NBER WORKING PAPER SERIES

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Working Paper 14365
http://www.nber.org/papers/w14365

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2008

We thank colleagues at IPEA, John Briscoe, Maureen Cropper, Sandy Caincross, Daniel Kammen, Margaret Kosek, Grant Miller, Anita Millman, Kara Nelson, Narayan Sastry, Werner Troesken, Tara Watson, seminar participants at University of Pittsburgh, Yale School of Forestry and Environmental Studies, AERE Health and Environment summer workshop, the Population Association of America and the Allied Social Science annual meetings. Errors are ours. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

The Impact of Piped Water Provision on Infant Mortality in Brazil: A Quantile Panel Data Approach
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NBER Working Paper No. 14365
September 2008
JEL No. H41,I18,Q53,Q56,Q58

ABSTRACT

We examine the impact of piped water on the under-1 infant mortality rate (IMR) in Brazil using a novel econometric procedure for the estimation of quantile treatment effects with panel data. The provision of piped water in Brazil is highly correlated with other observable and unobservable determinants of IMR -- the latter leading to an important source of bias. Instruments for piped water provision are not readily available, and fixed effects to control for time invariant correlated unobservables are invalid in the simple quantile regression framework. Using the quantile panel data procedure in Chen and Khan (2007), our estimates indicate that the provision of piped water reduces infant mortality by significantly more at the higher conditional quantiles of the IMR distribution than at the lower conditional quantiles (except for cases of extreme underdevelopment). These results imply that targeting piped water intervention toward areas in the upper quantiles of the conditional IMR distribution, when accompanied by other basic public health inputs, can achieve significantly greater reductions in infant mortality.

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1 Introduction

The Millennium Development Goals aim to reduce by two-thirds the under-five child mortality rate by 2015 from the base year 1990 (United Nations, 2005). In 2000, diarrhea caused approximately 22% of these deaths worldwide (Black et al, 2003).[1] About 1.5 million child deaths (or 88% of those from diarrhea) are caused by ingestion of unsafe water, inadequate availability of water for hygiene, and lack of access to sanitation (Black et al, 2003). A proposed strategy to achieve the Millennium Development Goals of reducing child mortality is to improve access to safe drinking water. Indeed, the Brazilian government has announced its goal to achieve universal coverage for piped water (World Bank, 2003). These proposals raise an important policy question – can provision from piped water from the network, hereafter "piped water", reduce the infant mortality rate (IMR).[2] For those populations at greatest risk (i.e. in areas that suffer severe infant mortality rates) can this provision reduce infant mortality rates, or is the provision of piped water effective only when accompanied by complementary income-related inputs at the household or community level?

In situations involving extreme inequality, it is possible for simple conditional mean estimates to mask the answers to these questions. Quantile estimation, which recovers the marginal impact of piped water on various quantiles of the conditional distribution of the IMR, can address this problem. Quantile regression is, however, not easily adaptable to dealing with problems of endogenous regressors. This presents a difficulty for most policy analyses, since policies are seldom applied randomly. When valid instruments are available, endogeneity can be addressed with instrumental variable quantile techniques (Abadie et al., 2002; Arias et al., 2001; Chernozhukov and Hansen, 2005, Khan

[1] These figures are for the 42 countries with 90% of the worldwide under-5 deaths in 2000 (Black et al., 2003).

[2] Our study is limited to the following measure: percentage of households that receive piped water from a network. We do not have data on the quality of that piped water. We also do not have data on the type of connection from the network to the home (i.e., whether through plumbing internal to the house or through standpipes external to the house). Furthermore, we do not evaluate the effectiveness of piped water interventions relative to other water-related interventions. Mintz et al. (2001) argue that "decentralized approaches to making drinking water safe, including point-of-use chemical and solar disinfection, safe water storage, and behavioral change merit far greater priority for rapid implementation."

and Tamer, 2008). The practical problem is that there are often no good instruments for many policies. The usual statistical approach in mean regression is to exploit panel variation and estimate fixed effects to control for time invariant sources of correlated errors. However, this approach is not applicable using standard quantile techniques.[3]

Using a new approach to quantile regression with panel data developed by Chen and Khan (2007), we examine the impact of provision of piped water on the under-1 infant mortality rate at various quantiles of the conditional IMR distribution using panel data for 3568 census units in all Brazil. We describe the effect of the treatment on various quantiles of the outcome distribution, making no assumption about the joint distribution of the treated and untreated distributions. Our interpretation follows that of Abrevaya (2001) and Bitler et al., (2005).

We find that an increase of one percentage point in the number of households receiving piped water in the group of counties with poor development indicators in the period 1980-1991 causes a decline of 1.25 deaths per 1,000 live births at the 90th percentile of the conditional IMR, but a decline of only 0.54 deaths at the 10th percentile.[4] The marginal effect at the mean (i.e., 0.72 deaths per 1,000 live births) turns out to provide a poor indication of the effect of water on much of the IMR distribution. The most important implication of this result is that the impact of a piped water provision policy is determined in large part by how those piped water connections are distributed. There is tremendous payoff to targeting water provision to the areas with the highest IMR (both conditional and unconditional). In practice, however, piped water interventions have tended to be in places with good indicators of development and which are low in the conditional IMR distribution.

