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Is Aid for Infrastructure Effective? A Difference-in-Difference-in-Differences Approach

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Abstract

The effects of foreign aid on the endowment of recipient countries with infrastructure have received surprisingly little attention in the empirical literature. This paper addresses this question by performing difference-in-difference-in-differences estimations, with the treatment defined as steep increases in aid for infrastructure since a distinct change in donor behavior in 2005. Mitigating endogeneity concerns in this way, we consistently find aid for infrastructure to be ineffective in improving the recipient countries' endowment with infrastructure. This finding holds not only for an encompassing index of economic infrastructure, but also for sub-indices of infrastructure in transportation, communication, energy, and finance.

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1. Introduction

In the early 1990s already, the World Development Report stressed the role of infrastructure, including transportation, communication, power and other public utilities, in determining "one country's success and another's failure – in diversifying production, expanding trade, coping with population growth, reducing poverty, or improving environmental conditions" (World Bank 1994: 2). According to Limão and Venables (2001: 451), insufficient infrastructure is a major reason for Africa's poor trade performance. Adam and Bevan (2006) show that short-run Dutch disease effects of aid tend to be offset by favorable supply-side effects to the extent that aid is used to improve the recipient countries' endowment with infrastructure.

Against this backdrop, the effects of aid on the recipient countries' endowment with infrastructure have received surprisingly little attention in the empirical literature so far. Vijil and Wagner (2012) and Donaubauer *et al*. (2016a) are notable exceptions. These two contributions regard infrastructure as a transmission mechanism, finding that aid enhances the recipient countries' export performance and the recipient countries' attractiveness to FDI, respectively, through improving their endowment with infrastructure.[1]

A more loosely related strand of the literature addresses the effects of (aggregate) aid on government spending and public investment (e.g., Gang and Khan 1991). Gomanee *et al*. (2005) find that aid finances public investment spending in sub-Saharan Africa. However, as stressed by Morrissey (2015), the distinction between public investment and public consumption may be misleading when it comes to a country's endowment with infrastructure. On the one hand, "government consumption includes expenditures to maintain and operate investment projects" (Morrissey 2015: 99); i.e., recurrent costs subsumed under public consumption may be essential for efficient infrastructure. On the other hand, according to Pritchett (2000), a large part of investment spending in developing countries does not have a positive impact on the public capital stock.

Public investment is also regarded as a channel through which aid may promote economic growth and alleviate poverty in the recipient countries (e.g., Agénor *et al*. 2008).[2] The simulations presented by Adam and Bevan (2006: 288) suggest that "there are potentially large medium-term welfare gains from aid-funded increases in public investment, despite the presence of short-run Dutch disease effects of aid." It is left open to question, however, whether and to which extent aid has positive effects on the recipient countries' endowment with infrastructure. Again, public

[1] Vijil and Wagner (2012) perform two-step cross-sectional estimations. Donaubauer *et al*. (2016a) estimate a system of simultaneous equations based on panel data for FDI, aid and infrastructure, but without accounting for country fixed effects in the equation with infrastructure as the dependent variable. See also Donaubauer *et al*. (2016b) who explicitly stress endogeneity concerns when assessing the effects of aid for infrastructure.

[2] Chatterjee *et al*. (2003) offer a theoretical analysis of the effects of capital transfers tied to investment in public infrastructure, compared to pure untied transfers. The long-run growth and welfare effects of tied transfers are shown to depend on the initial endowment with infrastructure, on co-financing arrangements, and on whether transfers are permanent or temporary; see also Chatterjee and Turnovsky (2005). Djajic (2009) assesses the welfare implications of reallocating aid from current consumption to infrastructure development; the focus of his theoretical analysis is on the conditions under which the donor and the recipient agree or disagree on how to divide aid between these two uses.

investment is not necessarily associated with better infrastructure.[3] For this reason, we focus on the effects of aid on a country's endowment with infrastructure.

What is more, the existing literature suffers from several shortcomings which we attempt to overcome in the present paper. First, the measurement of the recipient countries' endowment with infrastructure typically leaves much to be desired as it is often based on just a few selected indicators. In contrast, we employ a composite index of infrastructure – and four sub-indices of infrastructure in transportation, communication, energy, and finance – available from Donaubauer *et al*. (2016b). Second, the measurement of aid as the explanatory variable of principal interest is often deficient, especially in studies considering overall aid amounts. In contrast, we use disaggregated aid items specifically meant to improve the recipient countries' endowment with infrastructure. Third, we address endogeneity concerns by performing a difference-in-difference-in-differences (DDD) analysis.[4] This approach appears to be most appropriate, considering the relatively short time dimension of our analysis (1999-2011).

