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The Econometrics of Finance and Growth

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Abstract

This paper reviews different econometric methodologies to assess the relationship between financial development and growth. It illustrates the identification problem, which is at the center of the finance and growth literature, using the example of a simple Ordinary Least Squares estimation. It discusses cross-sectional and panel instrumental variable approaches to overcome the identification problem. It presents the time-series approach, which focuses on the forecast capacity of financial development for future growth rates, and differences-in-differences techniques that try to overcome the identification problem by assessing the differential effect of financial sector development across states with different policies or across industries with different needs for external finance. Finally, it discusses firm-level and household approaches that allow analysts to dig deeper into the channels and mechanisms through which financial development enhances growth and welfare, but pose their own methodological challenges.

This paper—a product of the Finance and Private Sector Team, Development Research Group—is part of a larger effort in the department to understand the link between financial sector development and economic development. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at TBeck@worldbank.org.

The econometrics of finance and growth

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1. Introduction

Economists have discussed over the past 100 years whether or not financial development has a causal impact on economic development. Theory suggests that effective financial institutions and markets that help overcome market frictions introduced by information asymmetries and transaction costs can foster economic growth through several channels. Specifically, they help (i) ease the exchange of goods and services by providing payment services, (ii) mobilize and pool savings from a large number of investors, (iii) acquire and process information about enterprises and possible investment projects, thus allocating society's savings to its most productive use, (iv) monitor investments and exert corporate governance, and (v) diversify and reduce liquidity and intertemporal risk. However, other models show that higher returns from better resource allocation may depress saving rates, resulting in overall growth rates actually slowing with more effective financial markets and institutions.[2]

While the finding of a positive correlation between indicators of financial development and economic growth cannot settle this debate, advances in computational capacity and availability of large cross-country data sets with relatively large time dimensions have enabled researchers to rigorously explore the relationship between financial development and economic growth. Further, as more disaggregated data sets have become available, the finance and growth literature has proceeded from using country-level data, to using industry- and firm-level data, to more recently using household data. While the cross-country literature has developed more sophisticated models to address biases introduced by measurement error, reverse causation and omitted variables, the progress to firm- and household-level data allows not only additional ways to address these biases, but also tests of the specific channels through which finance might enhance economic growth.

The econometrics of finance and growth can be summarized in the following simple regression model:

$$g(i,t) = y(i,t) - y(i,t-1) = \alpha + \beta_i f(i,t) + C(i,t)\gamma_i + \mu(i) + \varepsilon(i,t) \tag{1}$$

where y is the log of real GDP per capita or of another measure of welfare, g is the growth rate of y, f is an indicator of financial development, C is a set of conditioning

[2] See Levine (1997, 2005) for surveys of the theoretical literature.

information, μ and ε are error terms, i is the observational unit, be it a country, an industry, a firm or a household, and t is the time period. While ε is a white noise error with a mean of zero, μ is a country-specific element of the error term that does not necessarily have a mean of zero. The explanatory variables are measured either as an average over the sample period or as an initial value. The sign and significance of the coefficient β_i is at the center of the debate. As discussed in the remainder of this paper, the estimate of β_i can be biased for a variety of reasons, among them measurement error, reverse causation and omitted variable bias. While the cross-country literature assumes $\beta_i = \beta$, with some research supporting this assumption (Loayza and Ranciere, 2006), the time series literature does not impose this restriction. Further, several industry and firm-level studies test whether β varies across industries or firms with different characteristics, utilizing interaction terms.

This paper is concerned with an unbiased, consistent and efficient estimator of β_i.[3] In this context, we abstract from a number of other problems in the finance and growth literature. First, the paper does not cover problems arising from the lack of appropriate data, although we are concerned about measurement error in the financial indicators and the bias this introduces in the estimation. Second, while we are concerned about the bias introduced by the potential reverse causation from growth to finance, we are not concerned about this reverse causation per se, i.e. we do not discuss in depth the literature focusing on the impact of economic on financial development and bi-directional causality. Finally, the paper does not intend to be a fully fledged survey of the empirical finance and growth literature, as is Levine (2005), but rather focuses on studies with methodological contributions.

While this paper is concerned about estimating the relationship between finance and growth, some remarks about measuring financial development might be useful. While the theoretical literature links specific functions of the financial system to economic growth, data limitations have forced researchers to focus on variables capturing the size, activity or efficiency of specific financial institutions or markets. The first generation of papers in the finance and growth literature have built on aggregate data on financial institutions, mainly banks, available for 30 to 40 year periods for a large number

[3] For a broader survey on the econometrics of growth regressions, see Durlauf, Johnson and Temple (2005).

of developed and developing countries. Such indicators include monetization variables, such as M2 or M3 to GDP, or financial depth indicators, such as private credit (outstanding claims of financial institutions on the private sector) to GDP. Later papers have added indicators of the size and liquidity of stock markets, albeit available for fewer countries and shorter time periods. Indicators for the efficiency and competitiveness of financial systems, non-bank financial institutions such as institutional investors and, most importantly, the outreach of financial systems, are available for only a few countries and often do not have a time dimension.[4] Within-country studies allow researchers to utilize more micro-based data or focus on specific policy interventions or reforms.

The remainder of the paper is structured as follows. Section 2 illustrates the identification problem, which is at the center of the finance and growth literature, using the example of a simple Ordinary Least Squares (OLS) estimation of regression (1). Section 3 discusses instrumental variable (IV) approaches using cross-sectional and panel data. Section 4 discusses time-series approaches and section 5 differences-in-differences techniques. Section 6 discusses the use of firm- and household-level data and the methodological challenges this implies. Section 7 concludes and looks forward to new research directions.

2. Correlation vs. causality -- the identification problem

Goldsmith (1969) was the first to empirically show the positive correlation between financial development and GDP per capita, using data on the assets of financial intermediaries relative to GNP and data on the sum of net issues of bonds and securities plus changes in loans relative to GNP for 35 countries over the period 1860 to 1963. Such a correlation, however, does not control for other factors that are associated with economic growth and might thus be driven by other country characteristics correlated with both finance and growth. Second, such a correlation does not provide any information on the direction of causality between finance and growth. The early finance and growth literature has therefore used standard cross-country OLS regressions that

[4] See Beck, Demirguc-Kunt and Levine (2000) for an overview of different cross-country indicators of financial development and Beck et al. (2008) for a discussion of the different dimensions of financial development, such as depth, efficiency and reach. See World Bank (2007) for a discussion of financial outreach indicators.

build on an augmented Barro growth regression as in (1), with data for each country averaged over the sample period, assuming $\beta_i = \beta$ and $\gamma_i = \gamma$ for all countries, and including the lagged dependent variable as control variable:

$$g(i) = y(i,t) - y(i,t-1) = \alpha + \beta f(i) + C(i)\gamma + \delta y(i,t-1) + \varepsilon(i) \tag{2}$$

Unlike regression (1), regression (2) has thus only a cross-country, but not a time-series, dimension. The log of initial income per capita is included to control for convergence predicted by the Solow-Swan growth models. Including other country characteristics, such as initial levels of human or physical capital, and policy variables, such as government consumption or trade openness, in a set of conditioning information allows testing for an independent partial correlation of finance with growth. The coefficient β is of interest for finance and growth researchers, who interpret a positive and significant coefficient as evidence for a positive partial correlation between finance and growth.

Running this cross-country regression for a sample of 77 countries over the period 1960 to 1989, King and Levine (1993) found a positive and significant relationship between several financial development indicators and GDP per capita growth. Their study focuses mostly on monetization indicators and indicators measuring the size and relative importance of banking institutions. Using initial values of financial development confirms their finding. Levine and Zervos (1998) expanded the analysis to include measures of stock market development and found a positive partial correlation of both stock market and bank development with GDP per capita growth over the period 1976 to 1994.[5] Interestingly, they found a positive and significant link between liquidity of stock markets – as measured by a turnover indicator or value traded to GDP – and economic growth, but no robust relationship between the size of stock markets and economic growth. The empirical relationship between finance and growth, however, is not only statistically, but also economically significant. Levine and Zervos (1998) found that a one standard deviation in stock market liquidity and banking sector development explains an

[5] Other early finance and growth studies using cross-sectional OLS regressions include Atje and Jovanovic (1993) and De Gregorio and Guidotti (1995).

annual GDP per capita growth difference of 0.8 and 0.7 percentage, respectively, adding up to a total difference in GDP per capita of 31% over the 18 year sample period.

