Water for Life: The Impact of the Privatization of Water Services on Child Mortality

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While most countries are committed to increasing access to safe water and thereby reducing child mortality, there is little consensus on how to actually improve water services. One important proposal under discussion is whether to privatize water provision. In the 1990s Argentina embarked on one of the largest privatization campaigns in the world, including the privatization of local water companies covering approximately 30 percent of the country's municipalities. Using the variation in ownership of water provision across time and space generated by the privatization process, we find that child mortality fell 8 percent in the areas that privatized their water services and that the effect was largest (26 percent) in the poorest areas. We check the robustness of these estimates using cause-specific mortality. While privatization is associated with significant reductions in deaths from infectious and parasitic diseases, it is uncorrelated with deaths from causes unrelated to water conditions.

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At the 2000 Millennium Summit, member countries of the United Nations unanimously agreed on a set of eight goals to reduce poverty by 2015, among which are reducing child mortality by two-thirds and cutting in half the number of households that do not have access to safe water. These two goals are interrelated in that clean water is critical to containing the spread of infectious and parasitic diseases. Indeed, each year more than 3 million children die from preventable water-related diseases (World Bank 2002*b*), and a number of studies have found that access to safe water is associated with better child health (e.g., Merrick 1985; Behrman and Wolfe 1987; Esrey et al. 1991; Lavy et al. 1996; Lee, Rosenzweig, and Pitt 1997; Jalan and Ravallion 2003).

While most countries have committed to increasing access to safe water, there is little consensus on how to actually achieve this goal. One proposal under consideration by many governments is to turn water provision over to a regulated private sector. Governments that want to privatize water systems are typically motivated by potential efficiency gains. They hope that these efficiency gains will be translated into expanded access and enhanced service quality, and thereby improved health outcomes. While there has been little privatization of water services (World Bank 2002*b*), a number of authors have reported large gains in productivity and profitability associated with privatization in other sectors (e.g., Megginson, Nash, and van Randenborgh 1994; Barberis et al. 1996; Frydman et al. 1999; La Porta and Lopez-de-Silanes 1999).

In the water sector, however, it is not clear whether any efficiency gains from privatization would necessarily be translated into improved health outcomes or help to alleviate poverty. Indeed, recent public opinion polls and press articles report widespread discontent with privatization in general in Latin America (Finnegan 2002; Inter-American Development Bank 2002; Tagliabue 2002; McKenzie and Mookherjee 2003). Private water companies may provide suboptimal levels of service quality because they fail to take into account the significant health externalities that are present in this industry (Shirley 2000). In this case, privatization of water services may affect health outcomes negatively. In addition, privatization may hurt the poor through price increases, enforcement of service payments, and investment only in lucrative high-income areas (Estache, Gomez-Lobo, and Leipziger 2001; Birdsall and Nellis 2003). In this case, efficiency gains from privatization might be obtained at the cost of excluding the poor from access to water services, and thus health outcomes of the poor may actually deteriorate under privatization.

In this paper, we examine the impact of the privatization of water services on child mortality in Argentina. Our study focuses on young children because they are particularly vulnerable to water-related dis-

eases as a result of weak body defenses, higher susceptibility, and greater exposure from inadequate knowledge of how to avoid risks. There are two main disease transmission mechanisms generated by the lack of appropriate water systems: waterborne diseases that occur by drinking contaminated water and water-washed diseases that occur when there is a lack of water and sanitation for household hygiene. Young children worldwide suffer from several deadly diseases that could easily be prevented through the interruption of these transmission mechanisms by access to safe and sufficient water supply and provision for the hygienic removal of sewage (World Health Organization 2000). For example, diarrhea alone accounts for approximately 15 percent of all child deaths worldwide (UNICEF 2001). In Argentina, diarrhea, septicemia, and gastrointestinal infections are three of the top 10 causes of death for children under 5 (Ministerio de Salud 1999).

Our analysis takes advantage of the fact that local governments are responsible for delivering water services and only some municipalities privatized those services. During the 1990s, about 30 percent of municipalities covering approximately 60 percent of the population privatized their water services. This variation in ownership across time and space provides a potential instrument to identify the causal effect of privatization on child mortality.

A major methodological concern, however, is that local governments choose to privatize water services, and that choice may not be orthogonal to unobservable factors that also affect mortality. We address this concern in a number of ways that lead us to believe that the link between the privatization of water systems and child mortality is causal.

In the end, despite the concerns about potential negative health effects, we find that the privatization of water services is actually associated with a reduction in child mortality. Our main result is anticipated in figure 1, which depicts the evolution of the child mortality rates for municipalities with privatized and nonprivatized water companies. During the first half of the decade, the mortality rates of the municipalities that eventually privatized their water systems decreased at the same rate as the mortality rates of the municipalities that did not privatize. However, after 1995 the mortality rates of the municipalities that privatized decreased faster than the mortality rates of those that did not privatize. As we shall show, this timing is commensurate with the timing of privatization. Before 1995 only a few municipalities had privatized, whereas the bulk of privatizations occurred after 1995.

Our difference-in-differences models estimate that the effect suggested by figure 1 corresponds to a reduction in child mortality of approximately 8 percent. Moreover, we find that most of the reduction in mortality occurred in low-income areas (26 percent), where the expansion of the water network was greatest. Finally, we scrutinize the

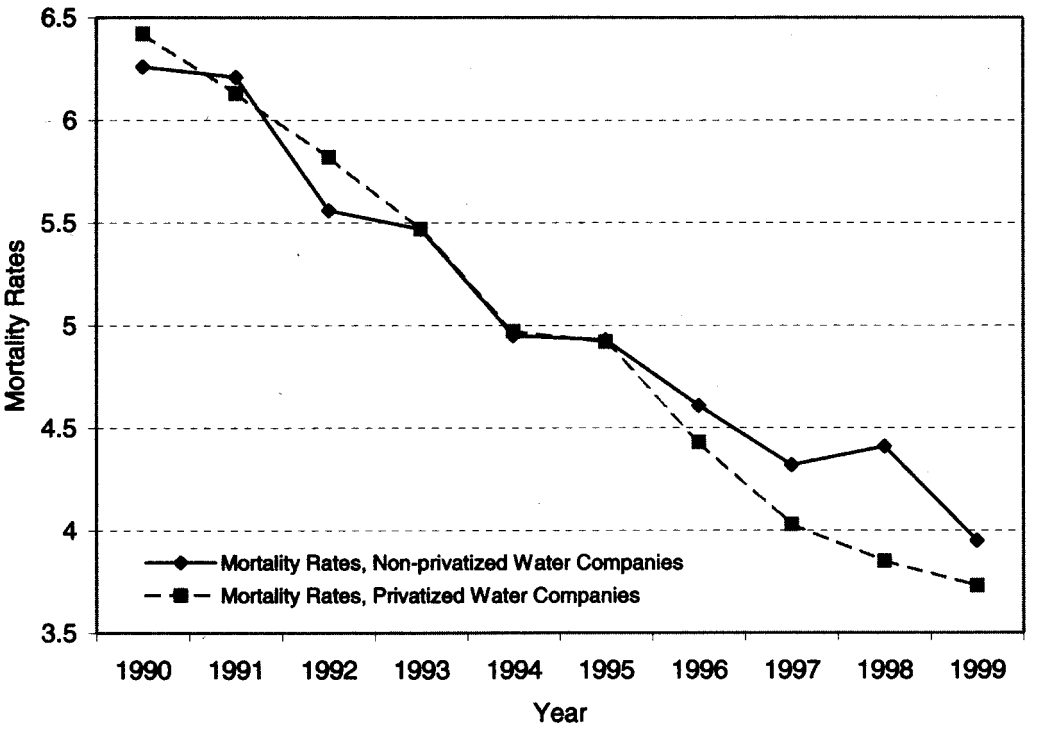

FIG. 1.—Evolution of mortality rates for municipalities with privatized vs. nonprivatized water services

validity of the causal interpretation of our estimates using cause-specific mortality. While privatization is associated with significant reductions in deaths from infectious and parasitic diseases, it is uncorrelated with deaths from causes unrelated to water conditions.

I. The Economics of Water Systems

Water systems include both the supply of clean water and the treatment and removal of sewage. These services are a natural monopoly involving large fixed costs and significant economies of scale (Noll, Shirley, and Cowan 2000). There is typically little competition to a well-functioning water system from alternative sources (Estache et al. 2001). The main alternative is household self-provision through pumped wells, rainwater catchments, cesspools, and septic tanks. Self-provision suffers from low quality and high cost (Abdala and Spiller 1999). Similarly, the sale of drinkable water from private vendors is substantially more costly and therefore does not present serious competition either. Finally, the average asset life of water systems' physical plant is very long and therefore impedes any potential dynamic competition.

The water sector is also characterized by the presence of significant externalities. Most water-related diseases are contagious. This generates positive externalities in the provision of clean water across society. Similarly, the proper elimination of sanitation residuals and treated industrial waste prevents negative externalities through the pollution of natural bodies of water and other natural resources.

