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Further Statistical Debate on "Too Much Finance"

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Abstract

This paper evaluates recent findings by researchers at the Organization for Economic Cooperation and Development (OECD) on "too much finance." It first critiques the OECD findings, which seem to imply that the optimal amount of finance is zero, given the linear specification of the main tests. It then finds that the negative impact of additional finance on growth is reversed when the appropriate (purchasing-power-parity) per capita income is applied and country fixed effects are removed. Separate tests for countries with intermediated finance below and above 60 percent of GDP show a significant positive effect of finance on growth in the lower group but an insignificant effect in the higher group. An appendix replies to critics of my earlier study (Cline 2015b) in which I argued that an estimated negative quadratic effect of finance on growth was likely to be a spurious correlation reflecting convergence-based lower growth at higher per capita incomes. It notes that the critics' own logarithmic tests, yielding a positive marginal impact of finance on growth even at high levels, achieve comparable explanation to their quadratic form yielding a negative marginal impact. It finds that adding dummy variables for below and above intermediate financial depth to the logarithmic form does not support the inverse U influence found in the quadratic form.

JEL codes: G10, O16, O43, O47, O57

Keywords: financial depth, too much finance, cross-country growth, private credit, convergence

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BACKGROUND

In a recent PIIE *Policy Brief*, *Too Much Finance or Statistical Illusion?* (Cline 2015b), I question recent statistical findings that financial depth has reached an excessive level in advanced economies and has contributed to a slowdown in growth as a consequence. Some studies have found that whereas a rising ratio of private credit to GDP initially spurs growth, once this ratio exceeds about 100 percent additional finance begins to reduce the growth rate. The key innovation in these studies is to add a quadratic term to the finance variable, and the key finding is that the quadratic term is negative so that the growth contribution of finance rises but then falls again after a peak (Arcand, Berkes, and Panizza 2012; Cecchetti and Kharroubi 2012; Sahay et al. 2015).[1] My critique of these studies is that there is an inherent mathematical bias toward finding a negative quadratic term on finance if there is a positive linear term and at the same time there is a positive trend relationship between financial depth and per capita income. I demonstrate this bias by running statistical tests that also find a significant negative quadratic term if growth is "explained" by doctors per capita, or R&D researchers per capita, or the number of telephones per capita. We should have no more confidence in the finding that too much finance spoils growth than in the finding that too many doctors, R&D researchers, or telephones spoil growth. Arcand, Berkes, and Panizza (2015b) have issued a comment on my analysis. Appendix A sets forth my reply to their comment.

Now a new study from another international financial organization has arrived at statistical findings with even more astonishing implications: For OECD countries, additional financial depth uniformly reduces growth.[2] In the main results of two researchers at the OECD (Cournède and Denk 2015), cross-country growth equations show a strictly negative coefficient on a linear variable for financial depth (ratio of private credit to GDP) without any quadratic term. If these results were taken literally, there would be a radical policy implication: *Growth would be maximized by completely eliminating credit finance*. The optimal amount of credit would be zero. The authors do conduct supplementary tests that suggest the influence of finance on growth is positive at initially low levels of finance, and this enables them to state in their abstract: "…finance has been a key ingredient of long-term economic growth in OECD and G20 countries over the past half-century…." But they then assert that "at current levels of household and business credit further expansion slows rather than boosts growth" (p. 3). However, they seek to support this conclusion on the basis of the strictly linear negative coefficient that is estimated for the full sample, including low-financial-depth observations, and that test inescapably implies that the optimal level of finance is zero. The authors cannot reject the fully linear results when it comes to the implication of optimal zero finance but at the same time use them as the basis for asserting that at "current levels" the

1. For my oral comments on Sahay et al. (2015), see the materials at www.piie.com/events/event_detail.cfm?EventID=396.

2. The three earlier studies just cited are by researchers at the IMF, Bank for International Settlements, and IMF, respectively.

impact of additional finance on growth is strictly negative. The authors' (proper) insistence on a positive growth influence of finance over an initial range seriously undermines the usefulness of their main estimates.

More fundamentally, even if attention is restricted to a range of private credit above say 60 percent of GDP, there is a major problem regarding causality. Higher per capita income is likely to drive relatively more demand for credit as, in effect, a luxury good. If so, when combined with the long-recognized "convergence" pattern of lower growth at higher per capita incomes, the effect will be that higher credit is observed to accompany lower growth but without causality. Reduction of credit would thus not boost growth because high credit is not causing low growth; instead, the maturing of the economy is slowing growth.

It turns out, moreover, that the study's main statistical finding does not hold up to certain key changes in specification. This working paper uses the same dataset as Cournède and Denk (2015), kindly provided by the authors, to examine this question.[3] The central findings here are that the results of that study are unreliable because first, the tests exclude the most important variable, real per capita income at purchasing power parity (ppp) comparable across countries; second, the tests apply country fixed effects and thereby throw out important information on cross-country variation; and third, incorporation of shift and slope dummy variables for lower financial depth removes the significance of the negative influence of higher financial levels on growth while tending to confirm the expected positive influence at low levels.