2. Approach and data

We perform a DDD approach to identify the treatment effect of sector-specific aid meant to improve the recipient countries' endowment with economic infrastructure in transportation, communication, energy, and finance.[5] We use data from the OECD's Development Assistance Committee (DAC) on foreign aid committed to economic infrastructure, as reported in the Creditor Reporting System (CRS) under CRS code 200, to a large sample of recipient countries. In additional estimations, we replace aid for all four aspects of economic infrastructure taken together by aid in specific sub-categories, i.e., CRS codes 210 (transport and storage), 220 (communications), 230 (energy), and 240 (banking and finance). All aid flows used in the DDD analysis are in US dollars at constant prices of 2013; aid is defined per capita of the recipient countries' population.

Our outcome variable is the change in the recipient countries' endowment with infrastructure (*Infra*) between two points in time. In contrast to the existing literature, we do not rely on just a few indicators to proxy the countries' endowment with infrastructure. Instead, we make use of systematic and comprehensive measures of infrastructure available from Donaubauer *et al*. (2016b). These authors construct a composite index of infrastructure covering the quantity as well as the quality of infrastructure in transportation, communication, energy, and finance. This approach ensures a comparable measurement of infrastructure for a large sample of countries and over a sufficiently long period of time. The index condenses various highly correlated indicators and thus avoids the identification problem that plagues regression analyses which include several

[3] Gang and Khan (1991: 365) conclude from their case study on India: "Just because there are foreign aid expenditures on investment does not mean India has anything to show for it. We are here dealing with budgetary data and not with real capital formation."

[4] Clemens *et al*. (2012) argue that previous attempts to control for the potential endogeneity of aid typically relied on invalid instruments. Instead of suggesting more valid ones, Clemens *et al*. address the potential endogeneity of aid by differencing the regression equation and lagging aid so that it can reasonably be expected to cause growth rather than being its effect. See Section 2 for details on differencing in the DDD approach.

[5] See Nunnenkamp and Öhler (2011) for a similar approach of assessing the effects of sector-specific aid in the fight against HIV/AIDS. The description of the DDD approach draws on Nunnenkamp and Öhler (2011).

indicators simultaneously.[6] To combine the information from 30 different indicators, Donaubauer *et al*. (2016b) use an unobserved components model; that is, observed data on each aspect of infrastructure are a linear function of an unobserved common component of infrastructure and an error term.[7] Using the same methodology, the index has been updated to 2011. The index ranges from -1.78 to 3.40 in our sample (based on annual values), with higher values indicating better infrastructure.

The DDD approach combines before-after comparisons and with-without comparisons. This appears to be most appropriate to assess the recent steep increase in aid for economic infrastructure and helps mitigate important limitations that plague both types of comparisons when employed in isolation.[8] The DDD estimator removes any fixed country effects (first differences) and any fixed time trends (second differences). According to Imbens and Wooldridge (2009: 67), this approach "is often associated with so-called 'natural experiments', where policy changes can be used to effectively define control and treatment groups." The great appeal of DDD analysis "comes from its simplicity as well as its potential to circumvent many of the endogeneity problems that typically arise when making comparisons between heterogeneous individuals" (Bertrand *et al*. 2004: 249), even though concerns about causal inference are not necessarily resolved completely.[9]

Our period of observation is 1999-2011.[10] We divide the overall period of observation into two equally long (6-year) sub-periods, i.e., 1999-2005 and 2005-2011 ('before' and 'after'). Considering 2005 as the dividing line between 'before' and 'after' is most plausible as this year marks a major shift in donor behavior. In the Paris Declaration on Aid Effectiveness, the donors committed themselves to improve the quality and, thus, the effectiveness of aid. At the same time, political leaders agreed at the G8 Summit in Gleneagles to substantially increase aid by about US$ 50 billion per annum (by 2010) and to double aid to Africa (http://www.unmillenniumproject.org/press/g8overview.htm).

