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An Empirical Assessment of Private Sector Participation in Electricity and Water Distribution in Developing and Transition Countries[1]

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ABSTRACT

We analyze a panel of 302 utilities with private sector participation (PSP) and 928 utilities without PSP in 71 developing and transition countries in order to evaluate the impact of PSP on firm performance in electricity distribution and water and sanitation services. We compare the change over time in a number of output variables for both groups of utilities and isolate the effect of PSP from time trends and firm-specific characteristics by using a series of econometric tools. We account for ex-ante differences between state-owned enterprises (SOEs) that were selected for PSP and those that were not, and correct for possible bias in the estimations induced by such differences. We distinguish between divestitures, concessions, and lease and management contracts in an attempt to evaluate the impact of different kinds of PSP. We find robust evidence in the global sample that PSP has a strong impact on the efficiency of utility operations; at the same time, the evidence suggests a decrease in employment due to PSP. PSP is associated with output increases in electricity, and connection increases in water and sanitation, an improvement in bill collection ratios and improvements in the quality of service in both sectors, the latter expressed as a reduction in distributional losses in electricity and an increase in hours of daily service in water. We also find a link between the form of PSP and the estimated performance impact, with the strongest effects in the electricity sector realized by utilities whose assets were divested to the private party, and by utilities managed under concession contracts in the water sector. The differentiation of results according to contract type is expected, as different kinds of PSP will respond to different contractual obligations and incentives. Two broader policy issues emerge: the lack of evidence for an increase in investment following PSP for any contract type except divestitures in electricity. This points to a lack of maintenance and expansion investment in the distribution networks even if PSP leads to an increase in operational efficiency. Moreover, there is no conclusive evidence for a change in prices as a result of PSP. This highlights the economic and political difficulties to align prices with costs in a large number of developing countries.

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1. Introduction and summary

The question whether privately managed utilities perform better than those run by the state has long been a source of debate. As a general rule, a change from public to private management is expected to lead to cost cutting and efficiency improvements driven by profit motives, and this has long been one of the strongest arguments used by proponents of privatization. When price exceeds marginal cost, a profit-driven operator will increase sales. In the context of competitive markets, cost savings and increases in labor productivity, service quality and investment have indeed been reported as a consequence of ownership change from public to private.[6] However, the empirical results of the impact of private sector participation (PSP) in electricity distribution and water and sanitation services are less clear-cut. These utility services are associated with features which have historically been used to justify public involvement: they are natural monopolies (when the service is provided through networks), generate externalities, and, in particular in the case of water services, display inelasticity of demand which conveys significant pricing power to the provider.[7] In the presence of such market failure, economic research into the impact of private management has often been inconclusive. The interpretation of available results is especially complex in the context of developing and transition economies, that is, in often very young institutional and legal environments. Nevertheless, the wide-ranging introduction of PSP in the sectors for electricity distribution and water and sanitation services in developing and transition economies during the second half of the 1980s and the 1990s constitutes an opportunity for studying the effect of private participation on enterprise performance in the context of essential utility services. At the same time, governments throughout the world have retained a multitude of utilities in state hands, thus providing a useful comparator group for evaluating the impact of PSP.

A major reason why studies in this research setting have often provided inconclusive or ambiguous answers have been data limitations. Contrary to studies focusing on manufacturing or industrial sectors which often analyze hundreds or even thousands of firms, and include relatively long series of annual observations,[8] studies on natural monopoly industries suffer by design from

[6] Gains in productivity and profitability associated with privatization have for example been demonstrated by Megginson, Nash, and van Randenborgh (1994), Frydman et al (1997); La Porta and Lopez-de-Silanes (1999), and Brown et al (2006).
[7] See Galiani et al (2005) for a discussion of these elements in the case of water services.
[8] Frydman et al. (1999) follow about 200 companies for a period of 4 years, Earle and Estrin (1997) follow 380 manufacturing firms for a period of 5 years. Brown et al (2006), have a total sample of several thousand companies, in 4 countries.

small sample size and often take the form of case studies.[9] Moreover, many studies fail to isolate the effect of private participation beyond a 'pre- versus post' comparison for a given set of companies, and do not observe state-owned comparators over the same time period.[10] These limitations have prevented many researchers from robustly identifying the PSP impact, as well as from accounting for pre-PSP differences across utilities that might lead to estimation bias. In addition, even where a sufficient number of observations and a sub-sample of state owned comparators are available,[11] oftentimes the question whether the control group of state-owned comparators can be considered a reliable counterfactual has been neglected.

Our aim in this paper is to analyze the performance effect of PSP in the context of electricity distribution and water and sanitation services, using longer time series and more comprehensive coverage than previous research. We also make use of a set of state owned comparators which are selected following a matching procedure to address concerns of like-with-like comparison and statistical bias. We develop a database that covers, as well as possible, the entire population of PSP experiences from the beginning of the 1990s to 2005. In rare cases, we also found data going back as far as the 1970s and 1980s. We cover the two sectors in all developing regions as defined by the World Bank, that is: East Asia and Pacific (EAP), Europe and Central Asia (ECA), Latin America and Caribbean (LAC), Middle East and North Africa (MENA), South Asia (SA), and, finally, Sub-Saharan Africa (SSA). [12] All PSP cases with at least 3 year post-PSP experience are targeted for inclusion in the sample. We address the question of the counterfactual by including in the database state owned enterprises (SOEs) operating in the same sectors and countries/regions. We use the term state owned enterprises in a broad sense, referring to utilities owned and controlled by all levels of government be they central, provincial or municipal. In this manner, we analyze a sample of 302 utilities with PSP and 928 SOEs in 71 developing and transition countries. Table 1A below gives an indication of the composition of the sample used.

[9] For example, Lipton et al (1990), Nellis (2003), Berg and Muhairwe (2006), etc.

[10] For example, Boubakri and Cosset (1998) and Galal et al (1994) compare private companies pre- and post-privatization without a sample of state owned comparators.

[11] For example, Megginson (1994), Brown et al (2006), etc.

[12] The sample size per region is thus determined by the prevalence of PSP in a particular region and not by other proportional indicators, for example the size of the region in terms of population, or electricity or water connections.

Table 1A. Sample Composition (number of utilities)

Region[1]	Electricity			Water and sanitation			Both sectors		
	PSP[2]	**SOE[2]**	**Total**	**PSP[2]**	**SOE[2]**	**Total**	**PSP[2]**	**SOE[2]**	**Total**
EAP	1	2	3	10	87	97	11	89	100
ECA	36	21	57	29	366	395	65	387	452
LAC	111	44	155	94	330	424	205	374	579
MENA	1	2	3	4	29	33	5	31	36
SA	3	3	6	0	0	0	3	3	6
SSA	9	19	28	4	25	29	13	44	57
Total	**161**	**91**	**252**	**141**	**837**	**978**	**302**	**928**	**1,230**

Note: [1] The regions included in the study are East Asia Pacific (EAP), Europe and Central Asia (ECA), Latin America and Caribbean (LAC), Middle East and North Africa (MENA), South Asia (SA), and Sub-Saharan Africa (SSA). [2] PSP stands for utilities with private sector participation; SOE stands for state-owned enterprise.

Table 1B shows that the final panel spans the years 1973-2005, but most of the data is concentrated in the period 1992 to 2004. The results should therefore be considered most relevant for this period.

In addition to the improvement in data coverage, another important contribution of the paper relative to previous studies is that it covers a range of ways in which PSP has been introduced in water and electricity utilities. This contrasts with research that examines “pure privatization”, i.e. permanent control of the private party over business assets and associated rights.[13] Due to the natural monopoly features and social and political considerations associated with utility services, full divestiture of assets occurs relatively rarely in their case.[14] The PSP experiences included in our study cover a range of legal arrangements defining the role of the private sector, namely management and lease contracts, concessions, and partial and full divestitures.[15] In essence, the criteria we use to define selection into the PSP sample is whether the private operator has control over the operating assets of the utility. In other words, we consider the transfer of *operating rights*

[13] Similar to many studies, Brown et al (2006) for example define privatization as over 50% private shareholdings.

[14] In recent years, a ‘reversed policy of renationalization’ has even taken place in utility sectors in developing countries, illustrating the strength of popular sentiment associated with the delivery of essential services to households. Examples of this would be the Argentinean government’s canceling of the national postal concession and the water concession in Buenos Aires; the early demise of the water concession in Bolivia; the end of the lease contract for DAWASA in Tanzania, etc.

[15] For the purpose of this study, full divestiture is defined as transfer of 100% of infrastructure assets, operating assets and operating rights to private hands for an indefinite period; partial divestiture is defined as transfer of between 51% and 100% of infrastructure assets, operating assets and operating rights to private hands for an indefinite period; concession as a transfer of the above assets and rights for a limited period; lease contract as state ownership of infrastructure assets, joint ownership of operating assets and private ownership of operating rights for a limited period; and a management contract as state ownership of infrastructure and operating assets and private ownership of operating rights for a limited period. In addition, the private side in a divestiture and a concession earns the full revenue, in a lease contract a percentage of revenue, and in a management contract a fixed or variable fee. For detailed discussion of PSP forms, see Delmon (2006).

as defining PSP criteria rather than the transfer of 'pipes and wires'. While this criterion is the same for all utilities with PSP in the sample, the different degrees of transfer of rights and assets conferred by the different legal PSP arrangements result in a range of contractual obligations and profit and investment incentives for private operators which this study examines in greater detail.

Table 1B. Sample Composition (number of utility year observations[1])

Year	**Electricity**	**Water and sanitation**	**Both sectors**
1973	1		1
1974	1		1
1975	1		1
1976	1		1
1977	1		1
1978	1		1
1979	1		1
1980	1	2	3
1981	1	2	3
1982	1	2	3
1983	1	2	3
1984	1	2	3
1985	4	2	6
1986	6	2	8
1987	34	2	36
1988	38	2	40
1989	40	3	43
1990	64	19	83
1991	78	36	114
1992	145	46	191
1993	144	50	194
1994	147	71	218
1995	181	183	364
1996	208	246	454
1997	233	433	666
1998	238	566	804
1999	246	598	844
2000	248	793	1,041
2001	248	855	1,103
2002	247	767	1,014
2003	226	728	954
2004	123	587	710
2005[2]	11	80	91
Sum	2,921	6,079	9,000

Note: [1] Number of utilities for which at least one of the core indicators (see Table 15) was observed in a given year. [2] The numbers for 2005 are low because the year was not targeted *per se* in the data collection effort; information was added opportunistically.

The empirical approach in the paper follows Brown et al. (2006) in their dealing with selection bias in privatization experiences by applying methods originally developed to address similar bias in evaluations of labor market programs. We similarly use not only model specifications with firm random and fixed effects, but also a model with firm-specific time trends. This allows us to control for fixed differences among firms as well as different trend productivity growth rates that may affect the probability of a company having been chosen for PSP in the first place. The firm-specific time trends also allow us to examine the probability of the PSP arrangement being a divestiture, concession, lease or management contract. In addition, we employ an instrumental variable (IV) procedure to address the issue that the choice of utilities selected for PSP is not random. We use regional measures of "privatization enthusiasm" (essentially the share of utilities already displaying PSP in the countries in the respective region) and monetary and fiscal pressures in the pre-PSP period (whether the country has run annual inflation higher than 20% for at least two years prior to the start of PSP in the country) and monetary and fiscal pressures in the pre-PSP period to instrument for whether the utility becomes a PSP, and what type of contract is adopted. As a robustness check of our findings, and to examine the issue of potential bias due to ex-ante differences between utilities that are selected for PSP and those that remain under state control, we implement a nearest neighbor matching procedure to choose the best *sub-sample* of SOE comparators and construct as similar as possible PSP-SOE pairs for the econometric analysis. We believe that the combination of the above methods addresses the concerns traditionally raised in the context of impact studies as described for example by Ravaillon (2001). It constitutes a qualitative improvement relative to studies which use as comparator group all state owned firms available, and hence introduce a potential estimation bias. We report the results of both the panel regression undertaken with the full sample and the nearest neighbor matching in order to illustrate the difference in the results.[16]

Similar to Andres (2004), we analyze the dynamics of firm performance as a response to PSP in two different periods: a 'transition' period, encompassing the years immediately before and after PSP, and a post-PSP period, ie, the period at least one year after the entry of the private operator. We adopt this procedure to compare the effects of full private control of operating rights against possible anticipatory effects and the impact of immediate pre-PSP utility restructuring.[17] As has been widely pointed out in the privatization literature, the announcement that a company will be transferred into private hands can result either in performance improvements due to increased

[16] Note that both estimation models applied have their drawbacks: the regression using the full sample suffers from an unbalanced and potentially too dissimilar control group, while the matching procedure leads to a considerable loss of data points and thus to overall lesser robustness and the loss of some differentiated results.

[17] See also Brown et al (2006) for a detailed discussion of timing effects and their econometric treatment.

managerial incentives, or performance deterioration as managers resort to asset stripping (Aghion et al. [1994], Roland and Sekkat [2000]). Alternatively, the state may implement a restructuring program in the immediate pre-PSP period in order to make the utility more attractive to private investors, or, to the contrary, withdraw financially in anticipation of its reduced role. As in Andres (2004) and Brown et al. (2006), we specifically try to isolate the "pure" effect of private participation from any transitory effect.

Finally, we report results on the range of effects observed in the context of the different types of private participation most common in utility industries: divestiture vs. concession vs. lease and management contracts. In the earlier literature, the statistical differentiation of results according to contract type has very much been hampered by small sample size. Contrary to other studies, our dataset contains a sufficient number of different PSP contracts, with distinct patterns in both sectors under examination: full and partial divestitures clearly dominate in the electricity distribution sector, and concession contracts in the sector for water and sanitation services. We would expect to find results differentiated by contract types because of different contractual obligations associated with different forms of private sector participation. For example, lease and management contracts may have explicit clauses that limit intervention by the private party in labor decisions, so we would expect estimation results in that type of setting to differ from a divestiture which gives greater management control to the private party. It can also be assumed that initial conditions on the ground will drive the selection of performance targets for certain contract types. For example, long-term concession contracts with explicit expansion targets may be favored in a situation where an increase in connections is a primary target, and we would expect to see such an increase in the estimation results. Overall, the variation in the data set allows us to examine the link between estimated impact of PSP and contract type. We draw preliminary conclusions regarding the extent to which it is initial sector conditions rather than the strength of incentives associated with different contract types that drive the contract-specific results. While further research into the question is recommended, prima facie facts suggest that it is not only initial sector conditions but indeed the varying degree of private control that is linked to the varying strength of performance results.

Several features and limitations of this research should be highlighted upfront. First, the study undertakes a partial analysis only and we are not attempting to assess the economy-wide welfare effects. Moreover, the approach is supply side focused, and the results interpreted as such. We acknowledge that in reality what we measure as supply side effect could well at least partly be demand driven, for example an increase in output might be due to higher demand, not a change on the supply side. Next, for reasons of clarity of argument, we use a terminology opposing 'utilities with PSP' to 'state-owned enterprises (SOEs)'. This terminology implies a simplification of reality in the sense that state ownership of assets is still the rule for most of our PSP cases, as mentioned already. Moreover, we understand the term SOE as utilities owned and controlled by the government, be it central, provincial or municipal governments. Finally, the study at hand does not explore the impact of regulatory and institutional arrangements, for example the presence of a sector regulator or how different tariff regulation regimes influence the results.

The major contribution of the study is the presentation of stylized facts distilled from a global sample of PSP experiences. For each individual case of PSP, the potential for performance change depends on a host of variables which will not have been individually taken into account for the global analysis. With this in mind, the results of the study can be summarized as follows. The analysis of the data shows that PSP is strongly associated with:

- an increase in output in the electricity sector;
- an improvement in operational performance in the electricity and the water sector, defined as increase in collection rates;
- a reduction of distribution losses in electricity;
- an increase in service quality in the water sector, defined as increased number of hours of daily service;
- an increase in the number of residential connections in the water sector and residential coverage in the sanitation sector;
- a decrease in employment in both water and electricity, leading to improved labor productivity indicators.

The findings above support the predictions of economic theory that private sector participation will improve efficiency and lead to lower costs. However, the data also fails to support other claims traditionally associated with the entry of the private sector. In particular, there is lack of robust evidence of an increase in investment for all contract types apart from divestitures (and the results for this type of contract are based on a small number of companies only). At the same time, we also fail to find a consistent result of a change in prices as a result of PSP. Arguably, this points to deeper policy issues known to bedevil the utility sectors in developing countries, namely the economic and political difficulty to align prices with costs even in the presence of PSP. Moreover, the investment result illustrates a lack of maintenance and expansion investment even if the introduction of the private sector brings operational improvements. Further research into this particular point is recommended as the study at hand arguably used simplified measures of prices and investment, and a more in-depth analysis seems indicated to confirm these important points.

Table 2A below gives a summary of the key results of the impact of PSP on select performance indicators. The two columns in the table show the quantitative effect the estimation coefficients imply: the percentage figures refer to the change in the average value of the impact variable between the pre-PSP and the post-PSP period, over and above what is observed for SOEs during the same period.[18] The table at the same time illustrates the effect of applying two different estimation approaches. The first results column displays the quantitative impact of PSP estimated on the basis of the full panel which allows maximizing the data points used. The second results column displays the quantitative figures using the best possible *subsample* of SOEs, ie the results from the matching procedure which accounts for pre-PSP differences between the treatment and the control group and keeps only the best-matched comparators. The comparison of the results from the panel regression with the results found under the matching procedure illustrates the potential bias that exists when no correction for pre-PSP differences is undertaken. However, while the matching procedure makes certain that the best available comparators are used, it comes at the cost of losing information from the full panel. This implies that certain results that will have been obtained on the basis of the panel regression cannot be validated by the matching procedure because too much data is being lost in the correction process. This is particularly relevant for the estimation of contract-specific results.

[18] To keep the table as simple and illustrative as possible, the exact numbers quoted often refer to the contract type which displays the most robust impact. See the results section for details.

Table 2A. Summary of results: the impact of PSP

Impact Variable	Quantitative change in the impact variable implied by the regression coefficient	
	Regression on full panel[1]	*Results using matching procedure*[2]
Electricity		
Electricity sold per worker	34.2% increase	38.9%-50.0% increase
Collection ratio	48.3% increase	69.9%-85.8% increase
Employment	30.4% decrease	43.8% decrease
Distributional losses	8.7% decrease	24.6% decrease
Water		
Residential connections	61.5% increase	13.1%-16.7% increase
Daily water supply	2.5% increase	11.5%-11.9% increase
Collection ratio	No statistically significant effect	15.1%-50.7% increase
Employment	No statistically significant effect	10.9%-16.9% decrease
Sanitation		
Residential coverage	12.0% increase	25.5% increase

Note: [1]Specification reported: fixed effect for utility, firm-specific time trend, instrumental variable correction for PSP endogeneity bias. The exact number quoted refers in most cases to the contract type which displays the most robust result. See the results section for details. [2] Matching undertaken to select the most comparable SOE comparators; a range of results indicates that several models were tested.

Table 2A reports only results that are significant across the most demanding specifications of the estimation approach; a comprehensive picture of the results is given below. The figures imply that there exists a strong effect of PSP over the panel period, but it is important to recall that the changes summarized in the table occur over the period 1992-2005 and do not imply a sudden jump following PSP. For example, the decrease in employment of the order of magnitude of 30-40% will refer to an average change from the pre-PSP period to the post-PSP period.

In order to examine more closely the bias that exists when utilities with PSP are compared in an indiscriminate manner to utilities without PSP, a complete picture of the variables analyzed is given in Table 2B. The results are presented in graphic format for easy reference; it is again possible to visualize the difference between the two estimation techniques, the first maximizing the sample information, the second using the most accurate comparator group and thus correcting for estimation bias caused by panel imbalance and ex-ante differences between utilities chosen for PSP and those that remain government managed. Table 2B illustrates that the estimation bias is not systematic in one direction. Some effects apparent under the initial panel specification disappear when matching procedures are applied, and other effects appear and become stronger. For example, while a positive effect of PSP on investment (CAPEX) per worker is observed in the water sample comprising all SOEs, this effect disappears under a matching procedure which matches SOE and PSP utilities based on pre-PSP characteristics, implying that there is no significant difference between privately and publicly managed utilities in this respect. In other cases, an effect of PSP which was non-existent in the panel, appears in the matching procedure, but the procedure yields too few observations to make the results robust for a definite statement.

For example, the effect of PSP on investment per worker in the electricity sample is significantly positive for the case of divestitures, but the result is based on 8 utilities only. The main message to retain is that the changes in results due to close matching suggest that previous studies on privatization and PSP may have mis-estimated the effect of the transfer of control by comparing utilities with and without PSP indiscriminately and not taking ex ante differences in the utilities chosen for PSP into account.[19]

Table 2B. Summary of results: Panel versus Diff-in-Diff Analysis

Impact Variable	Qualitative change in the impact variable implied by the regression coefficient	
	Regression on full panel[1]	***Results using matching procedure***[2]
Electricity		
Electricity sold per worker	↑	↑
Electricity sold per connection	↑	↑
Collection ratio	↑	↑
Employment	↓	↓
Distributional losses	↓	↓
Price	↔	↔
Annual interruption frequency	↔	↔
Electricity sold per connection	↔	↔
CAPEX per worker	↔	↑
Residential connections	↑	↔
Residential coverage	↓	↔
Water		
Residential connections	↑	↑
Daily water supply	↑	↑
Collection ratio	↔	↑
Employment	↔	↓
Water sold per worker	↑	↔
Water sold per connection	↔	↔
Water pipe breaks per connection	↓	↔
Output sold per connection	↔	↔
Price	↔	↔
Residential coverage	↓	↔
Investment per worker	↑	↔

[19] Table 2 suggests that in most cases nearest neighbor matching of utilities with PSP and SOEs reinforces the PSP effect, but it is important to note that the table does not report results which do not 'survive' the matching procedure. See the results chapter and the detailed estimation tables at the end of the report for a detailed discussion of the results.