OLS estimates, however, are only consistent if the following orthogonality conditions hold:

$$E[C(i)'\varepsilon(i)] = 0; E[\gamma(i,t)'\varepsilon(i)] = 0; E[f(i)'\varepsilon(i)] = 0 \tag{3}$$

A violation of this condition can arise for several reasons. First, the presence of an unobserved country-specific effect $\mu(i)$ – as in regression (1) – results in a positive correlation of the lagged dependent variable with the error term as, unlike the error term $\varepsilon(i)$, $\mu(i)$ does not have a mean of zero, so that:

$$E[\gamma(i,t-1)'(\mu(i)+\varepsilon(i))] \neq 0 \tag{4}$$

Omitted variable bias can also arise if other explanatory variables are correlated with the unobserved country-specific effect or if explanatory variables that should be included in regression (2) are (i) not included and (ii) correlated with included explanatory variables, so that:

$$E[C(i)'(\mu(i)+\varepsilon(i))] \neq 0 \tag{5}$$

Second, reverse causation from GDP per capita growth to financial development or another explanatory variable could violate the orthogonality condition and thus bias the estimator of β if $\varepsilon(i)$ and $\nu(i)$ are correlated with each other, as would occur if:

$$f(i) = \lambda y(i,t-1) + \nu(i) \tag{6}$$

Third, one of the explanatory variables could be mis-measured, so that:

$$f*(i) = f(i) + u(i) \tag{7}$$

where $f*$ is the true level and f is the measured level of financial development. This could result in attenuation bias, if the measurement error is correlated with f.

Several simple approaches to overcome these biases have been suggested. First, controlling for other country traits and policies can help minimize the omitted variable bias and allow testing for the robustness of the finance and growth link (Levine and Renelt, 1992). However, the number of observations, and thus degrees of freedom, severely limits this approach in a typical cross-country regression. Second, several studies have used initial values of financial development, rather than values averaged over the same period as GDP per capita growth. If the true time span over which an improvement in financial development results in higher growth is shorter than the sample period used in the regression, then using initial values might reduce biases stemming from reverse causation. On the other hand, using initial values does not correct for biases introduced by omitted variables, measurement error or the inclusion of the lagged dependent variable, and implies a loss of information to be used in the estimation. Third, using panel regressions with fixed country effects would eliminate any time-invariant omitted variable bias and time-invariant measurement bias. However, the correlation between the transformed lagged dependent variable and the transformed error term will make the fixed effect estimator biased, and this bias is only eliminated as the number of time periods goes towards infinity, which is certainly not the case for the typical growth regression with fewer than 40 annual data points. Finally, fixed effect regressions also have the conceptual shortcoming that they effectively limit the analysis to within-country variation in growth and financial development by differencing out cross-country variation.

3. Instrumental variable approach

The classical approach in cross-country growth regressions to overcome the biases related to OLS is to identify an instrument that helps isolate that part of the variation in the endogenous variable that is not associated with reverse causation, omitted variables and measurement error. Following the seminal work by La Porta et al. (1997, 1998), who identified variation in countries' legal origin as an historical exogenous factor explaining current variation in countries' level of financial development, an extensive literature has utilized this variable to extract the exogenous component of financial development.

To overcome biases related to the inclusion of the lagged dependent variable and omitted variable bias, while at the same time controlling for reverse causation and measurement error, researchers have utilized dynamic panel regressions using lagged values of the explanatory endogenous variables as instruments. Finally, to control for country heterogeneity in the finance-growth relationship, researchers have utilized Pooled Mean Group (PMG) estimators. We will discuss each methodology in turn.

3.1. Cross-sectional regressions

Underlying instrumental variable estimation is the following specification:

$$g(i) = \gamma(i,t) - \gamma(i,t-1) = \alpha_1 + \beta_1 f(i) + C(i)\gamma_1 + \delta_1 \gamma(i,t-1) + \varepsilon(i) \tag{8}$$

$$f(i) = \alpha_2 + Z(i)\beta_2 + C(i)\gamma_2 + \delta_2 \gamma(i,t-1) + \nu(i) \tag{9}$$

$$f^*(i) = f(i) + u(i) \tag{10}$$

where *C* are the included exogenous and *Z* the excluded exogenous control variables; the latter are also referred to as instrumental variables which allow us to extract the exogenous component of *f(i)* that is not correlated with $\varepsilon(i)$, i.e. $E[Z(i)'\varepsilon(i)] = 0$, and $E[Z(i)'u(i)] = 0$.[6] Estimating regression (8) with instruments can help alleviate biases arising from reverse causation, omitted variable and measurement error.

Regression (8) is typically estimated with a Two-Stage-Least Squares Estimator (TSLS). Unlike the OLS estimator, the TSLS estimator only uses the variation in the explanatory variables that is correlated with the instrument and therefore uses less information than the OLS estimator. If OLS is consistent, it is therefore more efficient than IV, whereas if OLS is inconsistent, the IV estimator is both consistent and efficient.[7]

The TSLS estimator can also be derived as a General Method of Moments (GMM) estimator that minimizes a set of orthogonality conditions (Hansen, 1982). In the case, where there are more excluded exogenous than endogenous variables, a weighting matrix has to be used. While the TSLS estimator uses a weighting matrix constructed under the assumption of homoskedasticity, the weighting matrix of the GMM estimator is constructed as the inverse of the variance-covariance matrix, thus assigning different

[6] Most of the papers using this approach assume that only financial development is an endogenous variable and thus treat all control variables as exogenous.
[7] The literature has developed several tests to resolve the issue of OLS vs. IV, including the Hausman test.

weights to the orthogonality condition, according to their variances. While the TSLS estimator is thus consistent, it is inefficient as it does not use all the available information. On the other hand, the GMM estimator relies on asymptotic characteristics and therefore suffers from a finite-sample bias as the optimal weighting matrix is a function of fourth moments (Hayashi, 2000).[8]

Using legal origin as an instrument for financial development, Levine (1998, 1999) finds a positive relationship between finance and economic growth. Researchers have also used other historical and exogenous country characteristics as instruments for financial development, such as settler mortality and latitude, to proxy for geographic conditions, ethnic fractionalization, religious composition of the population, and years since independence (McCraig and Stengos, 2005). Guiso, Sapienza and Zingales (2004) use sub-national variation in historical bank restriction indicators across 20 Italian regions and its 103 provinces as instrumental variables to assess the impact of financial development and competition on economic growth and other real sector outcomes.

IV regressions depend on the quality of the instrumental variables, independent of whether TSLS or GMM is applied. As discussed above, these instruments are typically exogenous country characteristics, such as geographic traits, or based on historical experience, such as legal origin. The challenge is to identify the economic mechanisms through which the instrumental variables influence the endogenous variable – financial development – while at the same time assuring that the instruments are not correlated with growth directly. An extensive literature has discussed the historic determinants of financial sector development and the channels through which, for example, legal origin has helped shape current financial sector development,[9] but there are also several formal econometric conditions to be fulfilled in order for an instrument to be valid. First, the exogenous variables cannot be correlated with error terms, i.e., $E\left[Z(i)'\varepsilon(i)\right]=0$ (orthogonality or exogeneity condition). Second, the excluded exogenous instruments have to explain the variation in the endogenous variables after controlling for the included exogenous variables, i.e. the F-test for *Z(i)* in (9) is rejected at conventional levels (relevance condition).

[8] The presence of heteroskedasticty can be examined with a test proposed by Pagan and Hall (1983).
[9] See Beck and Levine (2005) for an overview.

The orthogonality condition is typically tested with the Sargan (1958) test of overidentifying restrictions (OIR) if there are more instruments than explanatory variables, that is: $\hat{\varepsilon}' Z(Z'Z)^{-1} Z'\hat{\varepsilon}/\hat{\sigma}^2$, where $\hat{\sigma}^2 = (\hat{\varepsilon}'\hat{\varepsilon})/n$ and $\hat{\varepsilon}$ is the vector of residuals from estimating regression (8). This test can easily be calculated from a regression of the IV regression's residuals on included and excluded exogenous variables. It is distributed as χ^2 with (J - K) degrees of freedom under the null hypothesis that the residuals are not correlated with the exogenous variables, where J is the number of instruments and K the number endogenous variables.[10] Hansen's (1982) J-test is a generalization of the Sargan OIR test to the GMM context and is the value of the GMM objective function evaluated at the efficient GMM estimator: $\hat{\varepsilon}' Z(Z'\hat{\Omega} Z)^{-1} Z'\hat{\varepsilon}$, where $\hat{\Omega}$ is the estimated variance-covariance matrix of the residuals from regression (8). As with the Sargan test, Hansen's test is distributed as χ^2 with (J - K) degrees of freedom.

