Public Infrastructure, Education, and Economic Growth: Region-Specific Complementarity in a Half-Century Panel of States

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by

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Abstract

We find region-specific complementarity between investments in public infrastructure and education, both k-12 and postsecondary. The complementarity helps to explain how regions capture returns to investments in education even when residents are mobile, and is strong enough for the effect of tax-financed expenditures on either public infrastructure or education to be significantly *positive* when spending on the other is high, even though the independent effect of either one is *negative*. Effects are identified using a recursive structure, very long lags, GMM-instrumental variables, and multiple controls for heterogeneity. Estimates are robust across identification strategies, estimators, and instruments.

Introduction

The rapidly burgeoning literature on the effects of education on economic growth emphasizes two closely related channels: traditional production spillovers, the extent to which broad public investments in education raise the productivity of labor and other factors of production, as in Cohen and Paul (2004), Evans and Karras (1994), Duffy-Deno and Eberts (1991), and Pereira (2000); or more narrowly, the role of higher education in innovation and technological change, as in Aghion *et. al.,* (2009) and Ciccone and Papiannou (2009). Both channels are likely important, but neither helps to explain *why* regions invest in human capital, given that residents are mobile and that additional human capital makes them more mobile still. We pursue this question, and find evidence of region-specific complementarity between public investments in education and infrastructure, both physical and institutional. The localized complementarity provides one plausible explanation for how regions might retain returns to public investments in human capital: *coordinated* investments in public infrastructure and education induce locally specific spillovers, raising productivity, wages, and incomes relative to regions that do not make similar investments.

Contribution

A number of previous studies, including those cited above, find that increased regional spending on education or public infrastructure has positive effects on the performance of regional economies. None of these studies, however, explicitly account for the opportunity cost of the increased expenditures. All face the problem of the endogeneity of public expenditure, which is likely to bias coefficients in favor of finding a positive effect of public spending on growth. None account for the possibility that education and infrastructure are complements in the producing growth. Our study, then, is distinguished from prior studies in accounting explicitly for the opportunity cost of increased tax expenditures, adopting several different strategies to deal with endogeneity, and accounting for possible complementarity between public education and infrastructure.

Opportunity cost

We account explicitly for the opportunity cost of increased expenditures on public education or infrastructure via a fully specified government budget constraint: we estimate growth effects of state and local expenditures on public infrastructure and education, while simultaneously accounting for the opportunity cost (in terms of growth) of the corresponding taxes. By fully specifying the budget constraint, one can gauge the effect of a change in any element of the budget, net of the effect of a compensating change in any other element. Though widely ignored in empirical specifications for regional growth, the theoretical rationale for this approach to opportunity cost is set out in Barro (1989), and the corresponding empirical rationale is set out in Helms (1985), Mofidi and Stone (1990), Kneller *et al.,* (1999), and more recently, Bania, *et. al.,* (2007). Empirically, the fully specified government budget constraint is necessarily comprised of perfectly collinear elements, requiring that at least one budget element be omitted to identify the remaining parameters. Thus, the estimated parameters reflect the effect of a change in the corresponding variable, *given* a compensating change in the omitted budget element. We omit taxes and fees, so that parameters on expenditure categories, *e.g.*, public infrastructure and education reflect the joint effect of these expenditures and the corresponding taxes and fees necessary to support them.

Identification and endogeneity

A second factor distinguishing our work is that fifty years of panel data enable us to identify the spillover effects of public education and infrastructure on growth using several alternative empirical strategies. We begin by specifying a recursive structure with very long, generation-length lags and by incorporating both state and time fixed effects. We then assess the success of this strategy in three ways. First, we compare the equivalence of least-squares and GMM instrumental-variable estimates when the instruments are both empirically powerful and statistically exogenous, i.e., when one would expect a divergence if the least-squares estimator were biased. We then explore the sensitivity of our results to several controls for time-varying and spatial factors that could contribute to endogeneity bias. Finally, we exploit the half-century of data to apply dynamic time-series tests for Granger causality. Our standard for robustness is convergent evidence among all three approaches.

Complementarity

Our findings suggest that the direct effect on growth of tax-financed expenditures on either public education or infrastructure is significantly *negative*,[1] but the complementarity, or spillover effect of each on the other is positive enough when the level of the other is high to make their total effects significantly positive. Our findings are broadly consistent with the agglomeration-type economies found by Cohen and Paul (2004), the complementarities among various forms of both public and private capital found by Pereira (2000)—as well as the complementary spillovers between education and *institutional* infrastructure (e.g., judicial systems and public safety) found by Hanushek (2007) for a panel of developing countries.

