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Agglomeration and growth: Cross-country evidence ☆

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ABSTRACT

We investigate the impact of within-country spatial concentration of economic activity on country-level growth, using cross-section OLS and dynamic panel GMM estimation. Agglomeration is measured alternatively through urbanization shares and through indices of spatial concentration based on data for sub-national regions. Across estimation techniques, data sets and variable definitions, we find evidence that supports the "Williamson hypothesis": agglomeration boosts GDP growth only up to a certain level of economic development. The critical level is estimated at some USD 10,000, corresponding roughly to the current per-capita income level of Brazil or Bulgaria. Hence, the tradeoff between national growth and inter-regional equality may gradually lose its relevance. Our results also imply that, in terms of foregone growth, the cost of policies that inhibit economic agglomeration is highest in the poorest countries.

> It was thus that through the greater part of Europe the commerce and manufactures of cities, instead of being the effect, have been the cause and occasion of the improvement and cultivation of the country. This order, however, being contrary to the natural course of things, is necessarily both slow and uncertain.
>
> Adam Smith, Wealth of Nations

1. Introduction

Do economies grow faster if they are concentrated in space? This is one of the most fundamental questions posed by economic geographers. It is also an issue at the heart of a theoretical research programme that has emerged in the late 1990s by conjoining models from the "new" theories of economic growth and geography. This recent theoretical work generally supports the view that spatial proximity is good for economic growth. For example, Martin and Ottaviano (1999, p. 948) model growth and geographic agglomeration as "mutually self-reinforcing processes," Fujita and Thisse (2002, p. 391) find that "growth and agglomeration go hand-in-hand," and the review paper by Baldwin and Martin (2004, p. 2672) stresses the result that, given localized spillovers, "spatial agglomeration is conducive to growth." The complementarity of growth and spatial concentration, if found to be a general phenomenon, has strong implications for economic policy, as it entails a special kind of efficiency-equity trade-off, whereby policy makers may be forced to choose between supporting lagging regions and promoting growth at the national level (Martin, 1999).

Any link between spatial concentration and growth is unlikely to be a simple, ubiquitous regularity. We therefore consider the possibility that the effect of spatial concentration on economic growth may be non-linear and conditional on other factors. We focus on two prominent hypotheses.

First, Williamson (1965) suggests that agglomeration matters most at early stages of development. When transport and communication infrastructure is scarce and the reach of capital markets is limited, efficiency can be significantly enhanced by concentrating production in space; but as infrastructure improves and markets expand, congestion externalities may favour a more dispersed economic geography. This configuration is consistent with the model of urbanization and growth by Bertinelli and Black (2004). In that model, growth emanates from agglomerated regions ("cities"), as human capital accumulation is assumed to occur only there. These

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dynamic gains of agglomeration have to be weighed against the cost of static congestion diseconomies. The relative importance of these two effects changes across stages of development. Since individuals internalize neither the dynamic nor the static externalities generated by their location choices, the agglomerated region can be too small or too large in equilibrium. And since the potential gains from human capital accumulation are particularly large at initial stages of development while the congestion diseconomies do not change with the level of development *per se*, the relative importance of congestion will increase with the level of development. Agglomeration-induced human capital accumulation is shown to be particularly crucial at low levels of technological advancement, where the economy could otherwise remain in a development trap. More generally, geographic reallocation of economic activity is a slow process and urbanization trends take time to reverse.[1] It is therefore conceivable that countries find themselves with an excessively concentrated economic geography after periods of fast growth (possibly facilitated in its early stages by that very geographic concentration), and that concentration "overshoots" beyond the optimum level. According to the "Williamson hypothesis," therefore, agglomeration promotes growth at early stages of development but has no, or even detrimental, effects in economies that have reached a certain income level.

Second, Krugman and Elizondo (1996) suggest that countries' internal geography (and thus agglomeration) matter more to closed economies than to open economies, as domestic transactions become more important the less a country trades with the rest of the world and these transactions can in general be conducted more cheaply over shorter distances. Ades and Glaeser (1995), in a cross-section of 85 countries, indeed find a negative partial correlation between openness and urban concentration, but they remain skeptical as to the existence of a direct causal link.

While the literature now identifies a number of formally modelled channels through which agglomeration promotes economic growth, related empirical work remains comparatively scarce. There exists a body of historical scholarship on the link between urbanization, spatial inequality, industrialization and economic development, which strongly supports the view that these phenomena are positively related (Bairoch, 1993; Hohenberg and Lees, 1985; Hohenberg, 2004). Econometric studies of the impact of agglomeration on growth, however, are few in number. Henderson's (2003) extensive study remains the first and only rigorous cross-country analysis of the impact of urbanization on growth. He draws on a panel dataset covering up to 70 countries over the period 1960–1990, using dynamic panel estimation methods (difference GMM). He finds that urbanization *per se* has no significant growth-promoting effect, but that urban primacy (the share of a country's largest city) is advantageous to growth in low-income countries. His results support the Williamson hypothesis: interaction terms with initial per-capita income are negative for both urbanization and urban primacy. Our paper differs from Henderson's (2003) study in six main respects: (1) we expand the world cross-country data set to up to 105 countries over the period 1960–2000, (2) as an alternative to urbanization measures we also use Theil indices of intra-country geographic concentration, computed from regional data for 16 Western European countries over the period 1975–2000, (3) we additionally estimate "Barro-style" cross-section regressions of long-run growth, (4) we employ system GMM estimation, which has been shown to have superior small-sample properties to the difference GMM estimator, (5) we focus in particular on the interactions of agglomeration variables with income and with openness, and (6) we consider sector-level growth effects of agglomeration as well as aggregate patterns.

The only other explicit study of the growth effects of agglomeration, to our knowledge, is by Crozet and Koenig (2007), who exploit data for EU regions over the 1980–2000 time span to explore the effect of spatial concentration of economic activity within regions on the growth performance of those regions. Their evidence points towards growth-promoting effects of agglomeration: regions with a more uneven internal spatial distribution of production appear to grow faster.[2]

Our aim is to explore the causal link running from agglomeration to growth, mediated by stage of development and openness. We assemble the most comprehensive database used for this purpose to date, combining cross-section and panel data analysis of a large country-level dataset with panel analysis of sectorally and regionally disaggregated data for Europe. This allows us to experiment with a variety of agglomeration variables and model specifications. Since agglomeration is unlikely to have the same growth effects across sectors, we investigate the impact of agglomeration on the growth of individual sectors in addition to studying aggregate economic growth.

The paper is structured as follows. Section 2 discusses methodological issues concerning the specification and estimation of empirical growth models. Our results are reported in Section 3 (for a world-wide cross-country dataset) and in Section 4 (for a regionally and sectorally disaggregated European dataset). A concluding summary and discussion is provided in Section 5.

2. How to estimate the impact of agglomeration on growth

2.1. Choosing relevant determinants of growth

In the absence of an all-encompassing theoretical model, choosing the controls to include in an empirical growth model is far from trivial. Historically, there have been as many regression specifications as there have been empirical papers on the determinants of growth, and it is difficult to find variables whose influence on growth has not been found significant at least once in the literature.[3]

We base the selection of control variables on the study by Sala-i-Martin et al. (2004)—the most comprehensive specification search we know of. They explore the explanatory power of 67 variables in growth regressions covering a cross section of 88 countries over the 1960–1996 period (without however considering intra-country agglomeration measures). Drawing from the large number of possible permutations of variables allowed by this data set, they estimate 89 million randomly chosen regression specifications. Based on those estimations, they compute "posterior inclusion probabilities" as a gauge for the robustness of regressors.[4] They find that 18 of their 67 variables are "significantly related to growth," and that a further three variables are "marginally related to growth."

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[1] Henderson et al. (2001) discuss spatial deconcentration trends in fast-growing economies such as Korea, Brazil and Mexico, as well as associated impediments to optimal deconcentration.

[2] We might also mention Gallup et al. (1999), who, based on a sample of 75 countries, run a regression of GDP growth over the 1965–1990 period as a function of a number of explanatory variables measured in 1965. These explanatory variables include measures of output density in coastal areas and in interior areas. They find that, while high coastal density is conducive to growth, interior density has the opposite effect. Ades and Glaeser (1995) report a cross-section regression of growth from 1970 to 1985 across 85 countries on a set of initial-year variables that includes the population share of the country's largest city and the overall urbanization rate. Both urbanization variables are found to have a statistically significant negative effect on growth.

[3] For a comprehensive overview, see the relevant papers in the Handbook of Economic Growth (Aghion and Durlauf, 2005).

[4] The posterior inclusion probability measures the (weighted) average goodness-of-fit of models that include a particular regressor relative to the goodness-of-fit of models that do not include it.

Where possible, we therefore include the 18 variables singled out by Sala-i-Martin et al. (2004) as controls. For a list of these variables, see Appendix A. In addition, we include four explanatory variables that did not make it into the list of robust regressors reported by Sala-i-Martin et al. (2004) (*Population growth rate, Higher education, Fertility*, and *Investment share*).[5] These variables are considered in order to control as exhaustively as possible for within-country determinants of growth that are not related to agglomeration in the panel estimations. We also check for robustness of our results to the omission of all control variables.

Our main variables of interest, however, are those that represent agglomeration. One variable that could be considered as capturing agglomeration, *Population density*, is part of the Sala-i-Martin et al. (2004) list of regressors that are "marginally related to growth" (yielding predominantly positive estimated coefficients). This variable can be interpreted as capturing agglomeration *between* countries. While this is surely an effect we need to control for in an empirical model whose central hypothesis concerns the growth effects of spatially concentrated economic activity, our primary focus will be on variables that measure agglomeration *within* countries.

We employ two types of agglomeration measures, according to the availability of data in the two settings we work on. First, we draw on a "world sample" of up to 105 countries, allowing us to maximize the number of cross-sectional observations. In this sample, we use urbanization measures as proxies for agglomeration. We consider three variables: a country's population share living in cities whose population exceeds 750,000 in the year 2000 (*Urban750*), a country's population share living in areas described as cities by national statistics (*Urban*), and the share of urban population that lives in the largest city (*Primacy*). Second, we draw on an "EU sample" of 16 European countries. The appeal of this data set is that it provides us with regionally and sectorally disaggregated information. Thus we can compute geographic concentration indices explicitly rather than having to rely on proxy measures related to urbanization rates. Aggregate and sectoral geographic concentration is measured by Theil indices computed from regional employment data. These indices are scaled to regional area, thus measuring "topographic concentration": a uniform distribution of employment over physical space represents the zero-agglomeration benchmark and thus a zero value of the Theil index. The stronger the deviation of this distribution from uniformity, the higher the value of index (for details, see Brülhart and Traeger, 2005).