Our paper makes two methodological contributions to the program evaluation literature in developing countries. First, by using novel quantile

[3] Differenced regression cannot be applied in the quantile regression context, and simple fixed effects estimation suffers from incidental parameters bias unless the panel is very long in the time dimension.

[4] These indicators are described in Section 4 and Section 5.2.

techniques, we examine whether the provision of piped water can reduce infant mortality in the upper tail of the conditional IMR distribution. A priori, it is unclear whether the provision of piped water, without sufficient complementary health inputs, will yield a reduction in IMR at these quantiles. Previous studies' focus on the impact at the mean of the conditional distribution may obscure this policy-relevant heterogeneity.

Second, by applying panel data techniques to quantile estimation, we can estimate the impact of piped water on IMR while controlling for potential time invariant confounders. Areas with fewer piped water connections are also high IMR areas. These areas may suffer from systematic underreporting of infant deaths (Victora and Barros, 2001). At the same time, areas with more piped water connections are likely to benefit from other superior health inputs (these inputs are unobservables in our study, such as access to medical care, nutritional supplements, and public health infrastructure.) (Jalan and Ravallion, 2003; Weinreb, 2001). Our estimates will not suffer from the downward bias arising from the systematic underreporting of deaths or the upward bias arising from these time invariant inputs.[5] At present, only a few, albeit important papers, have applied quantile regression to program evaluation in developing countries (Djebbari and Smith, 2005) and fewer still have applied strategies to address time invariant confounders within the context of quantile regressions.

While instrumental variables may not always be available, the proliferation of quality panel data means that our methodological approach can be widely applied to the evaluation of other programs that provide health inputs or other public goods in developing countries. Our task of evaluating the impact of piped water on IMR shares two key characteristics with the evaluation of programs that provide health inputs in developing countries, such as the provision of nutritional supplements or medical assistance to populations at risk. First, from the policy perspective, it is important to understand the impact of these programs on the subpopulations that are most at risk; if unobservables are important determinants

[5] The quantile panel data technique we employ, like other fixed effect models, cannot correct the bias arising from time varying unobservables (Ahn, Lee, and Schmidt, 2001).

of the outcome variable, these subpopulations will tend to occupy the tails of the conditional outcome distribution. Mean impacts will fail to capture heterogeneous impacts across the conditional distribution. Second, the evaluation of these programs is complicated by their systematic placement in areas that receive superior health inputs. If these inputs are unobserved by the econometrician, they will cause an upward bias in the measurement of positive program impacts. At the same time, the systematic underreporting of outcome variables (e.g., mortality in higher mortality areas) (Victora and Barros, 2001), may attenuate the relationship between health inputs and mortality.

2 Piped water and infant mortality in Brazil

2.1 Infant mortality

Piped water supply reduces infant mortality directly by reducing the incidence of diarrhea that arises from the ingestion of contaminated water and food, and indirectly when caregivers are able to devote more time to childcare instead of water collection activities. Brazil serves as a case study for the impact of piped water on infant mortality for three reasons. First, diarrheal diseases are an important cause of infant mortality, accounting for 8% of infant death in Brazil in 1995-7 (Victora, 2001). In Northeast Brazil, the poorest area in the country, diarrhea accounted for 15% of infant mortality (Victora, 2001). Second, under-1 infants in Brazil are susceptible to water-borne diseases due to the relatively short duration of breastfeeding (Sastry and Burgard, 2005). Diarrhea is likely to increase when the infant is first exposed to supplemental liquids or solids, usually at ages below 1 year old (Sastry and Burgard, 2005). The 1989 Brazilian National Health and Nutrition Survey indicates that only 29.5% of infants aged 0-5 months were exclusively breastfed and 36.3% of those aged 0-23 months were breastfed (Senauer and Kassouf, 2000). In 1996, the Brazil-wide estimate of the duration for breastfeeding (both exclusive and supplemental) was 8.2 months

(Sastry and Burgard, 2005).[6] Third, our results from Brazil, particularly the Northeast, are potentially transferable to other developing countries.

2.2 Piped water provision: institutions and policies

During our study period, Brazil experienced three distinct regimes in piped water provision – the pre-1971, the 1971-1991, and the post-1991 periods. Prior to 1971, the responsibility for piped water provision rested with municipalities or counties.[7] In 1971, the federal government launched the National Sanitation Plan (PLANASA) with the goal of achieving universal supply of piped water in all urban areas. The state governments created state water companies (CESBs) and state water and sanitation funds (FAEs). Most municipalities signed 20-30 year concession contracts with the CESBs, transferring responsibilities for the extraction, treatment and distribution of piped water to latter. Under PLANASA, only municipalities with such contracts could gain access to federal funding (ERM, 2003).