[1] A standard Cobb-Douglas production function, for example, implies a non-positive productivity effect for investments in education or infrastructure when the level of the other is zero, which leaves only the negative burden of taxes required to finance the investments. Evans and Karras (1994) also find either insignificant or negative effects for public infrastructure alone.

Data

Our data for state fiscal variables are available for 1957-2007 from the Census of Governments at five-year intervals[2]; from the Bureau of Labor Statistics for the state unemployment rate; and from the Bureau of Economic Analysis, Department of Commerce for state real personal income per capita. We exclude Alaska as an outlier due to the idiosyncratic dominance of the Alaska pipeline and extreme variance in its state fiscal variables relative to the other 49 states. Thus, we have data for 49 states at five-year intervals from 1957 to 2007.

Table1 presents summary statistics for the dependent and explanatory variables. Our dependent variable is GROWTH, the (log percent) growth in real personal income per capita in each state. The average value for GROWTH for the five-year data interval is approximately 12.1 percent which corresponds to approximately 2.4 percent per annum. The key fiscal variables are TAXES_FEES, the ratio of all state and local taxes and fees to state personal income. As described above, taxes and fees become the reference variable when they are replaced by the remainder of the government budget constraint, and eliminated from the estimated equation. Thus, the effect of a change in any element of the budget constraint is estimated net of the effect of a compensating change in taxes and fees. Other fiscal variables are FED, the ratio of federal governmental transfers to personal income; EDUC, the proportion of personal income devoted to public expenditures on education (both higher education and K-12); INFRA, the proportion of personal income devoted to public spending on physical and institutional infrastructure[3] excluding EDUC; OTHER, redistributive and consumption-oriented expenditures[4]; and finally,

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[2] Equivalent data are not available annually until 1977.
[3]We distinguish between expenditures on physical infrastructure (e.g., highways) and institutional infrastructure (e.g., public safety) in the extensions of our primary empirical specification reported below. Consistent with the complementarity Hanushek and Woessman (2007) find for education and institutional infrastructure across a sample of developing countries, we find no significance in the distinction in our data.
[4] INFRA includes expenditures on transportation, public safety, environment and housing, government administration, interest payments, and a small residual category of expenditures ‘not elsewhere classified’. OTHER

SURPLUS, the budget surplus,[5] expressed as a percentage of personal income (negative if a deficit).[6]

Empirical Specifications

Theory suggests taxes and public investments in infrastructure and education have an effect on economic growth.[7] Consistent with Barro (1990), these can be permanent, as in other recent endogenous growth models, or apply only to transitions, which Barro, *et. al.*, (1995) and Turnovsky (2004) demonstrate could nevertheless, take decades.

Our primary specification has 25-year moving averages for each of the government fiscal variables, *i.e.*, we use average values over the previous 25 years, but we find qualitatively similar results using a specification with a discrete 25-year lag.[8] For education, this generation-length lag incorporates a full cohort of kindergarten through college students, plus several years of 'time-to-build' factors in education. Although the use of long lags in a recursive structure substantially mitigates the issues of endogeneity and the spurious correlation arising from short-term, cyclical, and other factors, our empirical specifications pursue several additional strategies to mitigate these issues. We include fixed effects for both state and time, observed time-varying controls (the state unemployment rate), and a lagged dependent variable. The latter helps to account for both temporal dynamics and unobserved time-varying heterogeneity, which might arise from state-specific variation over time in factors such as private and public capital stocks,

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includes expenditures on public welfare, hospitals, health, social insurance administration, and veterans services. INFRA is constructed by subtracting EDUC and OTHER from total public expenditures.

[5] For U.S. states, deficits and surpluses are typically small, averaging only about 0.2 percent of personal income. While we include the deficit/surplus in our primary specification as a control variable, our results are not sensitive to its inclusion.

[6] Also, consistent with other studies (*e.g.*, Helms 1985 and Mofidi and Stone 1990), we treat UI expenditures as outside the regular fiscal structure, in part because UI is largely driven federally, with separate accounting.

[7] As with most studies (an exception being Bania *et al.*, 2007) we focus only on linear effects at the margin, not on nonlinear effects.

