Private versus public provision of water services: does ownership matter for utility efficiency?

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ABSTRACT

Property rights theory posits that private firms are more efficiently managed than government enterprises. An important rationale for privatization, these ownership effects are ascribed to the fact that private firms, contrary to government enterprises, face competitive markets for capital and property rights. This article reviews a substantial body of water industry evidence on ownership effects and finds that ownership effects are neither independent nor overwhelming. Private water utilities are not more efficient than their public counterparts. Water utility privatizations sometimes produce efficiency gains but not always. Methodological shortcomings of published empirical work make it difficult to assess why this is so. For policy makers, the tentative conclusion to be drawn from this literature is that a change from public to private management will only yield benefits when accompanied by a comprehensive reform of the utility's external environment.

Key words | efficiency, ownership, privatization, water utilities

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INTRODUCTION

Privatization is intensely debated in the water industry. The argument for private ownership and management of water and wastewater systems rests on two arguments. First is the fiscal argument that a stronger reliance on private firms will relieve government of the burden of subsidizing investment and operations. Second is the efficiency argument that more private sector involvement will spur industry performance. Various arguments underpin the latter thesis: that private utility management is inherently more efficient than public management; that privatization goes hand in hand with efficiency-enhancing competition; that privatization catalyses much-needed institutional reform; and that privatization nurtures a healthy arms' length relationship between politics and business.

Our understanding of how privatization affects water utility performance is still incomplete (Braadbaart 2000). This article reviews what we know about the thesis that private utility management is inherently more efficient than public management. A growing body of economic theory and evidence centres on this so-called ownership effect on firm efficiency. The theory suggests that ownership effects appear both statically and dynamically. Static ownership effects would make private firms more efficient than public (government-owned and managed) enterprises. Dynamic ownership effects would produce efficiency gains in the course of the privatization process.

The aim of this article is to assess the evidence on ownership effects in the water industry. Have researchers detected evidence of *ownership effects*? If so, how strong are these effects? Moreover, are they independent from other forces bearing on utility efficiency? These are important questions. Economists believe the combination of ownership effects and competition makes privatization produce efficiency and other improvements. However, piped water and sewerage provision offers limited scope for competition. Therefore, much of the expected gains must stem from ownership effects. This article brings together a substantial but scattered empirical literature to assess whether this is so.

The next section offers a brief introduction to property rights theory. This is followed by a review of concepts and

methods employed in measuring ownership effects on firm-level efficiency. The subsequent section sums up the evidence on cross-sectional comparisons of public and private efficiency in the water industry, and the next section places the water industry evidence in the context of investigative work on other industries. This is followed by a review of longitudinal studies of ownership effects, and a section on counterfactual analyses of water utility reform. The next section holds the patterns emerging from the water industry against panel data evidence from other industries, and the final section sums up conclusions.

PROPERTY RIGHTS THEORY

Historically, economists have favoured privatization because they associate state ownership with monopoly and private ownership with a competitive market environment and ascribe superior welfare properties to the latter. A recent offshoot of economic theory takes a different tack. This theory, known as *property rights theory*, holds that private firms are more efficient than are state-owned enterprises regardless of the market environment. The reasoning is that private ownership entails an incentive structure that differs significantly from, and produces more economic welfare than, public ownership. The latter statement may strike non-economists as odd given that state-owned enterprises are, at least in name, managed by government officials in the public interest while private firms serve the private interests of their owners. Economists, however, argue that under private ownership, the interests of society—economic welfare maximization—are better aligned with those of the firm's owners/managers than is the case under public management. At the heart of their argument are two disciplining forces to which private firms are subject but from which state-owned enterprises are thought to be insulated, namely competitive markets for property rights and capital.

First, unlike private corporations, shares in state-owned enterprises are not marketable: the public as the virtual owner of the state-owned enterprise cannot signal its discontent with management by selling its stake in the enterprise. This insulates public managers from owners' claims. Likewise, state-owned enterprises do not face the threat of being taken over if they fare badly. Second, state-owned enterprises are thought to face distorted capital markets. Political concerns rather than managerial ones determine investment levels. State-owned enterprises typically operate under soft financial constraints. Governments are unwilling to let a mismanaged state-owned enterprise go bankrupt, as would private investors. In contrast managers of private firms face capital markets that reward profitable firms and punish those that run up losses. They are therefore more likely to strive for economic welfare maximization than are managers of state-owned enterprises (Vickers & Yarrow 1988; Galal *et al*. 1994; Shirley & Walsh 2000).

