is not enough to assume that β, δ, γ and Y are non-negative to prove that $\theta < 0$. It is also necessary to assume that π is non-negative.

But what is π? If we solve for π using the same procedure that we used to

[3] For the moment forget that Equation (4) (or Equation 3) cannot be estimated because F is a linear transformation of Y. We will discuss this issue in Section 4 below.

solve for θ we find that:

$$\pi = \frac{(\sigma - 1)\beta - \theta 2\delta \left[\gamma + \delta Y\right]}{\delta}. \tag{7}$$

Note that the denominator is positive (by assumption) and the first term of the numerator is negative by construction. Since $2\delta \left[\gamma + \delta Y\right]$ is positive, also by assumption, π can only be positive if θ is negative.

In other words, to prove that θ is negative, we need to assume that θ is negative! This is the flaw in Cline's proof.

In summary, Cline's "proof" shows that π and θ need to have opposite signs. As such, he implicitly **assumes** that $\pi > 0$ and hence $\theta < 0$. But of course this is no longer a proof but an assumption.

One may claim that given that π and θ need to have opposite signs, it is natural to assume that π is positive and θ negative. If not, one would get the counterintuitive results of a U-shaped relationship between finance and growth (instead of the inverted U-shaped relationship found in the too much finance literature).[4] However, this is exactly what we get when we estimate the Doctors and R&D regressions controlling for country-year fixed effects (see Table 2 below).

The Policy Brief has also implications for the overall literature on finance and growth (i.e. the literature that finds a positive effect of finance on growth, Levine, 2005). Assume that (1) and (2) hold and that we estimate the classic model in the finance and growth literature:

$$G = \omega + \kappa Y + \phi F. \tag{8}$$

Using the same reasoning as above, we can show that:

$$\phi = \frac{(\sigma - 1)\beta}{\delta} < 0. \tag{9}$$

Given that the literature finds positive values of ϕ, it is reasonable to conclude that the contemporaneous relationship between financial depth and initial income is more complex than what is suggested by Equation (2).[5]

[4] Personal conversation with William Cline.

[5] Actual regression models condition on a large number of covariates and (in a panel data setting) fixed effects that are correlated with both income and financial depth. See Section 6

Summing up, if one is willing to believe that Cline's first two assumptions hold, a linear model should also yield a negative estimate of π, which of course is not the case. The upshot is that either linear models are also wrong, or the assumptions are wrong.

There is a deeper point here. The too much finance literature did not just come up with an *ad hoc* model (like regressing growth on the number of doctors), but builds on an existing literature on finance and growth that stretches back to the 19th century. One may claim that there are problems with the finance and growth literature. However, these problems are relevant for the whole body of the literature on finance and growth and not only for its too much finance component.

In fact, there are theoretical arguments that are in line with the empirical finding that a large financial sector may hurt growth. Some of these theories predate the global financial crisis (i.e., Tobin 1984). In Arcand et al. we discuss some of these theories and look at the role of financial crises and income volatility. Cecchetti and Kharroubi (2015) test Tobin's hypothesis that a large financial sector may divert talents away from the productive sectors and find empirical support for this idea.

4 Non-linearities in initial income

There is another problem with the proof of Section 2. If Equation (2) is true, F is a linear (affine) transformation of Y. Therefore, $corr(F, Y) = 1$ and we cannot estimate Equation (4).

Things would be different if $F = \gamma + \delta Y + u$. But in this case, the standard irrelevant variables result would hold and Equation (4) would yield unbiased (albeit inefficient) OLS estimates. Specifically: $E(\widehat{\eta}) = \beta$ (implying $\sigma = 1$) and $E(\widehat{\pi}) = E(\widehat{\theta}) = 0$.

Therefore, the discussion of Section 2 only makes sense if $G = g(Y)$, where g is a non-linear function (and/or F is a non-linear function of Y).[6]

This is what Cline implies when he states that:

> A central proposition of this Policy Brief is that the supposed negative quadratic term on finance is picking up the influence of lower

for a discussion of the implications of including fixed effects in Cline's regressions.

[6] In this case, however, it may be hard to find closed form solutions for the bias.

growth at higher per capita income. In other words, the specification of the per capita income term itself is inadequate to capture convergence fully and leaves some false attribution of convergence to financial depth.

A way to address this problem is to allow for non-monotonicities in initial income. In Arcand et al. (2015) we do this by interacting initial income with financial depth (see Table 9 of Arcand et al.).

Alternatively, we could include the square of initial income in the regression. Table 1 estimates the same models as in the benchmark regressions of Arcand et al. (2015), but also controls for the square of the log of initial GDP. The too much finance result is robust to including this additional control.

Table 1: **Panel data regressions: 5-year periods.**

This table estimates the same models as in Table 6 of Arcand et al. (2015), but also controls for the square of (the log) of initial income. Column 3 excludes PC>165 percent, Column 4 excludes USA, IRL, ESP, ISL, and Column 5 excludes the top and bottom 1 percent.