The test of overidentifying restrictions, however, is relatively weak. First, the test only assesses the validity of any additional instruments, i.e. it cannot be performed if the number of excluded exogenous variables is the same as the number of endogenous variables. Further, the test tends to reject the null hypothesis of valid instruments too often in small samples (Murray, 2006). Most importantly, the test over-rejects if the instruments are weak, i.e. if they do not explain the endogenous variables in the first stage.

The second condition of instrument relevance can be tested in different ways. First, one can use an F-test of the joint significance of the instruments in (9); the critical values of this F-test for IV estimation, however, are larger than for OLS estimation; for the case of a single endogenous variable, Staiger and Stock (1997) show, using Monte Carlo simulations, that for most specifications and independent of the degrees of freedom a critical value of 10 is sufficient to reject the null hypothesis, and Stock and Yogo (2005) derive critical values for this F-test for the case of several endogenous variables,

[10] An alternative test was developed by Basmann (1960) and does not impose the overidentifying restrictions.

with the critical values increasing with the number of instruments.[11] Second, one can use a partial R^2 of the first-stage regression (9) that takes into account the intercorrelation among the instruments (Shea, 1997). Sepcifically, Godfrey (1999) shows that this statistic for endogenous regressor *i* is $\frac{\hat{\sigma}_i^{OLS}}{\hat{\sigma}_i^{IV}}[\frac{(1-R_{IV}^2)}{(1-R_{OLS}^2)}]$, where $\hat{\sigma}_i$ is the estimated asymptotic variance of the coefficient *i*. This measure thus tests for the relevance of the individual instruments, unlike the F-test, which tests for the overall relevance.

Weak instruments can bias the IV results towards OLS and turn them inconsistent. Further, weak instruments can result in an over-rejection of the overidentification test discussed above. If instruments are both invalid and irrelevant, the bias thus increases in a multiplicative way.[12]

Most of the cross-country finance and growth papers utilizing instrumental variables find that the IV estimator of β_1 is higher than the OLS estimator.[13] Manipulating regressions (8), (9) and (10), one can show that this implies:

$$\hat{\delta}_2 + \hat{\rho}\frac{\sigma(\hat{\upsilon})}{\sigma(\hat{\varepsilon})} < \hat{\beta}_1(1-\hat{\beta}_1\hat{\delta}_2)\frac{\sigma(\hat{u})}{\sigma(\hat{\varepsilon})} \tag{11}$$

where ρ is the correlation between ε and υ and the other parameters are taken from regressions (8), (9) and (10). There are several possible explanations for this finding and thus for inequality (11) to hold (Kraay and Kaufman, 2002). First, there could be negative reverse causation ($\delta_2 < 0$), which would bias the OLS estimator of the β_1 coefficient downwards. Given empirical studies showing the positive relationship between economic and financial development, this explanation seems rather unlikely (Harrison, Sussman and Zeira, 1999). A second explanation that makes inequality (11) hold is that omitted variables are correlated with growth and finance with opposite signs

[11] In the case of several endogenous variables, the Stock and Yogo test also requires each instrument to predict primarily just one of the endogenous variables.
[12] For further discussion on weak instruments and how to deal with them, see Murray (2006) and Baum, Schaffer and Stillman (2003).
[13] Most papers in the literature, however, do not formally test whether the difference between the OLS and the IV estimate is significant, which could be done with a Hausman test.

($\rho < 0$), an explanation for which, again, little evidence exists. A third – and most commonly adopted – explanation relies on attenuation bias, where measurement error in financial development ($\sigma(\hat{u})$) biases the OLS estimate downwards and makes inequality (11) hold. Critically, however, if the instrumental variables are positively correlated with omitted variables and the exclusion condition is thus violated, the IV estimator of β_1 is biased upwards. This is of concern, as a few instrumental variables, such as historical country traits, have been used for many different institutional variables in the context of growth regressions (Pande and Udry, 2006). Specifically, legal origin has been shown to be associated with an array of institutional arrangements, ranging from financial markets over general regulatory approaches, to labor market institutions. A significant correlation between institutional variables left out of the regressions and the instrumental variables can therefore also result in an upwardly biased IV estimator of β_1.

3.2. Dynamic panel analysis

While the cross-sectional IV regressions address biases related to omitted variables, reverse causation and measurement error, they do face several limitations. First, cross-country studies using cross-sectional IV regressions typically control only for the endogeneity and measurement error of financial development, but not of other explanatory variables entering the growth regressions. Second, in the presence of country-specific omitted variables, the lagged dependent variable is correlated with the error term if it is not instrumented.

As an alternative to cross-sectional IV regressions, researchers have therefore used dynamic panel regressions of the following format:

$$g(i,t) = \alpha + \beta f(i,t) + C^{(1)}(i,t)\gamma_1 + C^{(2)}(i,t)\gamma_2 + \delta y(i,t-1) + \mu(i) + \lambda(t) + \varepsilon(i,t) \qquad (12)$$

where $C^{(1)}$ represents a set of exogenous explanatory variables, $C^{(2)}$ a set of endogenous explanatory variables, and λ a vector of time dummies. Note that β is still assumed to be constant across countries, a restriction that we will relax further below.

Unlike the cross-sectional regressions, which use external instruments, i.e. variables that are completely external to the second stage regression, the dynamic panel regressions use internal instruments, i.e. lagged realizations of the explanatory variables. While this method does not control for full endogeneity, it does control for weak exogeneity, which means that current realizations of *f* or variables in $C^{(2)}$ can be affected by current and past realizations of the growth rate, but must be uncorrelated with future realizations of the error term. Thus, under the weak exogeneity assumption, future innovations of the growth rate do not affect current financial development.

In order to address the different biases in regression (12), Arellano and Bond (1991) suggest first-differencing the regression equation to eliminate the country-specific effect, as follows:[14]

$$\Delta g(i,t) = \beta \Delta f(i,t) + \gamma_1 ' \Delta C^{(1)}(i,t) + \gamma_2 ' \Delta C^{(2)}(i,t) + \delta \Delta y(i,t) + \Delta \lambda(t) + \Delta \varepsilon(i,t) \qquad (13)$$

where $\Delta x(t) = x(t) - x(t\text{-}1)$. This procedure solves the omitted variable bias, as described above, but introduces a correlation between the new error term, $\Delta\varepsilon(i,t)$, and the lagged dependent variable, $\Delta y(i,t\text{-}1)$. To address this correlation and the endogeneity and measurement problems, Arellano and Bond (1991) suggest using lagged values of the explanatory variables in levels as instruments for current differences of the endogenous variables. Under the assumptions that there is no serial correlation in the error term ε and that the explanatory variables *f* and $C^{(2)}$ are weakly exogenous, one can use the following moment conditions to estimate regression (13):

$$E\left[f(i,t-s)' \Delta\varepsilon(i,t)\right] = 0 \text{, for each } t = 3,...T, s \geq 2$$
$$E\left[C^{(2)}(i,t-s)' \Delta\varepsilon(i,t)\right] = 0 \text{, for each } t = 3,...T, s \geq 2$$
$$E\left[y(i,t-s)' \Delta\varepsilon(i,t)\right] = 0 \text{, for each } t = 3,...T, s \geq 2 \qquad (14)$$

Using these moment conditions, Arellano and Bond (1991) propose a two-step GMM difference estimator. In the first step, the error terms are assumed to be both independent and homoskedastic across countries and over time, while in the second step,

[14] Alternatively, one can use the forward orthogonal deviation transformation.

the residuals obtained in the first step are used to construct a consistent estimate of the variance-covariance matrix, thus relaxing the assumptions of independence and homoskedasticity. Simulations, however, have shown very modest efficiency gains from using the two-step as opposed to the one-step estimator, while the two-step estimator tends to underestimate the standard errors of the coefficient given that the two-step weight matrix depends on estimated parameters from the one-step estimator (Bond and Windmeijer, 2002).