Economists refer to these effects on management as ownership effects. For present purposes, this reasoning produces two premises which can be tested:

1. The static ownership thesis that private water providers are inherently more efficient than state-owned utilities.
2. The dynamic ownership thesis that water utility privatization always produces efficiency gains.

MEASURING OWNERSHIP EFFECTS: CONCEPTUAL AND METHODOLOGICAL ISSUES

I will use the term 'private firm' to describe a for-profit entity in which private actors have a controlling vote. Privatization denotes any increase in private sector involvement in the management of water and wastewater operations. The term divestiture is reserved for the special case of a permanent transfer of government-owned assets to a private party. This distinction matters because the lion's share of recent water privatizations has involved a temporary and partial shift of property rights to the private sector—strictly speaking an intensification of out-contracting. Privatization in this broad sense covers a vast range of contract modalities with different incentive structures.

Attempts to capture this contractual universe of *public-private partnerships* in classificatory schemes have

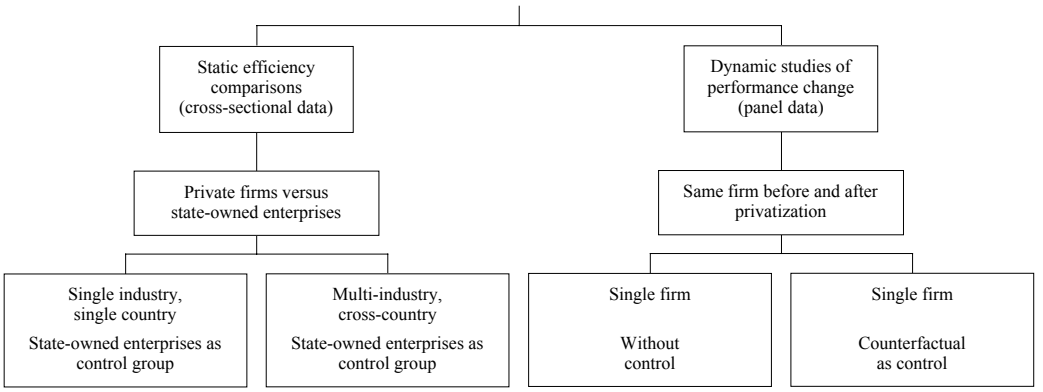

Figure 1 | Common methodologies employed in tests of ownership effects.

only been partly successful. However, three families of partnerships may be distinguished. First are lease and management contracts and other forms of outsourcing that transfer limited risk to the private sector. Second, Build-Operate-Transfer contracts and the many variants thereof that have the design and construction of green field plant at their core. Third are so-called concession contracts that shift the entire responsibility for service provision to a private operator for a lengthy period (Kerf *et al*. 1995; UNIDO 1996; Brook-Cowen 1997).

The term efficiency will be understood as the input-output ratio or the amount of inputs required for producing a given amount of output. This definition does not cover improvements in the service mix, that is, innovations that affect the quality of water services. In the context of this article, then, efficiency offers only a partial measure of the difference between public and private performance in water services delivery.

The latter observation brings us to a knotty methodological issue, namely how useful is a partial measure of performance. Economists have justly argued that public-private comparisons should encompass all welfare benefits and losses to society, that is, include both price and quality parameters for all stakeholders involved, including governments, customers, private investors and utility workers (Vickers & Yarrow 1988: p. 39; Galal *et al*. 1994). But while theoretically correct this so-called *total welfare* approach is difficult to put into practice, as analysis of individual privatization cases requires massive amounts of data and elaborate measurement protocols.

Economists rely on two basic methods for testing ownership effects (see Figure 1). Cross-sectional analysis is the method of choice for testing static ownership effects. Either this may involve a comparison of statistical data for samples of public and private firms operating in the same country and regulatory environment, or it may involve international, multi-industry samples of firms.

Tests of dynamic ownership effects likewise rely on two basic methods. Most studies take the form of longitudinal studies of changes resulting from privatization. Firm-level time series data (known as panel data in economics) compiled before and after privatization offer the basis for evaluating change with privatization. Sample sizes vary from in-depth case studies of single firms to statistical analyses of hundreds of privatized corporations. An alternative technique is the *counterfactual*. Essentially, the analyst compares the real performance of a divested firm over time against an alternative scenario in which the firm remains in government hands, measuring total welfare change under the real scenario against welfare change under the counterfactual scenario.