[8] We test the sensitivity of our results to the choice of 25 year lags and find similar results when using 20 and 30 year lags.

privately financed investment in education, and spatial interactions.[9] As a further sensitivity check, we account for regionally idiosyncratic effects among neighboring states by incorporating variables for tax-financed investments in public infrastructure and education by other states in the same Census region.[10] These specifications result in no substantial change in our primary estimates.

We use instrumental-variable techniques and generalized method-of-moment (GMM) estimators to account both for potential endogeneity and for the inconsistency of dynamic fixed-effects models in samples with a finite number of time periods. In our case, the number of periods (11) is substantially less than the number of states (49) included, so the Arellano-Bond (1991) and Arellano-Bover (1995) style GMM estimators are appropriate. These estimators use lagged values of the dependent variable (in our case 15 to 20-year lags), and other exogenous or predetermined regressors as instruments.[11] As previously noted, we test the power and exogeneity of the instruments and assess the success of our approach by the extent to which results using alternative identification strategies converge.

Regression and GMM Results

Table 2 presents least-squares and GMM instrumental-variable estimates explaining GROWTH, the log change in state real personal income per capita (times 100). The number of discrete five-year interval lags follow variable names in parenthesis and any other number following a variable name indicates the number of prior years over which the lagged value of a variable is averaged. The fiscal variable omitted from the estimated equation in Table 2 is TAXES_FEES, the ratio of state and local taxes and fees to personal income (times 100), so that

[9] Our results are not sensitive to the inclusion of the lagged dependent variable in the specification.
[10] We use the standard Census Bureau definition for the nine census divisions.
[11] Applications of GMM instrumental variables are often sensitive to the number of lags used in the instrument set. Here this issue does not appear to be serious since our primary estimates are not sensitive, for example, to whether or not a lagged dependent variable is included.

changes in expenditures for any fiscal category are implicitly financed by a change in taxes and fees. All the specifications reported in Table 2 include fixed effects for period and state, as well as time-varying effects captured by lagged GROWTH. Robust (panel-corrected) standard errors are presented.[12]

Key Findings

We begin with the simplest specifications in columns (1) and (2) and progress to our primary specifications in columns (3) and (4). In column (1), we include productive expenditures as a single variable (the sum of EDUC25 and INFRA25) and find a positive and marginally significant effect. In column (2), we enter EDUC25 and INFRA25 as separate variables and find that the coefficients on both variables are positive, but only the coefficient on INFRA is significant. We present OLS estimates of our primary specification in the third column, and GMM instrumental-variable estimates in the fourth column. The results in columns 3 and 4 are stable across the differences both in specifications and in the two estimation techniques. The coefficients for the independent, or direct, effects of lagged expenditures on education (EDUC25) and on public infrastructure (INFRA25) are negative and significant, while the coefficient on the interaction term capturing complementarity, (EDUC25*INFRA25), is significantly *positive*. The negative direct effects estimated for EDUC25 andINFRA25 suggest that any positive endogeneity bias is relatively small. The total impact of the direct and complementary effects is negative or positive depending on the levels of investment in EDUC25 and INFRA25. The growth effect of tax expenditures on health, welfare, transfer-payment and other entitlement programs (*i.e.*, OTHER25) is negative and significant; the coefficient for federal transfers (FED25) is positive, but insignificant, as is the coefficient for SURPLUS25.

[12] Estimations are performed in EViews 5.1. The robust standard errors, in this case Period SUR, are described in EViews 5: User's Guide (2004, p. 887).

For the education and infrastructure parameters, the GMM and OLS estimators yield roughly similar results. Given that the instruments are strong, with highly significant first-stage F-statistics, and that the J-statistic fails to reject the exogeneity of the instruments, the similarity in the OLS and GMM coefficients suggests that, in this case, the use of long lags with a recursive structure is a successful identification strategy. The robustness of the OLS estimates is bolstered, as well, by the results of tests of Granger causality, which indicate that tax investments in neither public infrastructure nor education *alone* 'Granger cause' growth (p= 0.095 and p= 0.2091, respectively), but the complementarity between them does (p= 0.0334.)[13]

Sensitivity Analysis

We subject our analysis to a variety of checks to determine how sensitive the results might be to various specifications. In the first column of Table 3, we estimate the model without the lagged dependent variable and find that the estimated coefficients are remain similar to our primary specification (presented in column 3 of Table 2). The second column of Table 3 presents the estimated coefficients for a specification without the inclusion of the surplus variable. Again, our results are essentially identical to our primary specification. Finally, we alter the model specification to allow for idiosyncratic effects among neighboring states. Specifically, we incorporate variables for tax investments in public infrastructure and education by states in the same Census region. These estimates are reported in the third column of Table 3, with no substantial changes from our primary specification.