There are several conceptual and econometric shortcomings with the difference estimator. First, by first-differencing we lose the pure cross-country dimension of the data. Second, differencing may decrease the signal-to-noise ratio, thereby exacerbating measurement error biases (see Griliches and Hausman, 1986). Finally, Alonso-Borrego and Arellano (1999) and Blundell and Bond (1998) show that, if the lagged dependent and the explanatory variables are persistent over time, i.e. have very high autocorrelation, then the lagged levels of these variables are weak instruments for the regressions in differences.[15] Simulation studies show that the difference estimator has a large finite-sample bias and poor precision.

To address these conceptual and econometric problems, Arellano and Bover (1995) suggest an alternative estimator that combines the regression in differences with the regression in levels. Using Monte Carlo experiments, Blundell and Bond (1998) show that the inclusion of the level regression in the estimation reduces the potential biases in finite samples and the asymptotic imprecision associated with the difference estimator. Using the regression in levels, however, does not directly eliminate the country-specific effect μ. Lagged differences of the explanatory variables can be used as instruments for the levels of the endogenous explanatory variables under the assumption that the correlation between μ and the levels of the explanatory variables is constant over time, such that:

$$E\left[f(i,t+p)'\mu(i)\right]=E\left[f(i,t+q)'\mu(i)\right], \text{ for all } p \text{ and } q$$
$$E\left[C^{(2)}(i,t+p)'\mu(i)\right]=E\left[C^{(2)}(i,t+q)'\mu(i)\right], \text{ for all } p \text{ and } q \qquad (15)$$

[15] Formal unit root tests as discussed in section 4 are not feasible in this context, as there are too few observations.

Under this assumption, lagged differences are valid instruments for the regression in levels, and the moment conditions for the regression in levels are as follows:

$$E\left[\Delta f\left(i,t-s\right)'\left(\varepsilon\left(i,t\right)+\mu\left(i\right)\right)\right]=0\text{, for each } t=3,...,T, s=2$$
$$E\left[\Delta C^{(2)}\left(i,t-s\right)'\left(\varepsilon\left(i,t\right)+\mu\left(i\right)\right)\right]=0\text{, for each } t=3,...,T, s=2$$
$$E\left[\Delta y\left(i,t-s\right)'\left(\varepsilon\left(i,t\right)+\mu\left(i\right)\right)\right]=0\text{, for each } t=3,...,T, s=2 \qquad (16)$$

The system thus consists of the stacked regressions in differences and levels, with the moment conditions in (14) applied to the first part of the system, the regressions in differences, and the moment conditions in (16) applied to the second part, the regressions in levels.[16] As with the difference estimator, the model is estimated in a two-step GMM procedure.

The consistency of the GMM estimator depends both on the validity of the instruments (exclusion condition) and the assumption that the error term, ε, does not exhibit serial correlation. Arellano and Bond (1991) propose two tests to examine these assumptions. The first is a Sargan test of over-identifying restrictions, which is constructed in a similar manner to the cross-sectional test discussed above. In the context of the system estimator, one can also compute a "difference-in-Sargan" test, the C-statistic (Eichenbaum, Hansen and Singleton, 1988), to test the orthogonality condition of a subset of instruments, such as the instruments applied to the level regressions. The C-statistic is computed as the difference of two Sargan/Hansen statistics, the one for the regression using the full set of instruments and the one using a smaller set of instruments. The C-statistic is distributed as χ^2 with the degrees of freedom equal to the number of instruments dropped from the second regression.

The second test examines the assumption of no serial correlation in the error terms, specifically whether the differenced error term is second-order serially correlated as, by construction, the error term $\Delta\varepsilon(i,t)$ from the difference regression is first-order

[16] Given that lagged levels are used as instruments in the difference regressions, only the most recent difference is used as an instrument in the level regressions, as using additional differences would result in redundant moment conditions (Arellano and Bover, 1995).

serially correlated and we cannot use the error terms from the regression in levels since they include the country-specific effect μ. This test is based on the standardized average residual autocovariances and, under the null hypothesis of no second-order serial correlation, has a standard normal distribution.

Rousseau and Wachtel (2000) use the difference estimator with annual data over the period 1980 to 1995 across 47 countries and find a positive link between indicators of bank and stock market development and economic growth.[17] Using five-year averages over the period 1960 to 1995 across 74 countries, Beck, Levine and Loayza (2000) and Levine, Loayza and Beck (2000) use both the difference and the system estimator and find a positive and significant relationship between indicators of financial intermediary development and GDP per capita growth, with the specification tests referred to above confirming the validity of both instruments and econometric model.[18] Beck, Levine and Loayza (2000) also find that the effect of finance on growth is through productivity growth, while there is no robust relationship between financial development and capital accumulation when controlling for biases due to simultaneity, omitted variables and measurement error.

The dynamic panel estimators have typically been applied to panels with few time periods and many countries. Further, the instrumental variable matrix Z is typically constructed with separate columns for instruments in different time periods, resulting in a quadratic increase in the number of columns of Z as the number of time periods increases (Roodman, 2007). This results in an overfit of the endogenous variables, biasing the coefficient estimates towards OLS estimates and biasing the Sargan/Hansen test for joint validity of the instruments towards over-accepting the null hypothesis (Bowsher, 2002). In order to avoid overfitting, one can limit the number of lags used in the difference regression or combine instruments into smaller sets, effectively imposing the constraint that instruments of each lag distance have the same coefficient when projecting regressors onto instruments (Beck and Levine, 2004, Roodman, 2007). In this case, the orthogonality conditions for the difference regressions are:

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[17] Rousseau and Wachtel (2000) was also the first paper to combine dynamic panel techniques with VAR techniques discussed in the next section.

[18] Other papers using dynamic panel techniques include Rioja and Valev (2004a,b) and Benhabib and Spiegel (2000). The latter, however, assume exogeneity of financial development and weak exogeneity only for capital accumulation, but not the other control variables.

$$E\left[f(i,t-s)'\Delta\varepsilon(i,t)\right]=0\text{, for each } s\geq 2$$
$$E\left[C^{(2)}(i,t-s)'\Delta\varepsilon(i,t)\right]=0\text{, for each } s\geq 2$$
$$E\left[y(i,t-s)'\Delta\varepsilon(i,t)\right]=0\text{, for each } s\geq 2 \quad (17)$$

and the orthogonality conditions for the levels regressions are:

$$E\left[\Delta f(i,t-s)'(\varepsilon(i,t)+\mu(i))\right]=0\text{, for each } s=2$$
$$E\left[\Delta C^{(2)}(i,t-s)'(\varepsilon(i,t)+\mu(i))\right]=0\text{, for each } s=2$$
$$E\left[\Delta y(i,t-s)'(\varepsilon(i,t)+\mu(i))\right]=0\text{, for each } s=2 \quad (18)$$

Given that data on financial sector indicators for a broad cross-section of countries are only available for a 25 to 40 year period, most studies split the sample period into non-overlapping five-year periods, thus controlling for business cycle effects, while at the same time having a reasonable number of time periods. An alternative to splitting the sample period into a number of five-year periods is to utilize overlapping five year periods, as proposed by Bekaert, Harvey and Lundblad (2005), thus allowing researchers to increase the number of time periods in the panel. In order to control for the MA(4) character of the data, the weighting matrix of the GMM estimator has to be adjusted accordingly.

Both the cross-sectional and the dynamic panel regressions discussed up to now assume a homogenous relationship between finance and growth across countries, i.e. β_i = β. At the other extreme, the time series approach, discussed in the next section, assumes complete country heterogeneity, but relies on a sufficiently large time series of data. When both cross-country and time-series dimension are sufficiently large, Pesaran, Smith and Im (1995) show that a consistent mean coefficient across countries is the unweighted average of the coefficients from independent country regressions (mean group, MG, estimator). The Pooled Mean Group (PMG) Estimator, introduced by Pesaran, Shin and Smith (1999), is in between these two extremes of cross-country and time-series approaches, as it imposes the same coefficient across countries on the long-run coefficients, but allows the short-run coefficients and intercepts to be country-specific.