All of the above techniques have drawbacks. Economists have criticized static ownership tests for their inability to capture the dynamic gains of privatization (e.g. Galal *et al*. 1994). Yet the 'single country, single industry' variant of this test remains the most robust method for

measuring ownership effects, involving a comparison of a 'treatment' sample of private firms with a 'control' sample of public firms. Because our treatment and control firms operate in the same market environment we can legitimately attribute observed efficiency differences to ownership effects.

The latter point is a drawback of dynamic tests of privatization. Published longitudinal tests capture the dynamics of privatization but cannot tell what causes welfare improvements when these occur. Privatizations are often directly preceded by reforms and usually go hand in hand with regulatory change, changes in competition policy, and so on. Most tests do not control for these intervening variables. This creates the problem that, for lack of a control group of similarly reformed but un-privatized firms, we do not know what portion of efficiency improvements to attribute to ownership effects. Economists have developed counterfactuals to solve this problem as well as for calculating total rather than partial welfare change (Galal *et al*. 1994: p. 21). As we will see, however, published counterfactuals do not solve the attribution problem in a satisfactory manner. They are apt to confuse privatization effects with the impact of reform in general. In sum, both static and dynamic tests are imperfect tools for the measurement of ownership effects. Whereas static tests control for intervening variables but fail to capture the dynamics of privatization, the reverse applies to longitudinal tests and counterfactuals.

STATIC TESTS OF OWNERSHIP EFFECTS: WATER INDUSTRY EVIDENCE

The majority of static econometric tests of ownership effects on water utility efficiency use a model specification:

$$Y = \text{intercept} + \beta_{1X\text{ownership}} + \beta_{2X2} + \ldots + \beta_{kXk} + \text{error term} \quad (1)$$

where the dependent variable Y is a proxy for firm-level efficiency or output, usually average water charge or volume of water produced. The independent variables comprise one variable denoting utility ownership form to which researchers add variables $x_2 \ldots x_k$ as controls for other influences on firm-level efficiency, e.g. utility size, service area profile or raw water quality. Multiple regression analysis is then applied to examine whether the ownership effect on utility efficiency is statistically significant.

A literature search yielded fourteen published static comparisons of public and private water utilities. Eleven tests draw on American statistics, the remaining three use data from Canada (Kitchen 1973), Asia (Estache & Rossi 1999) and Indonesia (Hermirasari 2000). I will omit the latter three studies. Kitchen relies on qualitative analysis only. Estache & Rossi's classification of utilities into public and private categories is questionable. Hermirasari's findings may be influenced by significant differences in scale between the private sample and the public control.

The set of eleven American studies offers a relatively robust sample of comparable static tests in the same country environment. Yet, as Table 1 shows, contradictory findings abound. Three investigators find that private water utilities are more efficient, four studies conclude that there are no differences in public and private efficiency, and four tests find that public water utilities are more efficient than private ones. This suggests that modeling procedures somehow impinge on test results. Close inspection reveals that model specification does indeed affect the outcome of static tests.

Consider first tests producing the counter-intuitive finding that public utilities are more efficient than private utilities. This finding appears to result from the use of water charges as an independent variable. American economists have long observed that public utilities profit from advantageous tax regimes (Kahn 1971). Neal *et al*. (1996) show how different tax regimes and lower (subsidized) costs of capital affect private and public water prices in California: public utility performance is inflated by subsidies that private utilities must do without.

Turning next to those tests that found private ownership was correlated with higher efficiency, these appear to suffer from model under-specification. The 1976, 1977 and 1978 studies listed in Table 1 use crude models and omit a

Table 1 | Results of eleven static ownership effect tests, water utilities, United States

	Investigator(s)	Data source	$N_{private}$	N_{public}	Efficiency indicators tested	Result
1	Mann & Mikesell (1976)	AWWA* (1970)	26	188	Operational cost	Costs of public utilities 20% higher than private utilities
2	Morgan (1977)	AWWA (1970)	44	99	Operational cost	Costs of public utilities 10% higher
3	Crain & Zardkoohi (1978)	AWWA (1970)	24	88	Cost and labour productivity	Lower costs and higher labour productivity for private firms
4	Bruggink (1982)	AWWA (1960)	9	77	Cost	Costs of public utilities 24% lower than private utilities
5	Feigenbaum & Teeples (1983)	AWWA (1970)	57	262	Cost	No difference between public and private utilities
6	Lindsay (1984)	State commission (1981)	17	61	Cost	Public utilities charge significantly lower prices than private utilities
7	Byrnes *et al.* (1986)	AWWA (1976)	59	68	Technical and scale efficiency	No difference between public and private utilities
8	Teeples & Glyer (1987)	Private survey (1980) and public data	52	67	Cost	No difference between public and private utilities
9	Lambert *et al.* (1993)	AWWA (1989)	33	283	Technical and scale efficiency	Public utilities more efficient than private utilities
10	Bhattacharyya *et al.* (1994)	AWWA (1992)	32	225	Cost	Public utilities more efficient than private utilities
11	Neal *et al.* (1996)	State Utilities Commission (1992–1994)	3	10	Cost	No price differences between public and private utilities but public utilities subsidised