Another special feature of water supply is that, as human life depends on access to drinkable water, the demand for water is perfectly price inelastic at survival levels. Of course, demand exhibits some price elasticity at levels for which water is used for other nonsurvival household and productive uses.

These features—natural monopoly, presence of significant externalities, and inelasticity of demand—have historically justified public intervention in the water sector. Most countries supply water services through the public sector, and private entry into water provision has been limited. However, there are growing calls to consider allowing a regulated private sector to deliver water services (World Bank 2002*b*).

Private supply has the advantage of providing strong incentives for cost reductions and other productivity enhancements. In contrast, these incentives are weak under public ownership, where typically agents cannot reap the results of their effort and innovation. In fact, empirical evidence from several sectors strongly suggests that service quality, productivity, and profitability rise significantly following privatization (Megginson et al. 1994; Barberis et al. 1996; Frydman et al. 1999; La Porta and Lopez-de-Silanes 1999).

Nonetheless, the weak efficiency incentives in public firms might be tolerable when cost reductions by private suppliers come at the expense of undesirable quality deterioration or reductions in access by the poor. In particular, unregulated private providers may undersupply the socially optimal quality of water in the presence of externalities because they fail to take into account the marginal social benefits in their decisions. Similarly, private owners may exclude low-income households from the network by raising prices, strictly enforcing payment, and concentrating their investments in high-income areas.

However, the fear of deterioration of quality or exclusion from access can be genuine only when supply conditions are noncontractible (Shleifer 1998). In the water industry, information asymmetries in service quality are relatively unimportant, and regulatory agencies can monitor water quality, pressure, repair delays, and shortages. Network expansions and universal coverage can also be enforced through regulation.

The arguments in favor of private provision are even stronger when we consider nonbenevolent governments. Politicians may use the control of state firms to channel benefits for themselves and their supporters (Shleifer and Vishny 1994). Excess employment, corruption, subsidies, and pork barreling are typical of state-owned enterprises (SOEs) around the world. As Shleifer (1998) explains it, state companies are unproductive not only because of the lack of managerial incentives but also because inefficiency results from the political use of SOE resources.

Finally, the process of resource allocation within the aggregated public sector does not guarantee the assignment of funds to the most profitable projects. The chronic underinvestment in physical capital that plagues many SOEs is aggravated for debt-ridden governments with large fiscal deficits. Privatization can significantly improve the access of firms to capital markets and therefore boost their ability to invest.

II. The Privatization of Water Services in Argentina

From 1870 through 1980, water services in Argentina were provided by the federal company Obras Sanitarias de la Nación (OSN) and a number of not-for-profit cooperatives. In 1980, OSN's jurisdiction was restricted to the federal district and 17 municipalities of the suburban Greater Buenos Aires area. While OSN remained under control of the federal government, the responsibility for public water services in the rest of the country was transferred to local governments (Artana, Navajas, and Urbiztondo 1999). Most of the companies provided both water and sanitation; however, a few supplied only water. In these cases, there was no sewerage service in the community.

In 1990, before privatization, public companies provided water services in two-thirds of the municipalities and not-for-profit cooperatives

TABLE 1
CHANGE IN OWNERSHIP OF WATER SYSTEMS, 1990–99

Ownership	Number of Municipalities	Percentage
Always public	196	39.7
Always private not-for-profit cooperative	143	28.9
Transferred from public to private for-profit	137	27.7
Always private for-profit	1	.2
No service or missing information	17	3.4
Total	494	100.0

NOTE.—In municipalities in which more than one company provides water services, we defined the ownership status of the municipality as the ownership of the company supplying the largest fraction of the population. Source: SPIDES, ENOHSA.

provided services in the remaining one-third. Between 1991 and 1999, about half of the public water companies servicing 28 percent of the country's municipalities and covering almost 60 percent of the country's population were transferred to private for-profit control (see table 1). The remaining municipalities continued receiving water services from either public companies or nonprofit cooperatives.[1]

A. Historical Context

The privatization of public water systems in Argentina represented a small part of a massive program that transferred almost all SOEs to private hands during the 1990s, which, in itself, was a part of a larger program of structural reform intended to reverse decades of economic decline. In the late 1980s Argentina was experiencing growing inflation driven in large part by printing money to finance huge fiscal deficits. The deficit averaged approximately 9 percent of gross domestic product during the decade. While federal and provincial overspending generated the lion's share of these deficits, a nontrivial portion was due to significant SOE losses. By the end of the decade the ruling Radical government was unable to balance the budget. Further deficit spending could not be financed through printing money or issuing new debt. In 1989 the country entered a period of hyperinflation that led the Radical government to resign six months before the official end of its administration.

The newly appointed Peronist government immediately launched an ambitious structural reform program designed to reduce the budget deficit, control inflation, and put the country back on a positive growth path. The program consisted of financial and trade liberalization, a monetary currency board, the decentralization of health and educa-

[1] The only exception is a small mining town in Jujuy, where a private mining company provided water service throughout the period of analysis.

tional services, the reform and privatization of the national pension system, the emancipation of the Central Bank, a general deregulation of economic activities, and the privatization of SOEs.

Argentina implemented one of the most ambitious privatization programs in the world. The privatized SOEs were mainly large natural monopolies in sectors such as electricity, oil and natural gas, telecommunications, transportation, mail service, and water systems. According to the official statistics (Centro de Estudios para la Producción 1998), 154 privatization contracts were signed during the 1990s. The privatization of the water sector was but a very small portion of the overall privatization program. In fact, the output of the water companies represented only a small fraction of the total SOE production (3.5 percent) and a tiny share of GDP (0.3 percent).

The privatizations were intended to reduce the budget deficit (Heymann and Kosacoff 2000). The acquiring firms paid the government substantial sums for the privatized companies in the form of cash and Argentine debt bonds. The privatization revenues collected by the federal and provincial governments reached U.S.$24 billion (Gerchunoff, Greco, and Bondorevsky 2003). As a percentage of public resources, privatization revenues were particularly important during the initial years of 1991 and 1992, when they represented approximately 10 percent of public revenues (Heymann and Kosacoff 2000). In addition to the revenues from privatization, the government no longer needed to cover SOE losses from the budget.

The privatizations were also intended to reverse a long period of neglect of the physical infrastructure (Chisari, Estache, and Romero 1999). During the 1970s and 1980s there was little capital investment in most public utilities, and indeed much of the physical infrastructure had seriously depreciated. After this long period of negative net investments, huge capital inflows were needed to improve both the quality of and access to SOE services. While the public sector had no capacity to finance those capital investments, private firms generating positive cash flows were able to obtain private financing. Indeed, the transfer of the SOEs to the private sector, mostly to large foreign companies, greatly improved the firms' investment and access to credit markets (Heymann and Kosacoff 2000; Galiani et al. 2005). Most of the privatized firms sold equity and bonds in international capital markets.

B. Why Did Local Governments Privatize Water Services?

Unlike most of the privatized sectors, which belonged to the federal government, the water sector is controlled at the local level, and therefore, the decision to privatize is a local one. In the early 1990s, the newly installed federal government focused its efforts on privatizing the larger

centrally controlled SOEs and did not put pressure on local governments to privatize their SOEs until later. Indeed, the privatization of water services accelerated after the elections in 1995, in which the ruling Peronist government was reelected. This is reflected in figure 2, which depicts the percentage of municipalities served by private water companies over time. Notice that the rate of privatization of municipalities was slow in the first half of the decade but accelerated in the second half.

In addition to the political influences from the central government, other factors could have affected local privatization decisions. For example, poorer municipalities with a lower tax base or underdeveloped infrastructure may have been more prone to privatize their water services. These are the municipalities that might have had the most to gain from privatization. This hypothesis states that when the whole country started privatizing the SOEs, the municipalities that jumped on the bandwagon were the poorer ones.

A different hypothesis is that the decision to privatize was made in response to an economic shock. As it occurred at the national level, local governments could have introduced privatizations to respond to regional recessions. Whether privatization is driven by time-varying shocks is important to the subsequent impact analysis. We propose to exploit the variability in firm ownership across time and space to identify the causal effect of privatization on child mortality using a difference-in-differences approach. While this approach controls for time-invariant heterogeneity, one of the main threats to its validity is the existence of time-varying unobserved covariates that are correlated with both privatization and mortality. For example, local economic shocks may affect both the privatization decision and child mortality. While we cannot test this directly, if we find that the decision to privatize is uncorrelated with observed time-varying covariates, then it is less likely to be correlated with unobserved ones.

In order to better understand why some local governments choose to privatize, in table 2 we estimate a discrete-time hazard model of the probability of transiting from public to private provision of water service using methods described in Prentice and Gloeckler (1978) and Jenkins (1995). We model the probability that a public water system in a given municipality and period of time is privatized as a function of a set of municipality time-invariant and time-varying covariates and allow for duration dependence.[2]

[2] Note that the sample utilized in the regressions of table 2 includes only the set of municipalities in which water services were operated by public companies in 1990 since nonprofit cooperatives were not at risk of privatization. The exact definitions of the variables and their sources are described in table A1 in the Appendix; descriptive statistics are reported in col. 1 of table 2.