Loayza and Ranciere (2006) use the PMG estimator on a sample of 75 countries and annual data over the period 1960 - 2000 and find a positive long-run relationship between financial development and growth, while the mean short-run coefficient on current financial development enters negatively.[19] Using the Hausman test that compares the MG with the PMG model, they cannot reject the hypothesis that the long-run coefficients on finance are the same in a cross-country panel growth regression. This is also evidence that the assumption that $\beta_i = \beta$ in the cross-country estimations discussed so far is a valid one, as long as the focus is on the long-term relationship between financial development and economic growth.

4. Time-series approach

The use of higher-frequency data, often limited to one or a few countries, and the concept of causality, are the main differences between the time series approach and the cross-country approach discussed in the previous section. First, the time-series approach relies on higher-frequency data, mostly yearly, to gain econometric power, while the cross-country approach typically utilizes multi-year averages.[20] Further, the time-series approach relaxes the somewhat restrictive assumption of the finance - growth relationship being the same across countries – i.e. $\beta_I = \beta$ – and allows country heterogeneity of the finance-growth relationship; most studies therefore focus their analysis on a few countries with long time-series data. The time-series approach also directly addresses biases introduced by the persistence and potential unit root behavior of financial development, as we will see in the following.

Second, and more importantly, different causality concepts underlie the two approaches. The time-series approach relies on the concept of Granger causality, as first developed by Granger (1969). A time series X is said to Granger-cause Y if, controlling for lagged Y values, lagged X values provide statistically significant information about the current value of Y. Granger causality tests are tests of forecast capacity; i.e. to what extent does one series contain information about the other series? Unlike the cross-

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[19] This negative short-run coefficient is consistent with the finding of the banking crisis literature. See for example, Demirguc-Kunt and Detragiache (1999).

[20] It is important to note, however, that the power of such high-frequency tests depends on the span of the time series rather than the number of observations.

country panel regressions discussed earlier, this concept therefore does not control for omitted variable bias by directly including other variables or by controlling with instrumental variables. Rather, by including a rich lag structure, which is lacking in the cross-sectional approach, the time series approach hopes to capture omitted variables. The cross-country approach, on the other hand, estimates the empirical relationship between finance and growth controlling for the different biases discussed in section 2, including the omitted variable bias, by extracting an exogenous component of finance that is related to growth only through finance.

In the context of the finance and growth literature, finance is said to Granger-cause GDP per capita if the inclusion of past values of finance in a regression of GDP per capita on its lags and the conditioning information set reduces the mean squared error *mse*. Formally:

$$mse[y(t+s)/\,y(t),y(t-1),\ldots] > mse[y(t+s)/\,y(t),y(t-1),\ldots,f(t),f(t-1),\ldots] \tag{19}$$

where the null hypothesis of no Granger causality is typically tested using F-tests on current and lagged values of *f*. Most studies test for bi-directional Granger causality using the following vector autoregression (VAR) system:

$$Y(t) = \alpha_1 Y(t-1) + \alpha_2 Y(t-2) + \ldots + \alpha_j Y(t-j) + \Sigma(t) \tag{20}$$

where *Y* is a vector comprising both GDP per capita and finance, as well as possibly other macroeconomic variables, and Σ is a matrix of error terms. Jung (1986) finds evidence for Granger causality from finance to GDP per capita for a sample of 56 countries, with some evidence of reverse Granger causality in the case of developed countries.

Testing for Granger causality between finance and GDP per capita using a levels VAR has the shortcoming that both finance and GDP per capita are nonstationary variables in most countries, as shown by standard tests for unit roots, such as the Augmented Dickey-Fuller (ADF) and Phillips and Perron (PP) tests, but stationary in first differences. However, only if two (or more) nonstationary series are co-integrated, i.e. if some linear combination of the series is stationary, can one use a levels VAR to test for Granger causality (Toda and Phillips, 1993, 1994). Cointegration thus implies a long-run equilibrium relationship between finance and GDP per capita. As in the case of Granger-causality, cointegration does not directly control for omitted variable or measurement biases, but rather exploits the long time-series of data to assess whether there is a stable relationship between these two variables.

If the vector Y is cointegrated, regression (19) can be re-written in the vector error correction (VEC) form (Engle and Granger, 1987):

$$\Delta Y(t) = \alpha_1 \Delta Y(t-1) + \alpha_2 \Delta Y(t-2) + \ldots + \gamma \delta' Y(t-1) + \mu(t) \tag{21}$$

where the vector γ of error correction coefficients (loading factors) indicates the direction and speed of adjustment of the respective dependent variable to temporary deviations from the long-run relationship, while the vector δ is the cointegrating vector. If there exists a non-zero cointegrating vector such that $\delta' Y(t)$ is stationary, the variables in Y are considered cointegrated. Testing for cointegration of the vector $Y(t)$ therefore is equivalent to a test that $\delta' Y(t)$ is stationary. If we can reject the null hypothesis that $\delta' Y(t)$ is stationary, we can also reject the null hypothesis that $Y(t)$ is cointegrated. In the case of two variables, this implies testing the residuals from a regression of $y(1,t)$ on $y(2,t)$ or $y(2,t)$ on $y(1,t)$ for stationarity (Engle and Granger, 1987). While the standard ADF test can be applied, the critical values are not the same as the test is performed on estimated residuals (Engle and Yoo, 1987). If there is no unit root, the two variables are cointegrated. In the case of more than two variables, inferences on the number and coefficients of the cointegrating vectors can be based on Johansen's (1991) Full-Information Maximum Likelihood approach. Johansen (1988) and Johansen and Juselius (1990) show that the Maximum Likelihood estimator of γ and δ can be derived as a solution of a generalized eigenvalue problem and likelihood ratio tests, based on these eigenvalues, can be used to test hypotheses on the number of cointegrating vectors.[21] The number of linear independent cointegrating vectors is equal to the rank of the matrix δ. Alternatively, one can test the hypothesis of a specific known cointegrating vector (Horvath and Watson, 1995), as done by Neusser and Kugler (1998).

[21] Specifically, the "trace" test can be used to test the hypothesis of r against zero cointegrating vectors, while the "λ-max" or maximum eigenvalue test can be used to test the hypothesis of $r+1$ cointegrating vectors against r cointegrating vectors.

Demetriades and Hussein (1996) and Luintel and Khan (1999) use the VEC specification and test for weak exogeneity of finance to GDP per capita by testing the null hypothesis that the corresponding loading factor in the GDP per capita regression in (21) is zero, while they follow Toda and Phillips' (1993) suggestion and use the product of loading factor and the cointegrating parameter to test for long-run causality. While Demetriades and Hussein (1996) find evidence for bidirectional causality and reverse causation from income to finance across a sample of 16 developing countries with at least 27 annual observations, with results varying substantially from country to country, Luintel and Khan (1999) find consistent evidence for bidirectional causality across a sample of ten developing countries with at least 36 years of data.

In the case of a cointegrating relationship between finance and GDP per capita, however, a levels VAR as in (20) can be used to test for short-term Granger causality, with conventional F-test statistics applying (Toda and Phillips, 1993, 1994; Sims, Stock and Watson, 1990)[22] and the VEC representation in (21) to estimate the adjustment speed γ. Rousseau and Wachtel (1998) use both the VAR specification of (20) and the VEC specification of (21) to determine the direction of causality between economic and financial development for five industrialized countries for the period 1870 to 1929. Specifically, using the VEC specification of (21), they find a co-integrating relationship for all five countries, while Granger causality tests suggest that finance leads GDP per capita in all five countries.[23] In addition, Neusser and Kugler (1998) apply the Granger and Lin (1995) test to measure the strength of causality from finance to GDP per capita at frequency zero, i.e. in the long-term, which is a function of the correlation of the errors in a bivariate VEC model and the adjustment coefficient vector γ.

In order to gain degrees of freedom, as unit root and cointegration tests have low power in the case of short time-series, several studies have expanded the time-series approach to panel data (Neusser and Kugler, 1998; Christopoulos and Tsionas, 2004). Averaging individual Dickey-Fuller unit root tests yields the Im, Pesaran and Shin (2003) test, while combining p-values from individual ADF tests yields the Maddala and Wu

[22] Specifically, Toda and Phillips (1993, 1994) and Sims, Stock and Watson (1990) show that in the case of cointegrated series the conventional Wald statistic converges to a χ^2 distribution.
[23] Following this approach, Rousseau and Sylla (2005) use data for the U.S over the period 1850 to 1997, Bell and Rousseau (2001) use data for India and Xu (2000) uses data for 43 countries over the period 1960 to 1993; all find robust evidence for a leading role of finance.