Our two main hypotheses are considered in the estimated equations as follows. An interaction term with initial per-capita GDP allows us to test for varying growth effects of agglomeration at different levels of economic development (the "Williamson hypothesis"). To test the "openness hypothesis," we interact agglomeration measures with variables that capture countries' openness to international trade.

Henderson (2003) finds that urbanization, measured as the ratio of urbanized population over total population, has less of an effect on economic growth than urban primacy, measured as the percentage of urbanized people living in the largest city. We therefore include the share of the biggest city over urbanized population (*Primacy*) as an additional regressor in the world sample. In order to capture possible non-linear effects, we also consider squared terms of the urbanization measures.

[5] Note that the Sala-i-Martin et al. (2004) results are based on cross-section regressions. An additional reason for including *Population growth* and *Investment share* is that they are the key variables of empirical Solow growth models. *Openness* and *Primary exports* are our panel data replacements for the variables *Years open* and *Mining* respectively in the cross-section model.

2.2. Estimation

2.2.1. Cross section regressions

Growth regressions are traditionally estimated employing one of two strategies: "Barro-type" cross-section regressions of long-term growth rates (typically over 25 or more years) on initial values or on long-term averages of explanatory variables; and panel regressions, using multiple-year time intervals to purge the dependent variable from short-term cyclical effects. The main advantage of cross-section estimation is that it allows us to draw on larger country samples and sets of variables, due to better data availability. In turn, the main advantages of panel estimation are that it allows us to control for omitted or unmeasurable country-specific time-invariant effects, and that it offers potentially valuable instruments for endogenous and/or mismeasured variables in the form of transformed lagged values of those variables (see, e.g., Temple, 1999).[6]

We adopt both approaches in conventional fashion. Our cross-section regressions are estimated via OLS, where average GDP growth of country i over period p, g_{ip}, is estimated as a function of log initial GDP, y_{i0} (to capture conditional convergence in income levels), an agglomeration variable, A_{i0}, and a set of control variables mainly based on the Sala-i-Martin et al. (2004) study (see above).[7] Hence, our estimating equation takes the form:

$$g_{ip} = \alpha y_{i0} + \beta A_{i0} + \boldsymbol{\gamma} \mathbf{X}_{ip} + u_i, \tag{1}$$

where $\mathbf{X}$ is the vector of the K control variables (measured during or at the start of period p), and u is a well behaved error term. The set of control variables includes the interaction terms with A_{i0} that represent our two hypotheses of main interest.

2.2.2. Panel regressions

We apply the "system GMM" approach initially proposed by Arellano and Bover (1995) for dynamic panel estimation. This requires us to rewrite Eq. (1) for a dynamic setting. The growth equation we seek to estimate has the following form:

$$y_{it} - y_{i,t-1} = \alpha y_{i,t-1} + \beta A_{i,t-1} + \boldsymbol{\gamma} \mathbf{X}_{it} + \mu_i + \nu_t + \varepsilon_{it}, \tag{2}$$

where t denotes 5-year intervals, and μ, ν, and ε are well behaved stochastic terms. This equation is equivalent to a simple AR(1) specification:

$$y_{it} = \alpha' y_{i,t-1} + \beta A_{i,t-1} + \boldsymbol{\gamma} \mathbf{X}_{it} + \mu_i + \nu_t + \varepsilon_{it}, \tag{3}$$

with $\alpha' = (\alpha + 1)$. The component of this specification that motivates the use of panel estimation is μ_i, a country-specific effect representing time-invariant determinants of income per capita that may or may not be correlated with agglomeration. If such effects exist and are important, any cross-section estimate of β (and of α' and $\boldsymbol{\gamma}$) based on lags of the same variables as instruments is bound to be biased.

Arellano and Bond (1991) propose a dynamic panel GMM estimator for models such as (3), based on first-differencing the data, thus eliminating the panel-specific effects (μ_i), and instrumenting all potentially endogenous variables ($y_{i,t-1}, A_{i,t-1}, \mathbf{X}_{it}$) with their own levels, lagged twice and more. This estimator relies on the assumption that the initial conditions are predetermined, so that

[6] Recall that in most relevant theoretical models, agglomeration causes growth and growth causes agglomeration (see Baldwin and Martin, 2004, for an overview). The latter effect arises for instance if growth is biased towards sectors that are subject to particularly strong agglomeration economies. We seek to isolate the impact of agglomeration on growth.

[7] To simplify exposition, we consider only a single agglomeration variable here, although our regressions will contain several variables designed to capture agglomeration effects.

$E[y_{i1}\varepsilon_{it}] = E[A_{i1}\varepsilon_{it}] = E[x^k_{i1}\varepsilon_{it}] = 0$, for $t = 2, \ldots, T$, $i = 1, \ldots, N$, and $k = 1, \ldots, K$ and it is consistent in N, the number of countries, given T.

The Arellano–Bond estimator is vulnerable to certain frequently encountered features of economic data. Specifically, it has been shown to behave poorly in small samples when α' approaches unity and/or when the variance of μ_i is large compared to the variance of ε_{it} (Blundell and Bond, 1998). Given the slow nature of changes in the internal geography of nations, these configurations would appear particularly relevant in our context. In such conditions, lagged levels represent weak instruments for the first-differenced variables, and Arellano–Bond estimates suffer from considerable finite-sample bias.

The system GMM estimator, initially proposed by Arellano and Bover (1995), is shown by Blundell and Bond (1998) to yield potentially dramatic improvements over the Arellano–Bond estimator in small samples. Bun and Windmeijer (2007) demonstrate that system-GMM consistently has the smallest bias of the dynamic GMM estimators, for the simple reason that it is a weighted average of difference GMM (Arellano–Bond) and levels GMM, and that the biases of those two estimators have opposite signs (with the former generally of larger magnitude).

System GMM is based on a system composed of first differences instrumented on lagged levels, and of levels instrumented on lagged first differences. For system GMM to be valid, the following assumption needs to hold:

$$E[\Delta A_{it}\mu_i] = E\left[\Delta x^k_{it}\mu_i\right] = E[\Delta y_{i2}\mu_i] = 0. \qquad (4)$$

The main aim of our estimation strategy is to minimize simultaneity bias, in order to isolate the causal effect that runs directly from agglomeration to growth. Hence, it is important that we scrutinize the assumptions underlying system GMM carefully in terms of our empirical context. A sufficient condition for (4) to hold is that A_{it}, x^k_{it} and y_{it} be mean stationary. This might be considered a strong assumption for our study, in particular as it would rule out, without theoretical or empirical justification, the possibility of secular income trends. However, the inclusion of the time effects ν_t allows for a common trend in income without violating (4). The stationarity assumption therefore reduces to positing that there are no secular diverging trends of relative international income levels.

Yet, this stationarity assumption might still appear constraining. We therefore note that assumption (4) does not necessarily require mean stationarity. As shown by Blundell and Bond (1998), (4) merely implies that the initial-period-specific disturbance ε_{i1} be uncorrelated with μ_i. Finally, if the underlying process has been generating the national income series for long enough prior to the observed period, or if the initial-period disturbances are randomly distributed, the true initial-period conditions become negligible, and conditional mean-stationarity of the agglomeration (and control variable) series is enough to satisfy (4). These assumptions can be considered weak enough to recommend system GMM for the estimation of empirical growth models (Bond et al., 2001).

In addition to providing a remedy for the simultaneity problem, dynamic panel GMM estimation presents two further advantages. First, it is more robust to measurement error than cross-section regressions. Time-invariant additive measurement error is absorbed into region-specific effects, and through suitably long lags delineating the instrument sets for y_{it} (in first-differences and in levels), dynamic panel GMM remains consistent even in the presence of country-year specific (but serially uncorrelated) measurement error. Second, dynamic panel GMM remains consistent even if agglomeration (as well as other controls) is endogenous in the sense that $E[A_{it}u_{is}] \neq 0$ for $s \leqslant t$, if the instrumental variables are sufficiently lagged.

Due to the bias implied by overfitting the endogenous regressors, it is standard to run tests of overidentifying restrictions after dynamic panel GMM estimation. We systematically report the Sargan test statistic and its associated p value. Furthermore, we limit the maximum lag length of the instrument sets to three throughout, which Bowsher's (2002) Monte Carlo results suggest to maximize the power of the Sargan test.[8]

3. Results: Urbanization and growth in a world sample

We first report estimations based on a world-wide cross-country sample. Our dataset contains up to 105 countries, over the period 1960–2000. Here we equate agglomeration with urbanization, focusing on *Urban750*, *Urban* and *Primacy* as our agglomeration variables.

3.1. Cross-country regressions

We begin with "Barro style" OLS cross-country estimations of the determinants of long-term growth (1960–1996). The size of our country sample for these estimations varies between 88 and 105, depending on the explanatory variables that are included in the regressions. Table 1 reports the relevant results with *Urban750* and *Primacy* measuring intra-national agglomeration. In columns (1) and (3), we show estimates of the full model, which includes all variables identified by Sala-i-Martin et al. (2004) as strongly related to growth, plus the four additional controls (*Higher education*, *Fertility*, *Investment share* and *Population growth rate*) that are included for the sake of consistency with our later panel specifications.[9] Columns (2) and (4) report results for a parsimonious model, excluding controls that are not found to be statistically significant in our data; and columns (3) and (6) report results with all controls excluded.