Impact Variable	Qualitative change in the impact variable implied by the regression coefficient	
	Regression on full panel[1]	***Results using matching procedure***[2]
Sanitation		
Residential coverage	↑	↑
Residential connections	↔	↔
Wastewater treated per worker	↔	↑
Wastewater treated per connection	↔	↔
Sewerage blockages per connection	↔	↔

Note: ↑ implies a statistically significant increase associated with PSP, ↓ implies a statistically significant decrease, and ↔ implies ambiguous or not statistically significant result.

We find robust evidence in the global sample that PSP has a strong impact on performance, with some of the efficiency gains driven by a reduction in the staff numbers. In the case of electricity, the estimates show that over the period 1973–2005, PSP is associated with an increase in electricity sold per worker that is 40–50% higher than the increase for SOEs, an increase in bill collection ratios that is up to 85% higher, and a reduction in distributional losses that is 25% more effective; at the same time, the evidence suggests a significantly stronger decrease in employment, with PSP associated with 40% higher staff reductions. There is also evidence for an increase of capital expenditure per worker, but only in the case of divestitures. In the case of water services, the results also show a robust increase in performance associated with PSP. PSP leads to an estimated increase in residential connections up to 16% higher than for SOEs, an increase in daily water supply up to 12% higher, and an improvement in the bill collection ratio up to 50% higher; as in the electricity sector, employment figures decrease more strongly under private management, by up to 16%. There is also some evidence of price increases in the case of water divestitures. Finally, for the provision of sanitation services, a 25% higher increase in residential coverage is estimated as consequence of PSP.

We also find a link between the form of PSP and the estimated performance impact, with the strongest effects in the electricity sector realized by utilities whose assets were divested to the private investor, and by utilities managed under concession contracts in the water sector. Several observations are warranted in the context of this result: a very small number of divestitures in the water and sanitation sector are present in the sample, and the fact that the contract specific results are not aligned between the two sectors needs to be interpreted with caution due to the different distribution of PSP types between the sectors. Importantly however, we would expect to find results differentiated by contract types because of different contractual obligations associated with the possible range of PSP and the matching of incentives to initial conditions on the ground. We consider the results found intuitive, however it is necessary to stress that the study distills stylized facts from a global sample and that in each individual PSP case, there are a host of specific factors that will lead to differentiated results.

Finally, as in Andres (2004) and Andres et al (2006), apart from evaluating the impact of PSP on utility performance in the period following the introduction of private participation, we also evaluate the impact of PSP during the two years before, the year of, and the year after the introduction of private participation (the "period of transition"). In most of the cases, we find that either there is no statistical impact of PSP during that period, or that the impact is negligible in comparison with the one observed during the post-PSP period, that is, the period starting in the second year after the entry of the private party. For example, the effect of PSP on firm-level employment in the electricity sample during transition, while statistically significant, is 7 to 8 times smaller than during the post-PSP period proper. On some occasions, however, we record a transition effect both substantial and comparable to or stronger than the one observed post-PSP: in the electricity sample, the collection ratio increases by 20.8% during transition in addition to the hefty 48% increase over the post-PSP period for which we have data; the average residential price increases by 6.3% in the transition period, in the post-PSP period it increases by 3.2%, but the effect is statistically indistinguishable from zero. For the water sample, water sold per worker increases during transition by 16.1% and wastewater treated per connection increases by 71.7%, implying that all of the increase in that measure was realized during the transition period. Consistent with Andres (2004), we thus also find that for certain variables, PSP already starts showing results in the years immediately before and after its introduction into the utility, possibly through restructuring and alignment of managerial incentives with profit considerations.

It is also interesting to note that in some cases we explore, the effect of PSP during transition has the opposite effect to the one expected and confirmed for the post-PSP period: power distribution losses and the annual electricity interruption frequency increase during the transitional period (by 13.4% and 37.9%, respectively) before dropping in the post-PSP years, water residential coverage decreases by 12.8%, and the number of residential connections in sanitation services decreases by 68.1% before increasing in the post-PSP period. We speculate that some of these opposite effects are due to the correction in base data for the utility that oftentimes occurs in preparation of the entry of a private operator. One of the effects of private participation picked up by the company data might thus be the adjustment to 'real levels of performance' occurring during negotiation of contractual arrangements between the private party and the government. Interestingly, the average residential water price also decreases by 11% in the transition period. Again, speculation is possible as to possible political motives for a decrease in tariffs prior to the entry of the private operator, but we recommend further in-depth examination of the question given the simply tariff measure of average revenue used for this study; the simplification might hide more complex tariff movements on a case-by-case basis.

In summary, the results obtained suggest that PSP does fulfill its promise of improved company performance. However, the results also give an indication of the reasons why private participation has often encountered practical problems: one of the most robust results found in the analysis is the decrease in the labor force occurring post-PSP. The lack of convincing evidence of an increase in investment linked to PSP also suggests a deeper rooted policy problem. Even in the presence of better operational results brought on by the private sector, public investment in the improved service does not seem to increase on the basis of the data examined. This result points to serious sustainability issues for the improvements achieved under PSP. Another result is conspicuous by its absence: there is no robust evidence in the data suggesting that PSP leads to a change in prices. Given that in the majority of developing countries the problem of public services is under-pricing, one would expect an increase of prices to improve the long term sustainability of the network services. The absence of such a result points to a tenacious problem in utility sectors that the entry of the private sector does not cure: prices remain a political issue and increasing them in a developing country setting is difficult.

The paper continues as follows. Section 2 summarizes previous empirical research with a focus on literature on utilities and less developed countries. Section 3 discusses the data selection criteria and process. Section 4 describes the sample and the variables used in the study. Section 5 lays out the empirical methodology. Section 6 describes the results from the econometric analysis. Finally, Section 7 concludes with the main findings of the paper.

2. Empirical literature techniques and findings

This paper aims to study the effect of PSP on firm performance under two particular circumstances: we study electricity and water distribution services which are associated with natural monopoly characteristics, and we do so for developing rather than developed countries. The empirical literature spans a wide range of results and techniques relevant to this context and some key results are summarized in what follows.[20]

The empirical techniques employed to study the impact of private participation in its different forms fall into three broad categories. The arguably most straightforward is an analysis of the statistical significance of the difference in average values of performance indicators between SOEs and private companies (e.g. Megginson et al. [1994], Boubakri and Cosset [1998], and

[20] See Briceño-Garmendia (2004) for a detailed survey of the literature in this setting.

Hodge [1999]); however, this technique suffers from an inability to control for other determinants of performance than the ownership variable and does not take differences in initial conditions between companies into account. Hence, other studies attempt to isolate the effect of PSP over time (e.g., Estache and Rossi [2002], Andres et al. [2006], Brown et al. [2006]), using a set of panel data techniques. These studies correct for omitted variable bias and consider initial conditions of companies, it is however noteworthy that they conduct a partial equilibrium analysis only, that is, they do not take into account general equilibrium considerations or welfare effects of PSP. The latter are addressed by studies like Galal et al (1994), McKenzie and Mookherjee (2003), Chisari, Estache and Romero (1999), Galiani et al (2005), and Clarke et al (2000) which specifically perform an empirical analysis with respect to a variety of economic agents affected. As to partial effects analyzed in the empirical privatization literature, these include changes in a number of partial performance measures: employment, output and coverage (for example, Ramamurti [1996], Ros [1999], Ros and Banerjee [2000], Estache and Rossi [2002] and Andres et al [2007 forthcoming]), degrees of efficiency and productivity (for example, productivity growth in Ehrlich et al [1994]), or labor productivity in Frydman et al [1997]).

Most of the literature related to the introduction of PSP in previously state-owned enterprises relates to manufacturing (for example, Vining and Boardman [1992], Frydman et al [1997], Brown et al. [2006]). Among multi-sector studies count for example Megginson et al [1994] who treat no less than 32 different industries. Regarding sectors traditionally counted among utility services, the telecommunications sector has arguably received most attention (for example, Ramamurti [1996] and Ros [1999]) followed by transportation (for example, Ramamurti [1996] and Laurin and Bozec [2001]). In recent years, several papers have made important empirical contributions regarding the public-private debate in the electricity and water sectors. Some of the studies are based on a specific case study (for example, in the books of Galal et al [1994] and La Porta and Lopez-de-Silanes [1997]), but others have produced more comprehensive cross-country analysis, like Estache and Rossi (2002) and Andres et al (2006) for the electricity sector, Galiani et al (2005) for the water sector, and Andres (2004) for water, electricity and telecoms.

The privatization impact literature displays a large majority of studies from the industrialized world. Representative studies are, for example, Haskel and Szymanski (1993), Galal et al (1994), (who present evidence from the developed and the developing world), and many others. Apart from privatization programs being introduced earlier in industrialized countries, traditionally insufficient data from the developing world explain this imbalance. However, in the last couple of decades, due to aggressively pursued privatization in many non-industrialized countries, the privatization literature has been enriched with empirical analysis of the effect of private participation in a transitional economy setting, albeit with a strong focus on Latin American countries. Moreover, many studies have amalgamated available data from developed and developing countries (for example, Megginson et al [1994], Bortolotti et al [2001] and Dewenter and Malatesta [2000]). All of these find important increases in productivity, profitability and access to services; however, from a viewpoint of interest in less developed countries, due to data aggregation, 'mixed' studies suffer from heterogeneity problems with potentially misleading averaged results.

Studies that have focused exclusively on developing countries have yielded interesting results. Boubakri and Cosset (1998) address the question of privatization of manufacturing firms in a sample of 21 developing countries over the period 1980 to 1992, and find significant improvements in profitability, operating efficiency, capital investment, output, and total employment; importantly, they show that these effects are larger in richer developing countries. Wallsten (2001) uses data on telecoms from 30 African and Latin American countries, and finds that privatization is associated with increased access to services and a reduced price; however, this is the case only when privatization is coupled with an increase in competition, and in the presence of independent regulation. Fink et al (2002) use International Telecommunications Union (ITU) data for 86 developing countries over the period 1985 to 1999, and again find that the largest increases in quality associated with privatization appear when coupled with independent regulation. Finally, Andres (2004), Andres et al (2006) and Andres et al (2007 forthcoming) find important increases in quality, investment and labor productivity and a decrease in employment in a sample of Latin American countries in the three sectors telcoms, electricity and water distribution services.

Thus, the overview of the empirical literature surveyed in studies on both developed and developing countries shows strong, though not conclusive, support of private participation or ownership.[21] In particular, those empirical studies mentioned, which research the effects of privatization or private participation in both in developed and in developing countries over time (for example, Ramamurti [1996], Ros [1999], Ros and Banerjee [2000], Estache and Rossi [2002] and Andres et al [2006]), all testify to the fact that private sector participation is unambiguously associated with: a decrease in labor force; an increase in labor productivity; an increase in output; and an increase in coverage, efficiency and output quality (measured as, for example, as a

[21] Of the empirical papers surveyed in Briceno-Garmendia (2004), more than half show better results for firms with some kind of private participation, while one third found ambiguous results and the rest favored state ownership.

reduction of child mortality as a result from the privatization of water distribution utilities in Galiani et al [2005]).[22]

Of all the studies that compare utilities with PSP to a sample of similar SOEs, trying to control for selection bias, some provide evidence to the effect of PSP on measures which our study does not cover, like an increase in firm-level productivity growth (for example, Ehrlich et al [1994]), but others find important effects on measures that we do study, like an increase in efficiency and labor productivity (for example, Frydman et al [1999]), albeit only for firms controlled by foreign owners. Megginson et al (1994) present an important departure from the aforementioned studies in that the authors measure a significant increase in the labor force as a result from privatization; that result may well be linked to the fact that they examine manufacturing companies which have expansion strategies subsequent to efficiency improvements. They also find a substantial increase in profitability, investment and efficiency. Finally, Brown et al. (2006) find important increases in manufacturing total factor productivity in Hungary, Romania and Ukraine in the post-1989 period.

The effect on the other measures, on which our study is focusing, is less clear cut. Ehrlich et al (1994) find a long-term decrease in total costs, while Frydman et al (1999) find no significant effect of ownership change on cost reduction. And both Estache and Rossi (2002) and Andres et al (2006) find an ambiguous effect of privatization on prices. The latter is along the lines of theoretical predictions, which point to two different effects of PSP: the reduction in price due to increased efficiency, and the increase in price due to the elimination of explicit and implicit subsidies and cross-subsidies often present in the sectors analyzed. Which of those two effects will dominate depends on the initial situation and the regulatory environment.[23]

3. Selection of treatment and control group

Empirical analysis into the issue of private participation traditionally suffers from selection bias. This problem arises when we are observing an independent variable not for the entire sample but only for a sub-sample. For example, we observe the price of electricity charged by the PSP utilities once they are under PSP, but do not observe it for identical utilities in which PSP has not been introduced. In such a setting, it is possible to examine the question whether a given variable

[22] However, Wallsten (2001) finds no significant effect of the change in ownership on coverage and labor efficiency when controlling for competition.

[23] Linked to this last point, empirical analysis seems to suggest that in developing countries, and especially in sectors with natural monopoly characteristics, private participation tends to bring larger efficiency gains when coupled with an independent regulation authority; we do not pursue the regulatory aspect in our analysis.

has increased or decreased following PSP; it is however not possible to ascertain whether a similar increase or decrease has not occurred in the state-owned companies as well.

Faced with this problem, our strategy is in essence to determine a sub-sample of utilities with PSP and a corresponding sub-sample of state owned utilities (SOEs), using qualitative criteria which make sure that the state-owned utilities are a valid counterfactual to PSP. The ideal for the sample construction would be to find pairs of PSP-SOE utilities in the same sector in the same country, and also otherwise sufficiently alike that any variation in performance can be closely linked to a variation in ownership. However, in practice the pool of available utilities is by nature limited to very few (often a single) per country and most often the available comparators vary widely across a number of dimensions, such as size or customer base. All these factors can influence company performance in the pre-PSP period, which may also affect post-PSP performance and bias the estimation. For instance, with decreasing returns to scale in labor, an identical percentage expansion in employment will bring lower labor productivity gains for a larger utility, so comparing a large PSP to a small SOE would introduce a downward bias in the estimation of the effect of PSP on labor productivity. Given the practical challenges posed by the construction of the comparator sample, pragmatism and opportunism alongside a number of qualitative criteria determine our initial SOE selection; in what follows we explain that the most important minimum threshold all SOE candidates have to meet at the initial stage is that they have been corporatized. The resulting sample shows a considerably larger number of SOEs than PSP cases as we deliberately ‘oversampled’ among all available SOEs to maximize the data for the econometric analysis. However, in a second step, we then employ finer estimation techniques which reduce the sample size but also make sure that utilities with and without PSP are matched as closely as possible, based on pre-PSP characteristics. We report results for both the full sample and the smaller, closely matched sample as the comparison of the two allows an examination of the potential bias introduced by comparing SOEs and companies with PSP indiscriminately.

Constructing a data panel by collecting data over a number of years for a number of utilities in different countries and regions as we do, allows comparing performance of the same utility before and after PSP, as well as juxtaposing companies with PSP with state owned comparators at a given point in time. Concretely, we determine a number of selection criteria that yield comparable samples of the group of PSP companies (our ‘treatment group’) and the SOEs (our ‘control group’). For both treatment and control group we consider only utilities engaged in distribution of electricity or water to residential customers, as well as sanitation services provided to households. It is important to stress that households must comprise at least a portion of the utility customer base; in other words, our sample excludes pure wholesale and industrial providers in electricity and water services. For the PSP group, we also only consider utilities for which information is available for at least three years after the entry of the private operator, to make the subsequent difference-in-differences estimation meaningful on the basis of sufficient data points. [24]

3.1. Treatment group: utilities with PSP

The aim for our treatment group is to comprise the entire population of companies in the water and sanitation and electricity distribution sectors that have experienced private participation in the pre-2003 period. The initial selection of PSP cases is based on the World Bank managed Private Participation in Infrastructure (PPI) database;[25] from this starting point, a number of regional experts and consultants undertook a country-by-country verification to make certain all PSP cases in a given region were accounted for.

The selection process of the PSP sample is rendered non-trivial by the fact that we consider a range of forms of private participation from divestitures to management contracts. In many cases, the variety of PSP options requires a close case-by-case examination in order to unequivocally discern whether a company can truly be considered to belong to our PSP sample; this is more particularly true as the sectors examined are such that the state is rarely absent from asset ownership and holds a range of supervision and control functions. Consequently, we chose to consider only those companies for our PSP sample where the private party has the power to make decisions that affect the firm's performance, such as output, inputs, technology, service quality, etc.

The determination of whether the private party exercises such managerial control turned out to be extremely utility-specific and no rule of thumb (such as possession of at least 50% of voting rights by the private investor(s)) proved to be always practicable. In fact, for the initial list of PSP candidates, not only the category of PSP the utility was labeled as in the initial source of information (e.g. management contract, lease contract, concession, joint venture or full divestiture), but also the availability of reliable non-conflicting information about the control mechanism within the utility, as well as the characteristics of the investor(s) ended up greatly

[24] This implies a "cut-off" date for entry of the private operator of 2002. Very few exceptional cases of private entry occurring after 2002 were included, justified on the basis of detailed and good quality post-PSP data.

[25] http://ppi.worldbank.org. The PPI database covers all low and middle income countries as classified by the World Bank.

influencing the selection decision.[26] We performed a case-by-case analysis for each identified PSP candidate to ascertain whether it should indeed be included in the treatment group. Finally, in order to ensure availability of PSP data, we targeted those firms that have been in private hands for at least 3 years, resulting in a cut-off date for private entry of 2002.

As for representativeness, the aim was for the treatment group to represent as close as possible the total population of utilities with PSP in the realm covered by the project, i.e. every case of PSP in electricity and water and sanitation distribution in the developing regions covered (subject to the condition of minimum 3-year PSP operation mentioned above). In the event, across all regions, we managed to cover, at least to some extent, 84% (302 companies) of the targeted PSP population, with 89% coverage (161 companies) in the electricity sector, and 79% coverage (141 companies) in the water sector. We exclude utilities for which we could not gather a critical mass of data. We assume that the non-inclusion of targeted companies with PSP into the final sample is randomized and thus does not introduce any bias.

The objective of the study was to maximize coverage of the PSP population; Annex 2 sets out what the coverage of our sample is in terms of the total population covered. For electricity, our sample covers 448 million people across all regions considered. This however only represents a coverage rate of 21% of the population in the countries covered, or a 9% coverage of the entire population in all regions. The highest coverage ratio is for LAC, where our sample covers 49% of the population in the region. For water and sanitation, the number of utilities included in the sample is a larger, but the total number of people covered is less than half the electricity number - 184 million. This represents a 8% coverage of the population in the countries covered, or a 4% coverage of the entire population in the region.

3.2. Control group – SOE choice

A first possible choice of control group is the same group of companies as selected for the treatment sample, but during the period when the firms were still under state control. The analysis performed in this case is of the 'before' versus 'after' kind. Since we are looking at the same firms, there are no concerns regarding the similarity (or lack thereof) of the two groups; by the same token, the downside of this comparator choice is that it assumes stationarity. A second option for control group takes into account this stationarity concern by attempting to estimate performance of the state company *had it not been privatized*. But this approach requires multiple

[26] It should be mentioned at this point that no co-operatives are considered for this study. By their nature, co-operatives are rarely, if ever, considered candidates for privatization and we exclude them from both groups under scrutiny, PSP or SOE.

assumptions to be made in the estimation of the fictional counterfactual behavior of the privatized firm and therefore it is not implemented in our study. The third option for control group, used by many privatization studies, is to select a control group composed of state companies in the same sector that have never been privatized. A common criticism of using such a control group is that the approach does not constitute a true randomized experiment, because the legal framework and policies state-owned firms operate under are different than those that apply to the firm with PSP. Some features of this different environment can result in competitive advantages for the state-owned company (such as soft budget constrains, low taxes and cost-of-capital), but others may represent disadvantages (such as lack of public investment, mandatory social pricing, or interference in employment policies). At the end of the day, the argument can be made that if a government allowed a SOE to operate as a private firm, i.e. without state interference linked to non-commercial objectives, the SOE could do just as well as the privately run company. Testing this argument is at the heart of all privatization impact studies.

Ideally, for the third kind of control group, we would thus want the SOEs in the control group operating "as if" they were privately owned and under similar institutional framework so that any differences in performance we observe can be attributed to PSP alone. This leads us to consider the factors other than private versus public control that influence performance, and how we can create a control group minimizing these factors. The first criteria we consider is for the state company to have been corporatized, i.e. the entity would be legally separate from the state and the company's accounts be separated from the state's. The SOE should also operate with a clear commercial objective and under a commercial law framework, i.e. be incorporated under general corporate law even if the precise relationship between the state as owner and the corporation (SOE) is set out in specific legislation/regulations. This is usually the position SOEs find themselves in that have undergone some kind of restructuring and/or are being prepared for privatization. These restrictions would ensure that we do not unfairly compare private companies with SOEs that operate under a non-discernible soft budget constraint, but that might also be given limited managerial tools, e.g. in the area of pricing and employment decisions.

From this starting point, one way of arriving at a comparable sample of state run companies would be the randomized selection from the comprehensive list of qualifying control group candidates, i.e. all state owned corporatized distribution companies in water and electricity in the countries/regions covered by the study. Apart from posing obvious practical problems by leading to selection from a very large number of SOEs, this method does not allow any control for the many other characteristics in which treatment and control group differ, such as political and legal climate, country wealth, or size and activity area of the utility, to name but a few. In the event, identifying and collecting a wide array of qualitative and quantitative indicators for every utility allowed for control over differences in utility characteristics at the data analysis stage.