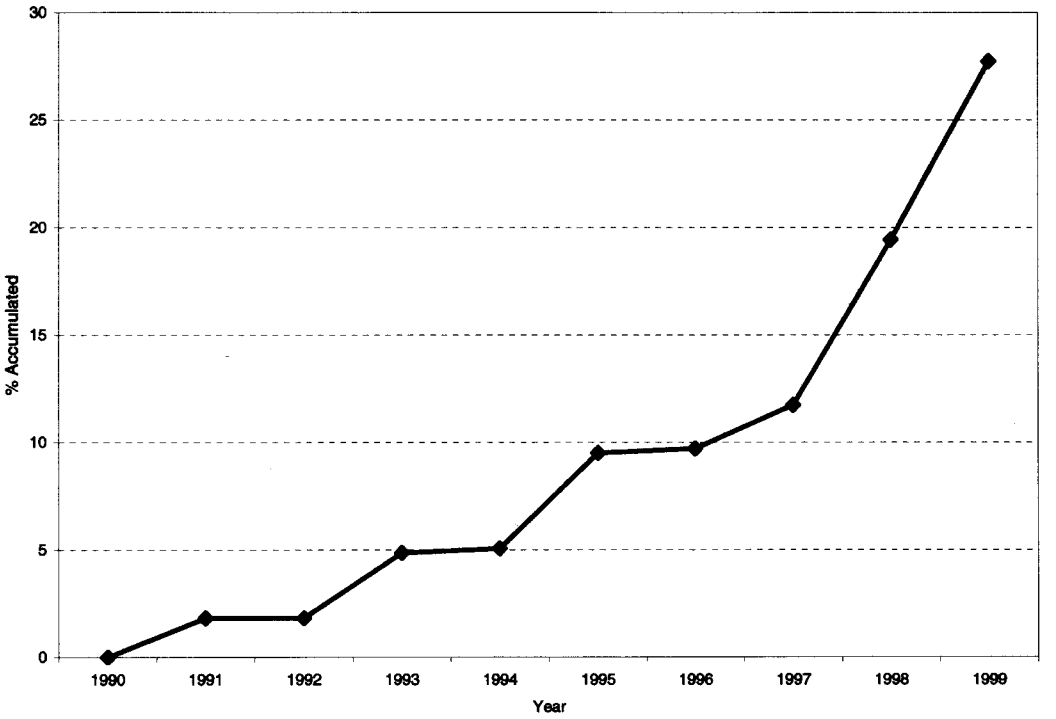

FIG. 2.—Percentage of municipalities with privatized water systems

First, we include a set of political variables that indicate whether the privatization decision was made by the central government or by a local government of a particular political affiliation. One would expect that the public water systems controlled by the federal government and local governments controlled by the party in power (Peronist) or provincial parties allied with the Peronists would be more likely to privatize than municipalities controlled by the opposition Radical party.

One might also expect that poorer regions are more likely to privatize. We measure the socioeconomic status of the areas using time-invariant covariates including GDP per capita, income inequality, and unemployment and a set of characteristics from the 1991 census such as the proportion of households that have unmet basic needs (UBN), housing characteristics, and demographic characteristics of the heads of households.

We test the hypothesis that socioeconomic shocks induced privatization decisions by including changes in GDP per capita, income inequality, and unemployment lagged one year. We use lagged shocks for two reasons. First, the privatization itself may have affected these time-varying variables, and, second, the length of time required since the decision to privatize a utility is made until the transfer is concluded suggests that the privatization decisions could not have been a response to contemporaneous shocks.

The results are reported in column 2 of table 2. First, as expected, we find that the likelihood of privatization is higher when the federal government or a local government run by the Peronist (or a provincial allied) party administers the public company than when the Radical party controls local government. This is consistent with the fact that the federal government launched the privatization wave of all SOEs, when the Peronists were the party in power and the Radicals were the loyal opposition. Second, we used a fifth-order polynomial to control for duration dependence, which shows that the likelihood of privatization increased over time. This is consistent with the sequencing of the overall privatization program in which the transfer of water systems to private operation occurred later in the decade. Third, the fixed baseline municipality socioeconomic characteristics are individually and jointly significantly different from zero and explain a good portion of the decision to privatize. Larger and less well off municipalities appear to be more likely to privatize. Fourth, and more important, none of the economic shocks are statistically significant.

While the results discussed above suggest that the decision to privatize is uncorrelated with economic shocks, it might be correlated with mortality for other reasons. Therefore, in column 3 of table 2 we include both the 1991 mortality rate and lagged changes to mortality. In this

TABLE 2
DISCRETE-TIME HAZARD ESTIMATE OF THE PROBABILITY OF BEING PRIVATIZED

	Mean (Standard Deviation) (1)	Model 1 (2)	Model 2 (3)
Time-varying covariates:			
Federal government operates services (=1)	.018 (.134)	15.975*** (2.719)	16.035*** (2.727)
Local government by Radical party (=1)	.139 (.346)	−3.198*** (1.067)	−3.204*** (1.067)
Local government by Peronist party (=1)	.719 (.449)	−.042 (.401)	−.054 (.402)
Δ log GDP per capita$_{t-1}$	.047 (.135)	4.294 (3.567)	4.259 (3.561)
Δ unemployment rate$_{t-1}$	.006 (.029)	−6.692 (5.696)	−6.805 (5.711)
Δ income inequality$_{t-1}$	.005 (.014)	.483 (7.483)	.139 (7.503)
Δ child mortality rate$_{t-1}$	−.266 (2.994)		.034 (.043)
Fixed pretreatment characteristics as of 1991:			
GDP per capita	60.601 (30.388)	−.022*** (.007)	−.022*** (.008)
Unemployment rate	.045 (.023)	12.871** (5.384)	12.790** (5.383)
Income inequality	.452 (.021)	−3.591 (5.820)	−3.469 (5.805)
Child mortality rate	6.208 (3.683)		−.009 (.036)
Population is 5,000–25,000 (=1)	.419 (.493)	.227 (.471)	.225 (.480)
Population is 25,000–50,000 (=1)	.202 (.402)	.106 (.535)	.110 (.540)
Population is 50,000–100,000 (=1)	.114 (.318)	−.261 (.605)	−.256 (.610)
Population is 100,000–250,000 (=1)	.079 (.269)	.663 (.612)	.668 (.615)
Population is more than 250,000 (=1)	.066 (.249)	1.159* (.631)	1.151* (.640)
Proportion of families with UBN	.246 (.151)	−13.660** (6.067)	−13.328** (6.226)
Proportion of families living in overcrowded housing	.097 (.059)	13.560* (7.150)	13.444* (7.200)
Proportion of families living in poor housing	.060 (.049)	6.980** (3.472)	6.987** (3.451)
Proportion of families living below subsistence	.036 (.022)	5.221 (7.418)	4.917 (7.449)
Proportion of houses with no toilet	.095 (.117)	10.143** (4.429)	9.798** (4.563)
No sewerage connection (=1)	.280 (.449)	−.182 (.323)	−.171 (.328)
Proportion of household heads with more than high school education	.025 (.012)	−27.242** (10.971)	−27.182** (11.003)

TABLE 2
(*Continued*)

	Mean (Standard Deviation) (1)	Model 1 (2)	Model 2 (3)
Mean household head's age between 45 and 52 (=1)	.653 (.476)	.279 (.343)	.288 (.343)
Mean household head's age above 52 (=1)	.144 (.351)	.506 (.456)	.513 (.456)
Duration dependence[a]		yes	yes
Observations		2,281	2,281

NOTE.—Standard errors are in parentheses.
* Statistically different from zero at the .1 level.
** Statistically different from zero at the .05 level.
*** Statistically different from zero at the .01 level.
[a] A fifth-order polynomial in time controls for duration dependence. Each coefficient in the polynomial is statistically different from zero at the .1 level.

model we find that both baseline mortality and lagged mortality are not correlated with the privatization decision.

III. The Effect of Privatization on Child Mortality

We evaluate the impact of the privatization of water services on the mortality of children under age 5. We focus on young children because they are particularly vulnerable to water-related diseases as a result of weak body defenses, higher susceptibility, and greater exposure from inadequate knowledge of how to avoid risks; and because water-related diseases can easily be prevented through access to clean drinking water, better hygiene, and better sanitation (World Health Organization 2000).

The dependent variable in our analysis is the child mortality rate constructed from information contained in vital statistics registries compiled by the Argentine Ministry of Health. We measure our dependent variable as the ratio of the number of deaths of children less than 5 years old to the total number of children less than 5 alive at the beginning of the year.[3] The database includes the 165,542 child deaths that occurred from 1990 through 1999 and are defined at the municipality level on an annual basis for 20 pathology groups.[4]

[3] Child mortality rate is traditionally defined as the probability that a child dies before she reaches age 5 and is usually approximated by the number of deaths of children less than 5 years old divided by the number of children born that year. When this definition of child mortality is applied to vital statistics, the mortality rate in Argentina has fallen from 72 per 1,000 live births in 1960 to 22 in 1999. Our results do not change when we use the traditional measure.