As usual, the cross-section growth regressions perform well, explaining up to 88 percent of sample variance in growth rates. When statistically significant, the estimated coefficients are stable across the six specifications, and all present the expected signs. The estimated coefficients on initial GDP strongly support the prior of conditional convergence. Other controls that are significantly and robustly related to growth are *Life expectancy*, *Mining*, *Years open*, and *Confucian*, which are found to have a positive effect on growth, and initial *Investment price* and *Tropics*, which have a negative effect. The fact that some of the controls turn out not to be statistically significant is not surprising, as Sala-i-Martin et al. (2004) use Bayesian averaging across some 89 million OLS regressions, whereas we report only four specifications.[10]

Turning now to our main focus of interest, we observe that the OLS results are consistent with the Williamson hypothesis: while the main effects of both *Urban750* and *Primacy* are positive, their interactions with initial-year GDP per capita are negative. These

[8] We report robust standard errors applying the correction suggested by Windmeijer (2005). We report Sargan test statistics rather than Hansen J tests, because we find a preponderance of implausibly good p values of 1.00, which is a well-known phenomenon related to the low power of the Hansen test when instrument sets are large (see Roodman, 2008). The drawback of the Sargan test is that it assumes homoskedasticity. As we consistently found the Sargan test to be more conservative than the Hansen test, we report the former throughout. While no formal test has yet been developed for weak instruments in system-GMM estimation, we find that our identifying instruments in the OLS equivalents of the implied first-stage regressions are statistically significant throughout. Furthermore, our system-GMM regressions systematically pass the "bounds test" on the lagged dependent variable, suggesting relative unbiasedness (see Bond et al., 2001). Estimations are performed using the xtabond2 package for Stata 9.0, written by David Roodman.

[9] Appendix A lists variable sources and definitions.

[10] In the basic kitchen-sink OLS regression reported by Sala-i-Martin et al. (2004), with the exception of the investment price, none of the 18 variables that are found overall to be "significantly related to growth" is statistically significant.

Table 1
World sample, cross-section estimation, *Urban750*

Dependent variable: per capita GDP growth rate, 1960–1996	(1)	(2)	(3)	(4)	(5)	(6)
Urban750	1.307e−03	9.734e−04	5.135e−04	8.549e−04	7.734e−04	1.239e−03
	(1.64)	(1.47)	(0.74)	(1.02)	(1.14)	(2.20)**
Urban750 squared	−7.828e−07	1.884e−06	−1.702e−06	−1.676e−06	8.507e−07	−4.210e−06
	(0.30)	(0.83)	(0.53)	(0.62)	(0.32)	(1.26)
Urban750 ∗ ln(*initial GDP per capita*)	−8.453e−05	−8.462e−05	−2.480e−05	−5.380e−05	−7.556e−05	−1.104e−04
	(0.90)	(1.04)	(0.25)	(0.52)	(0.88)	(1.27)
Urban750 ∗ *Years open*	−6.958e−04	−5.247e−04	4.930e−05	−3.439e−04	−2.719e−04	5.929e−04
	(2.40)**	(2.32)**	(0.37)	(1.16)	(1.07)	(6.55)***
Primacy	1.443e−03	1.596e−03	1.380e−03			
	(2.19)**	(2.70)***	(1.90)*			
Primacy squared	1.077e−06	4.135e−07	−1.381e−06			
	(0.62)	(0.24)	(0.65)			
Primacy ∗ ln(*initial GDP per capita*)	−2.406e−04	−2.540e−04	−2.113e−04			
	(2.88)***	(3.31)***	(2.48)**			
Primacy ∗ *Years open*	4.402e−05	4.710e−05	8.987e−05			
	(1.88)*	(1.99)*	(3.98)***			
Population density	4.548e−06	2.490e−06		3.971e−06	4.264e−06	
	(0.21)	(0.59)		(0.19)	(1.17)	
ln(*initial GDP per capita*)	−1.239e−02	−9.478e−03	1.154e−03	−1.726e−02	−1.526e−02	−3.140e−03
	(3.87)***	(2.98)***	(0.19)	(6.72)***	(5.36)***	(0.52)
Primary schooling	−2.778e−03			−2.467e−03		
	(0.34)			(0.32)		
Higher education	4.088e−03			1.320e−04		
	(0.41)			(0.01)		
Population growth rate	−1.019e−01			−8.952e−02		
	(0.74)			(0.59)		
Life expectancy	1.263e−03	1.124e−03		1.242e−03	1.021e−03	
	(4.58)***	(5.11)***		(4.21)***	(4.38)***	
Fertility	4.802e−04			9.352e−04		
	(0.33)			(0.63)		
Mining	6.164e−02	6.965e−02		5.614e−02	6.452e−02	
	(2.97)***	(4.54)***		(2.60)**	(4.18)***	
Investment price	−2.672e−05	−3.179e−05		−2.763e−05	−2.773e−	
	(2.11)**	(3.03)***		(2.32)**	05(2.57)**	
Investment share	5.962e−06			1.434e−06		
	(0.06)			(0.01)		
Government share	−1.398e−05			1.833e−07		
	(0.17)			(0.00)		
Years open	1.693e−02	1.497e−02		2.465e−02	2.552e−02	
	(1.82)*	(2.13)**		(3.68)***	(4.53)***	
Ethnolinguistic fractionalization	−1.410e−03			7.341e−04		
	(0.29)			(0.15)		
Spanish colony	−3.318e−03			−5.459e−03		
	(0.60)			(0.85)		
Buddhist	4.721e−03			8.480e−03		
	(0.71)			(1.35)		
Muslim	5.243e−03	5.477e−03		6.146e−03	6.279e−03	
	(1.16)	(1.65)		(1.32)	(1.82)*	
Confucian	3.947e−02	5.277e−02		3.852e−02	5.040e−02	
	(3.49)***	(7.61)***		(3.16)***	(7.10)***	
Malaria	1.227e−03			1.618e−03		
	(0.26)			(0.35)		
Tropics	−5.798e−03	−8.005e−03		−8.486e−03	−9.452e−03	
	(1.31)	(3.29)***		(1.81)*	(3.58)***	
Coast	−6.258e−07			7.373e−07		
	(0.03)			(0.04)		
East Asia	3.447e−03			3.822e−03		
	(0.83)			(0.89)		
Africa	−6.711e−03	−5.077e−03		−5.267e−03	−4.241e−03	
	(1.74)*	(1.90)*		(1.33)	(1.64)	
Latin America	−2.207e−03			4.973e−04		
	(0.34)			(0.06)		
Constant	4.052e−02	2.716e−02	−2.940e−02	6.769e−02	6.849e−02	−1.981e−02
	(1.62)	(1.36)	(1.23)	(3.32)***	(4.46)***	(1.13)
Observations	88	100	105	88	100	107
R-squared	0.87	0.81	0.40	0.85	0.79	0.33

Note: Estimation by OLS with heteroskedasticity-consistent standard errors (absolute values of t statistics in parentheses). The dependent variable is the average growth rate of PPP per capita GDP between 1960 and 1996. *Fertility*, *Population growth rate*, *Investment price*, *Investment share* and *Government share* are measured as averages over 1960–1964; all other time variant variables are measured in 1960, with the exception of the index of malaria prevalence, measured in 1966, and that of costal population, measured in 1965.

* Statistically significant at 10%.

** Idem, 5%.

*** Idem, 1%.

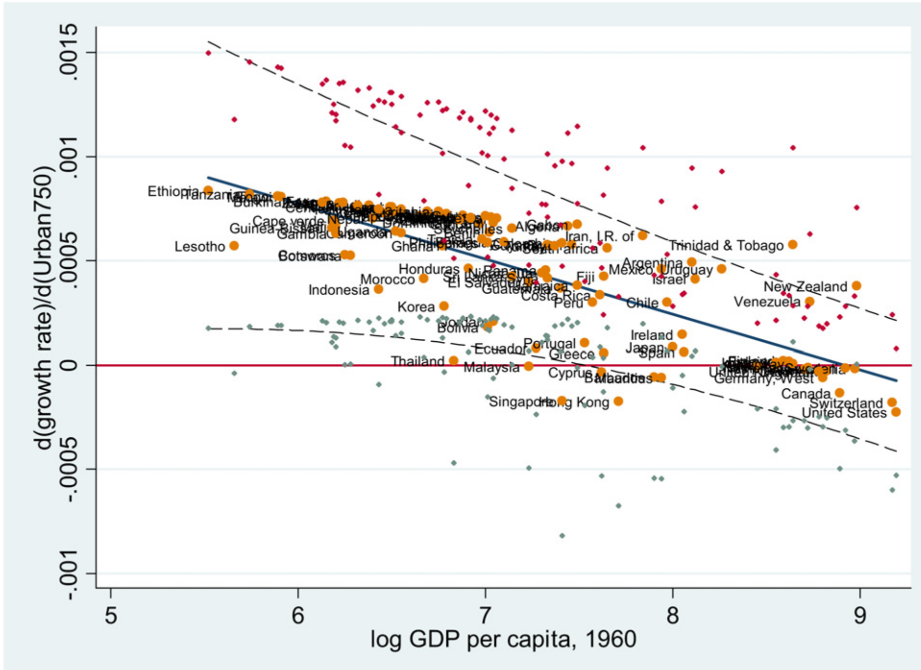

Fig. 1. Growth effects of urbanization implied by cross-section estimation in the world sample.

effects retain their signs across specifications, but they are statistically significant only in the case of *Primacy*.[11]

With respect to openness, our findings are not so consistent. In five of our six specifications, the estimated interaction terms with *Years open* are statistically significantly negative for *Urban750*, in line with our hypothesis on the role of openness. However, the corresponding coefficients are statistically significantly positive for *Urban750* in the most parsimonious specification (Table 1, column (6)) as well as for *Primacy* in all three estimation runs. These results run counter to the starting hypothesis.

Finally, we do not find strong evidence supporting non-linear effects, as square terms on *Urban750* are statistically significant only in the specifications featuring no controls, and square terms on *Primacy* are never statistically significant.

Due to our inclusion of interaction terms, we cannot read the average effect of urbanization on growth from the estimated coefficients directly. We can, however, compute the implied effects of variations in one particular regressor given the observed (or any other) value of the variables this regressor is interacted with. Fig. 1 provides an illustration. Based on the coefficients of Table 1, column (1), we show the implied derivatives of growth with respect to *Urban750* at actual levels of openness and initial-year GDP per capita. We sort countries horizontally by initial-year GDP per capita. We also report upper and lower 95-percent confidence bounds for each estimated point, and fit a quadratic line to those bounds to form an illustrative "pseudo confidence interval." This plot illustrates the consistency of our estimates with the Williamson hypothesis: while population concentration in large cities appears as a spur to growth in poor countries, it has no or even negative growth effects in rich countries. Fig. 1 also shows that, in our sample, most countries were still at a level of development where urbanization was positive for growth.