THE COURNÈDE-DENK RESULTS

Cournède and Denk use data for 33 OECD countries for the period 1961–2011 to estimate cross-country growth regressions incorporating a linear term on finance. I will focus on their results for "intermediated credit," credit to the private sector from either banks or other financial institutions.[4] Table 1 reports their coefficient estimates for three specifications of regressions for per capita GDP growth. In the simplest, only the credit variable is included. It has a significant negative sign.[5] In the second, once again annual data are applied, but other variables are added: the investment rate, average years of schooling, population growth rate, and a dummy variable for banking crisis. Again the credit variable has a signif-

3. The Cournède-Denk results presented in table 1 as well as several others of their main results were successfully replicated from this database. These replications are available on request.

4. Namely, lines 22d and 42d in the IMF's *International Financial Statistics*. The authors alternatively use the share of the financial sector in value added, which they also find has a negative effect on growth. In contrast, they find that a third financial variable, stock market capitalization as a percent of GDP, has a positive impact on growth.

5. Table 1 reports t-statistics in parentheses. For these sample sizes, the critical thresholds for significance at the 1 percent, 5 percent, and 10 percent levels are respectively: t = 2.6, 2.0, and 1.65.

icant negative coefficient. In the third variant, data are grouped into five-year averages. Once again the credit variable has a significantly negative impact.[6]

The size of the negative impact of credit is extremely large. In the second and third columns, this coefficient is –0.019. This magnitude means that if credit to the private sector were reduced from 150 to 100 percent of GDP, the annual growth rate per capita would increase by 0.95 percent. If credit for such a country were eliminated altogether, then according to this linear equation the annual growth rate would rise by nearly 3 percentage points. Both the linearity (and hence optimal level of zero for credit) and the magnitude of the impact are implausible.

Nor are these results consistent with saying that although by now the OECD has gone too far in finance, at earlier periods of lower financial depth more finance meant more growth, even though the authors seek to argue that this is the case. The absence of a quadratic term and the presence of a negative coefficient on the linear term for finance mean that additional finance reduces growth across the entire period and all OECD countries. Yet several countries had surprisingly low financial depth at the beginning of this period.

Thus, on average, Australia, Belgium, Greece, Israel, and New Zealand had private credit of only 14.5 percent of GDP in 1961–65. By 2001–05 the average for these countries had reached 83 percent of GDP. Applying the coefficient of –0.019 in the second column of table 1, the main results reported by Cournède-Denk would imply that rising financial depth in these five countries damaged their growth performance by an average of 1.3 percent per year.[7] By this diagnosis, the five countries' GDP levels would have been 112 percent higher than their actual levels in 2001–05 if they had kept financial depth frozen at the low levels of 1961–65.[8] Such a diagnosis, however, is not credible. The basic problem is that the authors reported as their main findings results that differ from their true judgement. Instead, they should have conducted and reported tests that distinguish between economies at low versus high levels of financial depth.[9]

Even if one were to distinguish between countries starting at higher levels of finance and those starting at lower levels, the estimated magnitudes of growth impact are implausible. Consider Japan and the United States, where private credit rose from a range of 70 to 80 percent of GDP in 1961–65 to 180 to 200 percent in 2001–05. Already in 1961–65 they would have been unnecessarily sacrificing about 1.4 percent in annual growth because of excess finance, according to the equation in the third column of

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6. Note that in the third variant, both schooling and population growth have the wrong signs, raising questions about the reliability of that test.

7. That is: $-0.019 \times (83 - 14.5) = -1.3$.

8. That is: $1.013^{40} = 2.12$.

9. As discussed in note 26 below, the graphical results they show on this issue do not fulfill this purpose.

table 1; by 2001–05, they would have been sacrificing 3.6 percent annual growth.[10] But if one adjusts this estimate because the negative effect really only would begin at finance of 60 percent of GDP (a threshold identified by the authors as discussed below), the resulting shift would still mean that by 2001–05 the two countries would have been sacrificing about 2.5 percent annual growth as a consequence of too much finance.[11] Such a loss would have meant that in the absence of financial deepening, their labor productivity growth by 2001–05 could have been an implausibly high 4.1 to 4.5 percent per year instead of the actual pace of 1.6 to 2.0 percent (OECD 2015).

Another central problem with the estimates is that they do not apply the most important variable typically included in cross-country growth analysis: the logarithm of real ppp per capita income. Some tests include per capita income but it is instead for the specific country and not comparable across countries.[12] Testing cross-country growth patterns without permitting a comparable cross-country level of real per capita income is a classic instance of staging *Hamlet* without the Prince of Denmark.