What is more, the Aid for Trade (AfT) initiative which focuses on aid for infrastructure was launched at the WTO Ministerial Conference in Hong Kong in 2005. The major objective of the AfT initiative was to overcome the supply-side and trade-related infrastructure constraints that had hindered the growth and diversification of exports of various developing countries (OECD and WTO 2011). As noted by Jouanjean (2013: 276), "the issue of infrastructure development resurfaced in the development agenda and poverty debate with the Aid for Trade (AfT) initiative." In current dollars, total AfT more than doubled after the WTO Ministerial Conference

[6] See Donaubauer *et al*. (2016b) for a detailed discussion of the advantages of the composite index of infrastructure, compared to specific indicators. See also Kaufmann *et al*. (2011) who stress that composite indices can be expected to be more precise and informative than any single indicator.

[7] This approach resembles the construction of the well-known Worldwide Governance Indicators by Kaufmann *et al*. (2011). Importantly, this approach increases the number of observations to be used for comparing the quantity and quality of infrastructure on an annual basis for a large sample of countries.

[8] The simple before-after approach would compare (the change in) infrastructure in aid recipient countries prior and subsequent to a distinct change in donor behavior. Clearly, the implicit assumption that no other omitted variable might have affected infrastructure over time is unlikely to hold. The simple with-without alternative of comparing (the change in) infrastructure between countries receiving high aid and those receiving low or no aid would ignore that infrastructure might have developed differently in the groups due to factors unrelated to aid.

[9] See also Meyer (1995) for a detailed discussion.

[10] More recent data on the index of infrastructure are not yet available. Reliable data on sector-specific aid are available from the CRS since 1995. We start our analysis in 1999 since the DDD approach requires equally long sub-periods before and after a distinct shift in donor behavior.

in 2005, exceeding US$53 billion in 2012, whereas AfT had hardly increased before the launch of the WTO initiative (in terms of commitments as reported in the CRS; see Hühne *et al*. 2015: Figure 10.3).

The with-without dimension of the DDD approach distinguishes between recipient countries with high increases in aid for infrastructure per capita and recipient countries with low increases or declines in aid for infrastructure per capita (treatment group, *T*, versus control group, *C*). For a start, we take the median of the change in aid for infrastructure per capita between 1999-2004 and 2006-2011 as the dividing line between the treatment and control groups.[11]

Formally, the DDD estimator for our baseline specification is as follows:

$$DDD = ((Infra^{T}_{2011} - Infra^{T}_{2005}) - (Infra^{C}_{2011} - Infra^{C}_{2005}))$$
$$- ((Infra^{T}_{2005} - Infra^{T}_{1999}) - (Infra^{C}_{2005} - Infra^{C}_{1999}))$$

The estimator corresponds to the coefficient of the interaction term between the dummy variable for the treatment group and the dummy variable for the second period in the basic regression without additional control variables. In extended specifications, we add the level of *Infra* at the beginning of the first and second sub-periods. In this way, we take into account that changes in *Infra* may depend on initial levels.[12] Furthermore, we include additional control variables $\boldsymbol{X}$, at the beginning of the first and second sub-periods, drawing on the specification of the infrastructure equation in Donaubauer *et al*. (2016a).[13] The extended regression equation reads as follows:

$$\text{Change in Infra} = \alpha + \beta\text{Treatment} + \gamma\text{2nd Period} + \delta(\text{Treatment*2nd Period}) + \lambda\text{Infra} + \rho\mathbf{X} + \varepsilon$$

3. Empirical results

Column (1) of Table I presents the results of the baseline DDD estimation for the overall sample of 92 aid recipient countries and the median of the change in aid for infrastructure representing the dividing line between the treatment and control group. In the basic specification, we only consider the two dummy variables (set to one for the second sub-period and the treatment group, respectively) plus the interaction between these two dummy variables. As can be seen, all three variables prove to be statistically insignificant at conventional levels. Importantly, we do not find aid for infrastructure to be effective in the sense of improving the treatment group's endowment with infrastructure after the change in donor behavior in 2005 and relative to the control group of aid recipients.

[11] Countries in the treatment group, on average, experienced an increase in aid for infrastructure by a factor of 2.23 when comparing the second sub-period with the first sub-period. In contrast, countries in the control group, on average, suffered a decline in aid for infrastructure by about nine percent. For details, see Appendix A. As explained in Section 3, we perform several robustness tests by modifying the dividing line between treatment and control groups.

[12] For example, positive changes in *Infra* become less likely once countries have achieved high levels of infrastructure.

[13] See Appendix B for summary statistics.