Fig. 2 provides an equivalent illustration of the growth effect of *Primacy*, again based on the estimates of Table 1, column (1). In line with Henderson's (2003) findings, we observe that, in spite of the positive estimated main effect, the interaction terms imply that *Primacy* has a negative impact on growth for the large majority of sample countries.

3.2. Panel regressions

Our second step is to explore the growth effects of our agglomeration proxies in the world sample using SYS-GMM estimation. This allows us to mitigate concerns about the influence of unmeasured time–invariant country-specific effects that could correlate with our included regressors and thus bias the cross-section estimates.

The panel sample comprises between 84 and 105 countries, depending on which regressors are included. The time span covered is 1960–2000, subdivided into eight five–year intervals. All variables are taken as deviations from their corresponding year means, thus controlling for ν_t in (2). Moreover, we treat all time dependent regressors as potentially endogenous; hence we instrument their first differences with past levels (from $t-2$ and backward for variables that are measured as averages over each five–year interval and from $t-1$ and backward for all variables measured at the start of each interval) and their current values in the level equations with lagged first differences.[12]

Table 2 reports our results for *Urban750* and *Primacy*. Columns (1) and (4) again show results for the full model, including all regressors we consider, columns (2) and (5) report comparable estimates with a parsimonious set of controls, and columns (3) and

[11] Appendix Table 1 shows the same regressions but with *Urban* instead of *Urban750*. Positive main effects on the agglomeration variables and negative interaction effects with initial GDP per capita dominate those estimations too. If we divide the sample into developing countries and other countries using current UN definitions, we find that *Urban750* has a statistically significantly positive growth effect in the former subgroup but no significant effect among the richer countries. The effect of *Primacy*, however, is estimated to be positive but not statistically significant in both subgroups. We have estimated a number alternative specifications (only *Primacy*, dropping squared terms, dropping interaction terms) and found the salient results to be robust.

[12] We limit the number of instruments by including a maximum of three lags, in order to avoid rejection of the null for the validity of overidentifying restrictions.

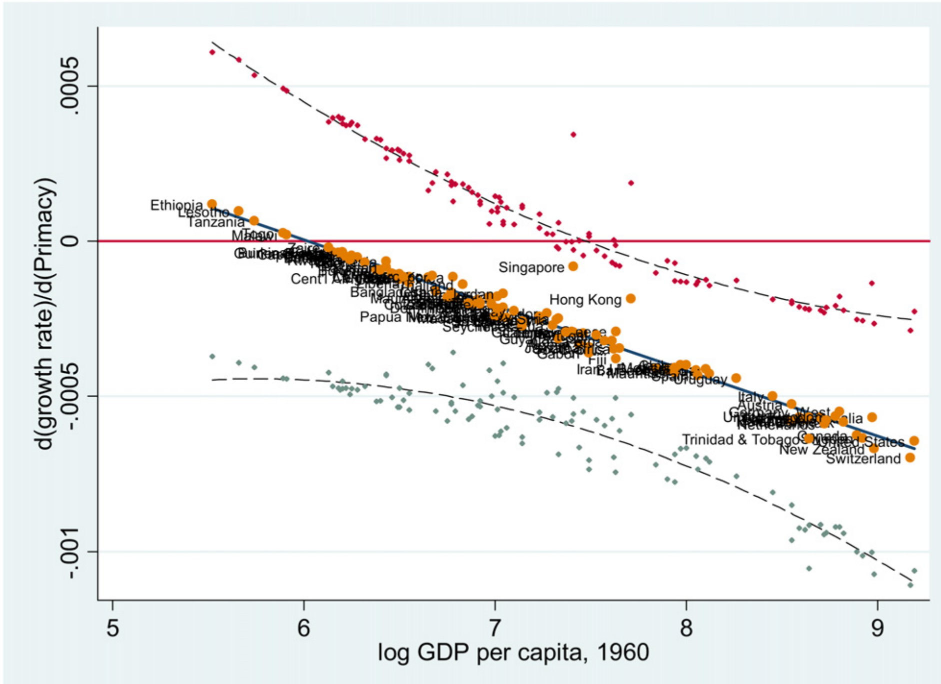

Fig. 2. Growth effects of urban primacy implied by cross-section estimation in the world sample.

Table 2
World sample, dynamic panel estimation, *Urban750*

Dependent variable: 5-year growth rates of per-capita GDP	(1)	(2)	(3)	(4)	(5)	(6)
Urban750	4.438e−03	3.433e−03	2.356e−03	4.871e−03	3.418e−03	2.723e−03
	(4.01)***	(3.78)***	(2.47)**	(4.52)***	(3.69)***	(2.82)***
Urban750 squared	3.992e−06	2.760e−06	1.054e−05	8.153e−06	8.898e−06	1.207e−05
	(0.72)	(0.54)	(2.35)**	(1.28)	(1.47)	(3.31)***
Urban750 ∗ ln(*lagged GDP per capita*)	−4.767e−04	−3.698e−04	−2.985e−04	−5.565e−04	−4.058e−04	−3.843e−04
	(3.38)***	(3.44)***	(2.58)**	(4.00)***	(3.77)***	(3.26)***
Urban750 ∗ *Openness*	−5.186e−06	−3.712e−06	−2.086e−06	−4.332e−06	−4.502e−06	−5.476e−07
	(3.87)***	(3.23)***	(1.46)	(3.64)***	(3.75)***	(0.51)
Primacy	5.187e−04	2.167e−04	8.969e−04			
	(0.68)	(0.36)	(1.09)			
Primacy squared	5.263e−08	2.868e−07	8.751e−08			
	(0.12)	(0.66)	(0.16)			
Primacy ∗ ln(*lagged GDP per capita*)	−8.114e−05	−3.898e−05	−1.466e−04			
	(0.93)	(0.58)	(1.55)			
Primacy ∗ *Openness*	2.627e−06	4.748e−07	2.160e−06			
	(1.83)*	(0.47)	(2.56)**			
Population density	3.530e−05	2.793e−05		3.998e−05	2.982e−05	
	(2.72)***	(2.74)***		(3.24)***	(3.13)***	
ln(*lagged GDP per capita*)	−7.242e−03	−1.093e−02	1.562e−02	−5.950e−03	−7.753e−03	1.606e−02
	(2.00)**	(3.04)***	(3.17)***	(1.71)*	(2.22)**	(4.69)***
Primary schooling	−4.376e−04			−9.683e−04		
	(0.25)			(0.54)		
Higher education	2.355e−03			3.178e−03		
	(0.27)			(0.30)		
Population growth rate	−6.412e−02			5.754e−02		
	(0.31)			(0.25)		
Life expectancy	3.134e−04			1.867e−04		
	(0.80)			(0.48)		
Fertility	−3.148e−03	−6.487e−03		−3.514e−03	−5.661e−03	
	(1.20)	(3.74)***		(1.26)	(3.08)***	
Primary exports	5.980e−03	1.281e−02		1.134e−02	1.309e−02	
	(0.92)	(1.61)		(1.69)*	(1.38)	
Investment price	−5.408e−05	−5.515e−05		−5.892e−05	−5.131e−05	
	(6.35)***	(5.47)***		(6.03)***	(4.41)***	
Investment share	6.242e−04	1.200e−03		6.326e−04	1.257e−03	
	(2.91)***	(6.11)***		(2.46)**	(5.34)***	
Government share	−4.054e−04	−2.458e−04		−3.988e−04	−3.247e−04	
	(3.36)***	(1.66)*		(3.16)***	(2.01)**	

Table 2 (*continued*)

Dependent variable: 5-year growth rates of per-capita GDP	(1)	(2)	(3)	(4)	(5)	(6)
Openness	7.585e−05	1.150e−04		1.480e−04	1.399e−04	
	(1.19)	(1.45)		(2.13)**	(2.27)**	
Ethnolinguistic fractionalization	−2.534e−03			−3.710e−03		
	(0.41)			(0.59)		
Spanish colony	−8.657e−03	−7.671e−03		−1.171e−02	−7.468e−03	
	(1.42)	(1.73)*		(2.14)**	(1.52)	
Budhist	5.659e−03			8.855e−03		
	(0.83)			(1.20)		
Muslim	−7.266e−03	−9.731e−04		−1.047e−02	8.438e−04	
	(1.55)	(0.19)		(2.12)**	(0.15)	
Confucian	−2.446e−03			−3.875e−03		
	(0.20)			(0.34)		
Malaria	1.126e−03			9.169e−04		
	(0.24)			(0.18)		
Tropics	−1.182e−02	−9.923e−03		−1.390e−02	−9.141e−03	
	(2.79)***	(2.47)**		(2.90)***	(2.14)**	
Coast	−3.511e−05	−2.271e−05		−4.310e−05	−3.028e−05	
	(2.20)**	(1.79)*		(2.61)**	(2.29)**	
East Asia	2.151e−03			9.549e−04		
	(0.35)			(0.14)		
Africa	−1.266e−02	−9.662e−03		−1.138e−02	−7.587e−03	
	(2.26)**	(2.40)**		(2.09)**	(1.87)*	
Latin America	−4.936e−03			−4.413e−03		
	(0.64)			(0.61)		
Constant	2.006e−02	1.304e−02	−6.981e−04	2.298e−02	1.288e−02	−5.964e−04
	(3.25)***	(2.99)***	(0.52)	(3.44)***	(2.49)**	(0.44)
Countries	84	104	113	84	105	114
Observations	567	678	898	567	683	906
Sargan	453.32	387.29	322.32	364.10	268.53	147.69
	(0.23)	(0.05)	(0.00)	(0.14)	(0.18)	(0.08)
AR1	−4.71	−5.09	−3.94	−4.74	−5.11	−3.95
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR2	−0.90	−0.21	1.31	−0.89	−0.01	1.44
	(0.37)	(0.83)	(0.19)	(0.37)	(0.99)	(0.15)

Note: Estimation by System GMM (absolute values of robust t statistics in parentheses). The time span goes from 1960 to 2000 and variables are calculated over 5-year intervals. The dependent variable is the annual average growth rate of PPP per capita GDP between year $t-5$ and year t. *Fertility, Population growth rate, Investment price, Investment share, Government share* and *Openness* are calculated as averages over each 5-year period; all other time dependent variables are measured at $t-5$. Instruments used for the equations in first differences are past levels of each time varying variable from $t-1$ for predetermined variables and from $t-2$ for the others up to the third lag. Variables in first differences starting at $t-1$ are used as instruments for level equations. In all equations the maximum number of lags of past variables used as instruments is limited to 3. P-values for the null hypotheses of the usual diagnostic tests are reported in parentheses at the end of the table.