*American Water Works Association.

number of important control variables to find that costs of private utilities are lower then those of public utilities. Bruggink (1982) is the first to spot these problems. Employing fine-grained data from a 1960 AWWA survey, he adds a number of independent variables to the regression (water source, purification technology, consumer mix, labour cost and type of regulation) and finds that public water utilities have lower operating costs than private utilities. Subsequent studies confirm that '(t)he methodology of comparing cost of water delivery functions for government versus private utilities can erroneously attribute cost variations to ownership if mis-specifications of the cost function result in the omission of variables that vary systematically with ownership' (Feigenbaum & Teeples 1983: p. 677).

Teeples & Glyer (1987) offer a graphic illustration of how model specification affects test results. Starting with a crude model that produces statistically significant differences in efficiency between a superior private sample and a control group of public utilities, they demonstrate how the ownership effect dwindles to insignificance if one progressively feeds control variables into the regression: '(d)isaggregating labor inputs, including missing water and energy inputs, and adding a control for service mix ... decreases ownership effects by a third to one-half' (1987: p. 404). From their careful study Teeples & Glyer conclude that 'as misspecifications are eliminated, apparent overall differences in the cost performances (author's note: between public and private utilities) disappear' (1987: pp. 407–408).

This work also demonstrates that the crucial ownership variable itself cannot be taken for granted. Economic theory posits a universe populated by two kinds of firms, namely public and private enterprises. Accordingly, water industry investigators insert ownership as a dummy variable (public/private) into their regressions. This simplification turns out to be problematic however. Teeples & Glyer (1987) distinguish between city public enterprises, city departments, two types of district-level public utilities and regulated private stock companies serving single service areas or multiple areas. They demonstrate that certain types of utility are associated with specific service environments. Corroborating evidence for this finding comes from Lambert *et al*. (1993) and Bhattacharyya *et al*. (1994) who abide by the dummy ownership variable but find a large dispersion of efficiency *within* the set of utilities they group together as public.

Summing up, efficiency differences between public and private utilities studies appear to result from either apples and oranges problems in the construction of the dependent efficiency variable or from model underspecification, that is, the confusion of service area effects with ownership effects. This body of work does not support the thesis that the efficiency of American water utilities varies with ownership form.

THE STATIC EFFICIENCY THESIS: EVIDENCE FROM OTHER INDUSTRIES

Is the above conclusion anomalous? Do ownership effects show up in static tests on other industries? The total number of published tests of static ownership effects exceeds 100. The most frequently cited meta-analyses of this large body of work are Borcherding *et al*. (1982) and Vining & Boardman (1992). The former compiled 50 industry studies, 40 of which report private firms to be more efficient than state-owned enterprises. Of the remaining ten studies, seven find no difference between private and public efficiency, and three report that state-owned enterprises are *more* efficient than their private counterparts. Vining & Boardman's more recent meta-analysis comprises 90 studies. The pattern that emerges is similar to the Borcherding analysis. Sixty-three studies confirm the private-is-more-efficient hypothesis while 20 investigations find no significant difference in efficiency between public and private enterprises in the same industry. Seven investigators report that the control group of state-owned enterprises is more efficient than the private sample.

Thus, a majority of static tests confirms the ownership thesis, but a significant minority does not. Reviewing this literature Galal and associates comment that 'the most striking characteristic of this body of work is its almost laughable diversity of results' (1994, pp. 11–12), and suggest that this diversity discredits static tests of ownership effects altogether. Other economists interpret the static

Table 2 | Market structure and ownership effects, infrastructure industries with competitive potential versus natural monopolies

	Investigator's conclusion		
Infrastructure industries grouped by market structure	**The performance of private firms is superior to that of public firms**	**No difference between public and private firms or public performance superior**	**Total number of investigations**
Natural monopolies:	10 (38%)	16 (62%)	26 (100%)
Water supply	4	4	8
Railroads	0	4	4
Electricity	6	8	14
Industries with competitive potential:	29 (81%)	7 (19%)	36 (100%)
Airlines	8	3	11
Refuse	9	4	13
Bus services	12	0	12

Source: based on Vining & Boardman (1992: pp. 214–215).

literature differently. They argue that this diversity may stem from omitted variable problems. Specifically they note that almost all published tests omit to factor the degree of competition to which firms are exposed into their regressions (Vickers &Yarrow 1988).