[4] We exclude from the analysis 5,042 child deaths for which the municipality is unspecified. The mortality data are not available at the municipality level before 1990.

A. *Identification and Estimation Methods*

Our objective is to identify the average effect of privatization on child mortality rates in the municipalities in which the water supply system has been privatized (i.e., the average impact of treatment on the treated). Specifically, we are interested in comparing mortality when water services are privately provided to the counterfactual, that is, mortality when services are publicly provided in the treatment areas at the same point in time. Since the counterfactual is never observed, we must estimate it. In principle, we would like to randomly assign private and public ownership across municipalities and compare the average outcomes of the two groups. In the absence of a controlled randomized trial, we are forced to turn to nonexperimental methods that mimic it under reasonable conditions.

A major concern is that the municipalities that chose to privatize could be different from the municipalities that chose not to privatize and that these differences may be correlated with mortality. For example, poorer urban areas in which mortality rates were higher may have been the ones that privatized. In this case, the correlation between privatization and mortality would be confounded with the wealth effect. In principle, many of the types of (unobservable) characteristics that may confound identification are those that vary across municipalities but are fixed over time. A common method of controlling for time-invariant unobserved heterogeneity is to use panel data and estimate difference-in-differences models.

Therefore, without the benefit of a controlled randomized trial, we turn to a difference-in-differences approach, which compares the change in outcomes in the treatment group before and after the intervention to the change in outcomes in the control group. By comparing changes, we control for observed and unobserved time-invariant municipality characteristics that might be correlated with the privatization decision as well as with mortality. The change in the control group is an estimate of the true counterfactual, that is, what would have happened to the treatment group if there had been no intervention. Another way to state this is that the change in outcomes in treatment areas controls for fixed characteristics and the change in outcomes in the control areas controls for time-varying factors that are common to both control and treatment areas.

Formally, the difference-in-differences model can be specified as a two-way fixed-effect linear regression model:

$$y_{it} = \alpha dI_{it} + \beta \mathbf{x}_{it} + \lambda_t + \mu_i + \epsilon_{it}, \tag{1}$$

where y_{it} is the mortality rate in municipality i in year t, dI_{it} is an indicator variable that takes on the value one if municipality i's water services are

privately provided in year t and zero otherwise, $\mathbf{x}_{it}$ is a vector of control variables that vary across both municipalities and time, μ_i is a fixed effect unique to municipality i, and λ_t is a time effect common to all municipalities in period t.

The error ϵ_{it} is a municipality time-varying error and is assumed to be distributed independently of all μ_i and λ_t. The errors ϵ_{it} might be correlated across time and space. For example, the persistence of regional epidemiological factors could induce time-series correlation at the municipality level. Error correlation could also be present in the cross-section dimension of the panel. Epidemiological factors present in one area could affect neighboring municipalities. Moreover, child health programs provided by provincial governments—the relevant policy unit after the decentralization of federal health services—typically apply to all the municipalities in a province at the same time. We take two approaches to avoid potential biases in the estimation of the standard errors. First, we allow for an arbitrary covariance structure within municipalities over time by computing our standard errors clustered at the municipality level. Second, we compute standard errors clustered at the province-year level.

In this model, α is the difference-in-difference estimate of the (average) effect of privatization of water services on mortality. The key identifying assumption for this interpretation is that the change in mortality in control areas is an unbiased estimate of the counterfactual. While we cannot directly test this assumption, we can test whether the secular time trends in the control and treatment municipalities were the same in the preintervention periods. If the secular trends are the same in the preintervention periods, then it is likely that they would have been the same in the postintervention period if the treated municipalities had not privatized.

Figure 1 already suggested that the levels and trends in mortality rates in treatment and control municipalities were the same before privatization. We formally test that the preintervention time trends for the control and treatment groups are not different by estimating a slightly modified version of equation (1). We use only the observations of the control and the treatment municipalities in the pretreatment period; that is, we use 1990–98 for all the control municipalities and only the preintervention years for the treatment municipalities. This covers nine of the 10 years since a number of treatment municipalities were not privatized until 1999, the last year of our sample. We modify equation (1) by excluding the privatization dummy variable and including separate year dummies for (eventual) treatments and controls. In this model, we cannot statistically reject the hypothesis that the preintervention year dummies are the same for both the control and (eventual) treatment municipalities at conventional levels of statistical significance.

This implies that the mortality rates in treatment and control groups had identical time trends (and levels) in the "pretreatment" period and validates our difference-in-differences identification strategy.

A related issue is that we are using both the municipalities that always had public provision of water services and the municipalities that had nonprofit cooperatives as controls. While the cooperatives were never susceptible to privatization, they are just as good as the always-public municipalities as controls for estimating the counterfactual. In fact, when we estimate separate year dummies for the always-public and cooperative municipalities in a mortality model for the whole sample period, we do not reject the hypothesis that these year dummies are the same at conventional levels of statistical significance. This implies that the mortality trends of the always-public and cooperative municipalities were the same over the sample period, and therefore they are equally as good in predicting the counterfactual.

B. Results

We present equation (1) estimation results for child mortality from all causes of death in table 3. Column 1 reports the results for a model using the whole sample and including no covariates except for municipality fixed effects and year dummies. We find that the privatization of water services is associated with a 0.33 reduction in the mortality rate, which amounts to a 5.3 percent reduction of the baseline rate.

One concern is that there may be municipality characteristics that vary across time and space and that are correlated with both mortality and privatization.[5] For example, it could be that the areas that privatized were also hit by positive economic shocks or there were improvements in the health care system or increases in public welfare programs at the time they privatized. Therefore, we directly control for a number of observed time-varying economic and political characteristics in models 2 and 3 in table 3.

We investigate whether positive economic shocks to the municipalities that privatized might have caused the reduction in mortality, including GDP per capita, unemployment, income inequality, and public spending per capita in a model reported in column 2.[6] The public spending variable controls for the possibility that the impact of privatization is coming from correlated improvements in the local public programs. The estimated impact of privatization is unchanged, but its statistical

[5] In Sec. II, we provide evidence consistent with the notion that privatization is driven by fixed characteristics and not by the observed time-varying variables. This suggests that privatization is also less likely to be correlated with time-varying location-specific unobserved shocks.

[6] The results in table 3 remain unchanged if the control variables are lagged.

significance drops to the .11 level when the standard errors are clustered by province-year.

A related concern is that the same political parties that choose to privatize might, in general, run better administrations or have stronger preferences for a reduction in child mortality in ways not properly captured by the public spending variable. In model 3 of table 3, we add dummy variables for the political party that controlled the local government. While it appears that mortality rates were marginally higher when the Radical party took over, the estimated impact of privatization was unaffected. Overall, we find that privatization is associated with a reduction in the child mortality rate of about 5 percent using the full sample regardless of the choice of controls.

C. Heterogeneous Response

Another concern is that the impact of privatization may not be homogeneous across municipalities, but rather may vary as a function of the characteristics of the municipalities. For example, the impact of the privatization may matter more in areas in which families are better educated. In this case, simple difference-in-differences estimates may suffer from two additional sources of bias (Heckman, Ichimura, and Todd 1997). The first bias arises when there are some municipalities in which privatization has taken place, but there are no comparable municipalities for which privatization did not occur and vice versa. The second bias may arise from different distributions of the vector of observable variables that affect mortality ($\mathbf{x}$) within the two groups of municipalities.[7]

Matching methods eliminate these two potential sources of bias by pairing privatized municipalities (treatments) with nonprivatized municipalities (controls) that have similar observed attributes. Using observations in the treatment and control groups over the region of common support in the distribution of $\mathbf{x}$ eliminates the first source of concern, whereas the bias due to different distributions of $\mathbf{x}$ between treated and untreated municipalities within this common support is eliminated by reweighting the control group observations.

In general, conventional matching methods assume that, conditional on the observed variables $\mathbf{x}$, the counterfactual outcome distribution of the treated units is the same as the observed outcome distribution of the units in the control group. This assumes that there is no selection into treatment on the basis of unobservables. To avoid the necessity for

[7] The vector $\mathbf{x}$ may include variables that vary only across municipalities and also across time and municipalities. With regard to bias, Heckman et al. (1997) suggest that, in practice, the first of these two sources of bias is likely to be the most severe.