* Statistically significant at 10%.
** Idem, 5%.
*** Idem, 1%.

(6) report estimates without consideration of any controls.[13] The statistically significant control variables again have their expected signs. While all tests for second-order autocorrelation are satisfactory, three of the six reported regressions are associated with significant Sargan test statistics, which suggests some caution due to potential overfitting bias.[14]

Our main results from the cross-section estimations are confirmed. The Williamson hypothesis is again supported. While, as in the cross-section regressions, the estimated main effects of *Urban750* and *Primacy* are positive, the interactions with per-capita GDP are consistently negative.[15] Contrary to the cross-section estimates, it is the coefficients on *Urban750* that are statistically significant here rather than those on *Primacy*.

Fig. 3 illustrates the implied effect of *Urban750*, analogously to Fig. 1, based on the estimates of Table 2, column (1). Again, we find that the growth effect of large cities turns negative beyond a certain GDP threshold. It is in fact striking how similar the critical threshold implied by the panel estimates is to that implied by the cross-section estimates. Both in Fig. 1 and in Fig. 3, the fitted lines cross the horizontal axis at a level just below 9, implying that the growth effect of large cities turns negative at an income level of some 10,000 dollars per capita (in 2006 prices), which roughly corresponds to the 2006 income levels of Brazil or Bulgaria.[16]

The corresponding effect of *Primacy* is shown in Fig. 4. This illustrates that, unlike *Urban750*, *Primacy* is estimated to have no systematic impact on growth in the panel regressions.

[13] Due to data availability, *Openness* now replaces *Years open* and *Mining* is now replaced by *Primary exports* (see Appendix A for definitions).

[14] Reducing lag lengths of the instrument set (from three to two or one) does not qualitatively change the coefficients in any of those three estimated specifications, while it does in some instances produce satisfactory Sargan test statistics. Therefore, while we cannot rule out overfitting, the resulting biases do not seem to be of first-order magnitude.

[15] Appendix Table 2 shows the same regressions but with *Urban* instead of *Urban750*. The same patterns regarding the agglomeration variables are evident in those sets of results. We have estimated a number alternative specifications (only *Primacy*, dropping squared terms, dropping interaction terms) and found the salient results to be robust.

[16] The critical threshold is given by $e^9 * 1.24 = 10{,}048$, where 1.24 is the progression of the US GDP deflator from 1996 to 2006. According to the IMF's World Economic Outlook Database, the 2006 PPP per-capita GDP of Brazil was USD 10,073 while that of Bulgaria was 10,022.

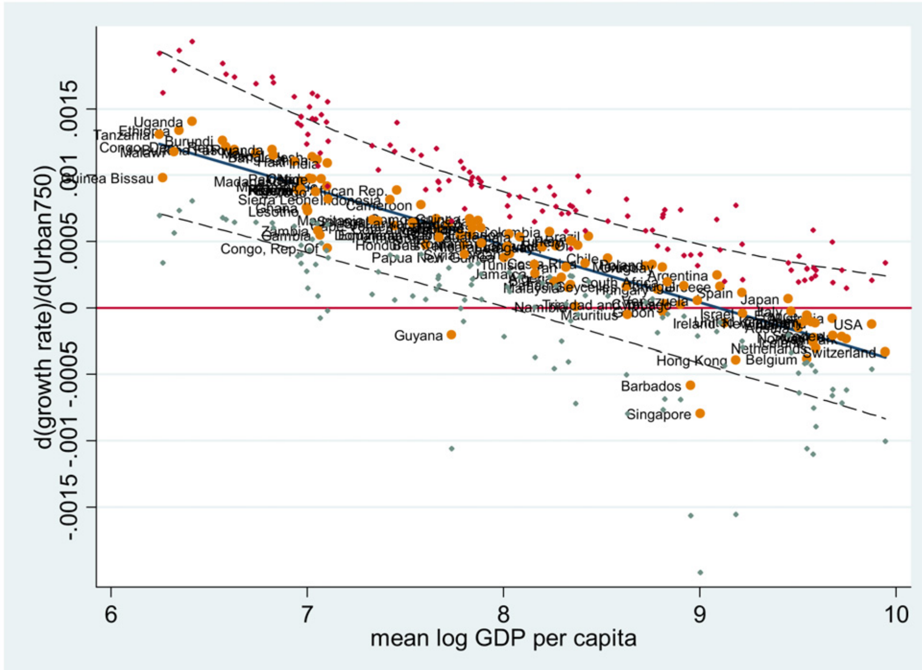

Fig. 3. Growth effects of urbanization implied by dynamic panel estimation in the world sample.

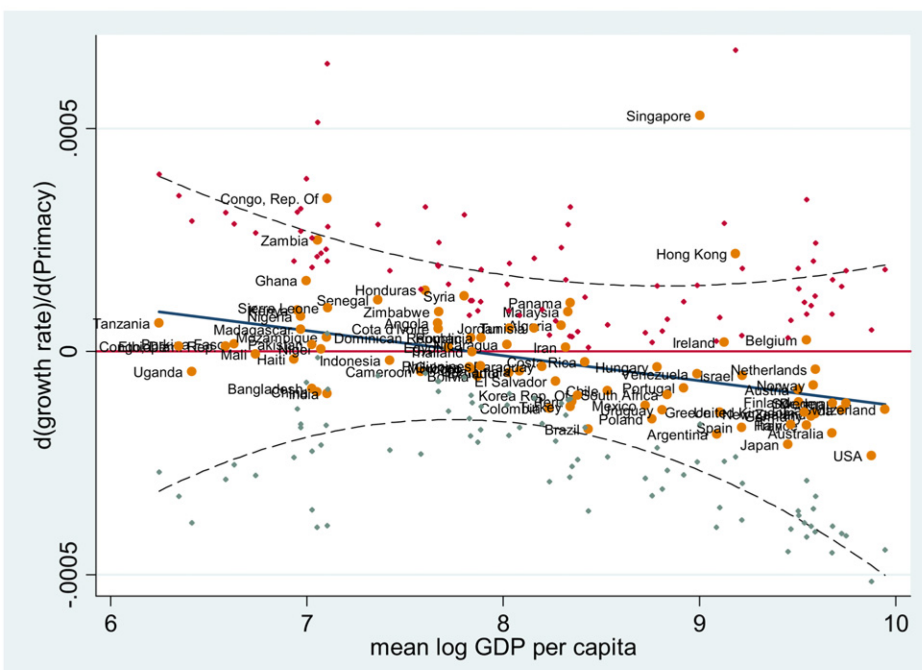

Fig. 4. Growth effects of urban primacy implied by dynamic panel estimation in the world sample.

While the Williamson hypothesis emerges strongly from the dynamic panel estimates, our findings for openness are again contradictory. According to our results of Table 2, greater trade openness reduces the growth-promoting effect of urbanization, which is in line with our working hypothesis. However, the results also suggest that openness enhances the growth-promoting effect of urban primacy, which runs against the hypothesis.

Except for the most parsimonious specification featuring no controls (Table 2, column (6)), the dynamic panel estimations also do not yield any statistically significant coefficient on the squares of our agglomeration variables, confirming the absence of such non-linear effects.

4. Results: Agglomeration and growth in an EU sample

As a complement to our world-wide cross-country dataset, we draw on information for 16 Western European countries for 1960 to 2000. We again organize the data into five–year intervals and express all variables as deviations from the relevant period means.

Table 3
EU sample, dynamic panel estimation, aggregate model

Dependent variable: per capita GDP growth rate	*Urban750*		*Aggregate Theil*	
Agglomeration index	7.936e−06	−1.267e−04	4.509e−01	2.934e−01
	(0.00)	(0.04)	(2.23)**	(1.62)
Agglomeration index ∗ ln(*lagged GDP per capita*)	−2.049e−05	−6.141e−05	−4.829e−02	−3.126e−02
	(0.05)	(0.18)	(2.35)**	(1.74)
Agglomeration index ∗ *Openness*	3.732e−06	1.658e−05	3.140e−04	−9.903e−05
	(0.35)	(2.23)**	(1.56)	(0.53)
Primacy	1.379e−03		4.108e−03	
	(0.51)		(0.85)	
Primacy ∗ ln(*lagged GDP per capita*)	−2.130e−04		−5.324e−04	
	(0.71)		(1.03)	
Primacy ∗ *Openness*	1.045e−05		1.789e−05	
	(2.01)*		(4.48)***	
Population density	1.063e−05	9.781e−06	2.705e−06	−5.089e−05
	(0.83)	(0.74)	(0.21)	(11.30)***
ln(*lagged GDP per capita*)	−3.600e−02	−3.337e−02	1.103e−02	9.846e−03
	(2.83)**	(4.69)***	(0.45)	(0.78)
Higher education	3.221e−02	1.985e−02	1.328e−02	−2.254e−02
	(2.59)**	(2.11)*	(1.04)	(2.29)**
Investment share	1.204e−03	8.575e−04	3.245e−04	−2.027e−04
	(2.96)***	(2.69)**	(0.85)	(0.66)
Government share	−7.551e−05	−3.084e−04	−1.839e−04	−3.111e−04
	(0.38)	(1.59)	(1.03)	(1.82)*
Openness	−2.022e−04	−1.512e−04	−3.297e−04	3.773e−04
	(1.33)	(0.97)	(1.80)*	(5.50)***
Countries	16	16	16	16
Observations	128	128	80	80
Sargan	125.16	112.26	67.00	70.00
	(0.07)	(0.23)	(0.34)	(0.34)
AR1	−2.58	−2.57	−2.17	−2.06
	(0.01)	(0.01)	(0.03)	(0.04)
AR2	1.50	1.44	0.77	0.57
	(0.13)	(0.15)	(0.44)	(0.57)

Note: Estimation by System GMM (absolute values of robust t statistics in parentheses). The time span goes from 1960 to 2000 for *Urban750* and from 1975 to 2000 for *Aggregate Theil*. Variables are calculated over 5-year intervals. The dependent variable is the annual average growth rate of PPP per capita GDP between year $t-5$ and year t. *Investment share, Government share* and *Openness* are calculated as averages over each 5-year period; all other variables are measured at $t-5$. Instruments are past levels of each variable at $t-1$ for predetermined variables and at $t-2$ for the others. P-values for the null hypotheses of the usual diagnostic tests are reported in parentheses at the end of the table.
* Statistically significant at 10%.
** Idem, 5%.
*** Idem, 1%.