(1999) test, both of which allow testing for a unit root in panels. To establish cointegration relationships in a panel, Pedroni (1997) suggests estimating the cointegrating regression by OLS separately for each country before a unit root test similar to the Phillips-Perron test is applied to the stacked residuals. Further, the VEC specification (21) can be extended to a panel with country-specific fixed effects to test for both long- and short-run relationships between finance and GDP per capita. Christopoulos and Tsionas (2004) find evidence for cointegration and long-run Granger causality from finance to GDP per capita for a sample of ten developing countries for the period 1970 to 2000, both for individual countries and for the panel. Unlike other studies in the time series tradition, they also confirm their findings by applying dynamic panel regression techniques using lagged values as instruments in the panel version of (21).

Using Geweke's (1982) measure of linear dependence, Calderon and Liu (2003) compute the relative strength of the Granger causality from finance to GDP per capita, from GDP per capita to finance and the instantaneous feedback between finance and GDP per capita. Specifically, using variance-covariance matrices calculated under different restrictions on the system (20) allows calculating a measure of the overall strength of the relationship between two variables and the three different sources. They find a stronger effect from finance to GDP per capita than for the reverse effect for developing countries, which increases when they average data over longer time periods. While they consider the linear decomposition in the context of panel regressions, with data averaged over five-year periods, they do not assess the finance-GDP per capita relationship at different frequencies.

5. Differences-in-differences estimations

While the cross-country IV approach focuses on identifying instruments to overcome the different biases found in an OLS regression, and the time-series approach focuses on the forecast capacity of finance in a VAR including GDP per capita, the differences-in-differences technique can be understood as a "smoking-gun" or controlled treatment approach. Specifically, traditional differences-in-differences estimation consists of comparing the difference between the treatment and the control groups before and after a treatment, such as a policy change, thus controlling for other confounding influences on growth.[24]

The seminal paper in this literature is Jayartne and Strahan (1996), who exploit the fact that states across the U.S deregulated intra-state branch restrictions at different times over the period 1970 to 1995 and relate this policy change to subsequent state-level growth. In this case the treatment and control groups are in flux; at any point in time, the treatment group consists of states that have deregulated, while the control group consists of those states that have not deregulated yet. By controlling for state- and year-specific effects, this approach effectively measures the impact of deregulation on state-level growth relative to the average state-level growth rate over the sample period and relative to the average growth rate in the U.S in this specific year. The specification is:

$$g(i,k) = \alpha(i) + \lambda(k) + \beta(External(k) * f(i)) + yShare(i,k) + \delta'(Industry(k) * Country(i)) + \varepsilon(i,k) \quad (22)$$

where $\alpha(i)$ is a vector of state dummies, $\lambda(t)$ a vector of year dummies, $C(i,t)$ a vector of time-varying state characteristics and d the treatment variable, which is branch deregulation in the case of Jayaratne and Strahan (1996), who found a positive and significant coefficient β, thus suggesting that branch deregulation led to higher growth.[25] They also find evidence for a large economic effect of branch deregulation, explaining an annual growth difference of at least 0.5 percentage points, compared to an average annual growth rate across states of 1.6%. Consistent with cross-country results, they also find evidence that the finance-growth nexus worked through improved lending efficiency rather than more lending and investment.

The differences-in-differences estimator reduces, but does not eliminate, the biases of reverse causation and omitted variables. Specifically, any omitted variable has to be time-variant in order to bias the results, because otherwise it would be picked up by the state dummies. Further, by considering sub-national variation, differences-in-

[24] While we treat such exogenous policy changes in the context of differences-in-differences estimations, one could also use them as instruments for financial development in the context of regular cross-sectional regressions (Guiso, Sapienza and Zingales, 2004).

[25] Following the model of Jayartne and Strahan (1996), Dehejia and Lleras-Muney (2007) show that, over the period 1900 to 1940 across states of the U.S, regulatory changes that allowed branching accelerated the mechanization of agriculture and spurred growth in manufacturing, while the introduction of deposit insurance had negative consequences.

differences estimation is less subject to biases introduced by unobserved heterogeneity across countries and measurement error is reduced as the focus is on one specific policy measure, implemented in the same way but at different times across sub-national units.[26] On the other hand, the events in different states, such as branch deregulation, were not independent from each other, but rather came in waves, which might bias the estimate of β (Huang, 2008). Further, the concern of reverse causation can only be addressed by utilizing instrumental variables or by showing that the decision to implement the policy change across states is not correlated with future growth rates, as was done by Jayaratne and Strahan (1996).

Apart from the problem of endogeneity, serial correlation of the error terms in differences-in-differences estimations can lead to underestimation of standard errors, as shown by Bertrand, Duflo and Mullainathan (2004).[27] This problem increases with the number of time periods and the persistence of the dependent variable and is exacerbated by the fact that the treatment variable, e.g. branch deregulation, shows little change across states, at most one change from zero to one. Using Monte-Carlo simulation, Bertrand, Duflo and Mullainathan (2004) show that collapsing data to before and after-treatment[28] or allowing for correlation within states (clustering) are solutions that resolve the problem of underestimated standard errors.

Going even more local, Huang (2008) uses county-level data from contiguous counties only separated by a state border in cases where one state deregulated at least three years earlier than the other. This helps reduce concerns of omitted variables, as one can assume a very similar structure of two contiguous counties and also helps reduce

[26] On the other hand, focusing on one country reduces the policy relevance of its findings, as the relationship might vary across countries with different economic and institutional settings. Further, subnational variation might not be independent from each other given the higher mobility of capital and labor within than across countries.

[27] Bertrand, Duflo and Mullainathan (2004) find overrejection of the null hypothesis using randomly assigned placebo treatments in Monte Carlo simulation

[28] Specifically, this would imply regressing growth on state and year fixed effects and other time-varying control variables, taking the residuals and averaging them for the period before and after the treatment for each state. The estimate of the treatment can then be obtained from a regression of this two-period state panel on the treatment dummy.

concerns of reverse causation, as expected higher future growth of a specific county is unlikely to affect state-level political decisions.[29,30]

A somewhat related differences-in-differences approach is suggested by Rajan and Zingales (1998), who conjecture that the effect of financial development should vary by sector or industry according to the financing need of each sector or industry. They thus assess the finance and growth link by focusing on a specific channel through which financial development should foster economic development, i.e. the channeling of society's savings to industries with the highest demand for external finance. Specifically, they use variation across industries in their dependence on external finance and variation across countries in their level of financial development to assess the impact of finance on industry growth, and apply the following specification:[31]

$$g(i,k) = \alpha(i) + \lambda(k) + \beta(External(k) * f(i)) + yShare(i,k) + (Industry(k) * Country(i))\delta + \varepsilon(i,k) \quad (23)$$

where g is growth of value added in industry k in country i; α and λ are vectors of country and industry dummies; *Share* is the initial share of industry k's value added in total manufacturing value added of country i; *External* is the external dependence of industry k; f is a measure of financial development in country i; *Industry* is a vector of other industry characteristics that do not vary across countries; and *Country* is a vector of other country characteristics that do not vary across industries. By including industry and country specific effects, the coefficient β measures the differential growth impact of financial development on high-dependence industries relative to low-dependence industries. When redefining this exercise in terms of a controlled experiment, we could see industries (rather than states) as the treated objects, some of which (high external

[29] This argument, however, is only valid if there is sufficient variation in growth across different counties within the state.

[30] Given the lack of randomness of the sample relative to the population, Huang (2008) constructs critical values from a distribution of the effects of fictitious placebo treatments on county pairs on non-event borders, taking into account spatial correlation across counties along the same borders. Only if 95% of all placebo treatments result in a growth difference below a certain value can this value be considered a significant growth difference for a real world treatment at the 5% significance level.