The authors explicitly include "country fixed effects," equivalent to a dummy variable for each country, and thereby throw out potentially important cross-country information. In the tests conducted here, it turns out that this important decision is responsible for turning the influence of finance from positive to negative. Yet it is arguably inappropriate, and at best the case for including country fixed effects is ambiguous.

REESTIMATING THE IMPACT OF PRIVATE CREDIT

In reexamining the Cournède-Denk results, a crucial first question is thus whether to include country fixed effects. Classic early studies of cross-country growth typically did not include country fixed effects. Instead, they sought to obtain more variation by allowing independent variables to vary both across and within countries. These studies include King and Levine (1993) on finance; Sachs and Warner (1999) on natural resources; and Mankiw, Romer, and Weil (1992) on human capital. Although some later studies have included country fixed effects, Barro (2012) has cast doubt on doing so. He observes:

> "Inclusion of country fixed effects…affects the estimated coefficients of explanatory variables… variables that have little within-country time variation cannot be estimated with precision. In effect, the inclusion of country fixed effects throws out much of the information in isolating the effects of X variables on growth rates."[13] (Barro 2012, 5)

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10. That is: –0.019 x 75 = –1.4; –0.019 x 190 = –3.6.

11. Since –0.019 x 60 = –1.1, the previous calculation of growth impact (–3.6 percent) would be shifted upward over the range by +1.1 percentage point, thus yielding –2.5 percent.

12. Thus even the scale of the variable differs sharply with the currency across countries.

13. In particular, Barro maintains that the failure of Acemoglu et al. (2008) to find statistically significant effects on democracy from per capita GDP and education stems from their inclusion of country fixed effects.

The most important driver of growth in cross-country tests has traditionally been the convergence factor captured by the logarithm of ppp income per capita. This variable becomes irrelevant when country fixed effects are applied, and Cournède and Denk instead apply the (lagged) logarithm of each country's own national real per capita income. Important variation is sacrificed as a consequence.[14]

Whether to use country fixed effects is related to whether growth convergence is "absolute" or "conditional." In absolute convergence, poorer countries would tend to grow faster than richer countries, and by implication all countries would eventually tend to converge to the same real per capita income. The early cross-country growth literature applying ppp income estimates when they became available instead confronted the paradox that for 1960–90, poor countries were not growing more rapidly than rich countries, and indeed were growing more slowly (Barro 1996, Sala-i-Martin 1996). This finding led to a focus on "conditional convergence," in which each country could be converging to its own individual long-term per capita income, which could differ from those of other countries. Such variables as saving rates, human capital formation, trade openness, legal institutions, and so forth were seen as influencing the long-term growth potential, and when such variables were included, the coefficient of per capita growth on the per capita income (lagged, logarithm) tended to revert to the expected negative sign rather than showing a positive sign. In this context it was a natural step to go further and apply a country fixed effect to capture still other unobservable (or "omitted variable") influences not captured in these and similar variables (Islam 1995).

It turns out, however, that beginning in the 1990s growth per capita in emerging-market and developing countries rose increasingly above that in advanced countries, such that the evidence shifted toward absolute rather than conditional convergence. Thus, per capita growth was only 1.9 percent in developing countries versus 2.4 percent in advanced countries in 1981–90 (which included Latin America's "lost decade" from the debt crisis). In successive decades, however, this comparison swung to 2.2 versus 2.0 percent, respectively, in 1989–98; 4.1 versus 2.1 percent in 1997–2006; and, in the Great Recession and its aftermath, 4.1 versus 0.27 percent in 2008–14 (IMF 1999, 169; 2015, 170). With less need to seek conditional rather than absolute convergence, the case for sacrificing variation in order to capture unobserved country-specific influences has presumably declined.

In the specific case of the Cournède-Denk estimates for the OECD, moreover, the countries included in the sample are much more homogeneous than in most cross-country tests, providing an important additional reason for excluding country fixed effects. An additional consideration is that in cross-country growth equations, country fixed effects are found in Monte Carlo experiments to exaggerate the speed of

14. Thus, for example, for the United States, average real per capita GDP in 2001–05 was 2.6 times as high as in 1961–65, a factor of 2.6. In comparison, at the period mid-point (1981–85), ppp per capita income in the United States was 4.9 times the level in Korea.

conditional convergence and thereby reduce the magnitudes and statistical significance of coefficients of explanatory variables (Hauk and Wacziarg 2009, 105).

In at least one regard, a plausible case can be made for including rather than excluding country fixed effects: The finance variable does move substantially, providing some potential basis for obtaining discrimination using within-country only variance. Nonetheless, because the key influence of cross-country difference in real per capita income is thrown out when country fixed effects are used, and because this influence is surely the most fundamental in the cross-country growth literature, this reason alone is sufficient to prefer tests omitting country fixed effects over tests including them. The first major decision in specification of the new tests conducted here, then, is to exclude country fixed effects.