In columns (2)-(4) of Table I, we test the robustness of this result by modifying the classification of aid recipients into the treatment and control groups.[14] We restrict the treatment group to the upper tercile of the overall sample with the highest increase in aid for infrastructure, and the control group to the bottom tercile with declining aid for infrastructure (or the lowest increase in aid) in column (2).[15] The treatment group is restricted to 40 recipients in column (3) by excluding six recipients which experienced exceptionally high increases in aid for infrastructure only because the starting levels in the first sub-period were extremely low. Column (4) returns to the full sample, but uses the difference in aid for infrastructure between the second and the first sub-periods, instead of the ratio of aid in these two sub-periods, to separate the treatment and control groups at the median. All three modifications have only marginal effects on the coefficient of the interaction term between the dummy variables for the second sub-period and the treatment group. Hence, aid for infrastructure continues to be ineffective in columns (2)-(4).

This result also carries over to columns (5)-(8) where we extend the specification of the estimation equation. We include the levels of *Infra* at the beginning of the first and second sub-period in column (5). In this way, we take into account that changes in the outcome variable may depend on initial levels; ignoring this possibility may bias the aid-related treatment effect. For similar reasons, the specification in column (6) includes the initial levels of additional control variables. The endowment with infrastructure is widely perceived to be better in richer, more populated, and geographically smaller countries.[16] Hence, these determinants may also affect the change in infrastructure in our DDD analysis. Likewise, better governance in aid-recipient countries may help improve infrastructure.[17] In further extensions, we also account for the interactions between the initial levels of *Infra* and the additional control variables with the dummy variable for the second period (columns 7 and 8).[18] The results for the control variables are generally plausible.[19] However, the extensions hardly affect the statistically insignificant coefficient on our variable of principle interest, the interaction term between the two dummy variables for the second sub-period and the treatment group.

In column (9) of Table I, we test for possible conditional effects of aid for infrastructure on the recipient country's endowment with infrastructure. Specifically, we assess whether the insignificant coefficients on the interaction between the dummy variables for the treatment group and the second period can be attributed mainly to recipient countries with poor governance. We

[14] See Appendix C for the classification of sample countries into the treatment and control groups.

[15] In other words, the tercile of 32 recipients around the median is excluded from the analysis in this robustness test.

[16] For details, see Donaubauer *et al*. (2016a) and the literature given there. The recipient countries' GDP per capita, population and geographical area are included in logged form in the extended specifications. The data are taken from the World Bank's World Development Indicators (WDI), available at: http://data.worldbank.org/data-catalog/world-development-indicators (accessed: January 2016)

[17] We owe this point to an anonymous reviewer. We considered three indicators from the World Bank's Worldwide Governance Indicators (http://info.worldbank.org/governance/wgi/index.aspx#home): rule of law, government effectiveness, and control of corruption.

[18] The identifying assumption of our DDD estimator is that, in the absence of the aid-related treatment, the difference in the outcome variable between the two sub-periods would have been the same for the treatment and the control groups. As argued by Abadie (2005), the plausibility of this assumption is open to question if the treatment and control groups differ with regard to factors that may be associated with the dynamics of the outcome variable. The interactions of *Infra* and the additional control variables with the dummy variable for the second sub-period account for such different dynamics in the treatment and control group.

[19] Table I shows the results for rule of law as the preferred indicator of governance. While rule of law enters significantly positive in column (6), the results for the two alternative indicators of governance proved to be weaker; these results are available from the authors on request.

exclude the tercile of countries in the treatment group with particularly poor scores with regard to rule of law from the estimation.[20] However, our major finding of ineffective aid for infrastructure still holds when restricting the treatment group to relatively well governed recipient countries.[21]

In Table II, we replicate the DDD approach with four sub-indices of infrastructure in transportation, communication, energy, and finance as alternative outcome variables. At the same time, we replace total aid for infrastructure by the corresponding sub-categories of aid for infrastructure in transportation, communication, energy, and finance, respectively, in order to calculate the increase in aid between the first and second sub-periods and classify the recipient countries into the treatment and control groups. For the sake of brevity, the estimations shown in Table II are restricted to the basic model for the full sample of recipient countries (as in column 1 of Table I) and the fully specified model (as in column 8 of Table I).[22] All interaction terms between the dummy variables for the second sub-period and the treatment group prove to be statistically insignificant at conventional levels in Table II, independent of the specific aspect of infrastructure and the specification of the estimation equation. Hence, we find not only total aid for infrastructure to be ineffective in improving the recipient countries' endowment with overall economic infrastructure, but also sub-categories of aid for infrastructure to be ineffective in improving infrastructure in transportation, communication, energy, and finance.