Practically, once the PSP sample is identified, we start by considering SOEs in the same country as first candidates for the control group. In case the country where the PSP company is based does not have an appropriate state counterpart, we specify criteria as to which countries can be considered reasonable candidates for substitution. Countries in geographical proximity with a similar economic and political environment are first choices. Ideally, both countries should be at similar stages of development, have the same market structure in the sectors considered, similar commercial laws under which firms operate and comparable political stability. Cultural characteristics might be important as well. For example, in some countries low bill collection rates are common because of historic reasons.[27] Given the range of criteria determining the possible choice of substitution countries, for the sake of practicability, the variables we eventually concentrate on are GDP per capita, to make sure that the stage of development of both countries is similar and that the purchasing power of households is similar as well; proximity to the country with PSP as proxy of regional characteristics; similar market structure (e.g. unbundling or not) and reform framework. In summary, the following empirical strategy is used to select the inital SOE control group:

- 1st , select similar SOEs in the same country, same sector and run "as if" private;
- 2nd, similar SOEs in the same country and sector, marked for privatization;
- 3rd, similar SOEs in a different but similar country, same sector and run "as if" private;
- 4th, similar SOEs in a different but similar country, same sector marked for privatization;
- 5th, similar SOEs in the same country, same sector (not "as if" private);
- 6th, similar SOE in a different but similar country, same sector (not "as if" private).

In addition to the sample selection criteria, availability and quality of the utility level data to be collected influence the composition of the final database. Most of the data is received directly from the utilities, regulatory agencies, existing national and World Bank utility databases, and relevant academic and consultant studies.[28]

If we had found the best matching SOE comparator for every PSP case, we would have ended up with the same number of SOEs and PSP companies in each country, region, and sector, and treatment and control group would be of equal size and composition. In practice, we fell short of this ideal since in many countries there were few qualifying or no SOEs at all because of the monopoly nature of the market, or alternatively, data was not available for the best comparator

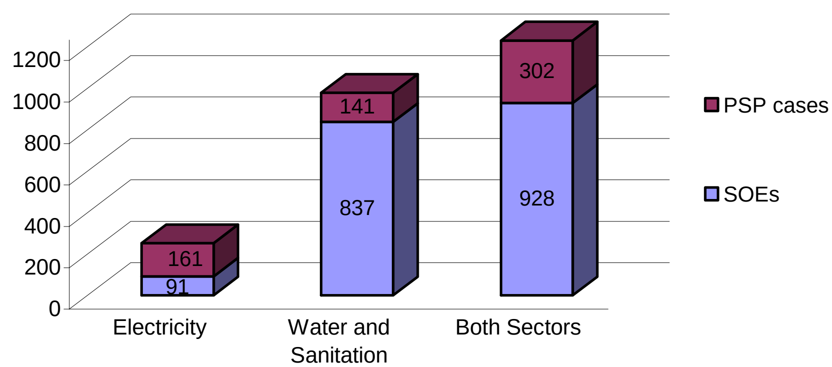
[27] Variables like this can directly affect the collection to billing ratio of the company and hence its profitability, even if all other characteristics are the same.

[28] We thank a multitude of local consultants and industry experts for their help in the data collection effort.

companies. The sampling thus was complemented by a dose of pragmatism and opportunism. Nonetheless, we remedy for this sampling deficiency by employing several types of empirical analysis, including nearest neighbor matching on the basis of propensity scores. The nearest neighbor matching ensures that we compare the treatment group to the best available control group (see Ravaillon 2001), but it comes at the cost of losing data points compared to the full sample estimates (see Section 5 for details).

As Figure 1 below shows, the final sample of electricity utilities includes 302 companies (161 with PSP experience and 91 SOEs) in 53 countries; the water sample consists of 978 utilities (141 with PSP experience and 837 SOEs) in 48 countries.[29] Overall, we were able to utilize data on 1,230 companies from 71 countries in both sectors, spanning the period from 1973 to 2005 (see also Table 1B).

Figure 1. Sample of PSP cases and SOEs by sector (number of utilities)



Note: The cut-off point for a PSP case to be included in the database is 2002; this ensures a minimum number of years with post-PSP data.

4. Data

4.1. Electricity sample description

We examine 161 companies under effective private management control and 91 SOE counterparts in the overall electricity sample. Let us recall that the PSP sample is intended to represent the total number of companies with private involvement in a given region and thus

[29] The large number of water SOEs is the result of a collaboration with the IBNET project, a benchmarking tool for water and sanitation utilities (www.ib-net.org). We are in particular grateful to Alexander Danilenko for his help and support.

mirrors the extent of private involvement in the electricity distribution sector in each region. Empirically, the regions Latin America and Caribbean (LAC) and Europeand Central Asia (ECA), followed by the Sub-Saharan Africa (SSA) region, have the highest numbers of utilities operated by private parties in the realm of electricity distribution. The rest of the regions, namely South Asia (SA), East Asia Pacific (EAP) and Middle East and North Africa (MENA) have experienced few cases with qualifying private participation in this sector. Consequently, 69% of our electricity PSP sample comes from the LAC region (111 utilities), 22% from ECA (36 utilities), 6% from SSA (9 utilities), with the rest of the regions represented only in a marginal manner because of the lack of qualifying PSP cases.[30] On the other hand, due to the pervasiveness of PSP in electricity distribution in LAC, there is a shortage of comparable state utilities to match all the selected PSP cases in this region. Therefore, while the number of PSP and SOE electricity companies is relatively well balanced outside LAC, there are considerably more PSP cases than SOEs within the LAC region for the electricity sample. Notwithstanding, we have collected a substantial amount of SOE data, with the greatest share of SOEs (48%) still coming from the LAC region (Table 3).

Table 3. Electricity sample in regional perspective

Region[1]	**PSP**[2]		**SOE**[2]		**TOTAL**	
	number	***% in PSP sample***	**number**	***% in SOE sample***	**number**	***% of Total***
LAC	111	*69%*	44	*48%*	155	*62%*
ECA	36	*22%*	21	*23%*	57	*23%*
SSA	9	*6%*	19	*21%*	28	*11%*
SA	3	*2%*	3	*3%*	6	*2%*
EAP	1	*1%*	2	*2%*	3	*1%*
MENA	1	*1%*	2	*2%*	3	*1%*
Total	161	*100%*	91	*100%*	252	*100%*

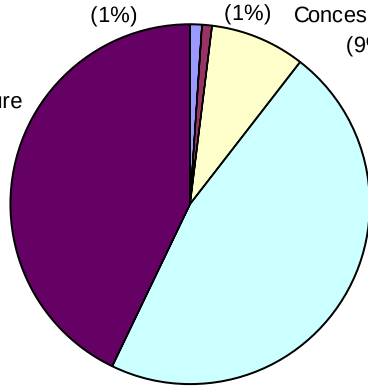
Note: [1] The regions included in the study are East Asia Pacific (EAP), Europe and Central Asia (ECA), Latin America and Caribbean (LAC), Middle East and North Africa (MENA), South Asia (SA), and Sub-Saharan Africa (SSA). [2] PSP stands for utilities with private sector participation; SOE stands for state-owned enterprise.

Divestitures, both full and partial, constitute 89% (144 utilities) of the PSP electricity sample.[31] Concession contracts account for 9% (14 utilities) of PSP cases; lease and management contracts appear less popular contractual arrangements chosen for private involvement in the electricity distribution sector in the timeframe considered for our study. Our sample of PSP experiences

[30] We are grateful to Luis Guasch and Luis Andres for making their LAC data on PSP cases in both electricity and water available to us.
[31] We make a distinction between divestitures where the private party(ies) take over 100% of asset and operating responsibility, and divestitures which are partial, resulting in mixed public-private responsibility. Partial divestitures are often labeled as Public-Private Partnerships (PPPs).

includes only two management and one lease contract (1% of the PSP sample each) as figure 2 below shows.

Figure 2. Sample of PSP cases by type of private participation, electricity
(number of respective PSP cases and percentage in overall PSP electricity sample)



Note: The cut-off point for a PSP case to be included in the database is 2002; this ensures a minimum number of years with post-PSP data.

4.2. Water sample description

In the sample of water and sanitation services (WSS), the number of SOEs is greater than that of PSP cases in all considered regions. Overall, the collected sample comprises 978 utilities, and allows us to compare the performance of 141 utilities with PSP with that of 837 SOEs. In addition to initially targeted SOE counterparts, we decided to include all extra SOE data available in existing databases as this greatly enhanced our ability to test the robustness of the results.[32] From the PSP point of view, the largest number of PSP cases in WSS exists in the LAC region, consequently almost 67% (94 utilities) of the PSP cases in the water sample comes from LAC. Respectively, the ECA region accounts for 21% (29 utilities) of the final water PSP sample, EAP for 7% (10 utilities), MENA and SSA account for 3% (4 utilities) of the sample each. There were no qualifying PSP cases in the SA region at the moment of data collection (Table 4).

[32] Collaboration with the IBNET team is gratefully acknowledged (www.ib-net.org).

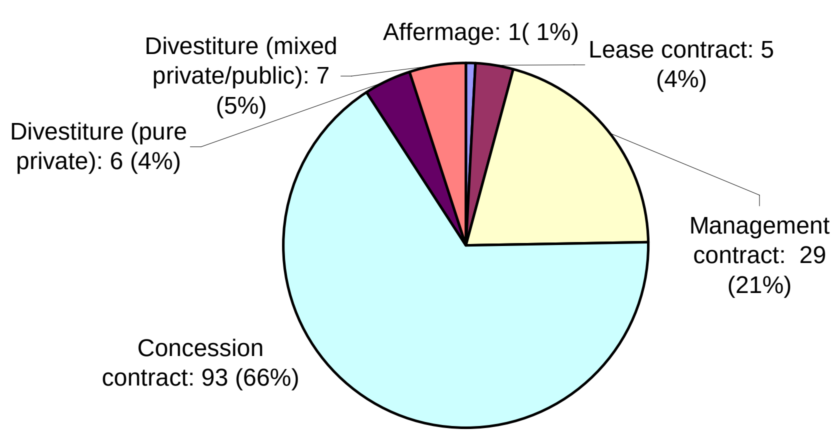
Table 4. Water and sanitation sample in regional perspective

Region[1]	PSP[2]		SOE[2]		TOTAL	
	number	*% of all PSP*	number	*% of all SOE*	number	*% of Total*
LAC	94	*67%*	330	*39%*	424	*43%*
ECA	29	*21%*	366	*44%*	395	*40%*
EAP	10	*7%*	87	*10%*	97	*10%*
MENA	4	*3%*	29	*3%*	33	*3%*
SSA	4	*3%*	25	*3%*	29	*3%*
SA	0	*0%*	0	*0%*	0	*0%*
Total	141	*100%*	837	*100%*	978	*100%*

Note: [1] The regions included in the study are East Asia Pacific (EAP), Europe and Central Asia (ECA), Latin America and Caribbean (LAC), Middle East and North Africa (MENA), South Asia (SA), and Sub-Saharan Africa (SSA). [2] PSP stands for utilities with private sector participation; SOE stands for state-owned enterprise.

Contrary to the electricity distribution sector where by far the most popular form of private participation are partial or full divestitures to private investors, in water distribution only a small portion of utilities is divested. Instead, operational control is transferred to private operators in a number of ways which allow the ownership of assets to remain with the state. Consequently, the water and sanitation PSP sample consists to 66% of concession contracts (93 utilities), 21% of management contracts (29 utilities), 4% of lease contracts (5 utilities), and 1% of affermages (1 utility). Divestitures, full and partial, constitute 9 % of the sample (13 utilities) as Figure 3 illustrates.

Figure 3. Sample of PSP cases by type of private participation, water and sanitation
(number of respective PSP cases and percentage in overall PSP WSS sample)



Note: The cut-off point for a PSP case to be included in the database is 2002; This ensures a minimum number of years with post-PSP data.

4.3. Access rates and contract type

The tables below show the prevalence of a given contract type per region and contrast it with the region-wide access rates for water and electricity. A main point of interest is the preliminary answer these tables allow regarding the question whether there is a direct link between initial conditions on the ground, in particular the availability of service, and the contract type chosen.[33] It is clear that in the electricity sector, divestures (both full and partial) are the preferred contract type, and the majority of these PSP cases occur in regions with high access rates (LAC and ECA). The majority of concession and lease and management contracts by contrast can be found in the low access region of SSA. The data thus suggests that a lower degree of private exposure is preferred in cases of little developed networks.

Table 5. Electricity sample by type of PSP and region

Region	Electricity access rate[1]	Number of utilities in the Electricity PSP sample					
		Divestiture (mixed private/public)	Divestiture (pure private)	Concession contract	Management contract	Lease contract	*Total*
LAC	88	58	51	2	0	0	*111*
ECA	97	13	18	5	0	0	*36*
SSA	24	0	0	6	2	1	*9*
SA	41	3	0	0	0	0	*3*
EAP	87	1	0	0	0	0	*1*
MENA	87	0	0	1	0	0	*1*
Total	**74**	***75***	***69***	***14***	***2***	***1***	***161***

Note: [1] Access to electricity is defined as the percentage of the total population that has electrical power in their home. It includes commercially sold electricity, both on and off the grid. For those countries where access to electricity has been assessed through government surveys, it also includes self-generated electricity.
Data are for the year 2000. Authors' calculations based on International Energy Agency (IEA). 2002. World Energy Outlook: Energy and Poverty. Available on-line at: http://www.worldenergyoutlook.org/weo/pubs/weo2002/EnergyPoverty.pdf. Paris: IEA.

The link between access rates and contract type is however less obvious in the case of water, as illustrated by Table 6. Similar to divestitures in electricity, concession contracts are the favorite PSP contract type in water and sanitation services across all regions. There is no immediate link between the access rate to improved water sources and the contract type. We would however expect our results to reflect likely contractual requirements imposed on the private party, such as a focus on improving collection rates in the case of lease and management contracts, or an increase in connections in the case of concessions. As we will see in detail later, this expectation is carried out by the results.

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[33] Ideally, this correlation should be calculated on a utility basis; however, this detailed data was not available to us to a sufficient degree.

Table 6. Water and sanitation sample by type of PSP and region

Region	Access to improved water source[1]	Number of utilities in the Water and Sanitation PSP sample						
		Concession contract	Management contract	Divestiture (mixed private/public)	Divestiture (pure private)	Lease contract	Affermage	*Total*
LAC	91	58	21	5	6	3	1	*94*
ECA	92	21	4	2	0	2	0	*29*
EAP	79	10	0	0	0	0	0	*10*
MENA	89	3	1	0	0	0	0	*4*
SSA	66	1	3	0	0	0	0	*4*
SA	84	0	0	0	0	0	0	*0*
Total	**80**	***93***	***29***	***7***	***6***	***5***	***1***	***141***

[1] Improved water source (% of population with access). Data are for the year 2004. Definition: Access to an improved water source refers to the percentage of the population with reasonable access to an adequate amount of water from an improved source, such as a household connection, public standpipe, borehole, protected well or spring, and rainwater collection. World Health Organization and United Nations Children's Fund, Meeting the MDG Drinking Water and Sanitation Target. Source: http://ddp.worldbank.org, CDF and WDI Central (April 2007) databases.

4.4. Variable description

A broad set of both qualitative and quantitative variables was collected for each utility in the sample. The qualitative set, common to both sectors, is designed to gather utility- and country-level characteristics. It notably contains the information on the type of PSP contract and the year of entry for the private sector. For our analysis, the qualitative variables are mostly used as determinants whether and for which period the utility can be considered a PSP and as control variables for the utility type and other differences among utilities. The quantitative variables include measures of outputs (e.g. water and electricity sold or produced), inputs (e.g. number of workers), physical capital (measured for example by network length), labor productivity (defined as output per worker), operational performance (e.g. level of network losses or collection rates), service quality (e.g. daily water supply and average electricity interruption frequency per year) and average prices (calculated as average revenues). Table 7 summarizes the variables used. While the qualitative variables are considered time invariant, the quantitative variables were collected on a yearly basis over 13 years, from 1992 to 2004, and where available, 2005. In a number of cases, earlier data than 1992 was available and was also included in the panel. Due to the challenging circumstances developing countries present in terms of data collection and verification, there are many gaps in the data; nevertheless, the overall size and range of the sample reached allows for robust statistical analysis. Tables 8 and 9 give the summary statistics of the variables employed in the statistical analysis for the electricity and water and sanitation sectors, respectively.

A graphical representation of the main variables underlying this study is also included in the back of the paper. The graphical representation allows to visualize the difference between the PSP and the SOE sample by following the selected variables over time.

Table 7. Core indicators description

Indicator	Electricity Distribution	Water Distribution
Output	Electricity generated; Electricity sold to residential and non-residential customers; Electricity purchased (MWh, total, per connection)	Water produced; Water sold to residential and non-residential customers; Wastewater treated ; Wastewater collected (m^3 , total, per connection)
Labor	Total number of employees; Average cost of labor per employee	Total number of employees (water and sanitation); Average cost of labor per employee
Labor Productivity	Total number of connections per worker; Total electricity produced per worker; Total electricity sold per worker; Total electricity purchased per worker (MWh)	Total number of connections (water and sanitation) per worker; Water produced per worker; Water sold per worker; Wastewater treated per worker (m^3)
Operational performance	Electricity lost in distribution (as % of electricity produced + purchased)	Number of pipe breaks (total, per water connection); Number of sanitation blockages (total, per sanitation connection)
Service quality	Average frequency of interruptions	Hours with water, daily
Investment, OPEX, revenues	Total investment per worker (USD) Total annual cost per worker (USD) Service revenues from residential and non-residential sector, absolute or per worker (USD)	
Coverage, capital, capacity	Total number of residential and nonresidential connections; Percentage of potential customers covered; Length of distribution network; Installed capacity	Total number of residential and nonresidential connections, water and sanitation; Percentage of potential customers covered; Length of distribution network, water and sanitation; Installed capacity
Collection ratio	Percentage of outstanding bills collected	
Price	Average residential tariff (USD) calculated as average revenue	

5. Empirical methodology

Our paper follows the broader literature on the impact of private ownership in estimating reduced form equations for a number of firm-level output variables, employment, labor productivity, price, coverage and service quality, while accounting for unobserved heterogeneity and selection bias. Two main strategies have emerged in privatization research to estimate the effect of introducing private management. Megginson et al (1994) gave rise to the first strategy, which

uses differences in means and medians between the respective samples, and then tests the statistical significance of that difference. This methodology can be used to estimate differences in distribution between, either, private and state-owned firms observed during the same period, or between private firms observed before and after privatization. The second strategy stems from the treatment literature (notably, Heckman and Robb [1986]). It uses a dummy variable equal to 1 for the post-privatization period, and then tests the statistical significance of the coefficient on the dummy, as well as different interaction terms, which include the dummy. A variation of the technique explicitly takes transition effects into account by adding to the specification a second dummy variable equal to one for the period immediately before and after the entry of the private party (e.g. Andres [2004], Andres et al [2006], Brown et al [2006]). In the context of a panel, most impact studies will employ a difference-in-differences technique to account simultaneously for the difference between pre- and post-event periods and between treatment and control group.[34]
The first, non-structural strategy is in particular helpful when there are too few years to carry out a full-fledged panel data analysis. As this paper makes use of a panel which spans a considerable number of years, we focus on the second, structural strategy in order to maximize the rich information at our disposal. The regression-based strategy also allows us to account for bias caused by the endogeneity of the PSP decision: we will address this issue by controlling for utility fixed effects, utility random growth and by using an instrumental variable procedure for the choice of PSP and the type of contract arrangement chosen.

While very large overall, our panel is nevertheless unbalanced. Where data gaps are concerned, in particular for the pre-PSP period, this is problematic to the extent to which, for example, a high labor productivity by a utility with PSP, observed in the post-PSP period only, may be overestimating the effect of PSP on labor productivity as a result of us not observing the (potentially even higher) labor productivity of this particular utility in the pre-PSP period. Second, the 'non-scientific' sampling of SOE companies we have described in section 4 may lead us to compare utilities which already differed on the basis of pre-PSP attributes; in other terms, because of the opportunistic oversampling we have ended up with a control sample that is too different from the treatment group to yield valid estimates.

To correct for the panel unbalances, we conduct an estimation enhanced by nearest-neighbor matching based on propensity scores. This is in the spirit of, for example, Card and Krueger (1993). We use the nearest neighbor matching as robustness check for the panel estimates calculated with the full sample; coefficients statistically significant in both approaches have a very high degree of confidence concerning their ability to assess the impact of PSP. Specifically,

[34] See for example Ravaillon (2001) for a discussion.

we proceed as follows for this estimation of the PSP effect while at the same time accounting for panel unbalances and pre-PSP differences between the treatment and the control group: following the difference-in-differences logic, for each utility the yearly observations are averaged into two mean observations, one post-PSP and one pre-PSP; for SOEs, the "PSP year" is defined as the average year in which PSP was introduced the country. If there are no PSPs in the particular country, then the PSP year attributed to the SOE is the average year for the sample. Then, a difference-in-differences estimation technique is used to isolate the effect of PSP on the treatment group. This technique allows us to eliminate utility-specific unobservables and to get the effect of PSP by regressing relevant output variables on a composite variable equal to 1 if the utility is both in the treatment group *and* is observed post-PSP. We perform the difference-in-differences estimation initially on the subset of companies only for which we have at least one pre- and post-PSP observation; moreover, we then test three specifications where utilities are matched to their nearest neighbor using a propensity score based on pre-PSP observables. In essence this means that we reduce the control group to strictly those SOEs only which are most similar to the utilities with PSP.