Another decision concerns the time period. The tests here end in 2007 growth, to avoid distortions from the Great Recession. Another key question is whether to use annual data or period averages. The main estimates here use five-year averages (the same approach adopted in both Cecchetti and Kharroubi 2012 and Arcand, Berkes, and Panizza 2012). The use of annual data instead will tend to introduce a bias toward a negative influence of finance on growth from cyclical patterns. Namely, in a recession, the magnitude of debt in the finance variable numerator will tend to rise from accumulation of unpaid balances, whereas the GDP denominator will tend to decline because of lower output.

Another question is whether to include investment as an explanatory variable. Because investment directly drives growth, presumably the most interesting question is whether greater financial depth benefits growth indirectly through facilitating higher investment. The tests here omit investment because otherwise there will be a tendency to understate the influence of finance working through facilitation of investment.

Table 2 reports the results of applying tests of per capita GDP growth on the same variable for finance as used by Cournède and Denk: the level of credit to the private sector as a percent of GDP, as well as the same variable they use for human capital (years of schooling).[15] The logarithm of ppp per capita income is taken from the Penn World Table (Feenstra, Inklaar, and Timmer 2015).[16]

The two preferred tests in table 2 are shown in columns A and B. In these tests, the independent variables are five-year (nonoverlapping) averages beginning in 1961 and the dependent variable (per capita growth) is the corresponding average for the period two years later (e.g., growth in 2003–07 regressed on 2001–05 independent variables). In column A, the finance variable turns out to have a positive coefficient, albeit not a significant one. As expected, the coefficient for the logarithm of lagged ppp per

15. These two variables are drawn from the dataset provided by Cournède and Denk.

16. In four variants corresponding to table 2, when population growth is included it is significant only in the annual test, and its inclusion makes almost no difference in the coefficient estimates for logarithm of ppp per capita income, finance, and schooling.

capita income is negative and highly significant.[17] The number of school years also has the correct sign and is highly significant.[18]

For purposes of the "too much finance" debate, the key finding is that the coefficient on the financial depth variable is positive, rather than negative. Column B reports the same test but with this variable stated as the natural logarithm of private credit as a percent of GDP. This time the t-statistic on the variable is considerably higher (but still below the level needed for even a 10 percent level of significance). The estimated coefficient is again positive. The size of the coefficient indicates that as credit to the private sector doubles from 50 to 100 percent, the per capita growth rate would be expected to increase by 0.19 percentage point.[19] It would take another doubling, to 200 percent, to boost the growth rate by another 0.19 percentage point, showing diminishing returns to additional finance, but not negative returns. The higher t-statistic and (albeit only slightly) R^2 than in the linear case (column A) support this common-sense finding of diminishing returns.

In contrast, columns C and D represent "misleading" results, either because they include country fixed effects or because they use annual rather than longer-period average data. In column C, inclusion of country fixed effects turns the coefficient of the finance variable negative using the five-year average data.[20] In column D, the use of annual rather than five-year data also turns the coefficient on financial depth negative, even without country fixed effects.

It is useful to apply tests that are more in keeping with what the authors truly think: tests with specifications that allow the influence of finance on growth to diverge between countries at lower levels of finance and those at higher levels, rather than the one-size-fits-all negative linear coefficient. For this purpose, the most reasonable dividing point is a ratio of private credit to GDP of 60 percent or less. This is the threshold at which their supplementary tests show inclusion of observations with greater financial depth begins to turn the coefficient on finance from positive to negative (Cournède and Denk 2015, 29).[21] A first approach for this purpose is simply to estimate the regression equation for two separate

17. Thus, other things being equal, at the per capita income level of the United States in 2001–05 (average natural logarithm = 10.642), per capita growth would be expected to be 2.7 percentage points lower than at the per capita income level of Mexico (average natural logarithm = 9.331). That is: –2.077 x (10.642 – 9.331) = –2.7.

18. Cross-country studies of returns to education have tended to find that the percent increase in wages holds relatively constant at, for example, about 10 percent, for each year of schooling added, and is actually higher at the tertiary level than at the secondary level (Montenegro and Patrinos 2013). The absolute average number of years of schooling of the adult population thus turns out to be a more appropriate specification than, for example, the percent change in number of years (which could be disproportionately larger for countries and periods with low initial average schooling levels).

19. That is: the natural logarithm of 2 is 0.6932. Multiplying by 0.277 yields 0.19 increase in the dependent variable for a doubling of the underlying finance variable (and hence an increase in its logarithm by the logarithm of 2).