4. Discussion and conclusion

Our finding of ineffective aid for infrastructure may not be particularly surprising for the skeptics of foreign aid (e.g., Easterly 2006). More specifically, even the World Bank argued in the 1990s in its report on 'Infrastructure for Development' that aid-financed projects suffered from serious cost and time overruns: "International donor policies and practices have sometimes reinforced distortions in recipient countries. Many donors have focused on financing new physical construction rather than on maintaining or improving existing infrastructure" (World Bank 1994: 91). In a similar vein, Agénor *et al*. (2008: 279) noted more recently: "In some countries aid inflows may have a large effect on the *flow* of public investment, but not on the *stock* of public capital, because of poor management."

The World Bank (1994) also observed that it was especially in infrastructure where the effectiveness of foreign aid has traditionally been eroded by full or partial tying, i.e., donor requirements to spend aid on goods and services from that particular donor. It is rather unlikely, however, that tied aid represents a major reason for our finding of ineffective aid for infrastructure in the more recent past. The OECD (2014) reports that most DAC donors have increasingly untied their aid since 2001 when they agreed on the Recommendation on Untying ODA to the Least Developed Countries. It is also questionable that the much debated fungibility of aid explains why we find aid for infrastructure to be ineffective. Clearly, it cannot be ruled out

[20] The following countries were excluded: Cambodia, Cameroon, Congo (Dem. Rep.), Congo (Rep.), Cote d'Ivoire, Haiti, Kyrgyz Republic, Libya, Myanmar, Nigeria, Paraguay, Sudan, Tajikistan, Togo, Turkmenistan, Yemen (Rep.).
[21] This result also holds when excluding the quartile of countries in the treatment group with particularly poor scores with regard to rule of law (results not shown).
[22] The estimations corresponding to columns (2)-(7) of Table I do not offer additional insights; these results are available on request. Note that the number of observations varies slightly across columns in Table II. This is due to data limitations which affected the number of countries for which the sub-indices of infrastructure could be calculated, notably in energy and finance.

that aid flows which are explicitly meant to improve the recipient countries' endowment with infrastructure are indirectly transferred to non-targeted sectors. The extent to which aid releases domestic resources in the recipient countries which can then be diverted to other uses is hard to measure. All the same, Morrissey (2015) concludes from the available evidence that the extent to which aid is fungible is over-stated.[23]

Arguably, it is mainly due to our DDD approach that our empirical results are in striking contrast with Vijil and Wagner (2012) and Donaubauer *et al*. (2016a) who find that aid enhances the recipient countries' export performance and their attractiveness to FDI, respectively, through improving their endowment with infrastructure. Compared to the few existing studies, the DDD approach provides a more rigorous set-up to assess the effects of aid on the recipient countries' endowment with infrastructure. In particular, the DDD approach appears best suited to mitigate endogeneity concerns. We may err on the conservative side, however, as our analysis covers a relatively short period of time since the change in donor behavior in 2005. Perhaps aid for infrastructure will prove to be more effective once longer time series become available for the index of infrastructure and sector-specific aid so that delayed effects can be captured fully.

[23] Morrissey (2015: 98) also argues that "even where it is fungible this does not appear to make the aid less effective."

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Table I — Total aid for infrastructure and overall index of infrastructure: estimations for the change in the overall index of infrastructure as the dependent variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2nd period	-0.0337	-0.0158	-0.0337	-0.0157	-0.0338	-0.0466	-0.0183	0.995*	0.978*
	(0.0461)	(0.0542)	(0.0462)	(0.0441)	(0.0462)	(0.0466)	(0.0625)	(0.521)	(0.558)
Treatment	-0.0342	-0.00757	-0.0478	-0.00692	-0.0332	-0.0294	-0.0352	-0.0150	0.0138
	(0.0482)	(0.0594)	(0.0511)	(0.0491)	(0.0489)	(0.0457)	(0.0497)	(0.0484)	(0.0578)
Treatment * 2nd period	0.0776	0.108	0.0981	0.0432	0.0777	0.0803	0.0814	0.0452	0.0250
	(0.0679)	(0.0799)	(0.0714)	(0.0684)	(0.0680)	(0.0659)	(0.0689)	(0.0723)	(0.0903)
Infra					0.00973	-0.138***	-0.00950	-0.205***	-0.187**
					(0.0399)	(0.0467)	(0.0654)	(0.0606)	(0.0744)
Infra * 2nd period							0.0341	0.171*	0.189*
							(0.0824)	(0.0920)	(0.109)
Area (log)						-0.00800		-0.0287	-0.0265
						(0.0161)		(0.0219)	(0.0239)
Area (log) * 2nd period								0.0452	0.0456
								(0.0311)	(0.0345)
Population (log)						0.0349**		0.0508**	0.0407
						(0.0174)		(0.0223)	(0.0251)
Population (log) * 2nd period								-0.0381	-0.0338
								(0.0341)	(0.0375)
GDP per capita (log)						0.0688***		0.119***	0.116***
						(0.0187)		(0.0233)	(0.0254)
GDP per capita (log) * 2nd period								-0.117***	-0.123***
								(0.0368)	(0.0402)
Rule of law						0.0885***		0.0708	0.0315
						(0.0306)		(0.0439)	(0.0524)
Rule of law * 2nd period								0.0245	0.0398
								(0.0611)	(0.0804)
Constant	0.0137	-0.0113	0.0137	0.0040	0.0181	-0.985***	0.00944	-1.400***	-1.252***
	(0.0329)	(0.0411)	(0.0329)	(0.0290)	(0.0382)	(0.277)	(0.0460)	(0.332)	(0.384)
Observations	184	120	172	188	184	178	184	178	148
Countries	92	60	86	94	92	89	92	89	74
R-squared	0.007	0.034	0.012	0.003	0.008	0.145	0.009	0.197	0.153