During PLANASA's operation between 1971 and 1991, investments in water and sanitation amounted to US$13.6 billion. The Employment Guarantee Fund (FGTS) financed 60% of the total investments, the FAEs and CESBs financed 29% and the federal budget and other sources funded the rest. The National Housing Bank (BNH), which managed PLANASA's operations, and the CESBs borrowed from international development banks (ERM, 2003).[8] The CESBs charged tariffs to users, allowing cross-subsidies between high- and low-income users (McNallen, 2006). PLANASA prioritized water services over sewerage and targeted larger and fast growing cities and metropolitan regions.[9] Rural areas received little service as most contracts between municipalities and the CESBs were restricted to urban areas, while rural villages remained under

[6] The average duration of breastfeeding did not differ dramatically between the Northeast and the rest of Brazil (Sastry and Burgard, 2005).
[7] We use the terms municipality or county, interchangeably, to correspond to the Brazilian-term "municipios"
[8] The FTGS is financed by taxes on employers (ERM, 2005).
[9] PLANASA invested 68% of funds into water and only 32% into sanitation.

municipal responsibility. Many municipalities, however, lacked the resources to provide piped water supply (ERM, 2003).

Brazil's economic upheavals in the 1980s contributed to PLANASA's demise in 1991. The CESBs, FTGS, FAEs and BNH all faced financial problems. PLANASA's operational functions were transferred to another federal bank, the National Economic Bank (CEF) (ERM, 2003). With the expiry of contracts signed under PLANASA, many municipalities are seeking to regain control of the water services (ERM, 2003). Several states and municipalities are locked in dispute over the asset ownership, regulatory authority, and concessionary powers (World Bank, 2003). The disputes have discouraged private investments (World Bank, 2003). Several of the more prosperous municipalities have opted to sign concessions with providers other than the CESBs. These municipalities have ended cross-subsidization, contributing to service deterioration in the poorest municipalities (ERM, 2005).

The Brazilian government has not implemented a consistent piped water program to replace PLANASA.[10] The debate at the federal level on proposed 2001 legislation for a comprehensive approach to water management ended in a stalemate (ERM, 2003). Today, the relative roles of the various federal institutions in the water sector remain poorly defined and uncoordinated[11] (World Bank, 2003).

Brazil's funding criteria for water investments have not consistently prioritized the poor (World Bank, 2003). PROSANEAR is one of the few projects that target water and sanitation services to the urban poor.[12] That project extended the existing public water network into poor urban neighborhoods. Between 1992-7, PROSANEAR I provided piped water to 0.9 million people and sewage facilities to 1 million people in 17 cities. Water agencies cross-

[10] PRONURB (the Sanitation Program for Urban Settlements) and PROSANITATION operated between 1990-4 and 1995-8, respectively (Garrido, 2006).

[11] The federal institutions include the Special Secretariat for Urban Development (SEDU), The Federal Economic Bank (CEF), the National Bank for Economic and Social Development (BNDES), the National Water Agency (ANA), the Ministry of Planning and Budget, the Ministry of Finance, and the National Health Foundation (FUNASA) (World Bank, 2003).

[12] The PROSANEAR I projects were financed by a World Bank loan, local water companies, state or municipal governments, and the CEF.

subsidized the low PROSANEAR tariff with revenues from their other customers and received through direct subsidies from the local governments. Two subsequent projects, PROSANEAR II and PASS/Comunidade Solidária, also target low income communities (Katakura and Bakalian, 1998). In addition, the Ministry of Health manages Program Alvorado, a social program with water supply and sanitation components (World Bank, 2003).

In the year 2000, out of the 5507 municipalities, about 3706 municipalities received piped water supply from state water companies and about 1676 municipalities received their supply from a mix of municipal water providers, private concessionaires, private and social organizations. In the remaining 2% of municipalities, water was supplied through standpipes and water tankers (in the small rural villages of the semi-arid area of the Northeast Region) and, through the use of private wells and individual extraction from surface water in the rest of the country" (ERM, 2003).

2.3 Patterns of piped water provision (1970-2000)

The provision of piped water by regions is tabulated in Table 1A. Specifically for this table, we classify counties as urban in a given year if 50% or more of their population live in urban areas. As evident from Table 1A, Brazil's policies have resulted in superior piped water coverage in urban counties situated in the more prosperous regions i.e., the Southeast and the South, but poor coverage in other less prosperous regions, i.e., the Northeast and the North. Coverage lags in rural counties across all regions, but is especially scant in the North and Northeast. In the 1970s, piped water coverage in the urban counties was low in the Southeast (51%), extremely low in the South (27%) and sparse the Center-West, North and Northeast (15-20%). Between 1970 and 1991, piped water coverage in urban areas grew to moderate levels in the Southeast and South (62-66%), to low levels in the Center-West and Northeast (39-44%), but remained extremely low in the North (27%). By 2000, coverage has grown to high levels in the South and Southeast (74-77%) and to moderate levels in the Center-West and Northeast (62-69%), while coverage lagged in the North (50%).