TABLE 3
IMPACT OF PRIVATIZATION OF WATER SERVICES ON CHILD MORTALITY

	Full Sample			Using Observations on Common Support			Kernel Matching on Common Support[a]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Private water services (=1)	−.334	−.320	−.283	−.540	−.541	−.525	− .604
	(.169)**	(.170)*	(.170)*	(.177)***	(.178)***	(.178)***	(.168)***
	[.157]**	[.163]**	[.162]*	[.191]***	[.198]***	[.195]***	
	{.195}*	{.203}	{.194}	{.261}**	{.274}**	{.266}**	
%Δ in mortality rate	−5.3	−5.1	−4.5	−8.6	−8.6	−8.4	−9.7
Other covariates:							
Real GDP per capita		.007	.009		.005	.006	
		(.005)	(.006)		(.006)	(.006)	
		[.006]	[.006]		[.007]	[.007]	
		{.007}	{.007}		{.007}	{.008}	
Unemployment rate		−.555	−.636		−.778	−.836	
		(1.757)	(1.758)		(1.797)	(1.802)	
		[2.161]	[2.166]		[2.249]	[2.263]	
		{2.862}	{2.846}		{2.635}	{2.635}	

Income inequality		5.171	5.085		2.932	3.052	
		(2.868)*	(2.880)*		(2.907)	(2.926)	
		[3.468]	[3.445]		[3.314]	[3.289]	
		{3.696}	{3.691}		{3.833}	{3.838}	
Public spending per capita		−.028	−.035		−.068	−.070	
		(.038)	(.038)		(.039)*	(.039)*	
		[.055]	[.055]		[.059]	[.059]	
		{.054}	{.055}		{.049}	{.050}	
Local government by Radical party (=1)			.482			.166	
			(.267)*			(.284)	
			[.281]*			[.301]	
			{.288}*			{.365}	
Local government by Peronist party (=1)			−.202			−.168	
			(.191)			(.193)	
			[.202]			[.230]	
			{.254}			{.309}	
R^2	.1227	.1256	.1272	.1390	.1415	.1420	
Observations	4,732	4,597	4,597	3,970	3,870	3,870	3,970

NOTE.—Each column reports the estimated coefficients of a separate regression model in which the dependent variable is the child mortality rate, whose mean was 6.25 per thousand in 1990. Standard errors are in parentheses. Standard errors clustered at the municipality level are in brackets. Standard errors clustered at the province-year level are in braces. All the regressions include year and municipality fixed effects. The sample includes the municipalities with always-public, privatized, and nonprofit cooperative water companies (see table 1).

[a] Standard errors for the kernel matching estimate are bootstrapped standard errors using 100 replications.

* Statistically different from zero at the .1 level of significance.

** Statistically different from zero at the .05 level of significance.

*** Statistically different from zero at the .01 level of significance.

this assumption, Heckman et al. (1998) propose a generalized difference-in-differences matching estimator that extends conventional matching methods to longitudinal data. By conditioning on fixed effects, the generalized difference-in-differences estimator identifies the parameter of interest without ruling out selection into treatment on the basis of time-invariant unobservables.

The objective, then, is to construct a control group by finding controls that have observed $\mathbf{x}$'s similar to those of the treatments. Rosenbaum and Rubin (1983) show that to match treated and untreated units on the basis of $\mathbf{x}$ is equivalent to matching them using a balancing score $B(\mathbf{x})$. The coarsest balancing score is the propensity score that gives the conditional probability of receiving treatment given the pretreatment values of the vector $\mathbf{x}$, that is, $P(\mathbf{x}) = \Pr(D = 1|\mathbf{x})$. Then, the method of matching assumes that, conditional on $P(\mathbf{x})$, the counterfactual outcome distribution of the treated units is the same as the observed outcome distribution of the controls. This result is very important in practice since it reduces the potential problem from matching on a high-dimensional vector $\mathbf{x}$ to matching on a scalar.

We estimate propensity scores from a logit model of the probability that a municipal water system that was public in 1990 was privatized sometime before the year 2000 as a function of the preintervention characteristics used in model 1 of table 2. These models are then used to predict the propensity (probability) that a municipality will privatize.

We identify control and treatment observations on a common support as follows. We exclude all control observations whose propensity scores are less than the propensity score of the treatment municipality at the first percentile of the treatment propensity score distribution and exclude all treatment observations whose propensity score is greater than the propensity score of the control observation at the ninety-ninth percentile of the control distribution. Then, our second set of estimates is obtained as difference-in-differences on the observations that lie on this common support. Finally, we use a kernel density weighting procedure to obtain the generalized difference-in-differences matching estimator (see Heckman et al. 1997).[8]

The results of these analyses are presented in models 4–7 in table 3. The estimated impact of privatization on mortality increases when we restrict the sample to observations only on the common support of the propensity score. In models 4–6, we report the results of estimating models 1–3 restricting the sample to observations on the common support. In the basic model of column 4, privatization is associated with an 8.6 percent fall in the child mortality rate. This estimate does not

[8] The bootstrapped standard errors of the matching estimates also address the potential serial correlation concern in difference-in-differences models.

change when we control for socioeconomic characteristics in column 5 and for political variables in column 6. Finally, the generalized difference-in-differences matching estimator, which uses kernel density weighted matches on the common support, is reported in column 7. The model estimates that privatization is associated with a 9.7 percent reduction in the child mortality rate.

D. Results by Cause of Death

In spite of the robustness of our results to the inclusion of economic and political controls, it is still possible that at the time of privatization there may have been other unobserved changes in the municipalities that privatized that are correlated with mortality in general. For example, there may have been enhancements in the health care system or increases in public welfare programs not captured by the public spending or political variables. It is also possible that there were different migratory trends among treated and untreated municipalities correlated with privatization.

In order to rule out possible unobserved changes correlated with privatization, we examine the impact of privatization on mortality by cause of death. The mortality data in Argentina are disaggregated for 20 specific pathology groups. The privatization of water provision on child mortality should mainly operate by affecting deaths from infectious and parasitic diseases. These deaths are classified into two of the pathology groups. If the death occurred after the first 28 days of life, it is classified in the infectious and parasitic diseases group. However, all deaths that occurred during the first 28 days of life are placed into the perinatal deaths category, regardless of the cause. Thus, even if the death occurred from an infectious or parasitic disease, it is assigned to the perinatal deaths during the first 28 days of life, and not to the infectious and parasitic diseases category. Therefore, if the observed reduction in child mortality is operating through improved access to and quality of water, then we should see significant negative effects on deaths in the perinatal deaths and infectious and parasitic diseases categories, and negligible effects on deaths from other causes such as accidents, cardiovascular diseases, or cancer.

We estimate the difference-in-differences models using municipalities with common support and all socioeconomic and political controls for child mortality rates for each cause of death.[9] The results are reported in table 4. As predicted, we find a statistically significant effect on mor-

[9] As we are analyzing child mortality, we exclude from this exercise the analysis of deaths from four causes that are relevant only for adults (suicides; homicides; other violent deaths; and pregnancy, labor, delivery, and puerperal diseases). We also exclude the residual category of undefined causes.

TABLE 4
IMPACT OF PRIVATIZATION ON CHILD MORTALITY BY CAUSE OF DEATH

	1990 Mean Mortality Rate	Estimated Impact Coefficients	%Δ in Mortality Rate
Infectious and parasitic diseases	.565	−.103 (.048)** [.055]* {.068}	−18.2
Perinatal deaths	2.316	−.266 (.105)** [.107]** {.123}**	−11.5
All other causes in aggregate	2.565	−.082 (.114) [.101] {.109}	−3.2
All other causes disaggregated:			
Accidents	.399	−.004 (.057)	…
Congenital anomalies	.711	−.022 (.056)	…
Skin and soft-tissue diseases	.000	.000 (.001)	…
Blood and hematologic diseases	.024	−.002 (.008)	…
Nervous system disorders	.163	.025 (.026)	…
Cardiovascular diseases	.236	.006 (.030)	…
Gastrointestinal tract disorders	.051	−.001 (.010)	…
Genital and urinary diseases	.020	−.006 (.007)	…
Osteoarticular and connective tissue diseases	.003	−.001 (.001)	…
Respiratory diseases	.511	−.038 (.051)	…
Immunodeficiencies, endocrine, and nutrition system diseases	.376	−.035 (.033)	…
Mental disorders	.002	.001 (.001)	…
Tumors	.068	−.006 (.015)	…

NOTE.—Each cell reports the estimated coefficient on the private water services dummy from a different difference-in-differences regression. Standard errors are in parentheses. Standard errors clustered at the municipality level are in brackets. Standard errors clustered at the province-year level are in braces. All the regressions include year and municipality fixed effects. All the regressions use the 3,870 observations on the common support and the socioeconomic and political covariates included in the regression in col. 6 of table 3.

* Statistically different from zero at the .1 level of significance.

** Statistically different from zero at the .05 level of significance.

tality from infectious and parasitic diseases and perinatal deaths, but no statistically significant effect on mortality from any other cause either separately or in aggregate.[10] The estimated effects correspond to a reduction of 18.2 percent in mortality from infectious and parasitic diseases and a reduction of 11.5 percent in perinatal deaths.

The importance of this result cannot be overemphasized. Privatization could be spuriously capturing the effect of unobservables if those uncontrolled variables are correlated only with deaths from infectious and parasitic diseases, but not with deaths from any other cause. This result rules out the presence of almost any other plausible explanation of our main results and leads us to believe in their causal interpretation.

E. Impact by Socioeconomic Status

We hypothesize that privatization should have had a higher impact on child mortality in poor municipalities than in wealthier ones. Middle- and high-income groups already had a high rate of connection to the water network prior to privatization. Even when they were not connected or when service quality was unsatisfactory, these income groups enjoyed better access to substitutes such as pumped wells, septic tanks, or bottled water than poor households. The main beneficiaries of network expansions and service enhancements, therefore, were low-income households, which also are the groups most vulnerable to child mortality.