20. The higher R^2 results mechanically from inclusion of the country dummies.

21. In view of their evidence of a positive relationship at lower financial depth, it is surprising that "no evidence is found of a quadratic relationship with GDP growth…." (Cournède and Denk 2015, 25).

samples: one including only country-periods with private credit less than 60 percent of GDP, and the other including all others. An alternative means of conducting this test is to include all observations, but to add a dummy variable for those cases with the finance variable less than 60 percent of GDP and allow it to interact with the finance variable.[22]

As shown in table 3, when these tests are conducted, and using the preferred model specification of column B in table 2, the results are much closer to what one would expect. The influence of additional finance is positive and statistically significant in the below-60 group (column A). The influence turns negative but is statistically insignificant in the above-60 group (column B). In the combined test, the influence of the logarithm of finance is again negative but statistically insignificant. However, the interaction term shows that this coefficient turns sizable and positive if the country-period has private credit of below 60 percent of GDP, although the coefficient is not significant at the 10 percent level.[23]

When the exact variables applied by Cournède and Denk are applied to the above 60 percent subsample, using five-year averages, the negative linear coefficient of growth on private credit as a percent of GDP is confirmed and is highly significant. The log of lagged (national) GDP per capita has the right sign and is significant. But investment is not significant; schooling has the wrong sign and is significant; and population growth has the wrong sign. For the below-60 observations, however, private credit still has a negative sign, although the coefficient is small and insignificant. Schooling, population growth, and lagged national GDP per capita all have the wrong signs. Only investment has the right sign.[24]

Two broad patterns can be seen in these tests. First, the results for the below-60 group are extremely weak, again strongly suggesting that the use of country fixed effects and the use of lagged national rather than ppp GDP per capita income robs the explanatory role of just about all influences (including credit) except investment. Second, the poor results on the other variables raise the question of why one should trust the significant negative effect on credit in the above-60 group.

In summary, the main results reported in Cournède and Denk (2015) do not provide a solid basis for concluding that "at current levels" OECD finance is so excessive that it depresses growth, because as the authors recognize it does not apply to lower levels of finance and therefore its test statistics are not

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22. That is: with $D_L = 1$ if private credit is 60 percent of GDP or less but 0 otherwise, and denoting the logarithm of private credit as a percent of GDP as *ln(pc)*, the regression equation of column B in table 2 adds D_L and D_L x *ln(pc)* to the set of explanatory variables.

23. The size of the coefficient becomes the sum of the first row entry in column C and the 5th row entry (−0.162 + 0.964 = 0.802).

24. If the same exercise is repeated but removing country fixed effects, the above-60 group again shows a significant negative coefficient on private credit, and investment has the right sign and is significant. But schooling, population growth, and the logarithm of lagged per capita GDP all have the wrong signs. Because the estimate for the most important variable, logarithm of lagged per capita GDP, is meaningless—likely reflecting the use of noncomparable national per capita GDP levels rather than ppp—the result for private credit warrants little credibility. These results and those just discussed (incorporating country fixed effects) are available on request.

reliable. Separate tests that distinguish between low and high levels of finance find a significant *positive* effect at low levels but do not find a statistically significant effect at high levels. Moreover, even if the full set of data is examined without distinguishing between low and high finance observations, the sign of the finance variable switches back to positive rather than being negative if an arguably more appropriate specification is applied.[25] This specification involves: (a) incorporating the traditional workhorse variable for cross-country growth regressions: logarithm of lagged ppp per capita income; (b) excluding rather than including country fixed effects; and (c) applying five-year averages rather than annual data. Their finding that more finance depresses growth is thus not robust to these three relatively basic alternatives (or, I would say, improvements).

Finally, it should be emphasized that even if a negative linear effect were robust to estimation just for the above 60 percent of GDP private credit group, applying the logarithm of lagged ppp per capita income and omitting country fixed effects, there would still be the problem of likely reverse causation. The authors do attempt to address causality by constructing an instrumental variable for private credit, based on changes in national financial regulatory requirements. However, not only do they again apply the analysis to the full sample rather than just observations above 60 percent of GDP in private credit, but in addition they do not include lagged per capita income in their growth equations at all (neither ppp nor national), a strange test considering the primacy of this convergence variable in the cross-country growth literature.

POLICY IMPLICATIONS

Literally accepting the main results reported by Cournède-Denk would have the radical policy implication that the ideal amount of financial intermediation is no financial intermediation at all. The authors themselves instead argue that at initially lower levels of finance, additional finance benefits growth, but they do not then conduct appropriate tests that verify a shift to statistically significant negative effects of finance at higher levels.[26]

In contrast to the main results in linear specification of Cournède and Denk (2015), the quadratic specifications in Arcand, Berkes, and Panizza (2012) and Cecchetti and Kharroubi (2012) indicate a large initial range over which more finance brings higher rather than lower growth. Even those formulations

25. Because the positive coefficient is not statistically significant, however, the respecification demonstrates only that the relationship of growth to finance is not significantly negative rather than that it is positive.