The advantage of the EU sample is that, for the sub-period 1975–2000, it offers intra-country information on the regional distribution of employment. This allows us to compute measures of agglomeration that come closer to capturing spatial concentration than the urbanization measures used in the world sample. Our principal agglomeration variables here are within-country topographic Theil indices of aggregate employment (*Aggregate Theil*) and of own-sector employment (*Sector Theil*), representing the degree to which aggregate and sectoral employment is concentrated over physical space within each country.[17]

Many of the controls introduced in the regressions based on the world sample are redundant in purely European data. We concentrate on two sets of controls. One approach is to introduce the traditional variables suggested by the Solow growth model, *Initial value added*, *Higher education* and *Investment share*. To these we add *Government share*, to take account of different degrees of public-sector involvement, and *Openness*, one of our variables of central interest.[18]

4.1. Spatial concentration, urbanization and aggregate growth

In order to examine the consistency of any results based on the EU sample with those based on the world sample, we first use *Urban750* as well as *Aggregate Theil* as our agglomeration measure and estimate the model for aggregate growth.[19]

These results are reported in Table 3. Given that we now have a much smaller sample, consisting of 16 countries, it is not surprising that we find relatively fewer statistically significant coefficients than in Table 2. The estimates on which statistical significance is found, however, mostly conform with expectations.

The estimates based on *Urban750* and *Total topographic* are qualitatively similar, and they are broadly in line with those found for

[17] The index is defined as follows (dropping country and time subscripts for simplicity): $AggregateTheil = \sum_r \frac{E_r}{\sum_r E_r} \log \frac{\frac{E_r}{A_r}}{\frac{\sum_r E_r}{\sum_r A_r}}$, where r denotes a sub-national region, E_r is the region's employment, and A_r is the region's area. *Sector Theil* is defined analogously. This index increases in the degree of concentration in employment across physical space. The Theil index has a number of desirable statistical properties compared to conventional alternatives such as the Gini. For a full exposition and discussion, see Brülhart and Traeger (2005).

[18] Square terms of the agglomeration variables, when included, were never statistically significant. We therefore report linear specification only. Instead of *Population density* we include *Aggregate Theil*, the within-country concentration index for aggregate employment. For the sector-level regressions, we retain initial-period per-employee sectoral value added as the sole control as an alternative approach, in order to save degrees of freedom and to explore the robustness of our estimated coefficients on the agglomeration variables.

[19] The two raw variables are very weakly correlated. The correlation coefficient of the measures, in differences from their country means, amounts to 0.07, which is not statistically significant.

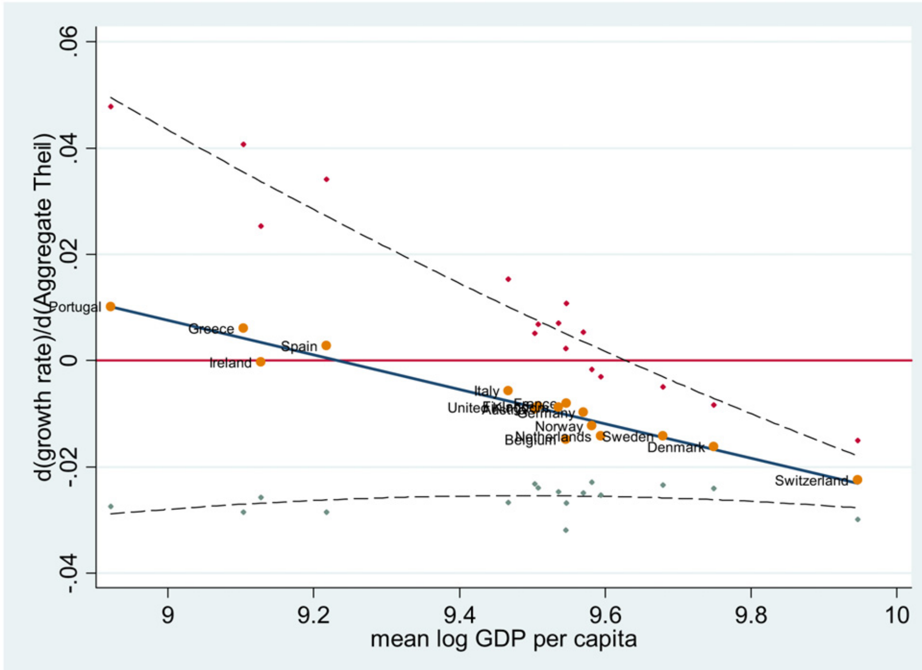

Fig. 5. Growth effects of agglomeration implied by dynamic panel estimation in the EU sample.

the world sample. The main effects are positive in three of our the four specifications, and the interaction effects with initial GDP per capita are negative throughout. Thus, we once more find support for the Williamson hypothesis.

The Williamson hypothesis is supported particularly strongly in the full specification with *Aggregate Theil* as our measure of agglomeration (Table 3, third column). The implied relationship between GDP per capita and the derivative of growth with respect to agglomeration is illustrated in Fig. 5. This suggests statistically significant growth-inhibiting effects for the five richest EU countries. The line fitted to the 16 sample points crosses the horizontal axis at a value of 9.2, implying that agglomeration turns negative for growth at a GDP per capita level of some 12,300 dollars in 2006 prices—remarkably close to the 10,000 dollar critical value estimated in a substantially different data set above (the value of 10,000 furthermore lies well within the pseudo confidence interval).

In the EU sample, five of our six estimated interaction terms of agglomeration measures with *Openness* are positive, with statistical significance found on three of them. These results run counter to the prediction whereby internal agglomeration becomes more important the less open a country is to international trade, thus confirming the conclusion that no clear-cut relationship can be discerned between openness and the growth effects of agglomeration.

4.2. Spatial concentration and sector growth

For the EU sample the data are disaggregated into eight broad sectors. We focus on the two sectors typically regarded as the main candidates for market-driven agglomeration forces: manufacturing and financial services.[20] The dependent variable considered is growth in sector-level value added in constant prices. We thus ask whether greater employment concentration across sub-national regions leads to faster growth in sectoral per-employee value added at the country level.

Our estimation results are shown in Table 4. For both sectors, we report regressions including the set of controls used in the aggregate regressions of Table 3, plus specifications without the four control variables. Furthermore, we consider specifications with and without controlling for the intra-country spatial concentration of aggregate activity (*Aggregate Theil*).

The results for manufacturing, reported in the first four data columns of Table 4, do not inspire confidence. The estimated coefficients on the Theil indices and their interaction terms are mostly statistically insignificant and very sensitive to the inclusion of controls. Furthermore, some estimates on the control variables, such as the negative effect of *Investment share*, are hardly plausible.

Somewhat stronger results appear for financial services. The signs of our estimated coefficients on the Theil indices and their interaction terms are robust to the exclusion of the controls. In three of the four specifications, the main effect as well as the interaction with lagged GDP per capita are estimated to be highly statistically significant. Interestingly, they suggest a "reverse Williamson hypothesis," whereby the growth effect of agglomeration would increase with income. We illustrate this effect, based on the estimates in column (5) of Table 4, in Fig. 6. This shows that spatial concentration of financial services begins to yield positive growth dividends at an income level of some 8000 dollars. Agglomeration effects in financial services would thus follow a different pattern along the development path from those of the economy as a whole. This finding, however, needs to be treated as suggestive at best, notably since the AR2 diagnostic test on all sector-level regressions is unsatisfactory.[21]

[20] See Appendix B.3 for a list of the eight sectors. Much of manufacturing and financial services activity may be considered footloose in principle but subject to agglomeration economies. Input–output linkages are important in much of manufacturing, while knowledge spillovers are likely to feature particularly prominently in advanced financial services. See Dekle and Eaton (1999), whose analysis of agglomeration effects also focuses on these two sectors.

[21] The "reverse Williamson" result furthermore is sensitive to the definition of the sectoral agglomeration measure. If, instead of the topographic definition of the *Sector Theil* used for the results shown in Table 4, we use a relative Theil index (i.e. scaled to aggregate regional employment rather than to area; see Brülhart and Traeger, 2005), the interaction with initial per-capita GDP becomes statistically insignificant.