To summarize, we proceed in several steps in our empirical strategy: first, we undertake a structural estimation of the difference between treatment and control group by performing a regression on the full panel. Second, we account for the possibility that the decision to privatize was not random and thus the estimation results are biased by adopting an IV approach. Third, we eliminate the destabilizing factor of working with an unbalanced panel by reducing the sample to one in which all surviving utilities have at least one pre- and one post-PSP observation. Fourth, we choose the best comparators by using a propensity score nearest neighbor matching procedure on the sample of companies with pre- PSP data.

In the first step, i.e. the testing of the significance of the dummy denoting PSP occurrence, we also attempt to distinguish between effects of pure PSP and effects that arose during the years immediately before the transfer of ownership, when the government might restructure the utility to make it more attractive for private investors (see also Brown et al [2006] for a discussion of this phenomenon). The basic equation we estimate for each of the two sectors can be written in the following form:

$$y_{ijt} = \beta_{PRIV} D_{ijt}^{PRIV} + \beta_{TRANS} D_{ijt}^{TRANS} + \gamma_{jt} D_{jt} + \delta_i \varphi_t + u_{ijt}, \qquad [1]$$

where y_{ijt} is ln(variable of interest) for firm *i* in country *j* at time *t*, and D_{ijt}^{TRANS} is a dummy variable equal to 1 if the utility is a PSP observed during the transition period, $-2 \le t \le 1$, where

$t = 0$ is the year of introduction of PSP; the estimation is performed on the full panel. D_{jt} is a $JT \times 1$ vector of country-year interaction dummies; γ_{jt} is the associated $1 \times JT$ vector of coefficients; and u_{ijt} is the idiosyncratic error. The dimensions of the other variables vary across specifications and tests. In particular, we use two basic specifications related to the form of PSP: in the first, we make no distinction between different PSP contract forms, in the second we do. D_{ijt}^{PRIV} is the measure related to the contract type: in the specification in which PSP contracts are aggregated, it is a dummy variable equal to 1 if firm *i* in country *j* has private participation at time $t \geq 2$, where $t = 0$ is the year of introduction of PSP; by contrast, in the specification in which PSP contracts are disaggregated, D_{ijt}^{PRIV} is a $IT \times 3$ matrix composed of the $IT \times 1$ vectors D_{ijt}^{Divest}, $D_{ijt}^{Concession}$ and $D_{ijt}^{LeaseMan}$, where the *ijt-th* element of D_{ijt}^{Divest} is equal to 1 if the utility is a divestiture at time $t \geq 2$ and to 0 otherwise, the *ijt -th* element of $D_{ijt}^{Concession}$ is equal to 1 if the utility is a concession at time $t \geq 2$ and to 0 otherwise, and the *ijt -th* element of $D_{ijt}^{LeaseMan}$ is equal to 1 if the utility is a lease or management contract at time $t \geq 2$ and to 0 otherwise.[35]

As for $\delta_i \varphi_t$, there are four specifications that we test. In the random effects RE model, $\varphi_t = 0$. In the fixed effects FE model, $\varphi_t = 1$ and consequently δ_i is the unobserved utility fixed effect. In the fixed effects with time trend FE+TT model, $\varphi_t = (1, t)$ and consequently $\delta_i = (\delta_i^1, \delta_i^2)$, where δ_i^1 is the utility fixed unobserved effect, and δ_i^2 is the utility-level random trend for utility *i*. The time trend is included to correct for the fact that some of the observed effects may be biased due to a natural demand-driven increase as the population grows, as well as to pick up some pre-PSP selection considerations. Finally, in the 2SLS fixed effects with time trend FE+TT+IV model, the specification of $\delta_i \varphi_t$ is as in the FE+TT model, but in addition the aggregate PSP dummy and the dummies for different contract types have been regressed on a set of instrumental variables in the first stage. The IV approach addresses endogeneity concerns about the non-random selection of companies with PSP which might lead to biased estimates. The instrumented variables are the degree of PSP enthusiasm in the region, measured by the percentage of utilities already subject to PSP in the other countries of the region in years *t=0*, *t=-1*, *t=-2* and *t=-3,*[36] as well as a dummy variable equal to 1 if the country ran an annual inflation superior to 20% for at least one year in

[35] We aggregate lease and management contracts because we have too few observations for each separately. This implies that we have to assume that the two contract types do not differ much in terms of the strengths of the incentives they offer to the private operator.

[36] The specification idea is akin to the one in Brown and Earle (1999).

the decade before the first PSP contract in the country. Both instruments are uncorrelated with which particular utility was chosen for PSP; however, all the IV variables are assumed to be correlated with the decision to engage in PSP, as well as with the predominant PSP contract chosen, by mirroring the reform and thus privatization pressure on the government.

As the model used in the panel data analysis implies a semi-logarithmic relationship between the dummies and the variables of interest in levels, it should be noted that the percentage impact of the change of the dummy value from 0 to 1 is given by $e^{\beta_{PRIV}} - 1$ and $e^{\beta_{TRANS}} - 1$, respectively (Halvorsen and Palmquist [1980]). For example, β_{PRIV} =1 would imply an increase in the value of the dependent variable by 1.72 percentage points from the pre-PSP to the post-PSP period. In addition, a Generalized Least Square (GLS) specification is needed in order to correct for possible non-spherical errors. As the true variance-covariance matrix is unknown, we replace it with a consistent estimator using the sample residuals, essentially employing a Feasible GLS (FGLS) procedure to estimate [1] in its different specifications.

Next in our estimation analysis we turn to Heckman and Robb's (1986) methodology in order to calculate difference-in-differences estimates by using a nearest-neighbor matching technique and thus enhance the robustness of our results. The rationale behind the nearest neighbor matching is two fold. For one, as pointed out before, our panel is unbalanced, and there are post-PSP utility-year observations for which we do not observe pre-PSP data. Hence, while we include these observations in the panel specifications in order not to lose information, the PSP effects estimated by a regular panel procedure may be contaminated by the fact that we may not be able to determine if the observed post-PSP value is an increase or a decrease for this particular utility relative to the pre-PSP period. Secondly, even if the utilities with missing pre-PSP observations are dropped, using indiscriminately all SOE utilities in the comparator sample may result in a under- or over-estimation of the impact of PSP because our PSP and SOE sample display systematic ex-ante differences. In other words, our treatment and control group are not sufficiently similar to produce non-biased results. If we compare, for example, measures of labor productivity for PSPs that are on average large to SOEs that are on average small we will introduce a bias in the estimation. In order to address these issues, we are implementing a nearest neighbor matching procedure in which utilities with PSP are matched to state-owned utilities on the basis of pre-PSP propensity score analysis. We use several variables to calculate the propensity scores, matching companies notably on the basis of pre-PSP customer or employment numbers, and we conduct the propensity score analysis only for those companies where we have pre-PSP observations. This reduces the sample size, but improves the robustness of the results by explicitly accounting for the concerns raised by the unbalanced sample. In practice, the following equation is estimated:

$$y_{ijt} = \beta_{PRIV} D_{ijt}^{PRIV} + \beta_{TREATMENT} D_{ij}^{TREATMENT} + \beta_3 D_{ijt}^{PRIV} D_{ij}^{TREATMENT} + u_{ijt}\,, \quad [2]$$

where $t=1,2$, D_{ijt}^{PRIV} is a dummy variable equal to 1 if utility i in country j is observed post-PSP at time $t=2$, and $D_{ij}^{TREATMENT}$ is a dummy variable equal to 1 if utility i in country j is a PSP. Hence, for each utility, the available yearly observations are averaged into two observations, one pre- and one post-PSP, where for state-owned utilities, the "PSP year" is defined as the average privatization year in the country, or, if there are no PSP cases in the particular country, in the sample. We carry out pre-PSP specification tests to calculate the probability of subjecting a utility to PSP on the basis of variables like customer base, employment size and labor productivity. On the basis of the variables identified as significant in the choice for PSP, we undertake a propensity score analysis and state-owned utilities are assigned in a nearest neighbor matching procedure to utilities with PSP. The procedure ensures that it is the closest matched SOEs only that act as control group. Finally, a difference-in-differences analysis as specified in [2] is performed on the sample of matched utilities. The variable of interest here is the difference-in-differences estimator β_3, which gives the effect of PSP, given that the utility is in the treatment group and is observed post-PSP.

Before we can perform a propensity score based nearest neighbor matching, we need to determine what pre-PSP criteria can best predict the probability that the utility would be selected for PSP in the future. To this end, we estimate a version of equation [1] to provide diagnostics about possible selection criteria used by both investors and privatization authorities. We restrict the sample to the pre-PSP period, that is, both firms that were never privatized and such that would be privatized in the future, observed at least 3 years before PSP (for utilities that would be privatized), and at least 3 years before the average PSP year in the respective country (for utilities that would remain state-owned). Then, we estimate the equation

$$PSP_{ij} = \Phi\left(\beta_{PREPSP} X_{ijt} + \gamma_{jt} D_{jt} + \delta_i \varphi_t + u_{ijt}\right) \quad [3]$$

where X_{ijt} is a $IT \times K$ matrix of K independent variables (size of the customer base, length of the distribution network, employment, labor productivity, etc.), observed over I utilities for T

years, and β_{PREPSP} is the associated $1 \times K$ vector of coefficients. As before, D_{jt} is a $JT \times 1$ vector of country-year interaction dummies; γ_{jt} is the associated $1 \times JT$ vector of coefficients; and u_{ijt} is the idiosyncratic error. The definition of PSP_{ij} varies across specifications: in the aggregated specification, PSP_{ij} is a dummy variable equal to 1 if the utility was ever subject to (any form of) PSP, and in the disaggregated one, PSP_{ij} is a dummy equal to 1 if the utility was ever subjected to PSP through divestiture, concession or a management/lease contract. The variables whose coefficients are significant in this procedure are the ones that we subsequently use in the propensity score for the matching procedures.

6. Empirical results

In order to justify the use of IV and of propensity-score matching, we first report the estimation of equation [3] as it demonstrates that indeed there is a non-randomness in the PSP choice. Tables 10A and 10B contain the estimates of equation [3] for a PSP-aggregated and a PSP-disaggregated specification in which $\varphi_t = 0$. The estimates imply that in the electricity sector, the probability of selecting a utility for PSP is not independent of its customer base, employment size and efficiency. Specifically, it is the utilities with larger customer base, fewer employees and, interestingly, larger distributional losses that are more likely to be chosen for private sector participation.[37] While it is easy to understand that utilities with lower labor expenses per customer are more attractive to private investors, the distribution loss result can be interpreted in two ways: utilities with higher distributional losses are more likely to be offered for PSP, even if not more likely to be desired by the private sector.[38] Alternatively, the private sector perceives high distribution losses as an area where improvements can be made relatively easily and they act as signal for potential for increased profitability under a change in management. While all the quoted results hold for utilities whose assets are divested, only the number of residential connections remains statistically significant as explanation for PSP for utilities for which concessions are negotiated.[39] In summary, we can be quite confident about the result that the size of the company plays a role in the decision to introduce PSP.

[37] This procedure and results are comparable to Brown et al (2006), who find that privatization was correlated with firm-specific pre-privatization output and productivity, although they find that the sign varies by country.

[38] We can imagine certain concessions being made to the private party to compensate for challenging initial conditions.

[39] There are not enough management and lease contracts to make conclusive estimates of the effect of those.

The estimation results are less straight-forward in the water sector. Utilities that sell more water are more likely to be chosen for PSP, but only when the resulting contract is a lease or a management contract. Again, it is on average the utilities with fewer employees that are more likely to be chosen for private management, as well as ones with higher labor productivity. However, utilities whose assets were divested were paradoxically larger in terms of employment and smaller in terms of customer base. This could have something to do with the fact that water is a sensitive political commodity, and potentially the most attractive utilities are offered as concessions and leases, leaving the government firmly in control over the assets of the company; by the same token, private involvement in form of divestitures might face lesser political barriers where the least efficient water utilities are concerned. Overall, customer base, employment size and labor productivity in the pre-PSP period seem to influence with fair certainty the probability of PSP in both the water and the electricity sector.

It is an important result we have arrived at: the estimates imply that the introduction of private participation in the water and electricity distribution sectors does not happen in an indiscriminate way, and PSP was not introduced in a random manner. For the purposes of our study it is irrelevant whether it is utilities with high labor productivity, larger size, or another characteristic that are more likely to be offered for PSP. What matters for our estimation procedure is the fact that utilities with certain characteristics influencing their performance have a larger equilibrium probability of being selected for PSP – the endogeneity concern is thus very real. In addition to that, the results imply that pre-PSP characteristics of the utility may determine the kind of PSP contract chosen for a utility, conditional on being elected for PSP in the first place. For our purposes, the data confirms that the use of IV for the estimation on the full panel is indicated, as is the estimation procedure as defined in equation [2]. In other words, matching the utilities with PSP to state-owned utilities which are similar to the PSP utilities across the range of utility-level characteristics defined by the estimation of equation [3], is a valid empirical exercise. Adopting this way of proceeding will correct the PSP impact results regarding bias caused by pre-PSP characteristics.

Having established the desirability of carrying out an IV correction and a refined propensity score matching in order to calculate robust estimates of PSP impact, we first turn to the estimation of equation [1], that is, the regression analysis where all utility/year observation available are included in the panel. This will establish the 'baseline' of the PSP impact results. We will then examine how the results change when the panel is balanced to utilities only for which we have pre- and post-PSP observations and when PSP propensity estimates are used to select the best possible subset of SOEs as comparators. Tables 11, 12 and 13 display regression estimates for the four different specifications of equation [1]: RE, FE, FE+TT and FE+TT+IV, respectively for electricity, water, and finally sanitation services. Panel A estimates the regression on the aggregated PSP, while panel B gives the estimates of the regressions in which PSP has been disaggregated by contract type. Then, Tables 14, 15 and 16 give the estimates of the difference-in-differences procedure. Model A denotes the estimation undertaken on the sub-sample of companies for which we have all pre- and post-PSP observations, Models B-D report the results of nearest neighbor matching, using the variables reported in Table 10 as relevant and statistically significant to calculate propensity scores.

6.1. Electricity

6.1.1. Total residential connections and output per connection

a) Full sample regression

Employing a panel regression on the full sample, the number of residential connections increases as a result of PSP, although the estimates are attenuated as we move from the simplest specification (RE) to the most specialized one (FE+TT+IV). This last, most demanding, specification implies that, all other things equal, the average number of connections is larger by 3.8% for privately managed utilities than for publicly managed ones. Disaggregating by type of contract shows that the biggest increase in residential connections occurs for utilities that were given out as concessions: for those, residential connections increase by 24.5% compared to public management while only by 2.3% for divestitures, which are the most prevalent type of PSP in electricity distribution. The disaggregated results are intuitive: concession contracts will often include explicit connection targets and we would expect larger increases for this type of PSP. By contrast, the fact that divestitures only show a modest increase in connection numbers can be linked to the fact that network expansion in a developing country context is risky and not necessarily profitable depending on the tariff regime in place. Divestitures will allow greater discretion to the private party to expand the connection base or not.

As far as output per connection in MWh is concerned, the regressions on the complete sample imply no change as a result of PSP. However, the specification that disaggregated according to contract type implies that output per connection actually *decreases* for utilities that were only leased out or for which a management contract was signed. A possible interpretations for this result is the following: if the entry of the private operator is accompanied by increasing bill collections, the result might be due to consumers becoming more sensitive to the cost of consumption.

b) Nearest neighbor matching

When we look at the corrected results in the balanced sample, i.e. the difference-in-differences estimation with nearest neighbor matching, we find that the positive effect of PSP on residential connections only holds for utilities whose assets are divested, no longer for those under concession contracts. As far as output per connection in MWh is concerned, there are not enough utilities with sufficient observations to evaluate that claim in the matching procedure.

In summary, using the most stringent model specification with matching, we find an increase in the number of connections over and above what was achieved by comparable SOEs in the same period for electricity utilities whose assets were divested; no statistically significant change in output per connection as result of PSP is found.

6.1.2. Employment (number of employees)

a) Full sample regression

Confirming one of the key results of previous privatization research, employment decreases substantially as a result of PSP throughout the range of our specifications. Interestingly enough, some of the total decrease in employment takes place in the transition period, implying that some restructuring is carried out to meet the expectations of investors. Still, in the transition period employment decreases by a mere 3.4% on average for the aggregate sample, while in the post-PSP period it decreases by 27.5%. The disaggregated regressions, however, imply that this effect is differentiated across types of PSP contracts: while employment decreases in the most demanding FE+TT+IV specification for utilities with divestiture by 30.4%, it actually increases for utilities under concession contract by 15.3% compared to SOE peers. Private incentives in divestitures are most aligned with pure privatization, so the result regarding staff reductions is consistent with results in the privatization theory; as to the other contract forms, it can be argued that the private operator has less control over the labor dimension and arguably employees enjoy greater job protection.

b) Nearest neighbor matching

All specifications of the matching procedure confirm that workers are shed under divestitures relative to SOEs in the post-PSP period but the result concerning concessions is ambiguous.

There is hence strong evidence that PSP is associated with greater employment attrition for utilities with divested assets.

6.1.3. Labor productivity (connections per worker, sales per worker)

a) Full sample regression

Another robust prediction of our analysis is that labor productivity in electricity distribution companies increases significantly as a result of PSP compared to SOEs. In the overall sample, the number of residential connections per worker increases significantly in the transition and post-PSP periods, and the volume of electricity sold per worker increases in the post-PSP period. However, both effects hold only for divestitures, for which the number of residential connections per worker increases by 31.5%, while the volume of electricity sold per worker increases by 34.2%. At the same time, for concessions, leases and management contracts both variables exhibit either a decrease or no change. Again, the results are intuitive and aligned with the earlier employment result: it is in the case of divestitures that we find the largest reduction in staff numbers and so we would expect increases in labor productivity. Given the weak or non-existent effect of PSP on output and the number of residential connections we have described earlier, we can be reasonably certain that the labor productivity results are strongly employment driven. Moreover, it is in the case of divestitures that the private operator has most control over staffing decisions.

b) Nearest neighbor matching

The corrected nearest neighbor matching confirms the result: there an increase in electricity sales per worker, but the results holds only for divestitures.

Hence, there is strong evidence that PSP is associated with a significant increase in employment-driven labor productivity for utilities whose assets were divested.

6.1.4. Collection rates, service quality and distribution losses

a) Full sample regression

Using the complete sample, collection rates increase substantially in the transition and post-PSP periods as a result of PSP. Moreover, it is not just utilities whose assets are divested that are more motivated to collect their arrears, but there is evidence that also lease and management contracts provide effective incentives to improve bill collection. Given the contractual obligations generally associated with the latter type of contract, these results confirm case-specific evidence. Specifically, while collection rates increase by an impressive 50.9% more for divestitures compared to equivalent SOEs, they increase by only a little less, more precisely 43.2% more, for concessions.[40] As for service quality, the regression applied to the whole sample implies an increase in the annual interruption frequency for utilities subject to concessions, leases or management contracts and no effect for divestitures, but as we see below, the corrected matching results do not uphold these conclusions but rather reverse them. Last, the full panel results indicate that distribution losses decrease for utilities with PSP, but only when assets were divested; in that case however the reduction is significant, 8.8% in the most demanding FE+TT+IV specification. It should be noted that we would expect that management contracts also lead to a reduction in distribution losses, as this is a frequent performance target included in these contracts. The data does not support that expectation however. Finally, there is some evidence that PSP is associated with a decrease in residential coverage, but it is not confirmed by the matching procedure.

b) Nearest neighbor matching

The matching procedure confirms the positive effect of PSP on collection rates and distribution losses. By contrast, the correcting nearest neighbor matching estimation does not confirm the result of negative or insignificant impact of PSP on service quality. On the contrary, when utilities are matched by pre-PSP employment size or customer base, divestitures exhibit a statistically significant decrease in interruption frequency, in other words, an increase in quality. The evidence that PSP is associated with a decrease in residential coverage is not confirmed either by the matching procedure, so the result on the complete sample may be driven by utilities with low coverage which we only observe post-PSP.

In summary, the conclusive results are that utilities with divested assets witness an increase in service quality, a reduction of distribution losses and an increase in collection rates, while for other types of PSP contracts only the result of increased collection rates holds.

6.1.5. Investment and Price (CAPEX per worker and average price[41])

a) Full sample regression

Our preferred FE+TT+IV regression specification applied to the overall sample leads to the conclusion that PSP does not lead to an increase in investment per worker as hoped by proponents of private involvement, but rather indicates a decrease for utilities subject to lease or management contracts. Given that employment does not increase for those, it would mean that there is a real

[40] That both divestitures and concessions are associated with an increase in collection rates is one of the most unambiguous results in the whole analysis, being confirmed by all matching specifications except when utilities are matched by pre-PSP distributional losses.

[41] Calculated as average revenue.

reduction in investment following the transfer of operating rights to the private sector. Neither lease nor management contracts award investment obligations to the private investor but it is a potentially significant result that the government does not seem to take up the investment obligations in this case either. This hints at a general problem related to PSP: even in cases where the private sector involvement leads to operational improvements, a lack of capital injections for maintenance and expansion purposes will make a long term improvement in service delivery very unlikely.