Piped water coverage in rural counties has lagged behind that in urban counties. In rural counties in the Southeast, South, and Center-West, piped water coverage grew from scant levels in 1971 (4-20%) to moderate levels (44-48%) by 2000. In contrast, the coverage in the rural counties in the North and Northeast, which has been sparse even as late as 1991 (13% and 19%), remains low in 2000 (31% and 42%).

3 Econometric issues in estimating the impact of piped water on IMR

3.1 Marginal effects of piped water – mean versus quantiles

We use quantile techniques to recover the marginal impact of piped water on various parts of the conditional IMR distribution. In contrast, previous studies of piped water have focused on the conditional mean of that distribution (Sastry, 1996; Merrick, 1985; Jalan and Ravallion, 2003). Only under the assumption that the marginal effect of piped water is a simple "common effect" or "location shift" will the impact at the mean be the same as the impact for the entire distribution (Heckman et al., 1997; Abadie et al., 2002). In other words, under the “common effect” assumption, the piped water intervention has the same impact on everyone with the same observed characteristics (Heckman et al., 1997).

Papers on health inputs have shown that estimates at the mean may obscure heterogeneous impacts at the various quantiles of the conditional distribution. Moreover, the heterogeneity in the conditional distribution of the outcome variable is relevant for public policy. For example, Abrevaya (2001) finds that prenatal care in the US has a significantly higher impact at lower quantiles of the conditional distribution of infant birthweight than at the higher quantiles. Moreover, he finds that the black-white differential in birthweight is larger at the lower conditional quantiles of birthweight.

Heterogeneity in the impact of piped water is relevant for policy decisions regarding piped water placement. On the one hand, targeting piped water to vulnerable households may improve their welfare significantly. Households or communities with low income typically have the fewest public resources for children's health.[13] In such cases, we would expect piped water to have greater protective effect among households or communities with lower incomes. On the other hand, targeting piped water to vulnerable households may be necessary but not sufficient to improve their welfare. In particular, their limited income or education may constrain their ability to exploit the benefits from piped water supply. In that case, water supply placement would need to be accompanied by other interventions (Jalan and Ravallion, 2003).

In exploring the impact of piped water on IMR, our study explores two types of heterogeneity that call for distinct policy responses: (1) heterogeneity along observable dimensions such as income and (2) heterogeneity due to unobserved factors. The policy response for the first type of heterogeneity is to target along observables such as income and education. The policy response to the second type of heterogeneity is more challenging. It would not be sufficient to simply consider income, education, and sewage in defining "vulnerable populations". Instead, in their task of allocating water, policy-makers need to look for other factors (i.e., unobserved factors in our analysis) that make IMR high. In this paper, we seek to also determine the return to targeting these unobservables in the placement of piped water, and we describe how such targeting might be accomplished.

The first type of heterogeneity can be explored by standard techniques in the literature i.e., by allowing the marginal impact of water to vary by the income variable. However, a mean regression with interaction terms would not address the second type of heterogeneity. In contrast, quantile techniques allow us to explore the second type of heterogeneity. A priori, it is unclear whether the marginal effect of water is greater in higher or lower income communities. Similarly, a priori, it is unclear whether, controlling for observables such as income, education and sewage network, the marginal effect of water is greater at higher or lower percentiles of the conditional distribution of the IMR.

[13] Thomas and Strauss (1992) make this argument for maternal education.

Previous studies suggest a complex relationship between health status, water supply and socio-economic status.[14] Shuval et al. (1981) propose a four-stage threshold-saturation model to explain the relationship between health status, water supply and socioeconomic levels reported in several empirical studies with seemingly contradictory results.[15] Shuval et al. (1981) propose that at the first stage, i.e., below a threshold of socioeconomic development, the provision of water does little to improve the health status of the community. Individuals have low disease resistance due to their extremely poor nutrition and personal hygiene and their exposure to multiple and simultaneous routes of disease transmissions. The provision of water alone, which addresses only one route of disease transmission, does not have a strong impact on health. Shuval et al.'s (1981) argument echoes that of Briscoe (1984a) – i.e., the improvements in drinking water supply in Matlab, Bangladesh did not cause major reductions in cholera incidence because complementary interventions were not undertaken to eliminate other important, albeit secondary, routes of cholera transmission (e.g., the ingestion of polluted water during bathing). Similarly, Esrey et al. (1992) find that water supply had a significant health impact only when accompanied by the presence of latrines in their study of infants in Lesotho.[16]

At the second stage, above that threshold but below the saturation point, socioeconomic development improves the standard of living and reduces the exposure to infection (Shuval et al., 1981). At this level of socioeconomic

[14] Consider, for example, hygiene behavior, which our study does not explicitly include but which can influence whether the provision of water supply translates to health benefits. It is likely that the provision of water supply encourages the adoption of hygienic behaviors. Cairncross (1990) argues that the provision of water leads to health impacts only when accompanied by the adoption of hygienic behavior. Citing Esrey et al. (1985), Cairncross (2003) argues that handwashing thus turns out to have an even greater impact on diarrheal disease than water supply or sanitation. Nevertheless, as noted by Cairncross (2003), "a convenient water supply makes handwashing easier to practice and hence more likely. Indeed, it has been confirmed by observation in developing countries that mothers of young children are more likely to wash their hands at critical moments if they have a piped water supply (Curtis et al., 1995)."