Notes: Columns (1) and (4)-(8): all sample countries with available data; column (2): excluding tercile of 32 countries around the median; column (3): excluding six countries with exceptionally high increase in aid for infrastructure due to extremely low starting levels in the first sub-period; column (4): difference in aid for infrastructure between the second and the first sub-period, instead of the ratio of aid in these two sub-periods, to separate the treatment and control group at the median; column (9): restricted treatment group by excluding countries with poor scores with regard to rule of rule. Robust standard errors in parentheses; ***, **, * when statistically significant at the 1, 5, and 10% level, respectively

Table II — Disaggregated aid for infrastructure and sub-indices of infrastructure: estimations for the change in the sub-indices of infrastructure in transportation, communication, energy, and finance as dependent variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Transportation		Communication		Energy		Finance	
2nd period	0.0532	0.831	0.0643	-0.181	-0.0422	-0.490	0.135	1.394
	(0.0720)	(0.659)	(0.0662)	(0.738)	(0.0617)	(0.624)	(0.0934)	(1.081)
Treatment	0.0223	0.00667	-0.0576	-0.0589	-0.0539	-0.0425	0.215**	0.206**
	(0.0608)	(0.0607)	(0.0562)	(0.0411)	(0.0475)	(0.0513)	(0.0983)	(0.0958)
Treatment * 2nd period	0.00826	-0.00954	0.0523	0.0507	0.0589	0.0450	-0.191	-0.180
	(0.109)	(0.109)	(0.0984)	(0.0773)	(0.0861)	(0.0953)	(0.132)	(0.134)
Infra		-0.247**		-0.395***		-0.171*		-0.233***
		(0.105)		(0.132)		(0.0986)		(0.0651)
Infra * 2nd period		0.152		0.153		0.111		0.186*
		(0.166)		(0.207)		(0.164)		(0.0982)
Area (log)		-0.0137		-0.0145		-0.0198		-0.00988
		(0.0254)		(0.0162)		(0.0233)		(0.0343)
Area (log) * 2nd period		0.0529		-0.0179		0.0514		0.00768
		(0.0409)		(0.0301)		(0.0505)		(0.0554)
Population (log)		0.00905		0.0415**		0.0256		0.0428
		(0.0289)		(0.0164)		(0.0254)		(0.0450)
Population (log) * 2nd period		-0.00878		0.00605		-0.0185		-0.0325
		(0.0473)		(0.0350)		(0.0488)		(0.0721)
GDP per capita (log)		0.0327		0.203***		0.0565*		0.0735
		(0.0332)		(0.0291)		(0.0323)		(0.0480)
GDP per capita (log) * 2nd period		-0.165***		0.0537		0.0210		-0.1000
		(0.0623)		(0.0715)		(0.0528)		(0.0671)
Rule of law		0.0470		0.0765**		0.0141		0.149
		(0.0564)		(0.0355)		(0.0532)		(0.0954)
Rule of law * 2nd period		0.0100		-0.0229		0.0216		0.0665
		(0.108)		(0.0675)		(0.0856)		(0.124)
Constant	-0.0424	-0.341	-0.0217	-2.068***	0.0194	-0.642*	-0.101	-1.227
	(0.0366)	(0.382)	(0.0416)	(0.317)	(0.0351)	(0.326)	(0.0631)	(0.778)
Observations	196	188	220	214	160	154	160	157
Countries	98	94	110	107	80	77	80	79
R-squared	0.007	0.162	0.019	0.382	0.006	0.116	0.035	0.147