With regard to prices, the estimation over the complete sample implies that there is a price increase during the transition period of 5.9%. It also implies that there is a significant increase in prices charged by concessions, leases and management contracts (but no significant change for utilities whose assets are divested); however, the propensity score nearest neighbor matching procedure does not confirm that result.

b) Nearest neighbor matching

The matching procedure implies divestitures may be seeing a statistically significant increase in investment as measured by CAPEX per worker, which is, however, partially an employment-driven result. Moreover, the data loss occasioned by the matching procedure implies that the result is based on data from eight utilities only, and a global generalization might require Nevertheless, the results are again intuitive: only in the case of divestitures has the private party investment interests. As already mentioned, the price result obtained under the full regression is not confirmed in the matching procedure. We thus conclude that there is no price change robust enough to be reported.

In summary, there is, hence, some evidence that divestitures result in higher per-worker investment, but no evidence that any type of PSP is associated with an increase in consumer price beyond what would be observed in SOE companies as well.

6.2. Water

6.2.1. Total residential connections and output per connection

a) Full sample regression

The initial, less demanding specifications of the regressions on the complete sample imply that the average number of residential connections increases during both the transition and the post-PSP period. However, the most demanding and preferred FE+TT+IV specification implies that the increase is only realized during the post-PSP period, but it is larger by a magnitude compared to the estimated effect from less demanding specification (61.5% compared to 16.2% under the FE+TT specification). Further disaggregating by types of contract suggests that this result is driven by an increase in the number of residential connections in areas served by water concessions, while for divestitures and lease and management contracts the increase is not significant.

b) Nearest neighbor matching

When utilities are matched nearest neighbor, it is only divestitures that exhibit an increase in residential connections in the post-PSP period. This is one of those instances which confirm that the full panel estimation may be overstating the effect of PSP due to the unbalanced nature of the sample which compares utilities without taking into account ex-ante characteristics. The same tests imply that output per connection increases beyond a normal trend only for concessions, only when utilities are matched by pre-PSP employment, and only significantly at 10%.

Thus, it can be concluded that there is some evidence that water divestitures result in an expansion of the customer base, but there is no conclusive evidence as to whether any type of PSP is associated with an increase in output.

6.2.2. Employment (number of employees, water and sanitation)

a) Full sample regression

In summary, the regression results on the full sample show results for the impact of PSP on employment which are highly variable between specifications and contract types. While the initial specifications which do not correct by using instrumental variables and do not disaggregate by contract type show that employment decreases for all types of PSP contracts, and this in both periods, once an IV correction of endogeneity is undertaken, it is only for water divestitures that employment (insignificantly) decreases by 39.1%, and it actually *increases* significantly for water concessions, water leases and water management contracts. Hence, we conclude that there is no conclusive evidence for an unambiguous change in employment, but this results changes under the matching specification.

b) Nearest neighbor matching

When utilities are matched by pre-PSP customer base and employment size, water divestitures exhibit a significant decrease in employment relative to utilities in state hands, the evidence related to other contract types is inconclusive. It is worth stating again that in the water sample the utilities with PSP are under-represented compared to the number of SOEs, and thus a matching procedure can yield greatly diverging results, if the characteristics of the PSP and SOE groups in the overall sample are very different. Nevertheless, the result from the matching procedure is aligned with theory and mirrors the results in the electricity sector: divestitures confer the incentive and the power to change employment numbers. The result is intuitive despite the fact that the divestitures in the water sector are rarely full divestitures.

I**t can be concluded that similar to the result in the electricity sector, utilities whose assets were at least partially divested exhibit the employment attrition effect predicted by the privatization literature.**

6.2.3. Labor productivity (connections per worker, sales per worker)

a) Full panel regression

All specifications on the overall sample imply that the number of residential connections per worker increases significantly, in both periods and for all types of PSP. As can be expected, in the most demanding FE+TT+IV specification, the estimates are significantly attenuated, and PSP is associated with an increase in the number of residential connections only in the post-PSP period, by 17.4%, and only for divestitures (by 23.3%) and concessions (by 22.6%). As far as water sold per worker is concerned, the un-matched difference-in-difference estimation implies that labor productivity increases for all types of contracts, but this result is not upheld in the matching procedure.

a) Nearest neighbor matching

Only the labor productivity result for divestitures holds in the nearest neighbor matching procedure, ie it is for this type of contract only that PSP leads to a significant increase in residential connections per worker. As mentioned above, as far as water sold per worker is concerned, the nearest neighbor matching procedure implies that there is no statistical change associated with any type of PSP contract relative to state-owned utilities, despite the positive results obtained under the full panel estimation.

Hence, PSP is associated with a higher number of residential connections per worker for divestitures, no other result is significant in all specifications.

6.2.4. Collection rates, operational performance, coverage and service quality

a) Full panel estimation

The full panel estimation results imply no impact of PSP on collection rates, but the matching procedure suggests that water concessions lead to a significant improvement in collection. The results on residential coverage are also ambiguous between the two estimation approaches: the panel regressions imply a decrease in coverage, but under the matching procedure, water concessions exhibit a significant increase in residential coverage. As far as service quality is concerned, the initial panel regressions imply that water concessions exhibit fewer pipe breaks per connection, but this is not confirmed by a finer matching procedure. Finally, water concessions see an increase in the number of hours per day with water by 19.7% on average, and this result is confirmed when utilities are matched by pre-PSP employment size.

b) Nearest neighbor matching

The matching procedure suggests that water concessions lead to a significant improvement in collection ratios. Surprisingly enough, this is not the case for divestitures, but only for concessions; this result may be partially driven by the fact that we do not observe many divestitures, and there are not enough observations to carry out finer propensity score matching. Regarding residential coverage, when utilities are matched by pre-PSP customer base and employment, water concessions exhibit a significant increase in residential coverage compared to state-owned utilities.[42] We already mentioned that pipe breaks show no significant change following PSP under the matching procedure, but there is a significant increase in service quality for water concessions measured as number of hours of daily service.

In conclusion, there is some evidence that utilities with PSP see an increase in collection rates and number of hours of daily supply and a decrease in pipe breaks, but these results only applies to water concessions.

6.2.5. Investment and Price (CAPEX per worker and average price[43])

a) Full panel regression

The panel regression implies that investment per worker increases in water concessions relative to state-owned utilities by an impressive 64.1%. However, the matching suggests that this result is largely overstated by including "worse" state-owned utilities in the un-matched specification. With regard to prices, the panel regression does not lead to the conclusion that prices change significantly between utilities with PSP and SOEs, but this result also changes under the matching procedure.

[42] But they exhibit a decrease when matched by pre-PSP output.
[43] Calculated as average revenue.

b) Nearest neighbor matching

As already mentioned, once only like-for-like PSP and SOE cases are compared, no significant difference in CAPEX per worker is found. With regard to prices however, when utilities are matched by pre-PSP customer base, water divestitures exhibit a significant increase in prices relative to state-owned ones, but there is no impact of private investor participation in the other types of contracts.

In summary, there is no evidence that the incentives from a transfer of assets and rights results in an increase in per-worker investment, while there is some evidence that those utilities which have seen their assets at least partially divested to a private investor are increasing consumer prices beyond what is done by SOEs.

6.3. Sanitation

6.3.1. Total residential connections and output per connection

a) Full panel estimation

In the full panel specifications without matching, there seems to be no effect of PSP in general or of any particular type of PSP on the number of sewerage connections. However, the results are more differentiated under the matching, if ambiguous as different for divestitures and concessions. The FE+TT+IV panel specification also implies that if there is any effect of PSP on output per connection beyond a normal trend, it is fully realized during the transition period. The disaggregated specification confirms that no increase in output per connection is found for any contract type, and this result holds for the matching specification.

b) Nearest neighbor matching

Once utilities are matched by pre-PSP customer base, employment size and volume of wastewater treated, the estimation results show that a water divestiture results in a significant increase in the number of connections, while a water concession results in a significant decrease of those. The nearest neighbor matching also confirms that PSP is associated with no increase in output per connection for any type of PSP contract.

Thus, the only effect of PSP on connections and output is an increase in the number of residential sewerage connections in utilities operated under a concession.

6.3.2. Labor productivity (connections per worker, sales per worker)

a) Full panel estimation

When we run the FE+TT specification, we find that the number of residential connections per worker increases during both the transition and the post-PSP period, and in the post-PSP period it does so for all three types of contracts. However, once the type of contract is instrumented for, the effects disappear or become insignificant. But once again, the matching will not validate this result – we will find a significant result for concession contracts. The effect of PSP on labor productivity is similar - wastewater treated per worker increases on average as a result of PSP in the overall sample specifications without instrumenting, but once we instrument for PSP and type of contract, the effects disappear. Again, the matching procedure yields the most robust result, and it is significant for divestitures as summarized below.

b) Nearest neighbor matching

When utilities are matched nearest neighbor by pre-PSP customer base and employment, the results show that in fact there is a significant increase in the number of residential sewerage connections per worker for water and sewerage concession. With regard to the effect of PSP on labor productivity, wastewater treated per worker increases when we only compare utilities for which we have a close match, but only for utilities with divested assets. They exhibit a larger increase in labor productivity than state-owned utilities, and the effect is statistically significant at the 1%.

It can be concluded that there is evidence that residential sewerage connections per worker increase for concessions, and wastewater treated per worker increases for utilities whose assets were at least partially divested to the private party.

6.3.3. Coverage and service quality

a) Full panel regression

The strongest piece of evidence from the analysis of the sanitation sample is that coverage increases as a result of PSP, but the results vary according to contract type and specification. While the FE+TT+IV un-matched specification implies that a coverage increase is only the case for water and sanitation divestitures, by 34.2% relative to utilities that remain in state hands, nearest neighbor matching implies that it is only water and sanitation concessions for which coverage increases. No conclusive result regarding service quality is found.

b) Nearest neighbor matching

Nearest neighbor matching shows an increase in coverage for water and sanitation concessions. However, there is also evidence that service quality decreases for concessions, as the number of sewerage blockages per connection increases for them under the matching specification.

In conclusion, while PSP is associated with an increase in residential sewerage coverage, there is no reason to believe that any type of PSP brings about an increase in the quality of sanitation services.

7. Conclusion

In this paper, we analyze a sample of 302 utilities with private sector participation (PSP) and 928 state owned enterprises (SOEs) in 71 developing and transition countries in order to evaluate the impact of PSP on output, employment, investment, price and service quality indicators in the electricity distribution sector and the sector for water and sanitation services. We use panel estimations to isolate the effect of PSP over a time period starting in the 1970s, with a focus on the years 1992 to 2005. In addition, we improve upon earlier privatization research in three ways: we disaggregate the analysis by PSP contract types; we use an IV procedure to extract the endogenous element of PSP; and we use a nearest neighbor matching procedure to correct for the possibility that standard regression analysis over- or understates the impact of PSP by comparing utilities with PSP to state-owned utilities that were inferior or superior to begin with. At the same time, this paper has not attempted to measure differences in the effect of PSP by world regions and by regulatory, legal and corruption environment. Future research can greatly benefit from addressing those questions.

Traditional privatization analysis is prone to a number of standard econometric problems, like measurement error and missing observations. We have gone to great lengths to ensure the validity of the data and to ensure that there are as few gaps in the panel as possible. The measurement problem has been aggravated in our case by the fact that the data originates from numerous country-specific sources, so each individual utility was checked to make sure that the quantitative variables are measured in the same units throughout (for example, megawatt-hours rather than kilowatt-hours, price per cubic meter rather than price per liter, etc), that the nominal values (tariffs, labor costs, etc.) are reported in local currency rather than already converted into dollars, etc. While the probability of measurement error in the final panel is clearly still non-zero, the empirical procedures include a correction for suspicious outliers (cutting off the tails of the distribution) to the point where the estimates become robust to further exclusion.

In summary, we find robust evidence in the global sample that PSP has a strong impact on performance, with some of the efficiency gains driven by a reduction in the staff numbers. In the case of electricity, the estimates show that over the period 1973–2005, PSP is associated with an increase in electricity sold per worker that is 40–50% higher than the increase for SOEs, an increase in bill collection ratios that is up to 85% higher, and an reduction in distributional losses that is 25% more effective; at the same time, the evidence suggests a significantly stronger decrease in employment, with PSP associated with 40% higher staff reductions. There is also evidence for an increase of capital expenditure per worker, but only in the case of divestitures. In the case of water services, the results also show a robust increase in performance associated with PSP. PSP leads to an estimated increase in residential connections up to 16% higher than for SOEs, an increase in daily water supply up to 12% higher, and an improvement in the bill collection ratio up to 50% higher; as in the electricity sector, employment figures decrease more strongly under private management, by up to 16%. There is also some evidence of price increases in the case of water divestitures. Finally, for the provision of sanitation services, a 25% higher increase in residential coverage is estimated as consequence of PSP.

In addition, while the effects in the electricity sector are unequivocally measured in most cases for utilities whose assets were fully divested to the private investor, the increase in the number of connections, and in collection rates, service quality and daily supply are measured in most cases for concession contracts in water and sewerage services. The different results on contract types in the two sectors may arguably be driven by the fact that for the water sector, divestitures are few and almost exclusively partial, with the state retaining control over the assets per se; the results also suggest that contractual obligations traditionally associated with concessions, such as connection expansion objectives, or lease and management contracts, such as increase in bill collection ratios, indeed produce measurable results in a global sample.

Our results corroborate earlier results about the impact of introducing the private sector into formerly state managed companies, but lend them a statistical robustness missing from case study and small sample research. Importantly, the results listed above confirm the premise that private incentives lead to cost savings and efficiency improvements. Two things are implied by them: first, PSP and contract type matter, with higher degrees of private participation associated with stronger gains in productivity and service improvements. We also find however that the true impact of PSP may have been over- or understated in previous research due to the non-random selection of utilities for PSP and ex-ante differences between utilities with PSP and SOE comparators. The three methodological improvements that our study undertakes, i.e. contract type disaggregation, IV, and nearest neighbor matching, correct for this bias. While the analysis implies in many cases that PSP is associated with a robust effect on a certain performance measure, the disaggregated procedure implies that in most cases, the performance of electricity concessions and water and sanitation utilities whose assets were partially divested, is statistically indistinguishable from that of state-owned utilities. It is however important to point out that there is a strong dominance of divestures in the electricity sector, and of concession contracts in the water and sanitation sector, and the observations points on the other contract types are significantly fewer. Extracting the endogenous element of the type of PSP contract chosen in several cases renders the measured effects of PSP from the model with fixed effects and time trends statistically insignificant, as in the case of the price effect – once utilities with PSP are matched to as-similar-as possible SOEs, there is no distinguishable difference anymore in the price movement of the two groups. Furthermore, the implementation of a propensity score-based matching procedure, in which utilities are compared based on pre-PSP similarities in customer base, employment size or labor productivity, leads to measured statistically significant effects of PSP, which the standard difference-in-differences procedure fails to measure (e.g., on employment in the water sector). A number of tests are implemented to ensure the validity of the procedures chosen, specifically, a Sargan and Hansen J test to check the exclusion restriction for the instruments, an *F*-test to check the validity of the inclusion of fixed effects, and a test of whether the propensity score balancing property is satisfied.

It can be concluded from the estimation procedures carried out in the paper that PSP matters and produces the efficiency improvements predicted by economic theory and demonstrated conclusively for sectors other than utilities. However, it is important to distinguish between the effects of the different types of PSP, and the effects of PSP over time. While some of the effects of PSP are realized during the transitional period, as a result of utility restructuring and managerial anticipation (for example in the case of employment reductions), most of the significant effects take place during the post-PSP period, when the private part is fully in control of the operating rights. This result is different from the one registered for example by Andres (2004). And while in the electricity sector only control over the infrastructure assets following divestiture results in collection and service quality improvements, it seems that in the water sector a simple control over operating rights granted by concessions leads to gains in those directions. However, it is the difference in the prevalence of the different contract arrangements that is thought to drive the contract type results to a considerable extent.

Statistical tables and econometric results

Table 8. Summary statistics, electricity

Variable	# of observations	Mean	Median	St. Dev.	Min	Max
Number of connection	1,252	784,032	274,917	2,111,579	105	22,299,698
Number of residential connections	2,184	620,580	262,866	1,413,477	945	19,591,838
Number of residential connections per worker	1,717	365.1	284.7	309.9	0.2	3,825.0
Length of distribution network (km)	658	34,729.2	13,392	84,026.9	48.3	632,400
Installed capacity (MWh)	604	2,921.3	440	13,906.9	0.2	299,323
Number of employees	1,997	3,553.1	1,352	8,237.5	10	80,257
Electricity sold (MWh)	2,606	4,800,000	15,760,500	470,000,000	153.6	244,000,000
Electricity sold per worker (MWh)	1,877	1,858.1	1,339.7	1,873.2	2.9	28,348.7
Electricity sold per connection (in MWh)	1,165	103.7	4.5	1,826.0	1.1	47,725.1
Electricity lost in distribution (%)	2,101	16.7	14.5	8.9	0.7	90.8
Annual interruption frequency	762	243.1	19.3	829.0	1.1	8,061
Labor cost per worker (USD)	355	9,129.2	5,291.4	12,303.4	531.2	95,609.8
Average residential tariff per MWh (USD)	1,383	96.1	77.7	105.5	1.6	754.5
Residential coverage (% of households)	473	83.8	100	26.5	3.7	100
Collection ratio (%)	456	75.2	83.8	28.4	95.9	112.3
Total annual operating expenses (OPEX) per worker (USD)	606	62,041.9	16,283	92,268.4	1.4	485,134.3
Total annual investment (CAPEX) per worker (USD)	454	10,675.2	5,194.4	13,275.2	0.1	76,233.1

Note: Collection ratio might be greater than 100% because of the prior year debts collected. Table reports variables before any outliers were cut.

Table 9. Summary statistics, water and sanitation

Variable	# of observations	Mean	Median	St. Dev.	Min	Max
Number of water connection	5,508	122,380	20,000	401,461	3	7,865,112
Number of sewerage connections	3,965	166,994	15,406	244,408	4	12,353,603
Number of water connections per worker	5,134	189.5	147.8	207.9	0.1	4,230.7
Number of sewerage connections per worker	3,688	577.2	69.2	18,424	0.1	1,076,165
Length of distribution network, water	5,089	1,160.5	268.5	3,578.5	3.5	57,321.5
Length of distribution network, sewerage	3,543	542.8	111.6	2.159.5	0.2	35,759.1
Number of employees	5,711	496.3	185	1,152.8	3	19,287
Water sold per worker (m^3)	4,980	45,672.3	32,374.5	54,619.3	4.5	942,056.1
Water sold per connection (m^3)	4,781	787.7	230.1	3,335.3	0.1	83,065.7
Wastewater treated per worker (m^3)	687	97,295.7	68,094	101,658	8.1	704,666.9
Wastewater treated per connection (m^3)	1,516	498.8	195.1	1,014.7	0.1	13,839.5
Number of pipe breaks per connection	2,619	0.2	0.2	1.9	0.1	81.4
Number of sewerage blockages per connection	1,499	0.3	0.1	2.4	0.1	54
Residential coverage (% of households)	4,677	90.2	86.5	822.7	0.1	56,311.9
Average number of hours with water daily	2,769	19.9	24	6.5	0.1	24
Labor cost per worker (USD)	4,789	4,838.8	2,700.4	5,787.0	0.1	51,201.7
Average water residential tariff (USD)	528	0.58	0.43	1.08	0.1	16.3
Collection ratio (%)	3,047	94.5	99.9	14.2	6.9	197.7
Total annual operating expenses (OPEX) per worker (USD)	4,636	13,657.4	7,374.3	17,843.1	0.1	341,976.8
Total annual investment (CAPEX) per worker (USD)	1,746	6,058.5	448.3	10,231.9	0.1	69,742.6

Note: Collection ratio might be greater than 100% because of the prior year debts collected. Table reports variables before any outliers were cut.

Table 10 A. Probability of PSP based on pre-PSP output, customer base, network, employment and labor productivity; electricity sector

Pre-PSP values			Private Sector Participation			
Electricity sold	0.234 (0.064)***					0.282 (0.439)
Nb of resid. connections		0.459 (0.091)***				3.761 (0.713)***
Electricity lost			0.675 (0.218)***			2.280 (0.432)***
Total number of employees				0.271 (0.106)***		-4.108 (0.614)***
Electricity sold per worker					1.090 (0.199)***	dropped
Observations	452	403	364	353	345	275
Pseudo R^2	0.17	0.19	0.45	0.16	0.25	0.51
			Divestiture			
Electricity sold	0.201 (0.064)***					-0.182 (0.505)
Nb of resid. connections		0.358 (0.091)***				3.754 (0.754)***
Electricity lost			0.522 (0.225)***			1.860 (0.460)***
Total number of employees				0.158 (0.110)		-3.633 (0.625)***
Electricity sold per worker					0.791 (0.201)***	dropped
Observations	432	377	332	313	305	209
Pseudo R^2	0.20	0.18	0.17	0.20	0.27	0.44
			Concession			
Electricity sold	0.314 (0.188)*					n/a
Number of residential connections		1.577 (0.444)***				2.141 (0.818)***
Electricity lost			6.916 (2.244)***			5.298 (4.418)
Total number of employees				n/a		n/a
Electricity sold per worker					n/a	n/a
Observations	60	52	49			45
Pseudo R^2	0.06	0.42	0.40			0.75

Note: The dependent variable is Private Sector Participation, which is a dummy equal to 1 if the utility was ever subjected to PSP, and to 0 otherwise. All regressions include fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level.