26. Their tests reported in graphical form for successively higher thresholds of credit include all lower-credit observations at each point, and at the upper extreme return to their main reported full-sample results. As a consequence that series of tests does not constitute evidence regarding a statistically significant negative influence of additional finance within a subset of observations over a 60 percent credit threshold versus that below this threshold.

yield what I suggest are unreliable turning points that advanced economies have supposedly already exceeded (Cline 2015b and appendix A below).

That three empirical studies by teams at three international organizations nonetheless come out with the same implication that there is already too much finance in such economies as that of the United States is potentially a powerful message for policy. In particular, in the Basel Committee reforms boosting required capital for banks, a key consideration has been whether too much required capital might discourage lending and curb growth because of a resulting decline in the rate of investment. But if the Too Much Finance results were taken literally, that outcome could be a good thing rather than a bad thing, because reducing the amount of finance relative to GDP would (at least over the highest range) boost growth rather than reduce it. A central purpose of this study as well as the analysis in Cline (2015b) is to show that these studies do not have sufficiently robust findings on negative effects of finance to warrant a general policy stance welcoming rather than seeking to avoid shrinkage of finance as a consequence of higher capital regulatory requirements.

Some might argue nonetheless that the absence of a statistically significant *positive* effect of finance on growth in the higher ranges of financial depth means that the benefits of lesser risk of financial crisis as a consequence of even sharply higher capital requirements for financial institutions could be obtained at no cost to the economy. But cross-country growth regressions, especially with insignificant coefficients, are not a reliable basis for evaluating optimal capital requirements. Instead, a more reliable basis for investigating this issue is to use a calibrated model comparing the costs associated with higher capital requirements against the benefits they provide through reduced risk of financial crises.[27]

27. Economic costs from higher cost of capital and lesser capital formation associated with a shift from debt to equity will be present if the Modigliani-Miller offset (reduction in unit cost of equity capital as leverage and risk decline) is incomplete (as found in Cline 2015a). Leading examples of the calibrated cost-benefit approach include Miles, Yang, and Marcheggiano (2012) and BCBS (2010).

Table 1 Cournède-Denk results: Explaining GDP growth per capita[a]

Variable	Annual	Annual	Five-year averages
Private credit (percent of GDP)	–0.031 (–6.2)	–0.019 (–2.1)	–0.019 (–3.2)
Investment rate		0.254 (5.0)	0.131 (2.8)
School years		0.222 (0.5)	–0.448 (–2.0)
Population growth		–0.710 (–1.9)	0.025 (0.09)
Ln (lagged national GDP per capita)			–2.029 (–1.7)
Bank crisis dummy		–1.183 (–2.5)	
Year fixed effects	No	Yes	Yes
Country trend	No	Yes	No
R-squared	0.183	0.520	0.652
Period	1961–2011	1970–2011	1961–2010
Observations	1,303	1,115	238

a. Ordinary least squares estimates for 33 OECD countries (excluding Chile due to incomplete data). T-statistics in parentheses. All tests have country fixed effects.

Source: Cournède and Denk (2015, 16 and 21).

Table 2 This study: Explaining GDP growth per capita[a]

Variable	A	B	C	D
Private credit (percent of GDP)	0.00119 (0.35)		–0.0115 (–1.6)	–0.00313 (–1.3)
Ln (private credit percent of GDP)		0.277 (1.3)		
Ln (ppp per capita income)	–2.077 (–5.9)	–2.267 (–6.4)	–3.175 (–3.3)	–1.282 (–5.2)
School years	0.191 (3.0)	0.193 (3.0)	0.019 (0.05)	0.126 (2.8)
Time-period fixed effects	Yes	Yes	Yes	Yes[c]
Country fixed effects	No	No	Yes	No
R-squared	0.351	0.356	0.556	0.248
Period	1963–2007[b]	1963–2007[b]	1963–2007[b]	1961–2005
Span	5–year average	5–year average	5–year average	Annual
Observations	222	222	222	1,186

a. Ordinary least squares estimates for 33 OECD countries. Simple t-statistics in parentheses.
b. Dependent variable. Independent variable lagged two years.
c. Year fixed effects.

Source: Author's calculations.

Table 3 Growth results for low versus high financial depth[a]

Variable	Low (A)	High (B)	All (C)
Ln (private credit percent of GDP)	0.967 (2.4)	–0.446 (–1.0)	–0.162 (–0.3)
Ln (ppp per capita income)	–2.719 (–5.1)	–0.557 (–1.1)	–2.175 (–6.1)
School years	0.232 (2.4)	0.015 (0.2)	0.180 (2.8)
Dummy low			–3.598 (–1.4)
Dummy low x ln (pc)[b]			0.964 (1.5)
Time period fixed effects	Yes	Yes	Yes
Country fixed effects	No	No	No
R-squared	0.443	0.293	0.368
Observations	106	116	222

a. Low: credit to private sector 60 percent of GDP or less. High: all others. Periods and lags are as in column B, table 2. Simple t-statistics in parentheses.
b. ln (pc): logarithm of credit to private sector as percent of GDP.