In table 5 we report the estimated impact of water privatization on child mortality at three different ranges of poverty at the municipality level. To estimate these heterogeneous impacts of privatization on child mortality, we interact the treatment dummy variable with a poverty indicator function from the 1991 census. We construct three ranges of poverty: municipalities with a percentage of households suffering from UBN lower than 25 percent, municipalities with UBN between 25 and 50 percent, and municipalities with UBN higher than 50 percent.

We find that the privatization of water systems does not affect mortality in those municipalities with low levels of poverty (UBN lower than 25 percent). The effect on the remaining treated municipalities is increasing in the level of poverty and is highly significant.[11] In fact, the privatization of water systems is associated with a 26.5 percent reduction

[10] When clustered standard errors are considered, the statistical significance of the coefficient for the infectious and parasitic diseases category falls to the .06 level for the municipality clustered standard errors and to the .13 level for the province-year clustered standard errors. The significance remains at the .05 level for perinatal deaths. The clustering of the standard errors does not alter the nonsignificance of the privatization effect for mortality from any other cause of death individually or in aggregate (for the sake of space we include only the clustered standard errors for all the other causes in aggregate).

[11] The statistical significance remains unaltered when clustered standard errors are considered.

TABLE 5
IMPACT OF PRIVATIZATION ON CHILD MORTALITY BY POVERTY LEVEL

	1990 Mean Mortality Rate	Estimated Impact Coefficients	%Δ in Mortality Rate
Nonpoor municipalities	5.07	.114	…
		(.233)	
		[.165]	
		{.159}	
Poor municipalities	6.97	−1.004	−14.4
		(.279)***	
		[.297]***	
		{.278}***	
Extremely poor municipalities	9.11	−2.415	−26.5
		(.544)***	
		[1.051]**	
		{.605}***	

NOTE.—Municipalities are divided into poverty groups using the government's index of UBN using data from the 1991 census. Nonpoor municipalities are defined as those in which less than 25 percent of households have UBN. Poor municipalities are defined as those in which 25–50 percent of households have UBN. Extremely poor municipalities are defined as those in which more than 50 percent of households have UBN. The reported coefficients are the interaction of the private water services dummy and UBN (recoded in a set of dummy variables in the three categories: below 25 percent, between 25 and 50 percent, and above 50 percent) in a difference-in-differences regression using only the 3,870 observations on the common support. The regression includes year and municipality fixed effects and the socioeconomic and political covariates used in the regression reported in col. 6 of table 3. Standard errors are in parentheses. Standard errors clustered at the municipality level are in brackets. Standard errors clustered at the province-year level are in braces.

** Statistically different from zero at the .05 level of significance.

*** Statistically different from zero at the .01 level of significance.

in child mortality in municipalities with high levels of poverty (UBN greater than 50 percent). This result is consistent with the predictions of our causal model. The effect of privatization on child mortality should be stronger for the groups that are more vulnerable to water-related diseases.

IV. Pathways

In Section III, we provide evidence that child mortality fell faster in areas that privatized water companies than in areas that did not privatize. In this section, we show that firms that privatized made choices that affected the pathways by which child health could be improved. There are a number of potential pathways by which the privatization of water systems might have induced the reduction in child mortality. First, privatization may have expanded the water supply and sewerage network providing access to service to households that were not previously connected to water and sewerage. Second, there may have been improvements in service quality in terms of reduced spillage of water and sewage, faster repair rates, fewer shortages, cleaner water, and better water pressure and sewage treatment. All these quality enhancements improve the epidemiological environment. In this section, we present evidence that privatization affected these pathways.

A. A Case Study

The largest water company privatization was the transfer of the federal company OSN that provided service in the Buenos Aires metropolitan area. The analysis of this privatization, described in Abdala and Spiller (1999), Artana et al. (1999), Shirley (2000), and Alcazar, Abdala, and Shirley (2002), illustrates the changes experienced by water systems in Argentina after the transfer to private operation.

Rather than assets being sold to private firms, water services in Argentina were transferred to the private sector through concessions.[12] In some cases, such as OSN, the royalty was set at zero and firms competed for the concession by offering the lowest tariff. In other cases, the privatized companies paid a canon to the government for the use of the public assets. For example, in the provinces of Cordoba and Corrientes, where a canon is paid on an annual basis, the royalty payments represented about 0.4 percent and 0.1 percent, respectively, of the provincial revenues in 1999. Thus the revenue from the royalties of the privatization of water service constituted at best a very small share of the public budget.

In May 1993, Aguas Argentinas, a private consortium led by the French company Lyonnaise des Eaux, won a 35-year concession to provide water services previously provided by OSN. The terms of the concession stipulated that 100 percent of households had to be connected to water service and 95 percent to sewerage service by the end of the 35-year period. It also established service quality and waste treatment standards.

Water use fees in Buenos Aires were initially lowered by 26.9 percent as a result of the privatization bid. However, 13 months after privatization, the regulator authorized a 13.5 percent increase in the usage fee and a significant increase in connection fees. This latter increase was particularly controversial since the connection fee almost reached a month's earnings for a household at the official poverty line. In response to protests, the connection fee was lowered to about one-tenth of the previous level, and a fixed charge was added to the water use bills for all clients as a cross subsidy (Alcazar et al. 2002). Indeed, the Buenos Aires water concession has been criticized for its prompt and frequent renegotiations (Gerchunoff et al. 2003; Clarke, Kosec, and Wallsten 2004).[13]

[12] This is the most common method of privatizing water services worldwide (Noll et al. 2000).

[13] Renegotiations of water concessions seem to be pervasive. Guasch, Laffont, and Straub (2003) report that 70 percent of water concession contracts in Latin America have been renegotiated. Criticisms of the Buenos Aires water concession also refer to the obscure tariff system that was inherited from the public era. Water use bills for unmetered customers are a complex function of property characteristics (Alcazar et al. 2002). Aguas Argentinas increased fees for 17 percent of the customers through property reclassifications (Clarke et al. 2004).

The enforcement of service payment was toughened after privatization. While delinquency was high for OSN, the private operator was allowed to cut service to customers with three unpaid bills (although it could be reconnected under the regulator's request). According to Artana et al. (1999), over 90 percent of customers regularly pay the service fees, although only about 60 percent pay them on time.

Privatization increased efficiency and profitability. Before privatization, OSN was overstaffed and absenteeism was high. During the first year under private management, the number of employees was reduced from 7,365 to 3,800. The reduction in employment, together with the increase in coverage and production, resulted in large increases in productivity. In fact, after a first year of negative returns, Aguas Argentinas turned into a highly profitable company.

A major question was whether these efficiency gains were translated into improvements in service quality. OSN had invested very little in infrastructure during the decade prior to privatization (Galiani et al. 2005). Low revenues and inefficiencies led to such low investment levels that they were not even sufficient to replace depreciating assets and maintain current supply. In 1985 OSN investment was 67.8 percent of what was needed to maintain current supply, and only 19.5 percent in 1990.[14] In the late 1980s, water coverage as a share of population was contracting, spilled water rates were very high, pressure and service quality were low, and summer shortages were frequent.

Things improved significantly after the privatization. The private company was able to invest a substantial amount in physical infrastructure and service quality. For the 10 years before the privatization, OSN invested an average of U.S.$25 million annually. From 1993 through 2000, Aguas Argentinas' investment jumped to around $200 million per year. Table 6 shows large increases in production of water and sewage, reductions in spillage, and significant service enhancements. In addition, summer water shortages disappeared, repair delays shortened, and water pressure and cleanliness improved.

The investments also paid off in terms of increased access to the network. The number of connections to the water and sewerage networks in Buenos Aires expanded by 30 percent and 20 percent, respectively, after privatization. Figure 3 depicts the log of the number of households connected to the OSN–Aguas Argentinas water and sewerage network by year from 1986 through 1999. While the number of households connected was relatively flat from 1986 to 1993, the network grew rapidly each year after privatization.

Moreover, the network expansion was concentrated in the poorer

[14] For the whole country, investment in the water sector as a percentage of total domestic investment fell from 1.5 percent during the 1960s to 0.5 percent in 1981–93 (Rey 2000).

TABLE 6
COMPARISON OF OSN (PUBLIC) VS. AGUAS ARGENTINAS (PRIVATE) PERFORMANCE, 1980–99

	OSN[a] (before Privatization)	Aguas Argentinas[b] (after Privatization)	Δ after Privatization (%)
Water production (1) (millions of m^3 per day)	3.56	3.89	9.3
Spilled water (2) (millions of m^3 per day)	1.49[c]	1.27	−14.8
Water supply (1–2) (millions of m^3 per day)	2.07[c]	2.62	26.6
Sewage drainage volume (millions of m^3 per day)	2.18	2.45	12.4
Water network extension (km of network)	10,148	13,287	30.9
Sewerage network extension (km of network)	6,875	8,312	20.9
Average delay in attending repair requests (days)	180[d]	32[e]	−82.2
Water leakages repaired per year	42,000[c]	96,383	129.5
Sewerage blockages repaired per year	100,000[c]	148,500	48.5
Percentage of clients with appropriate water pressure	17[c]	54[f]	217.6
Water turbidness (turbidness units)	7.5	2.3	−70
Usage fee index[g]	100	84	−16
Employees	9,300	4,000	−57

[a] Average for the period 1980–92.
[b] Average for the period 1994–99.
[c] 1993 only.
[d] 1992 only.
[e] Average excludes 1994.
[f] 1996 only.
[g] Corresponds to the "K" tariff factor. Source: Universidad Argentina de la Empresa–Centro de Estudios Económicos de la Regulación.

suburban areas of Greater Buenos Aires. Since 98 percent of households in the city of Buenos Aires were already connected to water services before privatization, most of the expansion in access necessarily had to occur among lower-income households in the suburban areas. Indeed, table 7 shows that middle- and low-income households accounted for 84.6 percent of the new connections.