This brings up an important issue: are researchers confusing ownership effects with effects resulting from increased competition or other external changes? Calculations based on the Vining & Boardman (1992) database suggest that this may indeed be the case. Table 2 takes 62 studies from the Vining & Boardman set that deal with infrastructure industries. I have divided this sub-set into two groups: industries in which competition is technically feasible and industries that qualify as natural monopolies, that is, industries in which one firm can supply the product at lower cost than two or more firms. The classification draws on Gomez-Ibanez & Meyer (1993), Kerf *et al*. (1995) and Webb & Ehrhardt (1998). Reality is of course more complicated than this binomial classification suggests. A degree of inter-modal competition is in evidence in railways, and limited competition is possible in the large-scale (industrial user) segment of water markets. An even more important caveat is that technical feasibility need not necessarily mean actual competition. Table 2 presents the results of this exercise. As may be seen, the evidence from competitive infrastructure markets is much more in line with ownership effects theory than that from natural monopolies. This lends some support to the idea that ownership effects may be confused with effects arising from the external environment.

What conclusions can we draw from the large body of static ownership tests? First, the water industry evidence *is* anomalous, belonging to a minority of static tests that do not confirm the ownership thesis. Second, economists disagree over the implications of the static evidence. Some argue that the presence of conflicting results invalidates this testing method. Others reason that these results prove that ownership effects cannot be studied in isolation from external influences on the firm, specifically competition. Preliminary probing suggests that some static tests may indeed confuse external (market) effects with internal (ownership) effects.

DYNAMIC OWNERSHIP EFFECTS: WATER INDUSTRY EVIDENCE

Summarizing the evidence on dynamic ownership effects in the water industry is difficult. Documented evidence on the longitudinal effects of privatization is frequently of the case study type. Much evidence is anecdotal. Many documented privatizations are recent and the time series therefore short. The number of cases covered in published work is small, raising sampling issues. With these cautions I will discuss three bodies of evidence: a recent survey of 29 privatizations in the United States; the effects of the 1989 England and Wales divestiture; and nine privatization cases in developing countries.

The Hudson Institute conducted a survey of 29 recent municipal privatizations in the United States in 1998 (Hudson Institute 1999). Many municipalities have engaged in public-private partnerships to solve problems of ageing infrastructure and water quality compliance problems caused by tightening national standards. The survey sample includes nine divestitures plus a variety of management, lease and BOT-type contracts. Many of the privatizations are recent making it difficult to gauge their impact on efficiency. As to immediate effects, four out of a sub-sample of 18 long-term contracts (10 years or longer contracts) reported water rate reductions after privatization. Water rates went up in another six cases but it is unclear whether these rate increases were less than they would have been without private participation. While the Hudson Institute survey does not elicit firm conclusions, it suggests that privatization can produce significant efficiency gains under specific circumstances. For example, a private operator serving multiple municipalities may reap economies of scale that are out of reach of the water department of a mid-sized municipality.

The divestiture of water and wastewater companies in England and Wales is probably the best-documented water privatization case available. In 1989 the British government transformed the water and sewerage divisions of what had up to then been public utilities into stock corporations and put up the shares for sale. To prevent these for-profit corporations from abusing their monopoly powers, an independent government agency, the Office of Water Services (OFWAT) was charged with the task of regulating the price and quality of water services provided.

Over the past decade, the now 11 private-owned utilities have successfully tackled the huge investment backlog that was an important reason for their privatization. At the same time, they have achieved substantial efficiency gains by retrenching labour, redesigning operations systems and improving incentives systems. It is difficult to specify the role played by ownership effects in these improvements. For one thing, the water industry underwent reorganization and saw its performance increase significantly in the run-up to divestiture (Lynk 1993). For another, a novel regulatory regime came into force concomitantly with the privatization. The British government appointed an independent regulator vested with extensive authority, assuming the role of the auctioneer in a surrogate market for water services. The price cap was the regulator's principal instrument, consisting of a ceiling on tariff increases set for five-year periods. The price cap system mimics a competitive market—the regulator sets price ceilings *ex ante* and firms are then allowed to reap the rewards from cost-cutting innovations (Helm 1995; Rees 1998; Braadbaart *et al*. 1999; Twort *et al*. 2000).