	(1)	(2)	(3)	(4)	(5)
LGDP(t-1)	0.706	-0.0742	-1.012	-0.323	-0.431
	(2.369)	(1.829)	(2.190)	(1.707)	(1.817)
LGDP2(t-1)	-0.0946	-0.0412	0.0255	-0.0277	-0.0184
	(0.159)	(0.112)	(0.129)	(0.105)	(0.113)
PC(t-1)	8.325***	3.462**	5.366**	3.523*	3.796**
	(2.217)	(1.657)	(2.306)	(2.098)	(1.727)
PC2(t-1)	-2.624*	-2.110***	-3.906***	-2.090	-2.229***
	(1.416)	(0.774)	(1.504)	(1.347)	(0.759)
LEDU(t-1)	1.121	2.331***	2.092***	2.474***	2.134***
	(0.739)	(0.640)	(0.679)	(0.687)	(0.622)
LGC(t-1)	-2.814***	-1.654***	-1.617**	-1.414**	-1.323**
	(0.744)	(0.633)	(0.709)	(0.683)	(0.629)
LOPEN(t-1)	1.586***	1.184**	1.578***	1.222**	1.533***
	(0.524)	(0.534)	(0.481)	(0.500)	(0.521)
LINF(t-1	-0.0474	-0.284	-0.122	-0.180	-0.243
	(0.202)	(0.190)	(0.196)	(0.180)	(0.167)
Observations	549	917	859	879	912
N. of countries	107	133	127	129	133
Sample	1960-95	1960-2010	1960-2010	1960-2010	1960-2010

In Arcand et al. (2011, 2015), we also estimate the model using industry-level data and the difference-in-differences approach of Rajan and Zingales. In one of the robustness checks, we control for GDP per capita and GDP per capita squared (interacted with external financial dependence; column 4 of Table 13 in Arcand et al., 2015) and show that the results are robust to including these controls in the regressions.

5 Implausibly large parameters

Cline suggests that some papers (Arcand et al., 2015, and Sahay et al., 2015) yield implausibly large estimates of the negative effect of finance on growth. For instance, he shows that the point estimates of column 4 of Table 6 of Arcand et al. (2015) imply that "Japan could achieve annual growth 1.6 percentage points higher if it would only reduce its ratio of private credit to GDP from 178 to 90 percent."

However, in Arcand et al. (2015), we show that there is substantial heterogeneity in the finance and growth relationship. This heterogeneity depends, among other things, on the presence of banking crises, income volatility, bank regulation, and the quality of institutions. Specifically, we show that during banking crises there is no statistically significant relationship (either positive or negative) between finance and growth (Table 11 and Figure 9).

Going back to the specific case of Japan, the large marginal effect implied by the coefficients of Table 6 in Arcand et al. (2015) could be explained by the fact that the regressions of Table 6 do not control for the heterogeneity brought about by the presence of a banking crisis.

If we apply the point estimates of Table 11 (column 4) to the case of Japan (which did have a banking crisis exactly in the period during which credit to the private sector peaked at 178 percent of GDP), we find that the growth differential brought about by reducing Japan's ratio of private credit to GDP from 178 to 90 percent is 0.95 percentage points. Still high, but much lower than what is implied by the point estimates of Table 6 that do not allow for heterogeneity.

Moreover, regressions capture average effects and are not meant to, and do not, fit all points (the regression's R^2 is never one). If this were the case, the returns to education estimated in thousands of papers in the labor economics literature, and which constitute one of the most robust empirical findings in the whole of economics, would be invalidated for the same reason which, to put it kindly, is neither here nor there. Therefore, it is singularly inappropriate to pick a specific data point to purportedly invalidate a result.

6 Doctors with fixed effects

In the growth and finance literature that uses panel data, it is standard to control for unobserved heterogeneity by including country and year fixed effects.

However, Cline estimates simple models that do not control for unobserved heterogeneity. Column 1 of Table 2 reproduces the Policy Brief's regression for the number of physicians. Column 2 shows, when we control for country and year fixed effects, that the linear and the quadratic effects of the number of physicians are no longer statistically significant and, if anything, they imply a U-shaped (instead than inverted U) relationship.

Columns 3 and 4 repeat the experiment for R&D. In this case the quadratic effect also disappears once we control for country and year fixed effects (and again, the relationship appears to be U-shaped, albeit not statistically significant).

Finally, columns 5 and 6 use the data for fixed telephone lines. In this case Cline's result survives controlling for country and year fixed effects. We do not know why this is the case (maybe because certain countries are leapfrogging the fixed line technology straight to cellular), but the general message of Table 2 is that, once one controls for country and year fixed effects, we do not always find a quadratic relationship between growth and any variable that might be correlated with initial income.

The moral of the story is that one ignores unobserved correlated unobservables that can be easily controlled for at one's own peril.

Table 2: **Panel data regressions: 5-year periods.**

This table estimates the same models estimated by Cline (columns 1, 3, and 5) but then also includes country and year fixed effects to control for unobserved heterogeneity.

	(1)	(2)	(3)	(4)	(5)	(6)
loggdp	-0.640***	-3.268***	-2.039***	-5.566*	-2.279***	-4.179***
	(0.229)	(0.904)	(0.415)	(2.858)	(0.328)	(1.124)
phys	0.960*	-1.743				
	(0.565)	(1.602)				
phys2	-0.227**	0.0471				
	(0.113)	(0.229)				
rd			0.164***	-0.235**		
			(0.0420)	(0.0934)		
rd2			-0.00202***	0.000596		
			(0.000521)	(0.000766)		
tel					0.208***	0.229***
					(0.0383)	(0.0607)
tel2					-0.00196***	-0.00220***
					(0.000463)	(0.000558)
Constant	7.374***	8.993***	20.07***	18.67***	19.80***	22.33***
	(1.713)	(2.692)	(3.532)	(5.044)	(2.528)	(5.750)
N Obs	290	290	132	132	290	290
N. of countries	50	50	44	44	50	50
Country FE	NO	YES	NO	YES	NO	YES
Year FE	NO	YES	NO	YES	NO	YES

7 Summing up

William Cline has written an interesting policy brief challenging the "Too Much Finance" result.

While more work certainly needs to be done to understand the links between finance and growth and assess the drivers of possible non-monotonicities, this note shows that the too much finance result easily survives Cline's challenges.

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