Table 10 B. Probability of PSP and pre-PSP output, customer base, network, employment and labor productivity; water and sanitation sector

Pre-PSP values						
			Private Sector Participation			
Water sold	0.358 (0.086)***					0.290 (0.133)**
Number of residential connections		-0.488 (0.180)***				n/a
Number of pipe breaks per connection			-2.181 (1.605)			n/a
Total number of employees				0.263 (0.096)***		-0.206 (0.089)*
Water sold per worker					1.015 (0.229)***	0.602 (0.298)**
Observations	387	195	19	449	354	449
Pseudo R^2	0.45	0.69	0.16	0.64	0.43	0.46
			Divestiture			
Water sold	n/a					n/a
Number of residential connections		-0.062 (0.072)				-1.589 (0.858)*
Number of pipe breaks per connection			n/a			n/a
Total number of employees				-0.135 (0.304)		1.856 (1.049)*
Water sold per worker					n/a	n/a
Observations		117		30		30
Pseudo R^2		0.04		0.02		0.15
			Concession			
Water sold	0.047 (0.155)					-0.001 (0.152)
Number of residential connections		0.005 (0.065)				n/a
Number of pipe breaks per connection			n/a			n/a
Total number of employees				0.134 (0.133)		-0.072 (0.190)
Water sold per worker					0.413 (0.634)	n/a
Observations	106	215		197	81	156
Pseudo R^2	0.45	0.27		0.60	0.57	0.19
			Lease or management contract			
Water sold	1.136 (0.291)***					1.325 (0.396)***

Number of residential connections		-1.156 (0.536)**				n/a
Number of pipe breaks per connection			-2.181 (1.605)			n/a
Total number of employees				1.054 (0.273)***		0.416 (0.511)
Water sold per worker					0.928 (0.267)***	-0.416 (0.511)
Observations	293	20	19	309	276	276
Pseudo R^2	0.57	0.34	0.16	0.45	0.36	0.57

Note: The dependent variable is Private Sector Participation, which is a dummy equal to 1 if the utility was ever subjected to PSP, and to 0 otherwise. All regressions include fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 11 A. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), electricity

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Electricity sold per worker	(6) Electricity sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Electricity lost in distribution	(11) Annual interrupt. frequency
Model A: Log levels with a random effect for utility											
PPD	0.589	0.474	0.778	-0.196	0.428	0.039	0.203	-0.072	0.008	-0.218	-1.082
	(0.072)***	(0.029)***	(0.351)**	(0.067)***	(0.043)***	(0.080)	(0.069)***	(0.067)	(0.017)	(0.028)***	(0.090)***
TD	0.273	0.211	0.576	0.142	0.226	0.087	0.065	-0.057	0.035	0.030	-0.433
	(0.073)***	(0.031)***	(0.349)	(0.068)***	(0.044)***	(0.078)	(0.071)	(0.044)	(0.019)*	(0.026)	(0.097)***
Observations	2,159	1,713	439	1,993	1,873	1,136	450	473	1,356	2,086	762
Log-likelihood	-3,261.8	-885.6	-909.6	-2,715.7	-1,700.9	-1,060.2	-219.0	47.5	57.1	-1,016.2	-990.9
Model B: Log levels with a fixed effect for utility											
PPD	0.035	0.267	0.067	-0.265	0.305	0.022	0.391	-0.081	0.031	-0.102	0.074
	(0.012)***	(0.026)***	(0.414)	(0.024)***	(0.033)***	(0.063)	(0.067)***	(0.026)***	(0.036)	(0.026)***	(0.066)
TD	0.014	0.044	0.204	-0.046	0.032	-0.060	0.186	-0.074	0.066	0.116	0.314
	(0.010)	(0.022)**	(0.338)	(0.020)***	(0.027)	(0.051)	(0.054)***	(0.018)***	(0.031)**	(0.021)***	(0.053)***
Observations	2,159	1,713	439	1,993	1,873	1,136	450	473	1,356	2,086	761
Log-likelihood	1,744.4	357.4	-585.3	443.2	-118.1	-18.8	109.5	539.9	223.3	178.1	-167.7
Model C: Log levels with a fixed effect for utility and a firm-specific time trend											
PPD	0.036	0.263	0.088	-0.260	0.301	0.021	0.391	-0.079	0.034	-0.091	0.075
	(0.012)***	(0.026)***	(0.418)	(0.024)***	(0.033)***	(0.064)	(0.067)***	(0.026)***	(0.036)	(0.027)***	(0.066)
TD	0.013	0.042	0.213	-0.043	0.029	-0.061	0.186	-0.071	0.066	0.122	0.314
	(0.010)	(0.022)*	(0.343)	(0.020)***	(0.027)	(0.052)	(0.054)***	(0.018)***	(0.031)**	(0.021)***	(0.053)***
Observations	2,147	1,701	431	1,968	1,850	1,107	450	458	1,338	2,055	756
Log-likelihood	1,759.7	351.2	-577.1	429.0	-126.5	-32.0	109.5	517.0	218.1	170.8	-168.2
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation											
PPD	0.037	0.251	0.087	-0.243	0.279	0.016	0.394	-0.074	0.031	-0.083	0.089
	(0.012)***	(0.026)***	(0.418)	(0.025)***	(0.033)***	(0.065)	(0.068)***	(0.027)***	(0.036)	(0.026)***	(0.066)
TD	0.016	0.037	0.212	-0.034	0.019	-0.066	0.189	-0.065	0.061	0.126	0.321
	(0.010)	(0.022)*	(0.340)	(0.020)*	(0.027)	(0.053)	(0.056)***	(0.018)***	(0.031)**	(0.021)***	(0.053)***
Observations	2,129	1,687	430	1,944	1,830	1,083	441	441	1,331	2,029	750
Log-likelihood	88.2	54.4	-702.1	556.2	-154.4	-65.7	123.3	497.5	143.3	175.4	-87.2

Note: All regressions include fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 11 B. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), electricity

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Electricity sold per worker	(6) Electricity sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Electricity lost in distribution	(11) Annual interruption frequency
Model A: Log levels with a random effect for utility											
PPD*Divestiture.	0.586	0.482	0.994	-0.195	0.431	0.052	0.184	-0.072	0.005	-0.222	-1.103
	(0.073)***	(0.029)***	(0.359)***	(0.066)***	(0.043)***	(0.084)	(0.072)***	(0.079)	(0.017)	(0.028)***	(0.091)***
PPD*Concession	0.679	0.014	1.505	0.741	0.283	0.049	0.381	-0.073	0.169	-0.123	-0.460
	(0.343)**	(0.193)	(1.949)	(0.401)*	(0.259)	(0.192)	(0.195)*	(0.109)	(0.085)**	(0.129)	(0.471)
PPD*L/M contract	0.728	-0.537	-3.450	-1.900	-1.629	-0.814	dropped	dropped	0.907	0.226	3.066
	(1.170)	(0.248)**	(1.495)***	(0.609)***	(0.367)***	(0.333)***			(0.150)***	(0.420)	(1.003)***
Observations	2,159	1,713	439	1,993	1,873	1,136	450	473	1,356	2,086	762
Log-likelihood	-3,261.7	-882.7	-905.3	-2.709.1	-1,700.7	-1,060.1	-218.5	47.5	59.0	-1,015.4	-989.9
Model B: Log levels with a fixed effect for utility											
PPD*Divestiture	0.023	0.292	0.105	-0.287	0.321	0.040	0.407	-0.089	0.013	-0.104	-0.027
	(0.012)*	(0.026)***	(0.418)	(0.024)***	(0.033)***	(0.069)	(0.075)***	(0.031)***	(0.036)	(0.027)***	(0.067)
PPD*Concession	0.210	-0.238	0.992	0.143	-0.006	-0.018	0.356	-0.067	0.293	-0.093	0.547
	(0.035)***	(0.094)***	(0.986)	(0.084)*	(0.113)	(0.091)	(0.098)***	(0.039)*	(0.082)***	(0.074)	(0.164)***
PPD*Lease or Man contract	0.117	-0.397	-2.079	0.041	-1.451	-0.714	dropped	dropped	0.895	0.340	1.608
	(0.113)	(0.138)***	(1.179)*	(0.224)	(0.219)***	(0.159)***			(0.133)***	(0.238)	(0.336)***
Observations	2,159	1,713	439	1,993	1,873	1,136	450	473	1,356	2,086	762
Log-likelihood	1,789.1	372.8	-582.9	457.0	-113.9	-18.6	109.6	539.9	229.5	179.9	-162.8
Model C: Log levels with a fixed effect for utility and a firm-specific time trend											
PPD*Divestiture	0.023	0.288	0.125	-0.282	0.317	0.038	0.407	-0.087	0.016	-0.093	0.028
	(0.012)*	(0.026)***	(0.423)	(0.025)***	(0.033)***	(0.070)	(0.075)***	(0.031)***	(0.036)	(0.027)***	(0.067)
PPD*Concession	0.212	-0.243	1.022	0.148	-0.010	-0.019	0.356	-0.066	0.295	-0.085	0.548
	(0.035)***	(0.094)***	(0.992)	(0.084)*	(0.113)	(0.093)	(0.099)***	(0.040)	(0.083)***	(0.075)	(0.164)***
PPD*Lease or Man contract	0.117	-0.336	-2.053	0.045	-1.393	-0.764	dropped	dropped	0.865	0.352	4.081
	(0.113)	(0.139)***	(1.186)*	(0.225)	(0.184)***	(0.162)***			(0.134)***	(0.238)	(0.345)***
Observations	2,147	1,701	431	1,968	1,850	1,107	450	458	1,338	2,055	756
Log-likelihood	1,774.4	366.7	-574.8	442.6	-122.4	-31.8	109.5	517.1	224.3	172.6	-163.3
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation											
PPD*Divestiture	0.023	0.274	0.124	-0.265	0.294	0.034	0.411	-0.081	0.014	-0.084	0.042
	(0.012)*	(0.027)***	(0.422)	(0.025)***	(0.034)***	(0.071)	(0.076)***	(0.032)***	(0.036)	(0.026)***	(0.068)
PPD*Concession	0.219	-0.215	1.021	0.142	0.015	-0.024	0.359	-0.059	0.294	-0.076	0.546

	(0.034)***	(0.094)***	(0.992)	(0.084)*	(0.113)	(0.094)	(0.099)***	(0.040)	(0.082)***	(0.072)	(0.165)***
PPD*Lease or Man contract	0.120 (0.112)	-0.337 (0.138)***	-2.053 (1.187)*	0.054 (0.223)	-1.394 (0.183)***	-0.763 (0.162)***	dropped	dropped	0.865 (0.133)***	0.359 (0.230)	4.059 (0.345)***
Observations	2,129	1,687	430	1,944	1,830	1,083	450	441	1,331	2,029	750
Log-likelihood	79.0	64.2	-507.7	556.7	-110.8	-72.0	16.2	2.8	101.7	90.2	-25.6

Note: All regressions include a Transition Dummy, fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 12 A. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), water

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Water sold per worker	(6) Water sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Water pipe breaks per connection	(11) Hours with water daily
Model A: Log levels with a random effect for utility											
PPD	0.161	0.381	0.588	-0.233	0.149	0.045	0.004	-0.053	0.391	-0.382	0.281
	(0.035)***	(0.046)***	(0.262)***	(0.028)***	(0.080)*	(0.090)	(0.020)	(0.027)*	(0.087)***	(0.301)	(0.049)***
TD	0.117	0.184	0.725	-0.118	0.009	-0.008	-0.020	-0.087	0.275	-0.010	0.095
	(0.031)***	(0.043)***	(0.294)***	(0.025)***	(0.082)	(0.091)	(0.020)	(0.024)***	(0.100)***	(0.291)	(0.041)***
Observations	2,365	2,365	1,748	5,447	4,749	4,560	2,797	4,405	1,330	2,514	2,730
Log-likelihood	-119.1	-3.9	-263.0	141.6	35.1	166.6	8.4	131.4	120.4	-56.5	-101.0
Model B: Log levels with a fixed effect for utility											
PPD	0.150	0.451	0.366	-0.251	0.030	0.025	0.037	-0.055	0.925	-0.038	0.345
	(0.033)***	(0.054)***	(0.503)	(0.028)***	(0.120)	(0.122)	(0.028)	(0.031)*	(0.231)***	(0.472)	(0.054)***
TD	0.110	0.244	0.453	-0.132	-0.078	-0.028	0.009	-0.086	0.786	0.297	0.140
	(0.029)***	(0.049)***	(0.474)	(0.024)***	(0.113)	(0.116)	(0.027)	(0.026)***	(0.223)***	(0.430)	(0.046)***
Observations	2,365	2,365	1,748	5,447	4,749	4,560	2,797	4,405	1,330	2,514	2,730
Log-likelihood	-56.5	-20.8	-24.9	14.6	1.1	14.9	5.3	10.3	11.0	-3.2	-9.8
Model C: Log levels with a fixed effect for utility and a firm-specific time trend											
PPD	0.150	0.451	0.366	-0.271	-0.140	-0.160	0.037	-0.073	0.927	0.073	0.193
	(0.032)***	(0.054)***	(0.503)	(0.028)***	(0.115)	(0.116)	(0.027)	(0.031)***	(0.233)***	(0.497)	(0.029)***
TD	0.110	0.244	0.453	-0.140	-0.155	-0.112	0.008	-0.095	0.787	0.340	0.077
	(0.029)***	(0.049)***	(0.474)	(0.024)***	(0.108)	(0.110)	(0.026)	(0.026)***	(0.226)***	(0.449)	(0.024)***
Observations	2,365	2,365	1,748	5,097	4,402	4,220	2,715	4,063	1,251	2,187	2,382
Log-likelihood	-0.1	-20.2	-24.3	18.6	1.2	2.4	5.6	10.3	10.9	3.3	-7.7
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation											
PPD	0.479	0.160	0.498	0.411	0.175	0.043	-0.009	-0.044	0.200	-0.380	0.025
	(0.131)***	(0.041)***	(0.165)***	(0.082)***	(0.055)***	(0.065)	(0.012)	(0.027)*	(0.052)***	(0.217)*	(0.032)
TD	0.012	-0.044	1.088	0.283	0.149	0.109	0.024	-0.120	0.016	0.030	-0.028
	(0.151)	(0.049)	(0.283)***	(0.099)***	(0.077)*	(0.092)	(0.017)	(0.031)***	(0.096)	(0.290)	(0.033)
Observations	2,365	2,365	1,748	5,083	4,393	4,210	2,715	3,924	1,251	2,181	2,370
Log-likelihood	-9.6	-15.6	-2.4	1.5	0.9	9.6	9.9	3.8	1.1	-2.2	-4.8

Note: All regressions include fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 12 B. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), water

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Water sold per worker	(6) Water sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Water pipe breaks per connection	(11) Hours with water daily
Model A: Log levels with a random effect for utility											
PPD*Divestiture	0.056	0.462	0.857	-0.332	0.662	0.017	-0.044	-0.153	0.285	0.484	0.128
	(0.042)	(0.070)***	(0.852)	(0.048)***	(0.272)***	(0.305)	(0.077)	(0.044)***	(0.280)	(0.777)	(0.128)
PPD*Concession	0.156	0.344	0.597	-0.184	0.103	0.072	-0.024	0.008	0.300	-0.707	0.253
	(0.034)***	(0.052)***	(0.276)**	(0.031)***	(0.089)	(0.098)	(0.020)	(0.029)	(0.101)***	(0.314)***	(0.053)***
PPD*Lease or	0.158	0.298	0.279	-0.216	0.175	-0.050	-0.042	-0.128	0.386	-0.097	0.336
Man contract	(0.050)***	(0.074)***	(0.759)	(0.043)***	(0.117)	(0.147)	(0.030)	(0.043)***	(0.101)***	(0.398)	(0.063)***
Observations	2,581	2,368	1,748	5,447	4,749	4,560	2,797	4,405	1,330	2,514	2,730
Log-likelihood	-178.3	-734.7	-936.9	-551.4	-439.2	-538.6	-118.3	-119.1	-831.0	-66.2	-51.1
Model B: Log levels with a fixed effect for utility											
PPD*Divestiture	0.048	0.499	0.680	-0.341	0.343	-0.547	-0.042	-0.158	0.524	dropped	0.174
	(0.040)	(0.074)***	(0.875)	(0.047)***	(0.398)	(0.400)	(0.079)	(0.046)**	(0.317)		(0.173)
PPD*Concession	0.142	0.381	0.0281	-0.206	-0.013	0.111	-0.022	0.002	0.578	-0.390	0.323
	(0.032)***	(0.059)***	(0.539)	(0.030)***	(0.130)	(0.130)	(0.026)	(0.033)	(0.200)***	(0.462)	(0.060)***
PPD*Lease or	0.153	0.369	dropped	-0.233	0.070	-0.172	-0.028	-0.134	0.623	-0.004	0.388
Man contract	(0.047)***	(0.085)***		(0.042)***	(0.156)	(0.191)	(0.038)	(0.047)**	(0.201)***	(0.482)	(0.067)***
Observations	2,581	2,368	1,748	5,447	4,749	4,560	2,797	4,405	1,330	2,514	2,730
Log-likelihood	-98.7	-18.5	-22.8	-14.1	-33.1	-12.70	-92.1	-10.3	-11.3	-38.1	-8.4
Model C: Log levels with a fixed effect for utility and a firm-specific time trend											
PPD*Divestiture	0.048	0.499	0.680	-0.363	0.181	-0.719	-0.042	-0.177	0.525	dropped	0.041
	(0.040)	(0.074)***	(0.875)	(0.047)***	(0.382)	(0.380)*	(0.077)	(0.046)***	(0.320)		(0.090)
PPD*Concession	0.142	0.381	0.281	-0.223	-0.171	-0.068	-0.022	-0.016	0.578	-0.334	0.180
	(0.032)***	(0.059)***	(0.539)	(0.031)***	(0.125)	(0.124)	(0.025)	(0.034)	(0.202)	(0.482)	(0.032)***
PPD*Lease or	0.153	0.369	dropped	-0.252	-0.108	-0.350	-0.031	-0.152	0.624	0.158	0.235
Man contract	(0.047)***	(0.085)***		(0.042)***	(0.150)	(0.182)	(0.037)	(0.047)***	(0.203)***	(0.514)	(0.035)***
Observations	2,581	2,368	1,748	5,097	4,402	4,220	2,715	4,063	1,251	2,187	2,382
Log-likelihood	-97.2	-18.0	-22.2	-17.5	-13.1	-2.2	-93.1	-10.2	-11.4	-2.3	-7.2
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation											
PPD*Divestiture	0.241	0.209	1.127	-0.329	0.856	0.652	0.024	-0.097	0.092	0.233	-0.116
	(0.315)	(0.102)**	(1.249)	(0.236)	(0.251)***	(0.299)***	(0.147)	(0.070)	(0.437)	(0.642)	(0.099)
PPD*Concession	0.764	0.204	0.495	0.586	0.116	0.010	-0.009	0.045	0.100	-0.639	-0.015
	(0.156)***	(0.048)***	(0.173)***	(0.096)***	(0.064)*	(0.074)	(0.013)	(0.31)	(0.066)	(0.243)***	(0.037)
PPD*Lease or	0.042	0.110	0.420	0.383	0.250	0.064	-0.025	-0.132	0.286	-0.076	0.126

Man contract	(0.240)	(0.074)	(0.468)	(0.152)***	(0.102)***	(0.129)	(0.026)	(0.054)***	(0.072)***	(0.478)	(0.053)***
Observations	2,580	2,368	1,748	5,083	4,393	4,210	2,715	4,054	1,251	2,181	2,370
Log-likelihood	-97.5	-6.5	-1.6	-1.4	-8.6	-29.5	-99.5	-5.9	-3.7	-1.7	-3.6

Note: All regressions include a Transition Dummy, fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 13 A. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), sanitation

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) Wastewater treated per worker	(4) Wastewater treated per connection	(5) Residential coverage	(6) Sewerage blockages per connection
Model A: Log levels with a random effect for utility						
PPD	0.033	0.269	-0.377	-0.047	0.221	0.177
	(0.066)	(0.079)***	(0.142)***	(0.215)	(0.043)***	(0.375)
TD	0.032	0.104	-0.568	0.054	0.189	-0.206
	(0.058)	(0.072)	(0.131)***	(0.213)	(0.036)***	(0.370)
Observations	1,828	1,718	678	1,516	2,796	1,499
Log-likelihood	469.9	505.4	-383.1	-376.6	-408.1	-126.9
Model B: Log levels with a fixed effect for utility						
PPD	0.035	0.342	-0.503	-0.305	0.253	0.336
	(0.065)	(0.086)***	(0.156)***	(0.311)	(0.045)***	(0.637)
TD	0.039	0.170	-0.673	-0.189	0.213	-0.106
	(0.056)	(0.077)***	(0.141)***	(0.294)	(0.037)***	(0.559)
Observations	1,828	1,718	678	1,516	2,796	1,4999
Log-likelihood	19.4	17.9	-12.5	-2.0	-8.8	9.3
Model C: Log levels with a fixed effect for utility and a firm-specific time trend						
PPD	0.035	0.341	-0.503	-0.305	0.254	0.601
	(0.065)	(0.085)***	(0.158)***	(0.315)	(0.046)***	(0.686)
TD	0.039	0.170	-0.673	-0.189	0.214	-0.091
	(0.056)	(0.076)***	(0.143)***	(0.267)	(0.038)***	(0.594)
Observations	1,828	1,718	620	1,458	2,708	1,207
Log-likelihood	19.1	17.7	-12.1	-2.2	-8.5	-2.3
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation						
PPD	0.030	0.076	0.168	0.188	0.118	0.012
	(0.147)	(0.085)	(0.127)	(0.140)	(0.056)**	(0.156)
TD	-0.519	-0.268	-0.055	0.540	0.014	0.018
	(0.178)***	(0.106)***	(0.195)	(0.227)***	(0.067)	(0.098)
Observations	1,828	1,718	678	1,516	1,795	2,795
Log-likelihood	96.9	18.1	-5.9	-1.4	-16.6	-7.7