[31] Rajan and Zingales (1998) compute the industry-level dependence on external finance from data of listed firms in the U.S, i.e. firms that should have the least problems in raising external finance and thus face a perfectly elastic supply curve, to get measures of industry-level demand for external finance. They conjecture that demand for external finance measured in this way proxies for the industry-inherent demand for external finance, rather than country- or firm-specific characteristics, in the U.S.

dependence) are subjected to the treatment of financial development. In a sample of 41 countries and 36 manufacturing industries, Rajan and Zingales (1998) find robust evidence for a significant and positive β, which is even stronger when focusing on young firms in the computation of external dependence. To gauge the economic significance, Rajan and Zingales (1998) assess the growth difference between the industries at the 75th and 25th percentile of external dependence in the countries at the 75th and 25th percentiles of their financial development indicator. Their results suggest that the annual growth difference between Machinery (75th percentile of external dependence) and Beverages (25th percentile of external dependence) is 1.3 percentage points higher in Italy (75th percentile financial development) than in Philippines (25th percentile financial development). This compares to an average industry growth rate of 3.4%, thus a relatively large effect.

As in the case of Jayaratne and Strahan (1996), regression (23) does not control for biases due to omitted variables or reverse causation. Rajan and Zingales (1998) address concerns of the endogeneity of the treatment, i.e. of financial development, by focusing on the smallest 50% of industries in terms of initial value added in each country, as it is less likely that the financial sector develops in response to the smallest industries. They address the omitted variable bias by including other interaction terms between industry and country characteristics that can explain cross-country, cross-industry growth variation and utilizing instrumental variables for financial development.[32] Critically, the differences-in-differences estimator depends on the assumption that there are industry-inherent characteristics that do not vary across countries and that they are properly measured by the data in the U.S (von Furstenberg and von Kalckreuth, 2006, 2007).

6. Firm- and household-level approaches

While the three approaches discussed so far – cross-country instrumental variable regressions, VAR models and differences-in-differences estimation – have tried to

[32] The differences-in-differences approach of Rajan and Zingales has subsequently been used by many other researchers interested in the linkage between financial development and growth and specific mechanisms and channels, including Beck and Levine (2002), Beck (2003), Beck, Demirguc-Kunt, Laeven and Levine (2005), Braun and Larrain (2005), Claessens and Laeven (2003) Fisman and Love (2003) and Raddatz (2006).

address the different biases resulting from the standard OLS cross-country growth regression, a fourth approach has used disaggregated firm and, more recently, household level data to assess the impact of access to financial services on firm growth and household welfare. The advantage of using micro-level data is that it allows more clearly the disentangling and testing of the mechanisms and channels through which financial development enhances economic growth. A disadvantage is that it focuses on the direct effect of finance on firm growth and household welfare but commonly does not consider spill-over effects on other firms and households and therefore does not allow for individual effects to be added up to an aggregate growth effect.[33]

Further, as in the case of cross-country regressions, biases due to omitted variables, measurement error and reverse causation have to be addressed. This section discusses several studies using micro-data that assess whether easier access to finance is associated with faster firm growth and higher household welfare. Unlike the previous section, this section does not introduce new methodologies, but rather discusses methodological challenges stemming from the use of micro, as opposed to country-level, data.

6.1 Firm-level approaches

The different approaches discussed in this section consist of relating firm-level growth or investment to country-level financial development measures. As in the case of cross-country regressions, however, this implies controlling for biases stemming from reverse causation and omitted variables. A first approach, suggested by Demirguc-Kunt and Maksimovic (1998), compares firm growth to an exogenously given benchmark. Specifically, they calculate for each firm in an economy the rate at which it can grow, using (i) only its internal funds or (ii) using its internal funds and short-term borrowing, based on the standard "percentage of sales" financial planning model (Higgins 1977).

[33] Indirect effects of financial development can be very important, as shown by Beck, Levine and Levkov (2007), who find that the main channel through which branch deregulation across U.S states led to lower income inequality was through labor market effects rather than through providing increased access to finance.

Given a set of simplifying assumptions, the external financing needs *EFN* at time *t* of a firm growing at rate *g(t)* is given by: [34]

$$EFN(t) = g(t) * Assets(t) - [1 - g(t)] * Earnings(t) * b(t) \tag{24}$$

where *b(t)* is the fraction of the firm's earnings that are retained for reinvestment at time *t*. Assuming that the firm retains all its earnings, i.e. *b(t)=1*, the internally financed growth rate *IG(t)* is the maximum growth rate that can be financed with internal resources only, that is:

$$IG(t) = ROA(t) / [1 - ROA(t)] \tag{25}$$

Demirguc-Kunt and Maksimovic (1998) then regress the percentage of firms in a country that grow at rates exceeding *IG(t)* on financial development, other country characteristics and averaged firm characteristics in a simple OLS set-up and show, for a sample of 8,500 firms across 30 countries, that the proportion of firms growing beyond the rate allowed by internal resources is higher in countries with better developed banking systems and more liquid stock markets.[35]

An alternative approach to assess the impact of access to finance on firm growth is the use of firm-level survey data, as done by Beck, Demirguc-Kunt and Maksimovic (2005), who use firm-level survey data for over 4,000 firms in 54 countries to run the following regression:

$$g(i,k) = \alpha + \beta_1 o(i,k) + \beta_2 f(i) + \beta_3 o(i,k) * f(i) + C^{(1)}(i,k)\gamma_1 + C^{(2)}(i)\gamma_2 + \varepsilon(i,k) \tag{26}$$

where *g* is sales growth of firm *k* in country *i* over the period 1996 to 1999, $C^{(1)}$ is a set of firm-level control variables, $C^{(2)}$ is a set of country-level control variables, *o* is the financing obstacle as reported by the firm and *f* is a country-level financial development indicator. The financing obstacle is the response by the firm to the question of whether

[34] The three simplifying assumptions are as follows: First, the ratio of assets used in production to sales is constant. Second, the firm's profits per unit of sales are constant. Finally, the economic deprecation rate equals the accounting depreciation rate.

[35] Subsequently, this technique has been applied by Demirguc-Kunt and Maksimovic (2002) and Guiso, Sapienza and Zingales (2004), among others.

financing is an obstacle to its operation and growth, and responses are coded as no obstacle (1) minor obstacle (2), moderate obstacle (3) and major obstacle (4). While β_1 indicates the relationship between the reported financing obstacle and firm growth, β_3 indicates whether this relationship varies across countries with different levels of financial development. Beck, Demirguc-Kunt and Maksimovic (2005) find a negative and significant coefficient on β_1 and a positive and significant coefficient on β_3, suggesting that firms reporting higher financing obstacles experience slower sales growth, but that this relationship is less strong in countries with better developed financial systems. Further, using triple interaction terms, they show that the mitigating effect of financial development on the relationship between financing obstacles and firm growth is stronger for small firms than for large firms.

Another methodology consists of assessing the relationship between country-level financial development and firms' financing constraints derived from a structural investment model, such as the Euler equation (Love, 2003; Laeven, 2003). Specifically, the Euler equation derives the optimal investment decision as the point where the marginal cost of today's investment is equal to the discounted marginal cost of postponing investment until the next period, which includes the marginal product of capital, the adjustment cost and the price of investment tomorrow. In the absence of credit market constraints, firms' investment decisions should thus be independent of firms' cash flow holdings, while the investment decisions of credit constrained firms should be a positive function of available cash. Financial sector development, on the other hand, should reduce the dependence of firms' investment on cash holdings. To test for the presence of credit market constraints and the impact of financial development on the relationship between credit market constraints and investment, the following regression is used:

$$I(k,t) = \alpha(k) + \lambda(t) + \beta_1 Cash(k,t-1) + \beta_2 Cash(k,t-1) * f(i,t) + C^{(1)}(k,t)\gamma_1 + C^{(2)}(k,t-1)\gamma_2 + \varepsilon(i,k,t) \tag{27}$$

where I is investment, $\alpha(k)$ is a vector of firm dummies, $\lambda(t)$ a vector of time dummies, *Cash* is liquid assets relative to total assets, $C^{(1)}$ and $C^{(2)}$ are sets of current and lagged firm-level control variables, such as investment-to-capital ratios and sales-to-capital ratios, and the subscript i refers to countries. The existence of credit constraints implies $\beta_1 > 0$, while the alleviating role of financial sector development implies $\beta_2 < 0$. As regression (27) poses similar problems in terms of the different biases identified in section 2 for cross-country growth regressions, most studies use the dynamic panel techniques suggested by Arrellano and Bond (1991) and Arrellano and Bover (1995) to control for these biases. Using data for 5,000 firms across 36 countries, Love (2003) shows that financial development reduces firms' dependence on cash holdings for investment, while Laeven (2003) shows, for a sample of 400 firms across 13 countries, that financial liberalization helped reduce small firms' financing dependence on internal cash, while it adversely affected large firms' financing possibilities. The effect of financial development and liberalization is also economically significant. Love (2003) shows that firms' financing constraints – as measured by the cost of capital – in countries with low levels of financial development are twice as high as in countries with average levels of financial development, while Laeven (2003) shows that financial liberalization had a significant economic effect on firms' financing constraints, reducing small firms' constraints by 80%.