Table 4
EU sample, dynamic panel estimation, sector level

Dependent variable: Per employee sector value added growth rate	Manufacturing				Financial services			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aggregate Theil	−0.0007		0.0431		−0.0487		−0.0401	
	(0.03)		(1.54)		(1.62)		(0.72)	
Sector Theil	−0.5418	−0.3924	0.3395	−0.0216	−0.6968	−0.6700	−0.4667	−0.3328
	(2.07)*	(1.65)	(0.94)	(0.05)	(3.25)***	(3.27)***	(2.40)**	(1.50)
Sector Theil ∗ ln(*lagged GDP per capita*)	0.0554	0.0402	−0.0426	−0.0028	0.0787	0.0714	0.0514	0.0338
	(1.96)*	(1.58)	(1.15)	(0.07)	(3.32)***	(3.27)***	(2.33)**	(1.51)
Sector Theil ∗ *Openness*	0.0001	0.0001	0.0002	0.0005	−0.0004	−0.0003	−0.00001	−0.0001
	(0.15)	(0.22)	(0.37)	(1.17)	(2.66)**	(1.25)	(0.09)	(0.50)
ln(*lagged sector value added*)	−0.0242	−0.0151	−0.0222	−0.0255	−0.0109	−0.0158	−0.0121	−0.0121
	(3.26)***	(1.78)*	(1.23)	(1.26)	(1.95)*	(2.15)**	(1.31)	(1.11)
Higher education	−0.0578	−0.0699			−0.0441	−0.0400		
	(2.95)**	(3.12)***			(2.22)*	(2.07)*		
Investment share	−0.0015	−0.0020			−0.0017	−0.0023		
	(2.46)**	(2.78)**			(2.51)**	(3.40)***		
Government share	−0.0016	−0.0013			−0.0002	−0.0006		
	(3.62)***	(3.42)***			(1.01)	(1.56)		
Openness	0.0004	0.0004			0.0002	0.0002		
	(5.07)***	(4.35)***			(1.88)*	(1.61)		
Countries	16	16	16	16	16	16	16	16
Observations	80	80	80	80	80	80	80	80
Sargan	57.33	45.84	48.43	44.01	60.34	58.18	37.03	33.44
	(0.46)	(0.64)	(0.06)	(0.03)	(0.36)	(0.20)	(0.37)	(0.22)
AR1	−1.50	−1.50	−1.59	−1.66	−1.95	−1.97	−1.97	−2.00
	(0.13)	(0.13)	(0.11)	(0.10)	(0.05)	(0.05)	(0.05)	(0.05)
AR2	1.52	1.51	1.67	1.78	1.65	1.71	1.65	1.70
	(0.13)	(0.13)	(0.10)	(0.08)	(0.10)	(0.09)	(0.10)	(0.09)

Note: Estimation by System GMM (absolute values of robust t statistics in parentheses). The time span goes from 1975 to 2000 and variables are calculated over 5-year intervals. The dependent variable is the annual average growth rate of gross sector per employee value added between year $t-5$ and year t. *Investment share*, *Government share* and *Openness* are calculated as averages over each 5-year period; all other variables are measured at $t-5$. Instruments used for the equations in first differences are past levels of each time varying variable from $t-1$ for predetermined variables and from $t-2$ for the others up to the second lag. P-values for the null hypotheses of the usual diagnostic tests are reported in parentheses at the end of the table.

* Statistically significant at 10%.
** Idem, 5%.
*** Idem, 1%.

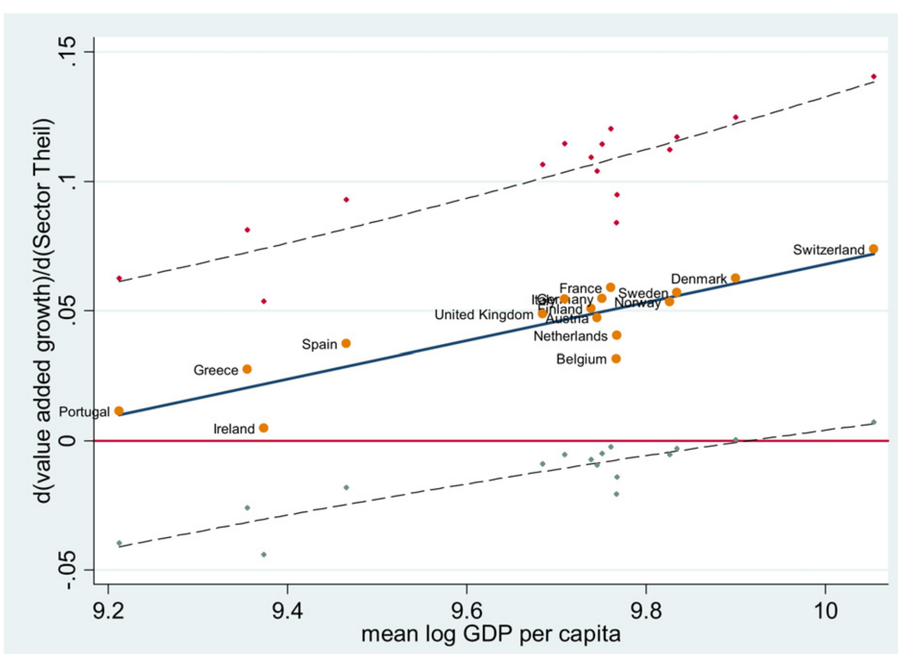

Fig. 6. Growth effects of sectoral agglomeration in financial services implied by dynamic panel estimation in the EU sample.

5. Conclusions

We empirically investigate the impact of within-country spatial concentration of economic activity ("agglomeration") on country-level growth, using cross-section OLS and dynamic panel GMM estimation and two complementary data sets, a large world-wide country sample and a data set for Western Europe featuring regionally and sectorally disaggregated information. Agglomeration is measured alternatively via measures of urbanization and through indices of spatial concentration based on data for sub-national regions.

Across estimation techniques, data sets and variable definitions, we find consistent evidence supporting the "Williamson hypothesis": agglomeration boosts GDP growth only up to a certain level of economic development. The critical level is estimated at around 10,000 US dollars in 2006 prices, corresponding roughly to the current development level of Brazil or Bulgaria. This implies that the benefits of agglomeration will become increasingly unimportant, and that the tradeoff between national growth and inter-regional equity may gradually lose its relevance as the world economy continues to grow. Conversely, it also means that it is in the poorest countries where policies aimed at inhibiting spatial economic concentration are most damaging in terms of foregone growth.

The aggregate pattern surely masks considerable heterogeneity across sectors and contexts. Our estimates for example also suggest that the growth effects of financial-sector agglomeration increase as countries get richer—consistent with a "reverse Williamson hypothesis" for this industry.

The hypothesis that increasing openness to trade weakens any growth-promoting effects of intra-country agglomeration, however, is not consistently supported by our estimations, and non-linear effects of agglomeration never turned out to be statistically significant.

Our results need be interpreted with a certain dose of caution. Although we took much care in trying to ascertain the robustness of the reported results, there are no limits to the number of additional sensitivity tests that could be applied in terms of data, variable definitions, model specification and econometric identification techniques. Our EU sample in particular is rather small for us to have strong confidence in the dynamic panel results. Even the seemingly weak identifying assumptions underlying panel GMM estimation may reasonably be called into question in this context (Duranton, 2008). Measurement is fraught with difficulties: the urbanization rate may confound spatial concentration with other economic or political features of a country, and our Theil indices of intra-country concentration may be affected by the modifiable areal unit problem (MAUP).

Perhaps the most delicate issue concerns spatial scale. All the measures we considered as proxies for agglomeration are constructed at the level of entire nations. However, there is mounting evidence that the most relevant spillover effects occur at a rather small spatial scale, within cities and regions rather than between them (e.g. Rosenthal and Strange, 2004, 2008; Duranton and Overman, 2005). Local clustering economies—which exist below the radar of this study—may be as strong as ever in developed as well as in developing economies.[22] Our research, however, suggests that spatial concentration *at the level of entire countries* may become increasingly irrelevant, or even detrimental, for economic growth.[23] This would imply that the tradeoff between national growth and inter-regional equity will lose its relevance as national economies continue to grow.

Given the statistical caveats that apply, our findings should be taken as suggestive at best. In view of the policy implications at stake, this question surely merits further scientific scrutiny.

Appendix A. Variable sources and definitions

Where applicable, we report the rank of a variable according to the posterior inclusion probabilities computed by Sala-i-Martin et al. (2004) in square brackets. Note that in building up variables for the EU and the panel sample, when data for year 2000 were not available, observations were replaced by data for 1999.

Dependent variable:

- *GDP per capita*: real GDP per capita in 1996 US$ (annual growth rate when used as regressand, base–year level when used as regressor). Source: Penn World Tables 6.1. [4]
- *Per-employee value added*: sector-level growth variable from Cambridge Econometrics Regional Database.

Agglomeration variables:

- *Urban750*: people living in cities with more than 750,000 inhabitants in year 2000, as a share of total population. Source: UN, World Urbanization Prospects, 2001 Revision.
- *Urban*: people living in areas defined as urban in each country, as a share of total population. Source: World Bank, Global Development Network Growth Database.
- *Primacy*: urban population living in the biggest city. Source: UN, World Urbanization Prospects, 2001 Revision.
- *Aggregate Theil*, *Sector Theil*: Theil index for intra-country spatial distribution of sectoral employment, "topographic" definition (Brülhart and Traeger, 2005). Source: authors' calculations based on the Cambridge Econometrics Regional Database.

Control variables:

- *Africa*: dummy for Sub-Saharan African countries. Source: World Bank, Global Development Network Growth Database. [10]
- *Buddhist*: Buddhists as share of population. Source: Sala-i-Martin et al. (2004). [16]
- *Coast*: costal (within 100 km of coast line) population per costal area in 1965. Source: Gallup et al. (2001). [6]
- *Confucian*: Confucians as a share of population. Source: Sala-i-Martin et al. (2004). [9]
- *East Asia*: dummy for East Asian countries. Source: World Bank, Global Development Network Growth Database. [1]
- *Ethnolinguistic fractionalization*: index for the probability of two random people in the same country not speaking the same language. Source: Easterly and Levine (1997). [17]
- *Fertility*: fertility rate. Source: World Bank, World Development Indicators. [36]
- *Government share*: government consumption as a share of GDP. Source: Penn World Tables 6.1. [18]
- *Higher education*: percentage of higher education attained in total population. Source: Barro and Lee (2001). [25]
- *Investment price*: price level of investment expenditure basket on PPP basis. Source: Penn World Tables 6.1. [3]
- *Investment share*: private–sector investment as a share of GDP. Source: Penn World Tables 6.1.
- *Latin America*: dummy for Latin American Countries. Source: World Bank, Global Development Network Growth Database. [11]

[22] Brülhart and Mathys (2008) find that the productivity-boosting effects of employment density at the level of sub-national regions in Western Europe have in fact been increasing over the last three decades.

[23] One possible explanation could be that the geographic scope of the relevant agglomeration economies shrinks with the level of economic development. Based on data for US states, counties and zipcode areas, Rosenthal and Strange (2001) found that agglomeration effects due to shipping costs and availability of inputs mainly arise at the largest spatial scale (states), but agglomeration effects due to knowledge spillovers only appear at the smallest spatial scale (zipcodes). Our results are consistent with a world in which the former type of agglomeration effects is more important at earlier stages of economic development while the latter effects come to dominate at more advanced stages of development.