Note: All regressions include fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 13 B. A panel data analysis of levels, with a Post-Privatization dummy (PPD) and a Transition dummy (TD), sanitation

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) Wastewater treated per worker	(4) Wastewater treated per connection	(5) Residential coverage	(6) Sewerage blockages per connection
Model A: Log levels with a random effect for utility						
PPD*Divestiture	0.017	0.334	0.552	-0.054	0.137	0.047
	(0.085)	(0.104)***	(0.225)***	(0.624)	(0.060)***	(1.067)
PPD*Concession	-0.011	0.183	-0.266	0.059	0.268	0.310
	(0.075)	(0.088)**	(0.149)*	(0.219)	(0.047)***	(0.397)
PPD*Lease or Man contract	0.025	0.217	-2.880	-1.176	0.169	-0.528
	(0.111)	(0.123)*	(0.316)***	(0.513)***	(0.066)***	(0.573)
Observations	1,828	1,718	678	1,516	2,796	1,499
Log-likelihood	454.3	504.9	-489.2	-381.7	-413.8	-128.4
Model B: Log levels with a fixed effect for utility						
PPD*Divestiture	0.014	0.370	0.497	-0.230	0.164	dropped
	(0.081)	(0.107)***	(0.225)**	(0.656)	(0.061)***	
PPD*Concession	-0.021	0.232	-0.383	-0.123	0.307	0.923
	(0.072)	(0.095)***	(0.161)***	(0.321)	(0.049)***	(0.715)
PPD*Lease or Man contract	0.042	0.299	-3.383	-1.431	0.204	-0.228
	(0.108)	(0.133)**	(0.335)***	(0.563)***	(0.069)***	(0.709)
Observations	1,828	1,718	678	1,516	2,796	1,499
Log-likelihood	17.8	16.5	-19.6	-2.2	-8.5	-7.9
Model C: Log levels with a fixed effect for utility and a firm-specific time trend						
PPD*Divestiture	0.014	0.370	0.467	-0.230	0.165	dropped
	(0.081)	(0.107)***	(0.227)**	(0.663)	(0.062)***	
PPD*Concession	-0.021	0.232	-0.383	-0.123	0.308	0.886
	(0.072)	(0.095)***	(0.163)***	(0.324)	(0.050)***	(0.753)
PPD*Lease or Man contract	0.042	0.300	-3.383	-1.430	0.206	0.254
	(0.108)	(0.133)**	(0.339)***	(0.569)***	(0.070)***	(0.783)
Observations	1,828	1,718	620	1,458	2,701	1,207
Log-likelihood	17.7	16.3	-19.1	-2.1	-8.2	-1.2
Model D: Log levels with a fixed effect for utility, a firm-specific time trend, IV estimation						
PPD*Divestiture	0.136	0.203	0.854	0.351	0.294	0.119
	(0.353)	(0.203)	(0.751)	(1.407)	(0.141)**	(0.934)
PPD*Concession	0.241	0.136	0.184	0.202	0.091	0.134
	(0.177)	(0.103)	(0.135)	(0141)	(0.063)	(0.270)
PPD*Lease or Man contract	-0.332	-0.057	-0.075	-0.617	0.087	-0.070
	(0.262)	(0.151)	(0.341)	(0.930)	(0.117)	(0.912)
Observations	1,828	1,718	678	1,516	2,795	1,498

Log-likelihood	9.7	18.1	-5.9	-1.4	-7.7	-18.0

Note: All regressions include a Transition Dummy, fixed effects for country and year, as well as country-year interaction dummies. *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 14 A. Difference-in-Differences with Matching, electricity

	(1) Number of total res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Electricity sold per worker	(6) Electricity sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Electricity lost in distribution	(11) Annual interruption frequency
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only											
PSP	0.012	0.281	1.997	-0.363	0.405	0.003	0.536	-0.179	-0.101	-0.220	-0.267
	(0.049)	(0.129)**	(0.827)***	(0.106)***	(0.133)***	(0.145)	(0.150)***	(0.107)*	(0.213)	(0.076)***	(0.202)
Observations	101	69	9	82	79	38	18	27	39	114	29
R^2	0.04	0.07	0.40	0.12	0.10	0.01	0.41	0.07	0.02	0.07	0.03
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers											
PSP	0.062	0.098	n/a	-0.088	0.171	0.106	0.619	n/a	-0.571	-0.140	-0.287
	(0.091)	(0.136)		(0.157)	(0.129)	(0.139)	(0.198)***		(0.552)	(0.132)	(0.238)
Observations	97	69		69	69	33	15		28	75	25
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment											
PSP	0.021	0.156	1.917	-0.367	0.323	-0.040	0.482	-0.006	-0.848	0.003	-0.506
	(0.157)	(0.186)	(0.330)***	(0.128)***	(0.201)*	(0.220)	(0.220)**	(0.006)	(0.536)	(0.139)	(0.234)**
Observations	70	69	8	82	79	27	14	18	25	79	25
Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP electricity lost in distribution											
PSP	0.052	0.131	1.924	-0.161	0.227	0.269	0.567	-0.045	-0.274	-0.145	-0.169
	(0.061)	(0.156)	(0.328)***	(0.143)	(0.149)	(0.314)	(0.205)***	(0.115)	(0.581)	(0.121)	(0.316)
Observations	75	64	8	76	74	30	18	20	26	114	22

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 14 B. Difference-in-Differences with Matching, by type of PSP contract, electricity

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Electricity sold per worker	(6) Electricity sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Electricity lost in distribution	(11) Annual interruption frequency
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only											
Divestiture	0.012	0.387	1.997	-0.441	0.486	0.074	0.649	-0.173	-0.109	-0.199	-0.335
	(0.048)	(0.115)***	(0.827)***	(0.102)***	(0.131)***	(0.255)	(0.200)***	(0.165)	(0.216)	(0.079)***	(0.192)*
Observations	94	64	9	77	74	33	13	21	38	107	25
R^2	0.01	0.15	0.40	0.19	0.15	0.01	0.45	0.05	0.02	0.06	0.06
Concession	0.009	-0.632	n/a	0.345	-0.337	-0.026	0.491	-0.184	0.155	-0.379	0.060
	(0.119)	(0.176)***		(0.202)*	(0.240)	(0.171)	(0.139)***	(0.142)	(0.912)	(0.161)***	(0.217)
Observations	39	26		36	33	36	16	22	9	60	10
R^2	0.10	0.35		0.08	0.06	0.01	0.43	0.08	0.01	0.09	0.01
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers											
Divestiture	-0.008	0.310	n/a	-0.236	0.297	0.191	n/a	n/a	-0.606	-0.197	-0.175
	(0.087)	(0.132)***		(0.153)	(0.120)***	(0.095)**			(0.570)	(0.102)**	(0.262)
Observations	90	64		64	64	28			27	68	21
Concession	0.006	-0.423	n/a	0.090	-0.324	-0.117	0.619	n/a	n/a	-0.119	n/a
	(0.163)	(0.119)***		(0.123)	(0.178)*	(0.225)	(0.198)***			(0.264)	
Observations	39	26		26	26	31	15			32	
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment											
Divestiture	0.369	0.281	n/a	-0.332	0.358	n/a	n/a	n/a	-0.895	-0.067	-0.648
	(0.172)**	(0.176)		(0.153)**	(0.217)*				(0.609)	(0.114)	(0.304)**
Observations	65	64		77	74				24	74	21
Concession	-0.167	-0.085	n/a	-0.107	-0.380	0.427	0.482	-0.011	n/a	-0.074	n/a
	(0.212)	(0.160)		(0.339)	(0.141)***	(0.184)***	(0.220)***	(0.011)		(0.368)	
Observations	27	26		36	33	25	14	15		37	
Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP electricity lost in distribution											
Divestiture	0.119	0.207	n/a	-0.312	0.252	0.009	0.665	n/a	-0.268	-0.035	-0.280
	(0.096)	(0.146)		(0.178)*	(0.153)*	(0.159)	(0.558)		(0.566)	(0.119)	(0.349)
Observations	68	59		71	69	25	13		25	107	18
Concession	-0.141	n/a	n/a	0.266	-0.589	0.307	0.527	n/a	n/a	0.132	n/a
	(0.199)			(0.394)	(0.464)	(0.161)*	(0.215)***			(0.232)	
Observations	32			35	33	28	16			60	

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 15 A. Difference-in-Differences with Matching, water

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Water sold per worker	(6) Water sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Water pipe breaks per connection	(11) Hours with water daily
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only											
PSP	0.051	0.154	0.123	-0.148	-0.011	-0.001	0.040	-0.018	0.231	-0.221	0.109
	(0.022)***	(0.052)***	(0.303)	(0.037)***	(0.083)	(0.104)	(0.009)***	(0.052)	(0.093)***	(0.176)	(0.028)***
Observations	240	234	200	449	374	361	244	227	78	180	196
R^2	0.02	0.04	0.01	0.03	0.01	0.01	0.09	0.01	0.01	0.03	0.07
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers											
PSP	-0.001	0.133	0.680	-0.156	-0.504	-0.508	0.041	0.083	0.222	0.430	0.170
	(0.037)	(0.076)*	(0.737)	(0.065)***	(0.680)	(0.799)	(0.023)*	(0.62)	(0.178)	(1.119)	(0.132)
Observations	240	234	181	254	201	216	204	71	31	61	33
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment											
PSP	0.072	0.123	0.081	-0.103	0.033	0.102	0.040	0.046	0.098	0.219	0.112
	(0.030)***	(0.070)*	(0.439)	(0.050)**	(0.101)	(0.163)	(0.017)***	(0.154)	(0.161)	(0.520)	(0.052)**
Observations	237	234	200	449	374	353	244	221	76	169	190
Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP water sold											
PSP	0.008	0.029	0.264	-0.006	0.027	0.126	0.014	-0.623	0.119	-0.084	-0.005
	(0.076)	(0.226)	(0.401)	(0.054)	(0.094)	(0.141)	(0.005)***	(0.188)***	(0.123)	(0.656)	(0.052)
Observations	203	201	196	390	374	361	233	182	78	167	165

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 15 B. Difference-in-Differences with Matching, by type of PSP contract, water

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) CAPEX per worker	(4) Number of employees	(5) Water sold per worker	(6) Water sold per connection	(7) Collection ratio	(8) Residential coverage	(9) Average residential price (in USD)	(10) Water pipe breaks per connection	(11) Hours with water daily
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only											
Divestiture	0.090	0.399	1.119	-0.386	0.305	-0.684	0.016	-0.123	0.291	n/a	-0.018
	(0.53)*	(0.127)***	(1.148)	(0.120)***	(0.528)	(0.632)	(0.029)	(0.150)	(0.366)		(0.026)
Observations	190	189	185	371	331	316	206	161	78		161
R^2	0.01	0.05	0.01	0.03	0.01	0.01	0.01	0.01	0.01		0.01
Concession	0.031	0.102	0.056	-0.129	-0.048	0.088	0.026	0.03	-0.108	-0.439	0.098
	(0.025)	(0.060)*	(0.312)	(0.043)***	(0.096)	(0.116)	(0.006)***	(0.062)	(0.134)	(0.471)	(0.032)***
Observations	222	217	199	424	331	352	236	206	39	166	185
R^2	0.02	0.01	0.01	0.02	0.01	0.01	0.06	0.01	0.01	0.05	0.05
Lease or Man contract	0.097	0.177	n/a	-0.124	0.075	-0.364	0.012	-0.058	0.325	0.208	0.169
	(0.049)**	(0.107)*		(0.074)*	(0.169)	(0.239)	(0.013)	(0.120)	(0.281)	(0.749)	(0.046)***
Observations	192	172		381	340	322	210	165	24	161	169
R^2	0.02	0.01		0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.07
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers											
Divestiture	0.561	0.451	n/a	-0.347	n/a	n/a	n/a	-0.027	n/a	n/a	n/a
	(0.150)***	(0.135)***		(0.151)***				(0.039)			
Observations	190	189		203				26			
Concession	-0.003	0.058	0.067	-0.047	0.021	-0.738	0.027	0.127	0.066	0.430	0.160
	(0.032)	(0.078)	(0.537)	(0.110)	(0.804)	(0.758)	(0.014)**	(0.073)*	(0.179)	(1.119)	(0.124)
Observations	222	217	180	237	196	211	200	56	22	61	28
Lease or Man contract	0.088	0.341	n/a	-0.287	n/a	n/a	n/a	-0.097	n/a	n/a	n/a
	(0.107)	(0.267)		(0.170)*				(0.069)			
Observations	192	192		206				25			
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment											
Divestiture	0.579	0.842	n/a	-0.232	n/a	n/a	n/a	0.010	n/a	n/a	n/a
	(0.166)***	(0.274)***		(0.109)**				(0.007)			
Observations	189	189		371				158	76		
Concession	0.051	-0.009	0.208	-0.078	0.023	0.303	0.023	0.129	0.284	-0.670	0.123
	(0.040)	(0.117)	(0.422)	(0.069)	(0.108)	(0.175)*	(0.013)*	(0.064)**	(0.369)	(0.661)	(0.059)**
Observations	220	217	199	424	362	344	236	201	76	1775	179
Lease or Man contract	0.025	0.372	n/a	-0.060	0.259	-0.170	-0.026	-0.018	n/a	-0.052	0.135
	(0.174	(0.251)		(0.080)	(0.183)	(0.133)	(0.046)	(0.081)		(0.618)	(0.093)
Observations	192	192		381	340	319	210	163		160	166

Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP water sold

Divestiture	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Observations											
Concession	-0.039	-0.156	n/a	-0.017	0.149	0.047	0.008	-0.708	-0.205	-0.514	n/a
	(0.088	(0.160)		(0.088)	(0.142)	(0.176)	(0.004)*	(0.324)**	(0.302)	(0.611)	
Observations	198	196		378	362	352	226	173	32	163	
Lease or Man	n/a	n/a	n/a	-0.110	0.279	-0.308	-0.046	0.083	n/a	-0.530	0.194
contract				(0.122)	(0.170)*	(0.123)***	(0.035)	(0.054)		(0.919)	(0.123)
Observations				353	340	322	207	170		158	154

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 16 A. Difference-in-Differences with Matching, sanitation

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) Wastewater treated per worker	(4) Wastewater treated per connection	(5) Residential coverage	(6) Sewerage blockages per connection
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only						
PSP	-0.077	0.005	0.230	-0.169	0.227	0.865
	(0.070)	(0.089)	(0.153)**	(0.171)	(0.101)***	(0.733)
Observations	163	160	24	114	138	71
R^2	0.01	0.01	0.11	0.01	0.04	0.02
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers						
PSP	-0.037	-0.034	n/a	0.136	0.106	1.568
	(0.063)	(0.127)		(0.340)	(0.304)	(1.741)
Observations	160	157		88	53	49
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment						
PSP	-0.187	-0.035	0.055	-0.061	0.082	2.109
	(0.173)	(0.133)	(0.164)	(0.228)	(0.267)	(0.719)***
Observations	162	160	24	113	133	71
Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP wastewater treated						
PSP	-0.190	-0.198	0.062	-0.271	0.189	2.612
	(0.093)**	(0.143)	(0.172)	(0.233)	(0.186)	(1.582)*
Observations	91	91	24	114	59	43

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Table 16 B. Difference-in-Differences with Matching, by type of PSP contract, sanitation

	(1) Number of res. connections	(2) Number of res. connections per worker	(3) Wastewater treated per worker	(4) Wastewater treated per connection	(5) Residential coverage	(6) Sewerage blockages per connection
Model A: Difference-in-Differences, utilities with pre- *and* post- observations only						
Divestiture	0.074	0.241	0.934	-0.266	0.072	n/a
	(0.184)	(0.216)	(0.317)***	(0.703)	(0.110)	
Observations	124	124	14	96	88	
R^2	0.01	0.01	0.37	0.01	0.01	
Concession	-0.131	-0.068	0.236	-0.146	0.293	1.088
	(0.084)	(0.108)	(0.149)	(0.181)	(0.118)***	(0.866)
Observations	148	145	23	112	122	69
R^2	0.01	0.01	0.12	0.01	0.05	0.02
Lease or Man contract	-0.045	0.024	n/a	-0.460	0.047	0.307
	(0.172)	(0.203)		(0.703)	(0.098)	(1.357)
Observations	125	125		96	90	66
R^2	0.01	0.01		0.01	0.01	0.01
Model B: Difference-in-Differences, matching by country, GDP per capita, pre-PSP customers						
Divestiture	0.247	0.719	n/a	n/a	n/a	n/a
	(0.127)*	(0.245)***				
Observations	122	122				
Concession	-0.201	-0.127	n/a	-0.014	0.338	1.568
	(0.082)***	(0.152)		(0.344)	(0.247)	(1.741)
Observations	145	142		87	41	49
Lease or Man contract	-0.245	-0.211	n/a	n/a	n/a	n/a
	(0.144)*	(0.204)				
Observations	123	123				
Model C: Difference-in-Differences, matching by country, GDP per capita, pre-PSP employment						
Divestiture	0.301	0.732	n/a	n/a	0.053	n/a
	(0.096)***	(0.200)***			(0.052)	
Observations	124	124			86	
Concession	-0.121	-0.102	-0.006	0.056	0.327	2.192
	(0.095)	(0.122)	(0.159)	(0.211)	(0.262)	(1.028)**
Observations	147	145	23	111	118	69
Lease or Man contract	0.101	0.433	n/a	n/a	0.054	n/a
	(0.142)	(0.278)			(0.054)	

Observations	125	125			89	
Model D: Difference-in-Differences, matching by country, GDP per capita, pre-PSP wastewater treated						
Divestiture	n/a	n/a	n/a	n/a	n/a	n/a
Observations						
Concession	-0.374	-0.428	0.002	0.139	0.236	3.334
	(0.146)***	(0.174)***	(0.168)	(0.243)	(0.173)	(1.871)*
Observations	90	90	23	112	55	42
Lease or Man contract	n/a	n/a	n/a	n/a	0.018	n/a
					(0.118)	
Observations					39	

Note: *** significant at 1% level; ** significant at 5% level; * significant at 10% level

Graphical representation of variables

Figure 4. Time trends of the selected variables, electricity, centered at the private participation year

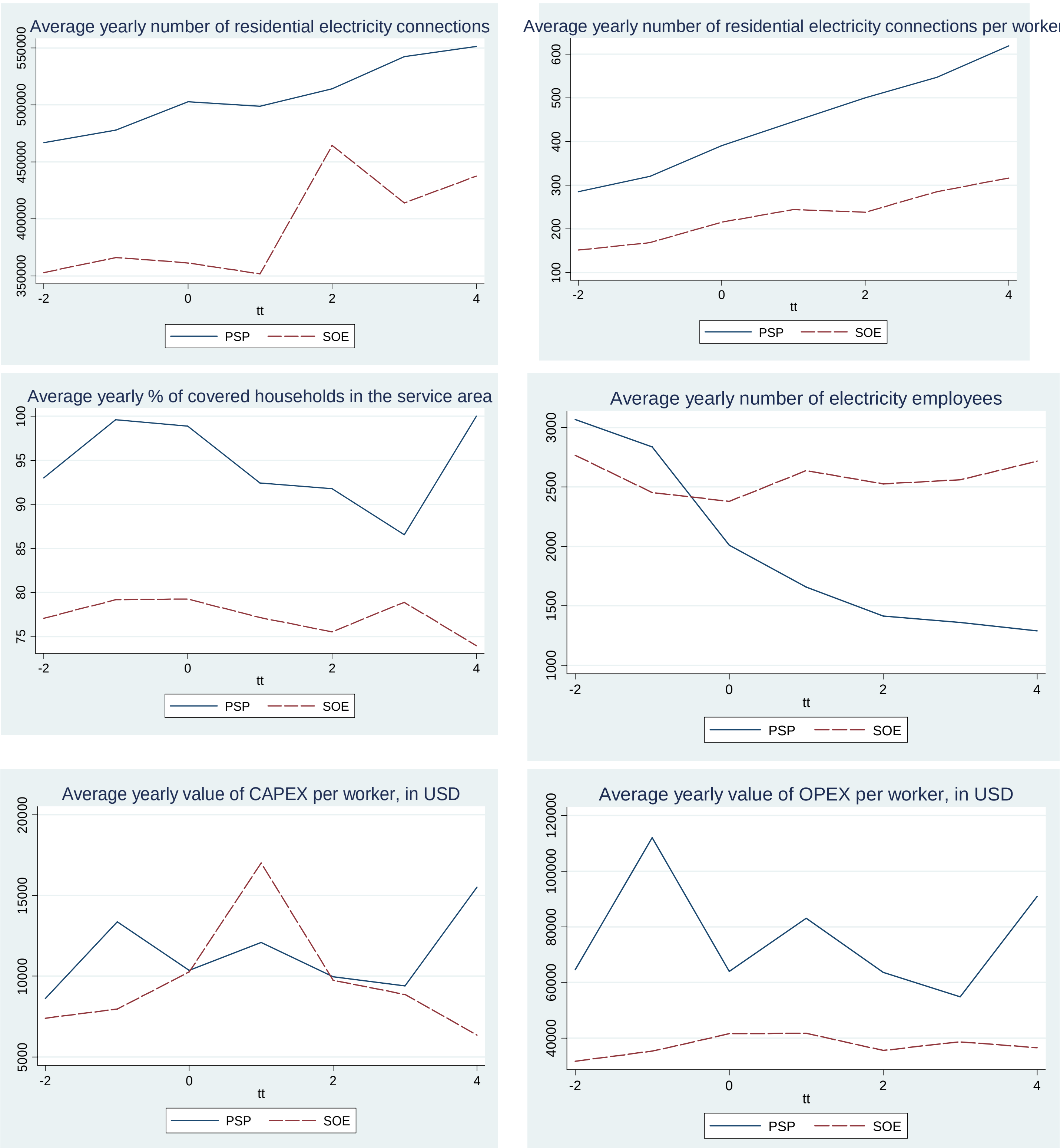


Figure 4. Time trends of the selected variables, electricity, centered at the private participation year