[15] Shuval et al. (1981) use country-level data for 65 developing countries from 1962. Life expectancy at birth measures health status, adult literacy rate measures socioeconomic status, and the proportion of the urban population having access to water supply by either household tap or standpipe measures the sanitation level. Shuval et al. (1981) reports that these data, though imperfect, are consistent with their model.

[16] Esrey et al. (1992) examine 119 infants who lived in 20 villages in Lesotho from a 6 month period in 1984-1985.

development, communities have a strong health response to investments in water supply. At the third stage, as communities develop further, they move towards a saturation point, whereby improvements in water supply have only a small impact on health. At the fourth stage, beyond the saturation point, communities have reached high levels of socioeconomic development. Improvements to water supply would not cause further improvements in health status (Shuval et al., 1981). The practical problem in testing this theory is that it is not clear what are all the variables that should be used to define "socioeconomic development". Our quantile approach allows us to measure the sensitivity of IMR to determinants of development not explicitly included in the analysis.

Previous studies show diverging results on the interaction between piped water and income. Whether piped water serves as a complement or substitute to household and community inputs may be specific to the level of income and education and overall institutional environment. In their study of 33,000 rural households in India in 1993-1994, Jalan and Ravallion (2003) find that while piped water did cause an overall reduction in diarrheal incidence, households in the bottom 40 percent of the income distribution did not experience significant health gains.[17] In their study of Brazil in 1974-5, Thomas and Strauss (1992) find that children in high income urban households benefit more from the availability of sewerage services and electricity. In contrast, several studies report that households' input and public infrastructure serve as substitutes. Thomas et al. (1991) find that children of uneducated mothers gained most from sewage networks in Northeast Brazil. Barrera (1990a) finds that children of less educated mothers in the Bicol region of the Phillipines benefit more from water connections and the absence of excreta in the environment.[18]

[17] Jalan and Ravallion (2003) argue that "policymakers trying to reach children of poor families – who are typically the most prone to disease – will need to do more than making facility placement pro-poor. The incidence of health gains need not favor children from poor families even when the placement favors the poor."

3.2 Selective placement of piped water

Studies relating water supply to health that fail to control for the selective placement of water supply would likely overstate the protective effect of water (Zwane and Kremer, 2007). Piped water is likely to be placed in areas that enjoy superior medical care provision, and where higher incomes are used to purchase other health-related inputs (Jalan and Ravallion, 2003; Weinreb, 2001). Both represent factors that contribute to low IMR (Rosenzweig and Wolpin, 1986). Our data indicate that piped water in Brazil is systematically placed in areas with superior observables. The correlation between the water and income variables is 0.71, 0.73, 0.78, and 0.61 and that between the water and education variables is 0.60, 0.63, 0.71, and 0.58, in 1970, 1980, 1991 and 2000, respectively.[19] It is therefore likely that the placement of piped water is also correlated with unobservable determinants of infant health.

To overcome the estimation problem of selective program placement, studies from developed countries have exploited the exogenous timing of water-related interventions to identify their impact. Troesken (2001) finds that municipal water provision in American cities around the early 20th century reduced typhoid rates in blacks. Cutler and Miller (2005) report that chlorination interventions in 19 US cities reduced infant mortality. Watson (2006) finds that a ten percentage point increase in the fraction of homes in American Indian reservations with sanitation improvements reduced infant mortality by 0.51 deaths per 1000 births.

However, few studies from developing countries (even those that focus on mean results) have been able to correct for non-random program placement. In their study of Bangladeshi and Filipino villages, Lee et al. (1997) correct for the selection bias stemming from conditioning on surviving children, but take the placement of piped water as given. In their Brazilian studies, Sastry (1996) and Merrick (1985) report positive association between piped water supply and infant

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[18] In the same study, Barrera (1990a) finds that children of more educated mothers derive greater benefits from health care facilities and toilet connections. Thomas et al. (1991) find that children of uneducated mothers gained least from health care facilities.

[19] These variables are defined in section 5.1.

mortality, but are not able to address the issue of program placement.[20] The study by Jalan and Ravallion (2003) uses propensity score matching techniques as a strategy to correct for the selective placement of piped water among rural Indian households in 1991. Comparing households with and without piped water, but which are similar on observable dimensions (and, by assumption, on unobservable dimensions), they find that the incidence of diarrheal diseases is higher in households without piped water. In contrast, studies testing the impact of point-of-use water treatment have been able to implement randomized trials. Clasen et al. (2004), Conroy et al. (1996), and Crump et al. (2005) find positive health impacts from the use ceramic filters, solar disinfection and chemical disinfectants, respectively. Kremer et al. (2007), using randomized trials to evaluate the impact of protecting naturally occurring spring water, find that spring protection improves source water quality and that reported child diarrhea incidence falls by one quarter.