6.2. Household-level approaches

While the availability of financial information for listed companies and survey data for non-listed companies has resulted in a rapid expansion of firm-level studies, the lack of comparable data for households has impeded similar research for the effect of access to finance on household welfare until recently. As in the case of aggregate and firm-level studies, the identification problem prevents inference from cross-sectional household surveys with data on welfare and access to finance variables. A final and very recent technique therefore uses controlled experiments with households and/or microentrepreneurs, whose financing constraints are randomly alleviated and who are then compared to a control group whose constraints were not alleviated. The challenges of these studies are less in estimation techniques than in the proper identification of treatment and control groups and of the experimental treatment itself. In the following, we will discuss three examples.

First, Pitt and Khandker (1998) use household survey data to assess the impact of microcredit on household welfare across several programs in Bangladesh. However, as in the case of cross-country regressions, omitted variable bias and reverse causation would bias the result of a simple OLS estimation, as illustrated by the following system:

$$\gamma(i,j) = C(i,j)\alpha_1 + \beta f(i,j) + \eta(i) + \varepsilon(i,j) \tag{28}$$

$$f(i,j) = C(i,j)\alpha_2 + Z(i,j)\delta + \mu(i) + \nu(i,j) \tag{29}$$

where *y* is a measure of household welfare of household *i* in village *j*, *f* is the amount of credit obtained by a household, *C* is a vector of household characteristics, and *Z* is a set of household or village characteristics that serve as instruments for the endogenous credit variable. μ and η are unobservable village characteristics, that are correlated with household welfare and credit, respectively. Correlations between μ and η and between ε and ν can result in a biased OLS estimate of β in (28). These correlation can arise because microcredit program placement is nonrandom, often related with specific village characteristics, such as poverty levels. Further, unmeasured household and village characteristics can influence both the demand for microcredit and household outcomes *y*. Pitt and Khandker (1998) therefore use the exogenously imposed restriction that only farmers with less than a half-acre of land are eligible to borrow from microfinance institutions in Bangladesh as an exclusion condition to compare eligible and non-eligible farmers in program and non-program villages. Using survey data for 1,800 households and treating landownership as exogenous to welfare outcomes, they exploit the discontinuity in access to credit for households above and below the threshold and find a positive and significant effect of credit on household consumption expenditures. Morduch (1998), however, shows that mistargeting, i.e. allowing farmers with landholdings above the threshold to access microcredit, violates the exclusion condition, and that different econometric techniques exploiting the landholding restriction lead to different findings.

Coleman (1999) exploits the fact that future microcredit borrowers are identified before the roll-out of the program in Northern Thailand and can thus exploit the differences between current and future borrowers and non-borrowers in both treated and to-be-treated villages.[36] His model is:

$$\gamma(i,j) = C^{(1)}(i,j)\alpha + \beta p(i,j) + C^{(2)}(j)\gamma + \delta M(i,j) + \varepsilon(i,j) \tag{30}$$

where y is an array of measures of household welfare, $C^{(1)}$ is a set of observable household and $C^{(2)}$ a set of observable village characteristics, M is dummy that takes the value one for current and future borrowers and p is a dummy that takes the value one for villages that already have access to credit programs. M can be thought of as proxy for unobservable household characteristics that determine whether a household decides to access credit or not, whereas β measures the impact of the credit program by comparing current and prospective borrowers. Coleman (1999) does not find any robustly significant estimate of β and therefore rejects the hypothesis that microcredit helps households in this sample and this institutional setting.

A final example is Karlan and Zinman (2006), who used a sample of marginally rejected applicants of a South African consumer credit institution. They convinced the credit institution to provide loans to a randomly chosen subset of these borrowers. Surveying both treatment and control groups six and twelve months after providing credit to the treatment group, they find that borrowers were more likely to retain wage employment and less likely to experience hunger in their household and be impoverished.

$$\gamma(i) = C(i)\alpha + \beta p(i) + \varepsilon(i) \tag{31}$$

where y is an indicator of household welfare, C is a vector of household characteristics and p is the treatment dummy that takes the value one if the individual surveyed has received a loan.

While controlled experiments can assess the effect of access to credit (or other financial services) on the growth of micro-enterprises or household welfare, there are shortcomings to this methodology. First, they are very costly to conduct. Second, they are environment-specific and it is not clear whether the results will hold in a different

[36] This technique is also referred to as pipeline matching (Goldberg and Karlan, 2005).

environment with a different sample population. Third, the controlled experiments, as they have been undertaken up to now, do not consider any spill-over effects of access to credit by the treated individuals or enterprises to other individuals or enterprises in the economy.

7. Concluding remarks

The finance and growth literature has come a long way from simple correlation and OLS regressions to dynamic panel regressions and the use of firm- and household- level data. While each of the different methodologies and aggregation levels has its shortcomings, the body of evidence accumulated over the past 15 years provides a strong case for a relationship between financial development and economic growth that is not driven by omitted variables, measurement error or reverse causation.

While the profession has made great progress in measuring financial development, especially by moving towards micro-data, this paper has focused on methodological advances to overcome the biases illustrated by a simple cross-country OLS regression. Most importantly, overcoming endogeneity and simultaneity biases with a proper identification strategy has been the main challenge for researchers. While the cross-country literature has focused on finding external and internal instruments, the time-series literature has exploited high-frequency data, a rich lag structure, and the forecast capacity of finance for GDP per capita. Differences-in-differences approaches address the identification challenge by assessing natural experiments, exploiting either exogenous policy reforms or inherent industry characteristics that result in a differential impact of financial development.

Using firm- and household-level data allows a deeper look into the mechanisms through which finance enhances firm growth and household welfare and thus provides additional evidence, but poses its own set of identification challenges. While many of the methodologies used at the cross-country-level, such as instrumental variables or differences-in-differences, can also be applied at the firm- and household-level, randomized controlled experiments with households and microentrepreneurs open new and exciting research opportunities, as they allow researchers to subject households and microenterprises to a specific treatment under the control of the researcher.

Different methodologies imply different aggregation levels. While assessing the finance and growth relationship on a more disaggregated level might allow better controlling for different biases – such as measurement error when considering a specific policy change on the sub-national level or simultaneity bias when using household data in a controlled randomized experiment – this has to be balanced with the limited extent to which we can draw policy conclusions from such a specification. Further, using firm-level or household level data does not properly control for spill-over effects, are often very costly exercises, and do not lend themselves easily to compute the aggregate growth effect of financial development. While randomized experiments have the advantage that they are the cleanest exercise possible, as they are controlled by researchers, they might not properly mimic the real world, and might not allow inferences outside the geographic and institutional experiment area.

While a wide array of cross-country techniques has been applied to the finance and growth field, some techniques have not been used yet, such as identification through heterogeneity in structural shocks (Rigobon, 2003). Further, it is easy to predict that there will be further advances in GMM techniques that control better for country heterogeneity and in techniques to assess the finance and growth relationship at different frequencies. As before, the finance and growth literature will benefit in the years to come from methodological advances in neighboring fields, especially in growth econometrics. Merging VAR and cross-country techniques – two literatures which have moved mostly parallel to each other up to now – also promises further methodological insights.

More important than these advances at the aggregate level, however, will be advances at the micro-level, and specifically on two fronts. First, randomized experiments involving both households and micro- and small enterprises will shed light on the effect of access to finance on household welfare and firm growth. One of the challenges to overcome will be to include spill-over effects and thus move beyond partial equilibrium results to aggregate results. Second, further studies evaluating the effect of specific policy interventions can give insights into which policy reforms are most effective in enhancing financial development and positive real sector outcomes.[37]

[37] One example assessing the effect of different legal reforms is Haselmann, Pistor and Vig (2005).

Advances in both areas, however, will depend on the collection of micro-based data on access to and use of financial services.

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