Both the above-mentioned pre-privatization reforms and the price cap mechanism must have had a positive impact on industry performance. To date, no investigator has measured the strength of these factors however. It therefore remains unclear what portion of observed efficiency gains stem from ownership effects.

Turning to the third piece of evidence on the dynamics of privatization, Table 3 presents data on efficiency changes in nine water privatizations in developing countries. This set of privatization cases comprises a variety of contracts with different divisions of risk and reward between public authorities and private providers. I have brought together these cases for the pragmatic reason that each reports two proxy indicators of efficiency: water charge and labour productivity change.

Table 3 shows water rate reductions in two cases while five cases show sharp increases in labour productivity after privatization. From this evidence, one is tempted to conclude that ownership change did indeed produce dynamic efficiency gains. This statement requires qualification however. Table 3 indicates that it was competitive tendering, not ownership effects that had a dramatic downward effect on water prices in two cases and that this

Table 3 | Dynamic effects of reform: price and productivity change in water and sanitation concessions, *N*=9, various years

	Starting date of private contract	Tariff reduction after privatisation?	Reason for yes/no reduction	Productivity improvement after privatisation	Main reason for change
Buenos Aires, Argentina	1993	Yes	Contract awarded after competitive tendering	Strong	Workforce reduced before and after privatisation (7,450→3,800)
Manila, Philippines	1997	Yes	Contract awarded after competitive tendering	Strong	Workforce reduced before and after privatisation (8,000→5,000)
Conakry, Guinea	1989	No	Negotiated contract	Strong but not lasting	Workforce reduced before privatisation
Cancun, Mexico	1994	No	Macro-economic setback (1995)	No	No workforce reduction
Gdansk, Poland	1992	No	Macro-economic setback	Moderate	Partial privatisation
Cartagena, Colombia	1995	No	Negotiated contract	Strong	Workforce reduced before privatisation (1,200→385)
Corrientes, Argentina	1991	No	Unclear	Strong	Workforce reduced before privatisation (600→250)
Jakarta, Indonesia	1997	No	Negotiated contract	Moderate	No workforce reduction
Aguas Calientes, Mexico	1993	No	Macro-economic setback (1995)	Moderate	No workforce reduction

Sources: Buenos Aires: Rivera (1996) and Alcazar *et al*. (2000); Manila: author's field survey end 1997 and Global Water Report (2000); Conakry: Menard & Clarke (2000a); Cancun: Rivera (1996) and personal communication, Lilian Saade-Hazin; Gdansk: Rivera (1996); Cartagena: Kennedy School of Government (1999) and World Bank (1998); Corrientes: Rivera (1996) and Natakura & Chang (1996); Jakarta: Braadbaart (2001); Aguas Calientes: personal communication, Lilian Saade-Hazin.

competition actually *preceded* the privatization. Both in Manila and in Buenos Aires private providers bid for concession rights. Fierce rivalry over monopoly rights brought down prices in both cases (Rivera 1996; Dumol 2000). This rivalry effectively ended after the awarding of lengthy (25-year) concession rights, however. The same applies to the impressive labour productivity gains recorded in five cases. Governments bought out large numbers of workers with golden handshakes *before* handing over the concessions to private operators.

All this is not to argue that the change to private management was immaterial to these efficiency changes. The point is that in those cases where efficiency did improve we cannot establish an unambiguous causal link between these improvements and ownership effects as defined in economic theory. Where efficiency changed for the better it did so following wide-ranging reforms that encompassed both the external (regulatory and market) environment as well as the internal organization of the utility (staff reductions, management going from public to private hands). Methodologically speaking these cases concern utilities that underwent a complex treatment of reforms, one component of which was a change from public to private management. For lack of a control group of utilities subjected to the same complex treatment minus the ownership change, we cannot say what part of the observed efficiency change resulted from ownership effects.

DYNAMIC EFFICIENCY TESTS: THE BUENOS AIRES AND GUINEA COUNTERFACTUALS

Counterfactuals are partly designed to address the above issues. To date counterfactual scenarios have been calculated for two water privatizations: Buenos Aires, Argentina and Conakry, Guinea. What do these two cases report on ownership effects?