3.3 Measurement error in IMR

Measurement error in the IMR poses a second problem in studies that investigate the relationship between health inputs and infant mortality, albeit the direction of bias is opposite to that discussed above for program placement. In particular, places that suffer from high infant mortality rates (and exhibit low rates of piped water provision) may suffer more severe under-reporting of those infant mortality rates. Even if there were an underlying negative relationship between the presence of piped water and infant mortality, the underreporting bias may conceal such a relationship. In the case of Brazil, Victora and Barros (2001), citing Simões (1999), note that under-reporting of infant deaths in the Northeast

[20] Merrick (1985) uses 1976 cross-sectional in Brazil data to estimate a structural model relating infant mortality to factors such as household-level access to piped water, state-level piped water supply, maternal and paternal education, and income. Merrick (1985) obtained piped water supply data from the 1970 Census that divided Brazil into 117 geographical units. In order to match the data to the Pesquisa Nacional Amostra de Domicilios (PNAD) household data, he was forced to aggregate the variable up to 25 observations corresponding to 25 states.

(where provision of piped water is low) is about 66.7%, while the under-reporting in the Southeast (where provision of piped water is high) is only 6.5%.[21]

3.4 Strategies to address non-random program placement and measurement error

To overcome the issue of non-random program placement, quantile studies from developed countries have been able to rely on experimental design such as in the evaluation of welfare reform or job training programs (Bitler et al., 2005 and 2006) or instrumental variables such as in evaluating the impact of childbearing on income[22] (Abadie et al., 2002), the returns to education (Arias et al., 2001), returns to job training programs (Chernozhukov and Hansen, 2005). In contrast, only a few quantile studies from developing countries have been able to rely on experimental design or instrumental variables. Djebbari and Smith (2005) use random assignment experimental data to examine the distributional impact of Mexico's program of education, health and nutrition (PROGRESA). They find that the program had a smaller impact on wealth and nutrition for households in the lower tail of the wealth and nutrition distribution.

A few studies, looking only at developed countries, have begun to explore the use of panel data in the context of quantile regressions. For example, Abrevaya and Dahl (2006) examine the impact of prenatal care and smoking on infant birthweight using panel data on maternally-linked births. They assume a correlated random effects model as is done here and in Chen and Khan (2007), but also impose the additional restriction of a linear structure on the individual specific effect. In this and other important policy contexts, randomized placements and instrumental variables are not readily available. In these situations, the use of panel techniques has the potential to correct the estimation bias from selective placement and systematic measurement error.

[21] The under-registration of infant deaths is estimated to be 52.2% in the North, 13.6% in the South, 23.9% in the Center-West, and 43.7% nationally. Most deaths that are not registered occur in the rural areas of the North and Northeast where rates of infectious diseases are higher. (Victora and Barros, 2001 citing Simões, 1999)

[22] "Childbearing reduces the lower tail of the income distribution considerably more than other parts of the income distribution." (Abadie et al., 2002).

3.5 Why means mask quantile results?

In this section, we illustrate why estimating mean effects can be significantly different from estimating quantile effects. To keep the discussion simple, we focus on the cross-sectional case. In particular, consider the linearly heteroskedastic model: $y_i = \beta_0 + x_i\beta_1 + x_i\Psi\varepsilon_i$, where y_i measures the infant mortality rate in county i and x_i measures the percentage of households there with access to piped water. We assume for this discussion that ε_i is independent of x_i (although relaxing this assumption with panel data is a major focus of the rest of the paper). Let μ_ε denote the mean of ε_i (i.e., zero) and let ρ_θ denote the θ^{th} quantile of the ε_i distribution. The variance of ε_i is one.

The marginal effect associated with the conditional mean function (which would be estimated were we to use simple OLS) is of the form ($\beta_1+\Psi\mu_\varepsilon = \beta_1$), whereas the marginal effect associated with the θ^{th} quantile is ($\beta_1+\Psi\rho_\theta$). The differences between these two measures will generally depend upon the skewness of the distribution of ε_i. For example, if Ψ is positive and the distribution of ε_i is skewed toward the right, then the marginal effect of x_i associated with the mean will exceed that associated with the median and the lower quantiles. On the other hand, if the distribution is skewed toward the left, the reverse will be true – marginal effects associated with the median and higher quantiles will exceed the marginal effect attained from OLS.

4 Data

We use newly available census data published by the Brazilian Institute for Economic Analysis (IPEA). These data are reported at the level of minimally comparable areas (MCA's) for the years 1970, 1980, 1991 and 2000. Previously, census data were available at the *municipio* or county level, which is the smallest political division in Brazil (Alves and Beluzzo, 2004). Changes in county boundaries between the decades had limited the comparability of the census data. To overcome this limitation, IPEA created the MCA dataset, in which geographical units share common boundaries across the decades. The MCA boundaries correspond to county boundaries for those counties whose borders did not change between 1970 and 2000. For those counties that changed their borders between 1970 and 2000, neighboring counties were dissolved into one larger MCA. Data from households were then aggregated up to the MCA level for 1970, 1980, 1991 and 2000.