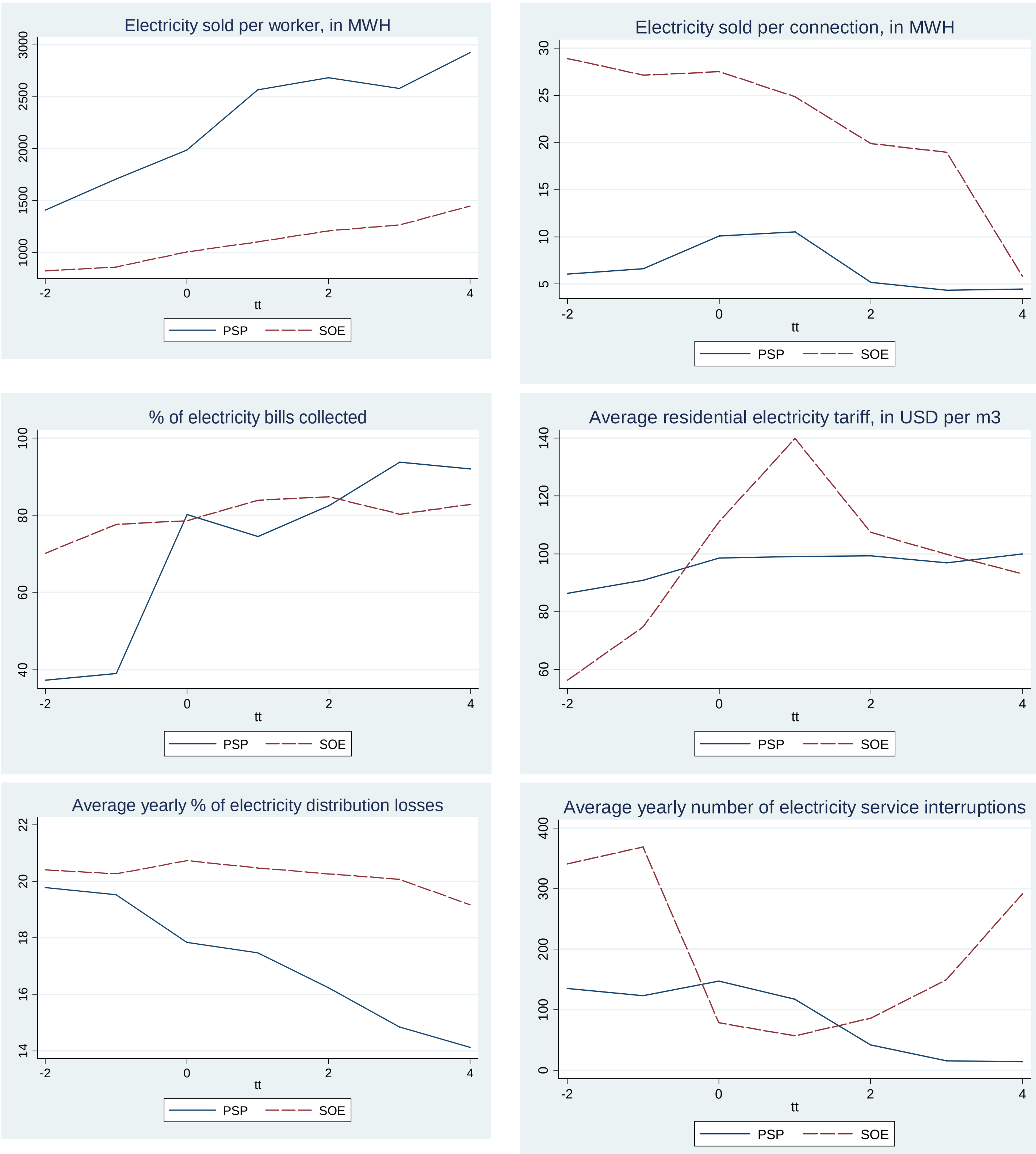


Figure 5. Time trends of the selected variables, water, centered at the private participation year

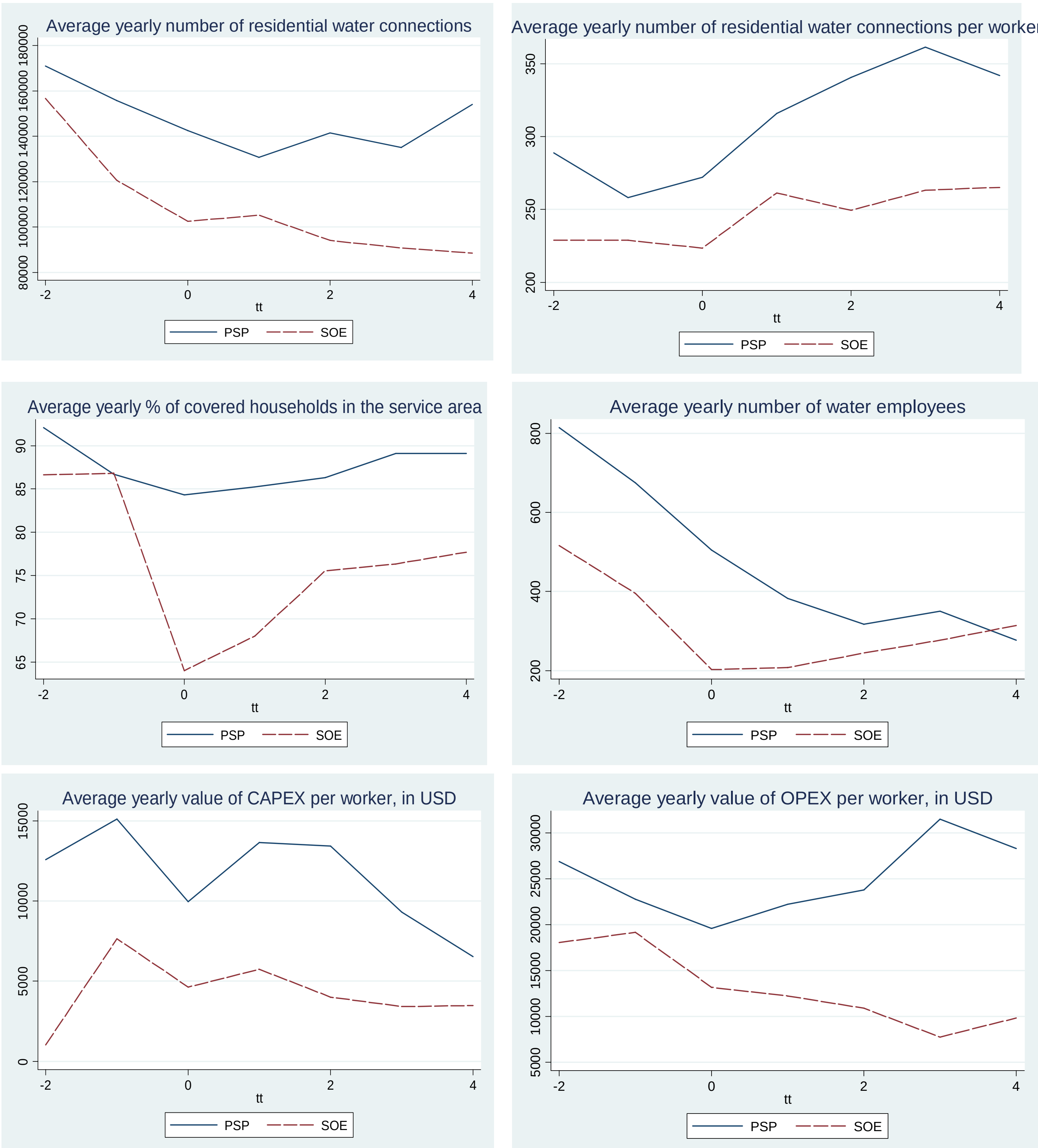


Figure 5. Time trends of the selected variables, water, centered at the private participation year

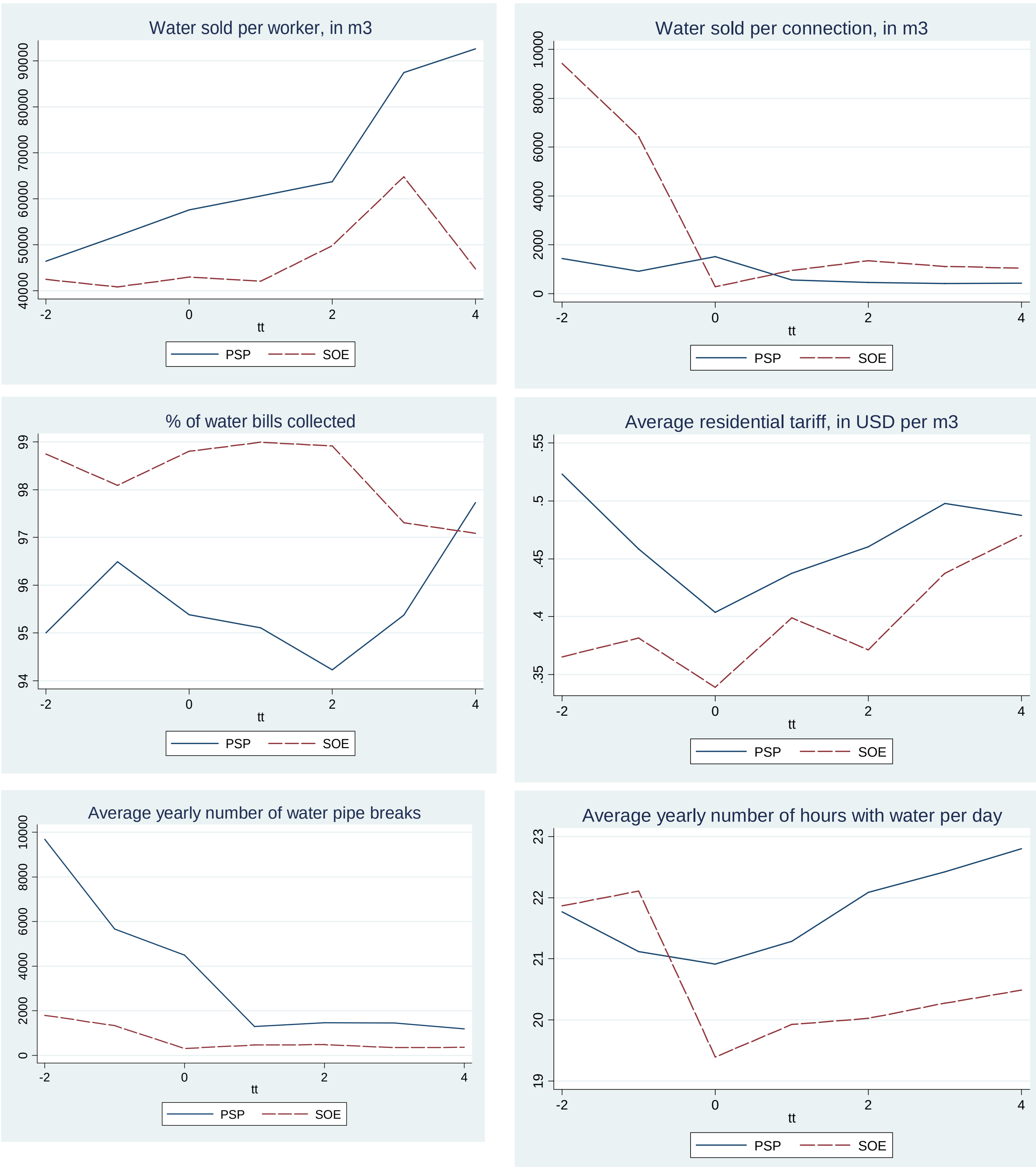
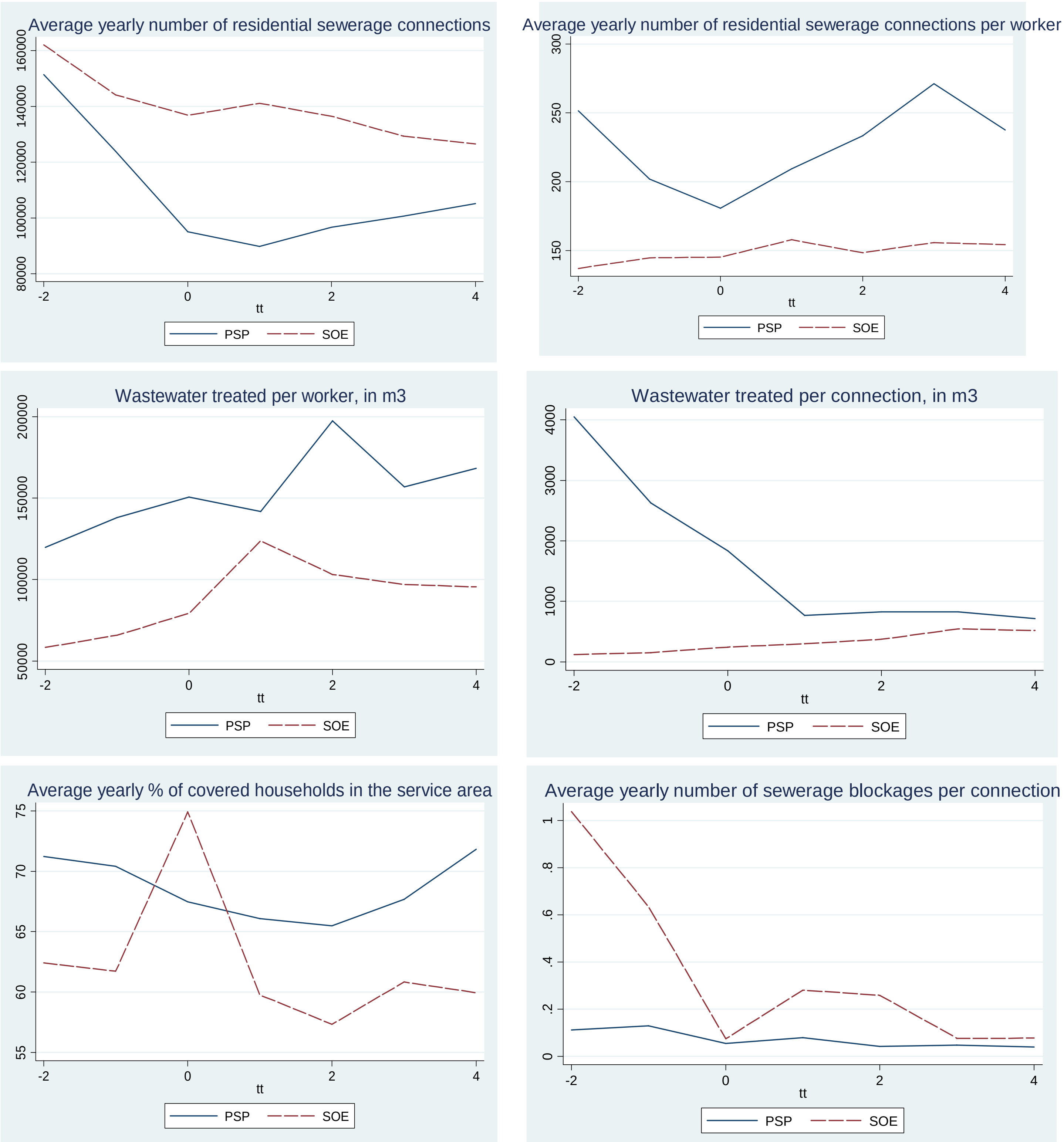


Figure 6. Time trends of the selected variables, sanitation, centered at the private participation year



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Appendix 1: Variables sources, construction and estimations

(1) All estimations are based on the software package 'Stata 9.0';

(2) In those cases when an observation was missing, but there were observations for the two adjacent years, the missing observation was filled assuming constant growth:

$X_t = \exp\left[\frac{\log(X_t) + \log(X_{t-1})}{2}\right]$, so that $\log(X_t) - \log(X_{t-1}) = \log(X_{t+1}) - \log(X_t)$

(3) Dollar-denominated monetary variables (profits, average tariffs, labor costs, etc.) were calculated as: $X_t^{REAL} = \frac{X_t^{NOMINAL}}{FXrate}$, where FX rate is the exchange rate of the local currency to USD as of Dec 31.

(4) The following sources were used for the non-firm level variables used in the estimations:

GDP per capita: WB development database, http://devdata.worldbank.org/edstats/cd5.asp

FX rate: Oanda FX history, http://www.oanda.com

Appendix 2: Percentage of population covered by sample, all countries

ELECTRICITY

Table A2.1. Estimated % of total population covered by electricity sample, by country

Region	Country	Year	Coverage of total population, %[1]
EAP	Malaysia	2002	6
EAP	Philippines	2002	25
ECA	Armenia	2002	100
ECA	Azerbaijan	2002	80
ECA	Czech Republic	2002	43
ECA	Estonia	2002	81
ECA	Georgia	2002	31
ECA	Hungary	2002	90
ECA	Moldova	2002	89
ECA	Poland	2002	5
ECA	Slovak Republic	2002	100
ECA	Ukraine	2002	94
LAC	Argentina	2002	76
LAC	Bolivia	2002	27
LAC	Brazil	2002	72
LAC	Chile	2002	55
LAC	Colombia	2002	65
LAC	El Salvador	2002	67
LAC	Guatemala	2002	57
LAC	Mexico	2002	23
LAC	Nicaragua	2002	44
LAC	Panama	2002	68
LAC	Peru	2002	55
LAC	Uruguay	1998	100
MENA	Morocco	2002	33
SA	India	2002	1
SSA	Botswana	2002	22
SSA	Burkina Faso	2002	12
SSA	Cameroon	2002	18
SSA	Cape Verde	2002	59
SSA	Cote d'Ivoire	1999	35
SSA	Eritrea	2002	9
SSA	Ethiopia	2002	4
SSA	Gabon	2000	46
SSA	Ghana	2002	14
SSA	Kenya	2002	8
SSA	Malawi	2002	4
SSA	Mali	2002	5
SSA	Mauritania	2003	13
SSA	Mauritius	2002	100
SSA	Mozambique	2002	4

Region	Country	Year	Coverage of total population, %[1]
SSA	Namibia	2002	1
SSA	Niger	2002	5
SSA	Senegal	2002	30
SSA	South Africa	2002	27
SSA	Tanzania	2001	0.04
SSA	Uganda	2002	4
SSA	Zambia	2002	12
SSA	Zimbabwe	2001	18

Note: [1] Derived by dividing a product of observed residential connections and average household size by total population.

Table A2.2: Estimated % of total population covered by electricity sample, by region, 2002

Region	Population, million people			% age of total population covered by sample[1]	
	Total covered by sample	Total in covered countries	Total in region	in covered countries	in region
EAP	22	103	1,839	21	1
ECA	97	134	472	72	21
LAC	260	444	529	59	49
MENA	12	30	289	38	4
SA	8	1,054	1,399	1	1
SSA	49	381	695	13	7
Developing countries total	**448**	**2,146**	**5,222**	**21**	**9**

Note: [1] Derived by dividing a product of observed residential connections and average household size by total population.

WATER and SANITATION

Table A2.3: Estimated % of total population covered by water sample, by country

Region	Country	Year	Coverage of total population, %[1]
EAP	China	2002	0.4
EAP	Malaysia	2002	72
EAP	Philippines	2002	6
EAP	Vietnam	2002	11
ECA	Armenia	2002	37
ECA	Bulgaria	2002	3
ECA	Croatia	2002	24
ECA	Czech Republic	2002	24
ECA	Estonia	2002	6
ECA	Georgia	2002	26
ECA	Hungary	2002	25
ECA	Kazakhstan	2002	12
ECA	Moldova	2002	10
ECA	Poland	2003	0.2
ECA	Romania	2002	6

Region	Country	Year	Coverage of total population, %[1]
ECA	Ukraine	2002	9
LAC	Argentina	2002	5
LAC	Bolivia	2002	10
LAC	Brazil	2002	35
LAC	Chile	2002	55
LAC	Colombia	2002	43
LAC	Costa Rica	2002	39
LAC	Ecuador	2002	9
LAC	Honduras	2002	1
LAC	Mexico	2000	5
LAC	Nicaragua	2002	43
LAC	Panama	2002	54
LAC	Paraguay	2002	14
LAC	Peru	2002	29
LAC	Trinidad and Tobago	2002	70
LAC	Uruguay	2002	88
MENA	Jordan	2002	36
MENA	Morocco	2002	10
SSA	Benin	2002	7
SSA	Burkina Faso	2002	4
SSA	Cote d'Ivoire	2002	24
SSA	Malawi	2002	1
SSA	Mali	2002	4
SSA	Nigeria	2002	2
SSA	Tanzania	2002	0.2
SSA	Togo	2002	4
SSA	Uganda	2002	1
SSA	Zambia	2002	6

Note: [1] Derived by dividing a product of observed residential water connections and average household size by total population.

Table A2.2: Estimated % of total population covered by water sample, by region, 2002

Region	Population, million people			% age of total population covered by sample[1]	
	Total covered by sample	Total in covered countries		Total covered by sample	Total in covered countries
EAP	36	1,475	1,839	2	2
ECA	17	170	472	10	4
LAC	117	454	529	26	22
MENA	5	35	289	14	2
SA	0	0	1,399	0	0
SSA	9	264	695	3	1
Developing countries total	**184**	**2,397**	**5,222**	**8**	**4**

Note: [1] Derived by dividing a product of observed residential water connections and average household size by total population.