- *Life expectancy*: life expectancy at birth in total years. Source: World Bank, Global Development Network Growth Database. [8]
- *Malaria*: percentage of 1995 population living in areas with malaria in 1966. Source: Gallup et al. (2001). [7]
- *Mining*: mining output as a share of GDP. Source: (Sala-i-Martin et al., 2004). [12]
- *Muslim*: Muslims as share of population. Source: Sala-i-Martin et al. (2004). [15]
- *Openness*: exports plus imports as a share of GDP. Source: Penn World Tables 6.1. [22]
- *Population density*: people per square km. Source: World Bank, World Development Indicators. Data for 1960 have been substituted by data for 1961. [19]
- *Population growth rate*: average annual population growth rate. Source: Penn World Tables 6.1. [56]
- *Primary exports*: primary (i.e. agricultural and raw materials) exports as a percentage of merchandise exports. Source: UNCTAD, Handbook of Statistics 2003. [27]
- *Primary schooling*: percentage of primary schooling attained in total population. Source: Barro and Lee (2001) and Barro and Lee (1993). [2]
- *Spanish colony*: dummy for former Spanish colonies. Source: Sala-i-Martin et al. (2004). [13]
- *Tropics*: percentage of land area in geographical tropics. Source: Gallup et al. (2001). [5]
- *Years open*: number of years a country has been "open" between 1950 and 1994. Source: Sachs and Warner (1995). [14]

Appendix B. Countries and sectors

The lists for the world data samples given below represent the total number of countries for which we obtained the required data. Some of the variables are reported missing for some of these sample countries. This is the reason why the number of observations in the regressions is not necessarily equal to the maximum sample size and may change according to the variables that are included as regressors.

B.1. World cross-section sample

Algeria, Angola, Argentina, Australia, Austria, Bangladesh, Barbados, Belgium, Benin, Bolivia, Botswana, Brazil, Burkina Faso, Burundi, Cameroon, Canada, Cape Verte, Central African Republic, Chad, Chile, Colombia, Comoros, Congo, Costa Rica, Côte d'Ivoire, Cyprus, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Ethiopia, Fiji, Finland, France, Gabon, Gambia, West Germany, Ghana, Greece, Guatemala, Guinea-Bissau, Guyana, Haiti, Honduras, Hong Kong, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Korea, Lesotho, Liberia, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Portugal, Rwanda, Senegal, Seychelles, Singapore, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syria, Taiwan, Tanzania, Thailand, Togo, Trinidad & Tobago, Tunisia, Turkey, Uganda, United Kingdom, United States, Uruguay, Venezuela, Zaire (Democratic Republic of Congo), Zambia, Zimbabwe (108 countries).

B.2. World panel-data sample

Algeria, Angola, Argentina, Australia, Austria, Bangladesh, Barbados, Belgium, Benin, Bolivia, Botswana, Brazil, Burkina Faso, Burundi, Cameroon, Canada, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Democratic Republic of Congo, Costa Rica, Côte d'Ivoire, Cyprus, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Ethiopia, Fiji, Finland, France, Gabon, Gambia, Germany, Ghana, Greece, Guatemala, Guinea, Guinea Bissau, Guyana, Haiti, Honduras, Hong

Appendix Table 1
World-sample, cross-section estimation, *Urban*

Dependent variable: per capita GDP growth rate, 1960–1996	(1)	(2)	(3)	(4)	(5)	(6)
Urban	−1.211e−04	6.789e−04	5.135e−04	−7.215e−04	3.820e−05	1.239e−03
	(0.21)	(1.46)	(0.74)	(1.45)	(0.08)	(2.20)**
Urban squared	5.699e−07	2.716e−06	−1.702e−06	−1.511e−06	8.690e−07	−4.210e−06
	(0.19)	(1.24)	(0.53)	(0.55)	(0.36)	(1.26)
Urban ∗ ln(*initial GDP per capita*)	7.323e−06	−1.097e−04	−2.480e−05	1.109e−04	−6.919e−06	−1.104e−04
	(0.08)	(1.48)	(0.25)	(1.35)	(0.09)	(1.27)
Urban ∗ *Years open*	−2.619e−04	−3.286e−04	4.930e−05	−1.788e−04	−2.487e−04	5.929e−04
	(1.35)	(2.63)**	(0.37)	(0.96)	(1.66)	(6.55)***
Primacy	1.030e−03	1.426e−03	1.380e−03			
	(1.52)	(2.32)**	(1.90)*			
Primacy squared	1.481e−06	6.641e−07	−1.381e−06			
	(0.88)	(0.36)	(0.65)			
Primacy ∗ ln(*initial GDP per capita*)	−1.767e−04	−2.306e−04	−2.113e−04			
	(1.87)*	(2.96)***	(2.48)**			
Primacy ∗ *Years open*	1.890e−05	3.011e−05	8.987e−05			
	(0.71)	(1.21)	(3.98)***			
Population density	−1.219e−05	6.428e−06		−9.285e−06	7.278e−06	
	(0.52)	(1.58)		(0.43)	(2.53)**	
ln(*initial GDP per capita*)	−1.191e−02	−5.445e−03	1.154e−03	−1.932e−02	−1.444e−02	−3.140e−03
	(2.22)**	(1.04)	(0.19)	(5.52)***	(3.27)***	(0.52)
Constant	3.936e−02	−2.710e−04	−1.529e−03	8.577e−02	6.391e−02	2.480e−02
	(1.07)	(0.01)	(0.04)	(3.19)***	(2.38)**	(0.66)
Control variables	22	8	0	22	8	0
Observations	88	100	105	88	101	107
R-squared	0.87	0.81	0.40	0.85	0.80	0.33

Note: Estimation by OLS with heteroskedasticity-consistent standard errors (absolute values of t statistics in parentheses). The dependent variable is the average growth rate of PPP per capita GDP between 1960 and 1996. Non-reported control variables, where included, are the same as in Table 1.

* Statistically significant at 10%.
** Idem, 5%.
*** Idem, 1%.

Appendix Table 2
World sample, dynamic panel estimation, *Urban*

Dependent variable: 5-year growth rates of per-capita GDP	(1)	(2)	(3)	(4)	(5)	(6)
Urban	1.809e−03	8.609e−04	2.014e−03	1.435e−03	1.094e−03	2.263e−03
	(3.03)***	(1.29)	(3.05)***	(2.19)**	(1.49)	(2.97)***
Urban squared	−2.300e−07	−2.930e−06	5.607e−06	−2.389e−06	6.261e−07	8.468e−06
	(0.06)	(0.49)	(0.90)	(0.54)	(0.10)	(1.24)
Urban ∗ ln(*initial GDP per capita*)	−2.090e−04	−7.040e−05	−3.017e−04	−1.318e−04	−1.268e−04	−3.686e−04
	(2.18)**	(0.53)	(2.31)**	(1.21)	(0.87)	(2.67)***
Urban ∗ *Openness*	−1.058e−06	−1.414e−06	2.664e−06	−6.944e−07	−2.505e−06	2.148e−06
	(1.03)	(1.19)	(1.68)*	(0.56)	(1.93)*	(2.15)**
Primacy	1.440e−03	1.094e−03	1.135e−03			
	(2.02)**	(1.59)	(1.75)*			
Primacy squared	−4.371e−07	−2.724e−07	−5.811e−08			
	(1.08)	(0.62)	(0.14)			
Primacy ∗ ln(*initial GDP per capita*)	−1.730e−04	−1.251e−04	−1.642e−04			
	(2.06)**	(1.56)	(2.10)**			
Primacy ∗ *Openness*	−5.157e−08	−6.512e−07	5.954e−07			
	(0.05)	(0.62)	(0.60)			
Population density	9.701e−06	4.479e−06		−9.000e−07	5.881e−06	
	(1.12)	(0.64)		(0.16)	(0.94)	
ln(*initial GDP per capita*)	−3.128e−03	−9.338e−03	2.148e−02	−1.089e−02	−7.900e−03	2.184e−02
	(0.57)	(1.03)	(2.75)***	(1.65)	(0.81)	(2.42)**
Constant	1.582e−02	1.051e−02	−5.069e−04	1.427e−02	1.100e−02	−3.510e−04
	(2.80)***	(2.33)**	(0.35)	(2.57)**	(2.32)**	(0.25)
Control variables	21	11	0	21	11	0
Countries	84	104	113	84	105	116
Observations	567	678	898	567	683	922
Sargan	458.22	391.06	345.09	385.51	292.75	216.95
	(0.18)	(0.04)	(0.00)	(0.03)	(0.03)	(0.00)
AR1	−4.65	−5.09	−4.15	−4.72	−5.04	−4.20
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR2	−0.78	−0.27	1.35	−0.81	−0.10	1.47
	(0.44)	(0.78)	(0.17)	(0.42)	(0.92)	(0.14)

Note: Estimation by system GMM (absolute values of robust t statistics in parentheses). The time span goes from 1960 to 2000 and variables are calculated over 5-year intervals. The dependent variable is the annual average growth rate of PPP per capita GDP between year $t-5$ and year t. Non-reported control variables, where included, are the same as in Table 2. Instruments used for the equations in first differences are past levels of each time varying variable from $t-1$ for predetermined variables and from $t-2$ for the others up to the third lag. Variables in first differences starting at $t-1$ are used as instruments for level equations. In all equations the maximum number of lags of past variables used as instruments is limited to 3. P-values for the null hypotheses of the usual diagnostic tests are reported in parentheses at the end of the table.

* Statistically significant at 10%.
** Idem, 5%.
*** Idem, 1%.

Kong, Hungary, Iceland, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kenya, Korea, Lesotho, Luxembourg, Madagascar, Malawi, Malaysia, Mali, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Romania, Rwanda, Senegal, Seychelles, Sierra Leone, Singapore, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syria, Taiwan, Tanzania, Thailand, Togo, Trinidad & Tobago, Tunisia, Turkey, Uganda, United Kingdom, Uruguay, United States, Venezuela, Zaire (Democratic Republic of Congo), Zambia, Zimbabwe (115 countries).

B.3. EU sample

Countries (number of regions): Austria (9), Belgium (10), Denmark (3), Finland (6), France (22), Germany (31), Greece (13), Ireland (2), Italy (20), Netherlands (12), Norway (19), Portugal (5), Spain (18), Sweden (21), Switzerland (7), United Kingdom (37); Total: 16 countries, 235 regions.

Sectors: agriculture, construction, manufacturing, distribution services, transport and communication services, financial services, other market services, non-market services.

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