We explore the sensitivity of our findings to alternative lag structures in Table 4 by estimating two alternative lag structures: a 30-year average (column 1) and a 20-year average (column 2) of the government fiscal variables. While the magnitudes of the coefficients vary

[13] Granger tests are performed using two 5-year lags, and EViews 5.1. Results are available on request. The p values reported are for tests of the null that the variable does *not* 'Granger cause' growth.

somewhat, the pattern of signs and statistical significance is not substantially different from the pattern in our primary specification.

Extensions

To test for possible differences between K-12 and higher education expenditures, we extend the primary specification in Table 2, replacing EDUC25 with separate measures of spending on K-12 and higher education in both the linear term and the interactive term. We find no statistically significant difference in the coefficients on the linear terms for K-12 and higher education. Similarly, we find no statistically significant difference in the interaction terms involving K-12 and higher education. Finally, we examine the total effects of K-12 and higher education on growth and again find no statistically significant differences, though this may be due to the large number of parameters estimated, collinearity in the two measures, or insufficient independent variation.

We follow a similar procedure to test the distinction between physical and institutional infrastructure. We separate INFRA25 into a broad measure of spending on public highways (HIGHWAYS25), which represents a substantial share of spending on physical infrastructure, and a residual (INFRA25-HIGHWAYS25), representing non-physical, or institutional, infrastructure.[14] As above, we replace INFRA25 in both the linear term and the interactive term in our primary specification with the two separate measures. In this case, as well, we find no statistically significant differences in the linear terms, the interactive terms, or the total effects of the separate measures. Here, too, the lack of significance in the distinction may be due to the number of parameters, collinearity, or insufficient variation.

[14] HIGHWAYS25 consists of average expenditures over the prior 25 years for the provision and maintenance of highway facilities, including toll turnpikes, bridges, tunnels, and ferries, as well as regular roads, highways, and city streets.

Conclusion

Evidence from a half-century panel of 49 U.S. states is consistent with significant region-specific complementarity between tax-financed expenditures on public infrastructure and tax-financed spending on both K-12 and higher education. The effect is identified using a recursive structure, very long lags with a half century of data, and extensive controls for both fixed and time-varying heterogeneity. Robustness is probed in several ways, including comparisons of the similarity of OLS and instrumental-variable estimates when the instruments are both empirically powerful and statistically exogenous, tests of Granger-causality, alternative lag structures for both the instruments and the tax expenditure variables, and inclusion of a set of variables to account for spatial interaction among neighboring states. The region-specific complementarity we find offers one plausible explanation for how regions can capture returns to public investments in education, even with mobile residents.

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Table 1. Summary Statistics, 49 U.S. States, 1962-2002 (N=441)

Variable	Definition	Mean	Median	Maximum	Minimum	Std. Dev.
GROWTH	log change in state real personal income per capita (multiplied by 100)	12.10	11.39	30.14	-9.93	5.56
TAXES	ratio of state and local taxes to state personal income (multiplied by 100)	10.07	10.01	17.75	7.13	1.33
FEES	ratio of state and local fees to state personal income (multiplied by 100)	3.86	3.72	12.85	1.02	1.52
EDUC	ratio of state and local expenditures on K-12 and higher education to state personal income (multiplied by 100)	6.43	6.38	11.58	3.70	1.19
INFRA	ratio of state and local expenditures on productive government services except education to state personal income (multiplied by 100)	7.43	7.17	14.81	4.70	1.51
OTHER	ratio of state and local expenditures on health, welfare, and other transfers to state personal income (multiplied by 100)	3.46	3.25	8.40	1.28	1.27
FED	ratio of net federal transfers to state and local government to state personal income (multiplied by 100)	3.57	3.37	7.68	0.91	1.27
SURPLUS	ratio of state and local government surplus to state personal income (multiplied by 100)	0.18	0.19	8.51	-3.09	0.99
UR	state unemployment rate	5.93	5.58	15.45	2.00	2.11