B. Access to Water Services

While the data for Buenos Aires show that the privatization improved service quality and expanded access to water services, we are unable to similarly assess the impact of privatization for the rest of the country. We are, however, able to say something about the effect of privatization on access to water services. Even though increased access may not be the only mechanism through which privatization can affect child mor-

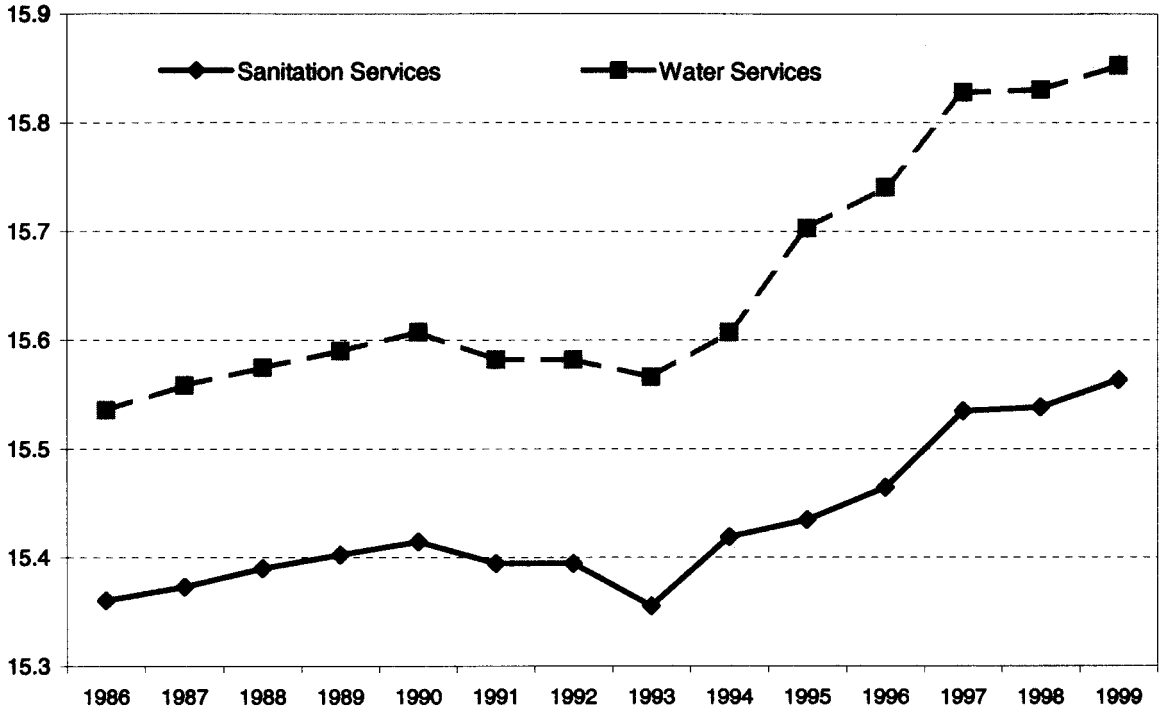

FIG. 3.—Logarithm of population connected to OSN–Aguas Argentinas network

TABLE 7
NETWORK EXPANSION BY INCOME GROUP IN GREATER BUENOS AIRES, 1993–2000

Income Level	New Connections	Percentage
High and upper-middle income	90,200	15.4
Lower-middle income	282,250	48.3
Low income	211,800	36.3
Total	584,250	100.0

SOURCE.—Subsecretaría de Recursos Hídricos, from Abdala and Spiller (1999).

tality, it is probably one of the most important causal channels. Indeed, acquiring water services for the first time is likely to imply a more important change in access to safe water relative to service improvements to households with existing water and sewerage connections.

We evaluate the impact of privatization on access to water services using data from the 1991 census and the 1997 Encuesta de Desarrollo Social (EDS). The EDS was a stratified random survey of about 30,000 households from urban municipalities with more than 5,000 inhabitants, and it asked questions about household connections to water services identical to those asked in the census.

To identify the effect of privatization on access to water, we exploit the fact that by 1997 a number of municipalities had already privatized their water services (fig. 2). Using the data from municipalities in the EDS survey, we calculate the difference-in-differences estimate of the impact of privatization on the proportion of households that had access to the water network. The difference-in-differences estimator compares the change in the proportion of households connected to water services in municipalities that privatized to the change in the proportion connected in municipalities that did not privatize water services. For this exercise, a municipality is in the privatized group if the privatization of water services occurred between 1990 and 1996.

The results, reported in table 8, show a significantly larger increase in the proportion of households connected to water services in the municipalities that privatized than in those that did not. The estimated impact is even higher when we exclude the capital city, where 98 percent of households were already connected to water service before privatization. In this sample, the results suggest that the number of households connected to the water network increased by 4.2 percentage points as a result of privatization.

This estimate, however, most likely underestimates the impact of privatization on access for two reasons. First, it includes the impact of privatization only through 1997. In Cordoba, for example, water services were privatized in that year, and coverage increased by more than 10

TABLE 8
DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE IMPACT OF PRIVATIZATION ON THE PROPORTION OF HOUSEHOLDS CONNECTED TO THE WATER NETWORK, 1991–97

	All Municipalities	Excluding Buenos Aires
	Municipalities That Were Not Privatized before 1997	
Proportion of households connected in 1991 (p_{91}^{public})	.866	.866
Proportion of households connected in 1997 (p_{97}^{public})	.898	.898
Difference 1997 − 1991 ($p_{97}^{public} - p_{91}^{public}$)	.032	.032
	Municipalities That Were Privatized before 1997	
Proportion of households connected in 1991 ($p_{91}^{private}$)	.730	.640
Proportion of households connected in 1997 ($p_{97}^{private}$)	.780	.714
Difference 1997 − 1991 ($p_{97}^{private} - p_{91}^{private}$)	.050	.074
Difference-in-differences ($p_{97}^{private} - p_{91}^{private}$) − ($p_{97}^{public} - p_{91}^{public}$)	.018	.042
Z-test for difference-in-differences estimate[a]	2.83***	5.78***

NOTE.—The preintervention connection rates are higher in control areas than in treatment areas in this table. However, this analysis takes into account privatization only through 1996, and therefore, the control group includes the set of municipalities that privatized later. When all the privatized municipalities are included in the treatment group, the preprivatization connection rates are the same in (eventually) treated and control groups. Specifically, in 1991, 74 percent of households were connected in eventually privatized areas and 70 percent in never privatized areas.

[a] The statistic of contrast is

$$z = \frac{(p_{97}^{private} - p_{91}^{private}) - (p_{97}^{public} - p_{91}^{public})}{\sqrt{[p_{97}^{private}(1 - p_{97}^{private})/n_{97}^{private}] + [p_{97}^{public}(1 - p_{97}^{public})/n_{97}^{public}]}},$$

where p_t is the proportion of households with access to water connection in year t in a municipality in which water has been privatized (private) or has not been privatized (public), and n is the number of observations. Note that there is no sample variability when we estimate p for 1991 since these statistics are estimated from census data.

*** Statistically different from zero at the .01 level of significance.

percentage points in the first three years of concession. Second, the EDS grossly undersampled poor areas,[15] and access expanded most in poor areas in which fewer households were connected at the baseline. For example, table 7 showed that connections increased the most among the poor in Greater Buenos Aires, whereas Artana et al. (1999) report that after privatization in Corrientes, one of the poorest provinces in the country, the number of connections to the water network in the

[15] Specifically, when we split the sample into three groups—nonpoor municipalities in which less than 25 percent of households have UBN in the 1991 census, poor municipalities in which between 25 and 50 percent have UBN, and extremely poor municipalities in which more than 50 percent have UBN—we found that the EDS does not include any extremely poor municipalities and includes only a few poor municipalities.

TABLE 9
SHARE OF HOUSEHOLDS CONNECTED TO WATER AND SEWERAGE IN 1992 AND IN 2002 BY INCOME QUINTILE

		Income Quintile				
	All	Poorest	II	III	IV	Wealthiest
	Share of Households Connected to Water					
1992	.74	.61	.71	.75	.77	.83
2002	.88	.82	.85	.88	.92	.91
Change 1992–2002	+.14	+.21	+.14	+.13	+.15	+.09
	Share of Households Connected to Sewerage					
1992	.54	.35	.47	.51	.56	.74
2002	.64	.51	.57	.60	.68	.79
Change 1992–2002	+.10	+.16	+.10	+.09	+.12	+.05

SOURCE.—World Bank (2002*a*).

province rose by 22 percent and the number of sewerage connections increased by 50 percent.