The MCA dataset divides Brazil into 3568 MCAs, a number which compares favorably with the 4500 counties in Brazil in 1998 (Mobarak et al., 2004) and 5560 in 2000 (Alves and Beluzzo, 2004).[23] While the MCA dataset is imperfect in that it sometimes aggregates several counties which may differ in their policy and institutional context, we believe that this dataset represents the best demographic panel dataset currently available for Brazil. The finer resolution of the MCA data relative to other available Brazilian panel census data lessens the degree of within unit heterogeneity.[24]

Table 1B presents summary statistics. The mean infant mortality rate declined from 125 deaths per 1000 live births in 1970, to 87 deaths in 1980, to 49 deaths in 1991 and to 34 deaths in 2000. At the same time, we see improvements in other development indicators. The percentage of households with piped water has increased fourfold from a mere 15% in 1970 to 62% by 2000. The percentage of households connected to the sewage network, starting from a lower baseline of 5% in 1970, has increased six-fold to 29% by 2000. Total fertility rate has more than halved from 5.9 births in 1970 to 2.8 births by 2000. Both the income-related Human Development Index and the education-related Human Development index show improvement between 1970 and 2000.

[23] In the 1980s, Brazil had 4088 municipalities, with an average population of 29,800 and an average area of 2118 km^2 (Sastry, 1996). We drop one observation in our analysis because of missing values.

[24] Potter et al. (2002) use the previous version of decennial data (terminating in 1991) that divides Brazil into 518 microregions. Another data source, the PNAD, suffers from municipio boundaries that are not consistent from one survey to another.

5 Method

5.1 Estimation

Our dependent variable is the number of deaths of infants under one year of age per thousand live births. Reviews of active surveillance of developing areas and of studies published between 1990 and 2000 indicate that the under-1 age-group experience the highest diarrhea specific mortality rates (Kosek et al, 2003). Our analysis focuses on all-cause infant mortality. Brazilian vital statistics data (except when the information is specifically collected by researchers) are notoriously unreliable on cause-specific deaths, and the unreliability is worse in high mortality areas (Sastry and Burgard, 2002). By focusing on infant mortality, we avoid the potential bias inherent in studies that examine child health. Studies that use child health (e.g., height-for-weight scores) need to correct for the selection on surviving children in order to avoid underestimating the overall impact of piped water on child health (Lee et al., 1997).[25] We interpret the coefficient on piped water to capture the impact of piped water on infant mortality, typically through reduced risk of death from diarrheal diseases.

Our study is limited to the analysis of one aspect of the quantity of piped water. The definition for the water variable is the percentage of households with piped water from the general network.[26] As in Sastry (1996) we focus on households' source of water, i.e. from the network, and not on the type of connection. Our focus on this variable has two limitations. First, we are not able to provide separate estimates for piped water delivered to the household through external plumbing (e.g. communal standpipes) and for piped water delivered to the household through internal plumbing.[27] Second, we do not study the quality of piped water.[28]

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[25] Child health data from the PNAD, 1996 Demographic and Health Survey and 1989 the Brazilian Health and Nutrition fail to provide municipal-level information. Cause-specific vital statistics data are not publicly available for all of Brazil.

[26] The IPEA definition is "numero domicilios com água canalizada de rede geral". We divide this figure by the total number of households in that MCA.

[27] Data on water quality and the type of connection is unavailable at the MCA-level. Our study therefore cannot overcome an important limitation in current studies. As noted in Fewtrell et al. (2005). "there are currently too few data to disentangle satisfactorily the role of service level (i.e., community *versus* household connection) and the health effect of water supply interventions." The type of connection may influence the health effects of piped water provision. Victora et al.

In addition to piped water, we include several covariates to account for other time-varying factors that influence the IMR. Income-based Human Development Index (which we refer to simply as "income") is added as a covariate as higher income levels are associated with improved chances for child survival[29] (Sastry, 1996 citing Merrick 1985, Thomas et al., 1990 and Victora et al., 1986).

Our regression model should control for improved sanitation,[30] as the latter influences infant mortality rates (Habicht et al., 1988). We explicitly include in our model one type of improved sanitation – i.e., the percentage of households with network sewage.[31] This type of sanitation is considered the only adequate kind in urban areas (UNICEF, 1997). Network sewage is the predominant

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(1988) find in their study in the metropolitan area of Southern Brazil that "compared to those with water piped to their house, those with piped water to their plot but not to their house are 1.5 times more likely to suffer infant death from diarrhea (95% confidence interval 0.8 to 3.0) and those without easy access to piped water had 4.8 times greater risk (95% confidence interval 1.7 to 13.8). Esrey and Habicht (1986) note that "in the studies reporting health benefits, the water was piped into or near the home, whereas in those studies reporting no benefit, the improved water supplies were protected wells, tube wells or standpipes." While the absence of data on the type of connection is a limitation to our study, it is useful to note that Sastry (1996), in his study of Brazil, reports that infant mortality levels in his data are more strongly correlated with the source of water (water from the general network versus other sources) than the type of water connection (internal or external plumbing). Sastry then confines his study the analysis of piped water from the network, regardless of the type of connection.