Table 2. Regression and GMM Estimates
(robust standard errors)

Variable	(1-OLS)	(2-OLS)	(3-OLS)	(4-GMM)
Constant	11.776	13.369	81.713 ***	
	(9.697)	(9.807)	(21.531)	
GROWTH(-1)	-0.121 **	-0.121 **	-0.132 **	0.014
	(0.054)	(0.054)	(0.052)	(0.106)
EDUC25+INFRA25	1.501 *			
	(0.776)			
EDUC25		0.367	-9.337 ***	-9.274 ***
		(1.364)	(3.117)	(3.417)
INFRA25		2.064 **	-6.854 **	-6.272 **
		(0.955)	(2.671)	(2.950)
EDUC25*INFRA25			1.201 ***	1.188 ***
			(0.357)	(0.374)
OTHER25	-1.364	-1.111	-2.743 **	-3.631 **
	(1.408)	(1.421)	(0.137)	(1.449)
FED25	-0.601	-0.388	1.872	1.638
	(1.541)	(1.544)	(1.550)	(1.711)
UR	-2.344 ***	-2.362 ***	-2.357 ***	-3.377 ***
	(0.225)	(0.225)	(0.216)	(0.399)
SURPLUS25	1.138	1.281	-0.440	-0.151
	(1.065)	(1.065)	(1.094)	(1.162)
Fixed period effects	yes	yes	yes	yes
Fixed state effects	yes	yes	yes	yes
Adjusted R-squared	0.468	0.469	0.504	0.464
J-statistic				16.388
Number of observations	294	294	294	245

* significant at the ten percent level.
** significant at the five percent level.
*** significant at the one percent level.

Table 3. OLS Regression Estimates (Sensitivity Analysis)
(robust standard errors)

Variable	(1)		(2)		(3)	
Constant	75.956	***	78.764	***	91.379	***
	(21.442)		(20.212)		(23.926)	
GROWTH(-1)			-0.132	**	-0.150	***
			(0.052)		(0.052)	
EDUC25	-8.717	***	-8.909	***	-7.762	**
	(3.119)		(2.893)		(3.120)	
INFRA25	-6.286	**	-6.401	***	-5.966	**
	(2.665)		(2.424)		(2.656)	
EDUC25*INFRA25	1.155	***	1.141	***	1.017	***
	(0.348)		(0.308)		(0.347)	
OTHER25	-2.646	*	-2.624	**	-2.899	**
	(1.346)		(1.330)		(1.341)	
FED25	1.453		1.663		2.460	
	(1.530)		(1.451)		(1.529)	
UR	-2.438	***	-2.362	***	-2.337	***
	(0.217)		(0.216)		(0.214)	
SURPLUS25	-0.358				-0.245	
	(1.095)				(1.066)	
REGION_EDUC25					-4.238	*
					(2.257)	
REGION_INFRA25					1.086	
					(1.439)	
Fixed period effects	yes		yes		yes	
Fixed state effects	yes		yes		yes	
R-squared	0.490		0.506		0.511	
Number of observations	294		294		294	

* significant at the ten percent level.
** significant at the five percent level.
*** significant at the one percent level.

Notes: REGION_EDUC and REGION_INFRA represent average EDUC and INFRA for other states in the same census region.

Table 4. OLS Regression Estimates, Specifications with Alternative Lag Structures (robust standard errors)

Variable	(1)		(2)	
Constant	115.153	***	49.059	***
	(27.995)		(14.085)	
GROWTH(-1)	-0.204	***	-0.160	***
	(0.054)		(0.046)	
EDUCn	-13.888	***	-4.221	**
	(3.939)		(2.030)	
INFRAn	-11.216	***	-3.156	*
	(3.448)		(1.732)	
EDUCn*INFRAn	1.962	***	0.554	**
	(0.423)		(0.226)	
OTHERn	-5.674	***	-1.668	*
	(2.031)		(0.991)	
FEDn	1.921		2.041	**
	(2.435)		(1.007)	
UR	-2.320	***	-2.326	***
	(0.253)		(0.201)	
SURPLUSn	0.330		-0.779	
	(1.753)		(0.696)	
Lag specification (n)	30 years		20 years	
Fixed period effects	yes		yes	
Fixed state effects	yes		yes	
R-squared	0.552		0.491	
Number of observations	245		294	

* significant at the ten percent level.
** significant at the five percent level.
*** significant at the one percent level.