Finally, results from a recent World Bank (2002*a*) household survey confirm that network expansions during the privatization period were concentrated in the poorer income groups. The survey inquires about connections to water and sewerage services in 1992 (prior to almost all water privatizations) and again in 2002 (well after privatization). Table 9 reports the share of households connected to the water and sewerage networks in both years. Overall household connections to the water network increased by 14 percentage points and to the sewerage network by 10 percentage points. However, most of the increase came from households in the lower-income groups. Indeed, connections to the water network increased by 21 percentage points and to the sewerage network by 16 percentage points among households living in the poorest quintile of the income distribution (see fig. 4). Table 9 demonstrates that while the poor still suffer the lowest connection rates, they have had the largest gains in access after 1993.

V. Conclusions

During the 1990s Argentina launched a massive privatization program as part of a large plan of structural reforms. The program included the privatization of local water companies providing service to approximately 30 percent of the municipalities and 60 percent of the population. Available information from a number of case studies demonstrates that the newly privatized water firms were more efficient, invested more in physical infrastructure, and provided better service quality than their previous public incarnations. Indeed, our evidence on access to

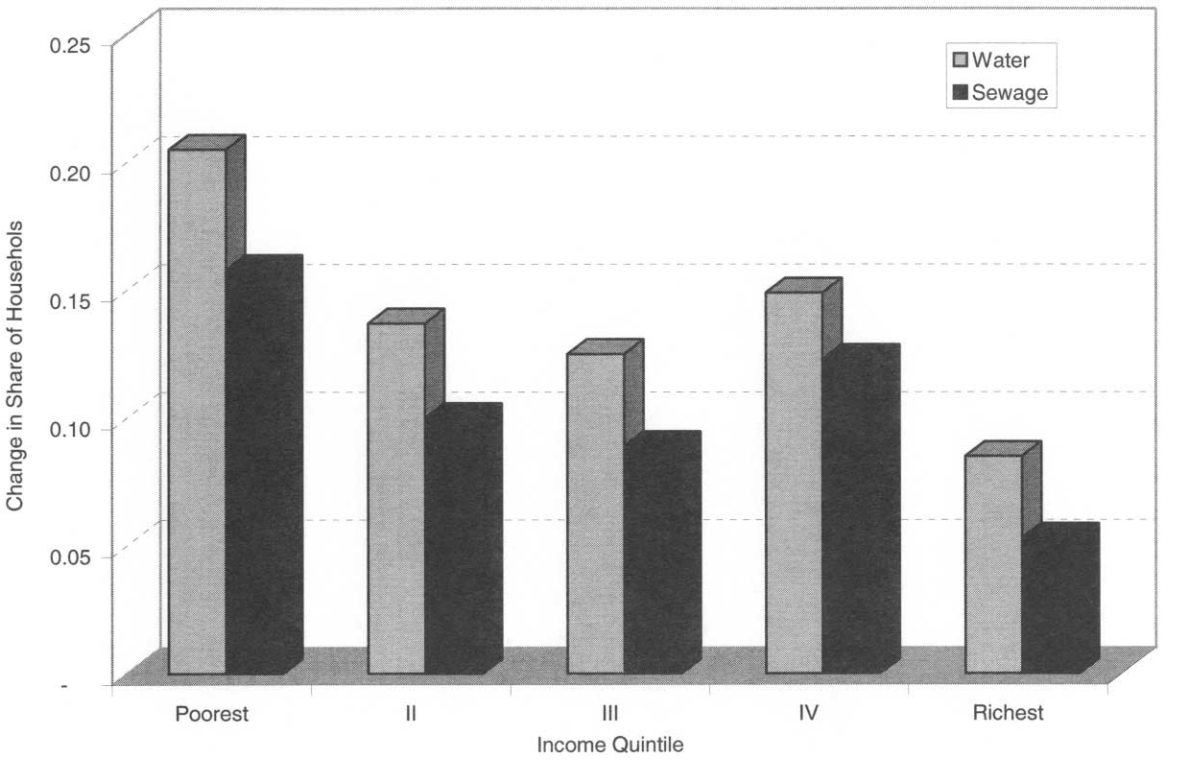

FIG. 4.—Change in share of households connected to water and sewerage, 1992–2002

service shows that the network connections increased significantly in the areas that privatized.

We hypothesized that increased access to the water and sanitation network, and potential changes in service quality, improved health outcomes of young children. Using a combination of methods, we find that child mortality fell by approximately 8 percent in the areas in which water systems were privatized. A number of factors lead us to believe that the link between the privatization of water systems and the decrease in child mortality is causal. First, privatization decisions across municipalities and time do not depend on time-varying variables that may also affect mortality rates. Second, the treatment and control groups showed similar trends in the preintervention period. Third, water privatization affected child mortality from water-related diseases, but it showed no effect on deaths from other causes. Fourth, the impact of privatization was largest in poorest areas.

Our results shed light on a number of important policy debates. First, while the previous privatization literature (including the case studies on Argentine water companies) demonstrates that private operation reduces costs and raises firms' productivity and profitability, it does not address the question of whether privatization actually increases social welfare. We show that privatization reduces child mortality, a direct and tangible welfare indicator.

Second, many fear that private operators would fail to take into account the significant health externalities that are present in this industry and therefore underinvest and supply suboptimal service quality. On the contrary, our evidence suggests that the deterioration in performance of water systems in Argentina under public management was so large that it allowed for a privatization that generated private profits, improved access, expanded service, and reduced child mortality. While the regulated private sector may not be providing first-best services, it seems to be doing a much better job than the public sector.

Finally, there is a growing public perception that privatization hurts the poor. This perception is driven by the belief that privatized companies raise prices, enforce service payment, and invest only in lucrative high-income areas. In contrast, we find that the poorest population experienced the largest gains from privatization in terms of reduction in child mortality. Privatization appears to have had a progressive effect on reducing health inequality.

Appendix

TABLE A1
VARIABLE DEFINITIONS AND SOURCES

Variable	Definition	Source
Child deaths	Number of deaths of children less than 5 years old by municipality by year by cause of death	Ministerio de Salud de la República Argentina
Child population	Number of children less than 5 years old by municipality by year; obtained by linear extrapolation from the 1991 census using the 1990–2000 INDEC (National Institute of Statistics and Census) estimates of total municipality population	INDEC, Censo Nacional de Población y Vivienda 1991; INDEC, Proyecciones de Población por Localidad, 1990–2000
Child mortality rate	Child deaths/child population	
Private water services	Dummy variable = 1 if the largest fraction of the population in the municipality is supplied by a private water company, and 0 otherwise	Sistema Permanente de Información de Saneamiento (SPIDES), Ente Nacional de Obras Hídricas de Saneamiento (ENOHSA: http://www.enohsa.gov.ar)
Real GDP per capita	Per capita gross geographic product in hundreds of constant pesos in the province in which the municipality is located	Consejo Federal de Inversiones and INDEC
Unemployment rate	Unemployment rate (May and October average) for households in the surveyed cities of the province in which the municipality is located	Permanent Household Survey INDEC
Income inequality	Gini index (May and October average) for households in the surveyed cities of the province in which the municipality is located	Permanent Household Survey INDEC
Public spending per capita	Public spending per capita in hundreds of constant pesos in the province in which the municipality is located	DataFiel and INDEC
Local government by Radical party	Dummy variable = 1 if the Union Civica Radical party governs province in which the municipality is located, and 0 otherwise	Ministerio de Interior de la República Argentina

Local government by Peronist party	Dummy variable = 1 if the Peronist party governs province in which the municipality is located, and 0 otherwise	Ministerio de Interior de la República Argentina
Federal government operates services	Dummy variable = 1 if the company providing water services depends on the federal government, and 0 otherwise	SPIDES, ENOHSA
Unemployment 1991	Municipality unemployment rate in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
Population	Total population in the municipality in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
Overcrowded housing	Fraction of municipality's households with an average of more than three people per room in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
No toilet	Fraction of municipality's households with no fecal evacuation system in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
Poor housing	Fraction of municipality's households living in poor housing in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
Below subsistence	Fraction of municipality's households with four or more members per working member and low household head education in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
UBN	Fraction of municipality's households with unmet basic needs (i.e., at least one of the following: overcrowded housing, no toilet, poor housing, or below subsistence) in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
No sewerage	Dummy variable = 1 if sewerage was not provided in municipality in 1991, and 0 otherwise	INDEC, Censo Nacional de Población y Vivienda 1991
Head education above high school	Fraction of households in which head has educational level above high school in 1991	INDEC, Censo Nacional de Población y Vivienda 1991
Household head age	Mean age of household heads in the municipality in 1991	INDEC, Censo Nacional de Población y Vivienda 1991.

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