Source: Author's calculations.

APPENDIX A REPLY TO ARCAND, BERKES, AND PANIZZA

In Cline (2015b), I argued that statistical findings of a negative quadratic influence of finance on growth were questionable. I showed that if causation went the other way, from rising per capita income to rising financial depth, for example, because home mortgages are a luxury good, a quadratic term relating growth to finance would have a spurious negative coefficient. Arcand, Berkes, and Panizza (2015b), authors of one of the key papers in this literature, have responded in an indepth (10-page) comment.

Before turning to the critiques in their comment, it is important to emphasize that even in their own study, the three authors (referred to as ABP hereafter) found virtually equal statistical performance of a formulation in which the influence of finance was logarithmic rather than quadratic. As a consequence, their own results suggest that there is little basis for stating that the influence of extra credit turns negative beyond some point rather than that this influence tapers off but remains positive. Thus, their table 1 shows in the quadratic formulation that a country with credit to the private sector at 150 percent of GDP could *increase* its growth rate by 1.57 percentage points by reducing credit back to an optimal 83 percent, but in the logarithmic formulation that reduction in credit would *reduce* the growth rate by 0.44 percentage point.[28] The R^2 in the quadratic formulation is 0.458, almost indistinguishable from the R^2 of 0.435 in the logarithmic formulation. The finance variable is significant at the 5 percent level in both formulations.

The authors do apply a statistical test to verify that their quadratic function relating growth to finance has a positive linear and negative quadratic term, yielding an inverse-U shape.[29] However, they do not apply a similar test for whether an inverse-U is significantly superior to the monotonic logarithmic form. When I apply such a test to the OECD data by adding low- and high-side dummy variables to the equation using the logarithm of private credit relative to GDP, the results do not provide much if any support for the inverse-U effect.[30]

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28. The quadratic formulation has coefficients of 5.815 on PC and –3.503 on PC^2, where PC is the ratio of private credit to GDP. The logarithmic formulation has a coefficient of 0.743 on the logarithm of PC. The tests are on average per capita growth in 1970–2000 across 66 countries.

29. The test is that proposed by Sasabuchi (1980) and Lind and Mehlum (2010). It involves determining, given the covariance-covariance matrix of βx_i and $2\gamma x_i$ (respectively the linear and quadratic contributions to the derivative of growth with respect to finance, where β is the linear coefficient and γ the quadratic), whether the derivative is significantly different from zero and positive at a low level of finance but negative at a high level within the sample.

30. I set the low and high dummies at D_L =1 when private credit is less than 65 percent of GDP and D_H = 1 when it is more than 124 percent, the significance thresholds identified by Arcand, Berkes, and Panizza (2015a). Inclusion of these dummies has practically no effect on the estimates of the coefficients on either the logarithm of per capita ppp GDP or schooling, although it raises the coefficient on the logarithm of finance and turns it significant. However, the coefficient on the low-end dummy turns out to be positive and thus has the wrong sign for a strong inverse-U form, and although the high-end dummy is negative, it is not significant at even the 10 percent level.

If policies were aggressively pursued based on the inverse-U influence of finance on growth, punitive taxes could be required to shrink credit by more than 60 percent in such economies as the United States and the United Kingdom (where the 2010 levels reached 195 and 202 percent of GDP, respectively).[31] But a difference in explanatory power that does not show up until the second decimal place in the R^2 is too slim to warrant such policies.

Regarding the critiques of my policy brief in their recent comment, ABP (2015b) first noted that my equations demonstrated not that the quadratic term on finance was necessarily negative and spuriously so, but rather that *if* the linear term was positive, *then* the quadratic term had to be negative. But the signs could also be the reverse. That observation is true, but it is also trivial and does not constitute a meaningful critique. Inspection of my relevant equation makes it clear that the sign of the linear term does indeed have to be the opposite of the sign of the quadratic term (subject to an additional specific threshold if the linear term is negative). Namely, I showed that if half of the observed reduction in growth as per capita income rises is spuriously attributed to a quadratic influence of finance (rather than underlying convergence), then:

$$A1)\,\theta = \frac{-0.5\beta - \pi\delta}{2[\gamma + \delta x]\delta}$$

Here, θ is the coefficient of growth on the quadratic term of finance and π is the coefficient on the linear term of finance, in a regression equation explaining growth per capita on three variables: the level (or more accurately, logarithm) of per capita income; finance (e.g., private credit as a percent of GDP); and finance squared. The other terms are per capita income (or its logarithm), x; the true parameter relating growth to per capita income, β; and the terms in a simple linear relationship showing the response of finance to per capita income (constant γ and linear coefficient δ on per capita income). With all terms in the equation except either θ or π positive, then if π is positive, the numerator on the right side is strictly negative and so θ on the left side must be negative. However, it is also possible that the quadratic coefficient θ will be positive, so long as not only π is negative but also $|\pi\delta| > 0.5\beta$.