In the late 1980s, the water industry of Guinea was performing poorly by any standard. Water services were inadequate, financial discipline woeful, illegal connections rife and the state-managed piped water system highly dependent on government subsidies. This changed with a comprehensive package of reforms that took effect in 1989. First, the concession rights for Conakry were transferred from a national public agency to a newly created public enterprise, SONEG. Second, the water system received a substantial financial injection through a World Bank-led water supply project. Third, international donors closely monitored the project and used their political influence to ensure implementation of the reforms. The loan, combined with the fact that Guinea had been under a structural adjustment programme since 1986, gave donors substantial leverage. Fourth, the government signed, as a condition for the water supply loan, a ten-year lease contract with a majority private-owned enterprise, SEEG, for operation and maintenance of all urban water systems. This limited form of private sector participation was put forward as a compromise between proponents and opponents of privatization. Fifth, the workforce was reduced from 504 to 355 leading to an immediate leap in labour productivity. Sixth, the average water tariff increased six-fold between 1989 and 1996 (Menard & Clarke 2000a,b).

This series of measures greatly improved the performance of the Guinea system. Table 4 shows the results of a counterfactual scenario for Guinea calculated by Menard & Clarke (2000b). The counterfactual assumptions are that without these reforms there would have been no productivity gains, no financial injection, no large price increases and therefore increased subsidies. Undoubtedly, both government and consumers are better off than they would have been under a continuation of the old system even though, as the researchers observe, many problems remain to be solved.

A similarly comprehensive set of reforms was applied in the second case, Buenos Aires. The Buenos Aires water and wastewater system was managed by a state-owned enterprise propped up by government subventions and suffering from low connection rates, non-viable tariffs, low productivity and poor quality of services. From 1992, the following reforms were introduced. First, a regulatory agency with wide-ranging powers was created. Second, private operators were invited to bid competitively for the concession rights in 1993. Rivalry among bidders led to a dramatic reduction of water tariffs. Third, concession rights were bid out for a 25-year period, with the winning

Table 4 | Welfare effects from reform: the Guinea and Buenos Aires counterfactuals

	Conakry, Guinea, 1989–1999		Buenos Aires, Argentina, 1992–1996	
Stakeholders	**NPV in 1996 US$ mi.**	**Gain as % of 1988 output**	**NPV in 1992 US$ mi.**	**Gain as % of 1992 output**
Government	9.8	37	– 137.9	– 4
Consumers	19.5	74	1326.6	44
Foreign investors	3.9	14	349.6	11
Domestic investors			66.8	2
Workers			49.5	1
Competitors			2.3	0
Total domestic change	29.3	111	1656.8	55

Sources: Alcazar *et al*. (2000); Menard & Clarke (2000b).

consortium assuming full responsibility for investment and operations over the contract period. End-of-contract technical targets ensured that the winning operators would fulfill the government's objectives. Tariff changes were tied to a formula set out in the concession contract. Fourth, the government reduced the workforce from 7,400 to 5,800 workers through a voluntary retirement programme. The private operators subsequently further reduced the workforce. Finally, the water and sanitation system obtained a substantial loan after contract closure (Rivera 1996; Alcazar *et al*. 2000).

This amalgam of reforms produced impressive performance gains (see Table 4). The assumptions underlying this calculation are that without these reforms there would have been no increase in investments; no gains in labour productivity and other inputs; and no price reduction in 1993. As may be seen, all stakeholders but the government gain from the reforms and consumers benefit the most.

In sum, the Guinea and Buenos Aires cases show positive welfare change because of structural changes in the organization of water and wastewater service delivery. These changes included the introduction of arms' length governance regimes, the introduction of competition for the market in the Buenos Aires case, mass redundancies, financial injections, and a delegation of management to private operators.

Neither case study attempts to measure the impact of individual components of these comprehensive reform packages on performance change. Therefore, we cannot single out the effect of ownership change on utility efficiency. Both teams of researchers appear to struggle with this attribution problem. In the Buenos Aires analysis, Alcazar and associates use the terms of reform, concession and privatization interchangeably. In their paper on Guinea, Menard & Clarke consistently (and correctly) ascribe the observed performance changes to reform. The paper, however, carries the misleading title 'The Welfare Effects of Private Sector Participation in Urban Water Supply in Guinea'.

THE DYNAMIC EFFICIENCY THESIS: EVIDENCE FROM OTHER INDUSTRIES

How does the dynamic water industry evidence fit in the overall picture of privatization and efficiency change? At first glance, this supports the ownership thesis. Many

Table 5 | Meta-analysis of dynamic privatization tests

Test results	Number of investigations	Percentage
Privatization produced significant efficiency gains	14	66.7
Privatization did not produce efficiency gains	7	33.3
Total no. of tests reviewed	21	100.0

Source: Shirley & Walsh (2000: pp. 50–51).