Notes: Infra denotes infrastructure in transportation in columns (1) and (2), communication in columns (3) and (4), energy in columns (5) and (6), and finance in columns (7) and (8). Robust standard errors in parentheses; ***, **, * when statistically significant at the 1, 5, and 10% level, respectively

Appendix A – Infrastructure and aid for infrastructure for the treatment and control group

	Treatment group	Control group
Difference in infrastructure		
• Before (2005-1999)	-0.020	0.014
• After (2011-2005)	0.023	-0.020
Average inflow of aid for infrastructure per capita		
• Before (1999-2004)	5.024	11.961
• After (2006-2011)	11.208	10.887

Note: Sample as in Table 1, column 1

Appendix B — Descriptive statistics (year 1999)

	Treatment group				
	Obs	Mean	Std. dev	Min	Max
Infra total (level)	45	-0.542	0.441	-1.401	0.573
Infra transportation (level)	46	-0.471	0.616	-1.299	1.821
Infra communication (level)	53	-0.413	0.293	-0.974	0.373
Infra energy (level)	40	-0.432	0.465	-1.817	0.785
Infra finance (level)	39	-0.319	0.910	-2.085	1.732
Area (log)	45	12.902	1.431	9.233	15.939
Pop (log)	45	16.398	1.460	14.000	20.758
GDP per capita (log)	45	7.204	1.205	4.887	9.449
Rule of law	45	-0.538	0.716	-2.080	1.180
	Control group				
	Obs	Mean	Std. dev	Min	Max
Infra total (level)	44	-0.441	0.433	-1.175	0.840
Infra transportation (level)	48	-0.421	0.493	-1.175	0.889
Infra communication (level)	54	-0.483	0.276	-1.335	0.194
Infra energy (level)	37	-0.584	0.512	-2.688	0.291
Infra finance (level)	40	-0.336	0.686	-1.542	1.184
Area (log)	44	11.998	1.957	6.565	16.055
Pop (log)	44	16.158	1.608	13.215	20.949
GDP per capita (log)	44	7.365	0.991	5.627	9.820
Rule of law	44	-0.432	0.524	-1.220	0.985

Appendix C — Countries in the respective treatment and control groups

	Table 1					Table 2			
	Columns (1), (5), (7)	Column (2)	Column (3)	Column (4)	Columns (6), (8)	Column (1)	Column (3)	Column (5)	Column (7)
Afghanistan						1			
Albania	0		0	1	0	1	0	0	1
Algeria	0		0	0	0	1	0	0	0
Angola	0	0	0	0		1	0	0	1
Argentina	0	0	0	0	0	0	1	0	0
Armenia	0		0	1	0	1	0	0	0
Azerbaijan	0	0	0	0	0	1	1	0	1
Bahrain	0	0	0	0	0				
Bangladesh	0		0	0	0	0	1	0	0
Belarus				1					
Benin	0		0	1	0	0	1		1
Bhutan	0		0	1	0	0	0		1
Bolivia	0	0	0	0	0	0	0	1	0
Bosnia and Herzegovina	0	0	0	1	0	1	0	1	
Botswana	1	1	1	0	1	0			1
Brazil	1	1	1	0	1	1	0	1	1
Burkina Faso	0	0	0	0	0	0	0		0
Burundi							1		1
Cabo Verde						1	1		
Cambodia	1		1	1	1	1	1	0	
Cameroon	1		1	1	1	1	1	0	0
Central African Republic							1		
Chad							1		0
Chile	1		1	0	1	0	1	1	0
China	0	0	0	0	0	0	0	0	1
Colombia	1	1		1	1	1	1	1	1
Comoros							0		
Congo, Dem. Rep.	1		1	1	1	1	0	1	
Congo, Rep.	1		1	1	1	1	1	0	
Costa Rica	0	0	0	0	0	0	1	0	0
Cote d'Ivoire	1	1	1	1	1	0	0	1	1
Croatia	0	0	0	0	0	0	1	1	0
Cuba	0		0	0	0	0	1	0	
Djibouti							1		
Dominican Republic	0		0	1	0	1	0	0	0
Ecuador	0		0	0	0	0	1	1	0
Egypt	1		1	1	1	1	1	1	0
El Salvador	1	1	1	1	1	1	0	1	1
Eritrea							0	0	
Ethiopia	1	1	1	1	1	1	0	1	
Fiji	0	0	0	0	0	0	1		1
Gabon	1		1	1	1	1	0		0
Gambia						0	1		
Georgia	1	1	1	1	1	1	0	0	1
Ghana	1		1	1	1	0	1	1	1
Guatemala	0		0	0	0	0	1	1	0
Guinea						0	1		
Guyana	0	0	0	0	0	0	0		0
Haiti	1	1		1	1	1		1	1
Honduras	0	0	0	0	0	0	0	0	1
India	1	1	1	0	1	1	0	0	1
Indonesia	0	0	0	0	0	0	1	0	1
Iran	0	0	0	0	0	1	0	0	
Iraq						0		0	
Jamaica	0	0	0	0	0	0	0	0	0
Jordan	1		1	1	1	0	0	1	1
Kazakhstan	0		0	1	0	0	1	1	1
Kenya	1	1	1	1	1	1	1	1	1
Korea, Dem. Rep.	0	0	0	0			1	0	
Kyrgyz Republic	1		1	1	1	1	1	1	0
Lao						0	0		
Lebanon	1	1	1	1	1	1	1	1	1
Lesotho							0		1
Liberia							1		
Libya	1	1		0	1		1		