But the latter condition is only a curiosity. In the large empirical literature relating growth to finance, there is to my knowledge not a single significant finding that additional finance at first *reduces* growth but eventually (say after finance reaches 100 percent of GDP) *increases* growth again, such that $\pi < 0$ while $\theta > 0$. So in the relevant application, estimating the influence of finance on growth, it is strictly the case of positive linear influence that would generate a spurious negative quadratic influence. In any event, it is unclear why ABP should be so content that any spurious influence would have to show up with an

31. The cuts needed to reduce credit to 76 percent of GDP, the turning point identified in Arcand, Berkes, and Panizza (2015a). Data are from World Bank (2015).

opposite sign for the quadratic term from that on the linear term. If they agree to that proposition, then they would have to recognize that their own tests preclude the result that both the linear and quadratic terms are positive, yet that is the implied alternative to their test results in which the linear is positive and the quadratic negative. If that alternative is impossible, they have set up a test that cannot be rejected, and it is not a meaningful test.

The second critique of ABP is that if the quadratic term on finance is spuriously negative because of a pattern of greater financial depth as a consequence of higher per capita income, then by the same approach I used to arrive at equation A1, it would follow that in a simple linear regression of growth on per capita income and a simple (linear only) finance variable the coefficient on the latter would be negative, yet the empirical literature finds it is positive. In other words, if growth decelerates as per capita income rises, and if finance deepens as per capita income rises, then any spurious attribution would find that deeper finance reduces rather than increases growth.

But a more fruitful way to think about this issue would be to consider two types of finance: one that causes growth and the other that responds as a luxury good to rising relative demand as per capita income rises. For simplicity, the first would be business loans and the second, home mortgages and loans for consumer durables. The system I spelled out would apply to the second category of finance, not the first. Empirical results primarily capturing the first type would find a positive coefficient of growth on finance. Those primarily reflecting the second type would find a negative simple linear coefficient of growth on finance, which may explain the negative coefficients in the Cournède-Denk (2015) study. Importantly, in this interpretation, reduction of finance will not increase growth, any more than a luxury tax shifting consumption away from any other luxury good would increase overall growth.

Third, ABP take issue with my critique that their parameters indicate that implausibly large increases in growth rates could be achieved by shrinking the financial sector. I illustrated my point using the case of Japan, which (using their coefficients) could supposedly raise the growth rate by 1.6 percentage point by reducing credit to the private sector from 178 to 90 percent of GDP. They cite an alternative set of estimates, controlling for banking crises, in which reducing credit from 178 to 90 percent of GDP in Japan would boost growth by "only" 0.95 percentage point. I would consider this alternative also implausible. They go on to insist that "regressions … are not meant to, and do not, fit all points (the regression's R^2 is never one) … [so] it is singularly inappropriate to pick out a specific data point to purportedly invalidate a result." But the Japan example I gave does not involve Japan's actual residual but instead simply applies the regression line to two alternative credit levels (which the line would also predict for any other country at comparable credit levels). Nor is the large impact for Japan unrepresentative. Thus, if we take ABP's apparently preferred model (controlling for banking crises, table 11 column 4 in Arcand, Berkes, and Panizza 2015a) and apply the 2006 data for private credit relative to GDP (table

16), we obtain implausibly large estimates for the increase in growth rates that can be achieved by cutting credit back to only 90 percent of GDP for a long list of important countries.[32]

Finally, ABP run new tests finding that if I had run my regressions of growth on doctors, R&D technicians, and telephones in what they believe is the right way—including country fixed effects—the spurious but statistically significant negative quadratic terms would have disappeared in two of the three cases. I have argued above that on this issue it is more appropriate not to include country fixed effects, so my results arguably remain more relevant than their reversal of two out of three of them.

32. United States: 3.1 percentage points (with private credit at 195 percent of GDP in 2006); Canada, 2.5 percentage points; Denmark, 2.1; Netherlands, 1.8; Ireland, 1.7; Switzerland, 1.6; and the United Kingdom, 1.5 percentage points (private credit at 160 percent of GDP). Ironically, the same table shows Japan's private credit at only 99 percent of GDP, much lower than the 178 percent shown for 2010 in the latest World Bank (2015) financial structure database (the number I had used), so Japan is no longer among the implausibly overstated in this exercise. ABP apparently used an earlier version of the database (Beck et al. 2010), which gave identical figures for bank credit to the private sector and total credit to the private sector from banks and other financial institutions (GFDD.D1.01 and GFDD.D1.12, respectively), whereas the most recent database places the total at 191 percent versus 99.7 percent for credit from banks alone (World Bank 2015).

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