researchers find evidence of efficiency gains after privatization. Closer inspection reveals a number of methodological issues however. Researchers struggle with the same omitted variables that create problems in static ownership tests. A much-cited review of 61 divested firms by Megginson *et al*. (1994), for example, does not control for country, competition or other environmental variables yet heroically attributes all observed positive efficiency changes to privatization. This notwithstanding many descriptive accounts that note how privatizations are preceded by intensive corporate and market reform. For example, Ramamurti observes that before privatization '(g)overnments had to restructure the firms, negotiate deals with workers and their union leaders, clean up the firm's finances, redefine the regulatory environment . . . helped by foreign management consultants, accounting firms, lawyers, and investment bankers' (1996: p. 13). Galal *et al*. (1994) report similar observations in their path-breaking counterfactual analysis of 12 privatizations. Here, as in the two water counterfactuals discussed above, authors struggle with the problem of what part of the welfare gains arising from across-the-board reform to ascribe to a single component of that reform, namely privatization.

Table 5 presents the findings of a meta-analysis conducted by Shirley & Walsh (2000). Out of 21 dynamic tests of ownership effects reviewed by Shirley & Walsh, 14 find that privatization did produce efficiency gains while seven do not arrive at this conclusion.

Summing up, the dynamic efficiency literature confirms a tendency in evidence in water industry tests, namely that researchers attribute observed efficiency gains resulting from across-the-board reform erroneously to a single component thereof: ownership change. Not all studies find evidence of efficiency gains.

CONCLUSIONS

This article has reviewed the economics literature on the relationship between ownership form and efficiency. Property rights theory posits that managers of state-owned enterprises are less likely to maximize economic welfare than are managers of private enterprises, the former not being exposed to the disciplinary force of property rights and capital markets. The article has addressed three important questions. Have water industry researchers detected ownership effects? If so, how strong are these? Moreover, are they independent from other forces bearing on utility efficiency? An extensive body of work has tested for ownership effects in both static and dynamic settings. From this empirical literature, we can draw the following conclusions:

1. Measuring ownership effects is difficult. Ideally, one should compare a treatment group of private utilities with a control group of government-managed, but otherwise similar, utilities operating in similar environments. These criteria can be readily met in a static setting but they pose problems in a dynamic context. Tests of ownership effects occurring in the course of a privatization programme require one to collect data on ownership effects as well as on all other variables thought to influence efficiency change, for example changes in market structure,

regulation or the macro-economic climate. Most published dynamic tests do not control for these intervening variables. This makes it difficult to link performance improvements to their causes.

2. Static tests of the single country/single industry variety come closest to the ideal of a controlled experiment. A series of static tests carried out on large samples of American water utilities yields inconclusive results—it is doubtful whether ownership effects exist; if they do there is little reason to believe they are powerful.
3. Dynamic tests of ownership effects suggest that these become manifest in some but not all water privatizations. When utility performance does increase, it is not clear why this happens. In successful cases, we observe the implementation of a comprehensive package of reforms, ownership change being only one component thereof. Published tests do not reveal the explanatory power of the ownership variable nor do they show how crucial the privatization component was to the overall success of the reform package. This observation exemplifies the extent to which the privatization debate suffers from terminological inflation (Heald 1984; Galal *et al*. 1994; Dominy 1999). If privatization is used as a synonym for any and all beneficial reform, from regulatory change and the reduction of political interference to increased competition and financial transparency, privatization becomes a good thing by definition. This dilution renders the privatization debate empty however.

In sum, neither the static nor the dynamic water industry evidence provides convincing support for the ownership effects thesis. Cross-sectional comparisons do not show private water utilities to be consistently more efficient than their public counterparts. Some water privatizations produce efficiency gains but others do not. When efficiency does improve it is not clear what role privatization plays.

What are the implications of these findings? On the policy side, they confirm earlier suggestions that privatization will effect positive change in the water industry only in conjunction with supportive industry reforms (Klein; Rees 1998; Webb & Ehrhardt 2000). Increased private sector involvement, in other words, is no silver bullet. This review also suggests a research agenda for water industry investigators. Much of the literature on the dynamics of divestiture has so far skirted a fundamental issue, namely how crucial is a privatization component to the success of reform? One report argues that privatization is the only way to get rid of counterproductive government interference (World Bank 1995). Other literature, pointing to evidence of high-performing state-owned utilities, holds that successful reform may not always require privatization (Rees 1998; Blokland *et al*. 1999). This is an important empirical issue: is increased private sector involvement indispensable for successful utility reform? If so, why? Providing convincing answers to these questions will not be easy. For investigators the task ahead is to develop dynamic models of utility performance that incorporate both changes internal to the utility and external factors such as competition and regulation.

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