Appendix C — continued

	Table 1				Table 2				
	Columns (1), (5), (7)	Column (2)	Column (3)	Column (4)	Columns (6), (8)	Column (1)	Column (3)	Column (5)	Column (7)
Macedonia	0	0	0	0	0	0	0	1	0
Madagascar	0	0	0	0	0	0	1		0
Malawi							0		1
Malaysia	0	0	0	0	0	1	0	0	1
Mali							1		1
Mauritania						0	0		
Mauritius	0	0	0	0	0	1	0		0
Mexico	1	1	1	0	1	0	0	1	1
Moldova	1		1	1	1	1	0	0	0
Mongolia	1		1	1	1	1	1	0	0
Morocco	1	1	1	1	1	1	1	0	0
Mozambique	0	0	0	0	0	0	1	0	0
Myanmar	1		1	0		0	1	0	
Namibia	1	1	1	1	1	0	1	1	
Nepal	0		0	1	0		0	0	0
Nicaragua	0		0	1	0	0	0	1	
Niger	1		1	1	1	1	0		0
Nigeria	1	1	1	0	1	1	1	0	1
Oman	1	1		1	1	1	1	1	1
Pakistan	1		1	0	1	1	0	1	0
Panama	1	1	1	1	1	0	0	1	1
Papua New Guinea	0	0	0	0	0	0	1		0
Paraguay	1	1		1	1	1	0	1	1
Peru	0	0	0	0	0	0	1	0	1
Philippines	0	0	0	0	0	0	0	0	0
Rwanda						1	1		
Saudi Arabia	1	1	1	0	1	0	1	1	
Senegal	1		1	1	1	1	0	1	1
Serbia									1
Sierra Leone						0			
Solomon Islands						1	1		
South Africa	1	1	1	0	1	1	1	1	1
Sri Lanka	0	0	0	0	0	1	0	0	0
Sudan	1	1		1	1	1	1	1	
Suriname						0	1		
Swaziland						1	0		1
Syria	1	1	1	1	1	1	0	1	
Tajikistan	1	1	1	1	1	1	0	0	
Tanzania	1	1	1	1	1	1	1	1	1
Thailand	0	0	0	0	0	0	1	0	0
Togo	1	1	1	1	1		0		1
Trinidad and Tobago	0	0	0	0	0	0	1	0	0
Tunisia	0		0	1	0	0	0	1	0
Turkey	1	1	1	1	1	1	1	1	1
Turkmenistan	1	1	1	0	1	0	1	1	
Uganda							0		0
Ukraine				1					
Uruguay	1	1	1	0	1	0	1		0
Uzbekistan	0	0	0	0	0	0	0	0	
Venezuela	0	0	0	0	0	0	0	0	0
Vietnam	0		0	1	0	1	0	0	0
Yemen, Rep.	1	1	1	1	1	1	0	1	
Zambia	0		0	1	0		0	1	0
Zimbabwe							0	1	

Notes: 1 if country is in the treatment group, 0 if country is in the control group. Groups for columns (2), (4), (6), and (8) of Table 2 are not shown in order to save space.