[28] While several point of use studies, such as Kremer et al. (2007), have been able to collect data on water quality and water quantity, others have not been able to separate the two impacts. For example, Esrey (1996) reports that although health benefits from optimal water service (piped water) were found, it was not known whether these benefits were due to improvement in the quality of water, usage of more copious quantities of water, both factors, or some other mechanism. The few studies that have been able to separate the influence of the quality and quantity of water find that water quantity has a greater impact than water quality on health and mortality (Sastry, 1996; citing Bourne, 1984, Esrey and Habicht, 1988, and Victora et al, 1988). In another study, Esrey et al (1991) note that "interventions to improve excreta disposal and water quantity, which are associated with better hygiene practices, produce greater impacts than improvements in water quality, …particularly so, in highly contaminated environments where diarrhea rates are high." However, more recent studies, such as Fewtrell et al (2005) and Clasen et al (2008), argue that water quality interventions at the household level are as effective as improvements in sanitation and water supply in reducing the incidence of diarrhea.

[29] The definition for HDI_income = ln (observed value of RFPC) – ln (lower limit of RFPC) / [ln (upper limit of RFPC)-ln(lower limit of RFPC) where RFPC is the family per capita income.

[30] Improved sanitation is defined by the World Health Organization as connection to a public sewer, connection to a septic system, a pour-flush latrine, a simple pit latrine or a ventilated improved pit latrine (UN Millennium Project, 2005)

[31] The IPEA definition is "numero domicilios instalações sanitárias de rede geral". We divide this figure by the total number of households in that MCA. Victora et al. (1986) and Victora et al. (1988), both cited in Sastry (1996), find that household toilet facilities are related very weakly to child mortality risks. instalaciones

method of improved sanitation in urban areas (WHO/UNICEF, 2000). We rely on the panel method of our analysis to control for cross-sectional variation in other types of improved sanitation in Brazil. Nevertheless, the panel method can adequately control for the cross-section variation in improved sanitation other than network sewage only to the extent that that variation is fairly constant over the decade. In rural areas, an estimated one-fifth to one-half of households use basic latrines (WHO/UNICEF, 2000). Use of pit latrines, while not as effective as the sewage network, is correlated with some declines in morbidity (Esrey et al, 1991). To address the shortcoming in sanitation data for rural areas, we repeat our regression analysis with the urban only sample.

Maternal education, by improving mother's access to health-related information and her ability to make better use of health inputs, influences the reduction in the infant mortality rate. (Sastry, 1996 citing Barrera (1990a), Rosensweig and Schultz (1982), and Thomas et al. (1991)) In the absence of women-specific education or literacy data, we use the education-based Human Development Index (which we refer to simply as "education") variable. The education variable has been constructed by IPEA from a 2:1 weighting of the index for literacy rate and the index for school attendance rate.[32] As seen in Table 1C, while men's and women's literacy rates are positively correlated, one limitation in using the non-gender specific education variable proxy variable is the presence of some regional variation in the gap between men's and women's literacy rates.

The total fertility rate is used a co-variate in order to control for the association between fertility and IMR (Barnum, 1988).[33] The total fertility rate is a measure that summarizes the rate of childbearing in a year. It is derived by summing the age-specific birth rates for a population of women in a given period.

[32] The HDI_education variable includes current schooling, which captures MCA-level investment in education of children. The index of literacy rate or the index of school attendance rate = (observed rate – minimum rate) / (maximum rate – minimum rate).
[33] We use this variable to control for the association between higher fertility and higher IMR. We do not aim to explain the relationship between them. The association could be due to high fertility households having fewer inputs per child or due to households choosing to have more children when faced with higher infant mortality rates.

That variable is available at the county-level for 1991 and 2000 only and at the region level for 1980 and 1970.

Finally, our panel data procedure controls for county-specific time-invariant unobservables. One such time-invariant characteristic that influences infant mortality is the climate – greater seasonality in temperature and precipitation is associated with greater infant mortality from infectious diseases (Sastry, 1996).[34] Some variables that vary in the cross-section and that we aim to control for using the panel procedure (e.g., access to healthcare and breastfeeding behavior), are not strictly speaking time-invariant. There are, however, no county-level data maintained on breastfeeding behavior. While there are data available that describe the number of doctors, nurses, and hospitals at the county level, we found that these variables had no explanatory power after controlling for the county effect, α_i.

The basic panel data model to be estimated is of the form:

$$(1) \qquad y_{i,t} = \alpha_i + x'_{i,t}\beta + \varepsilon_{i,t} \qquad t = 1, 2$$

where $y_{i,t}$ denotes the under-1 infant mortality rate in county i and year t, defined as the number of deaths for every 1000 live births before the end of the first year. $x_{i,t}$ includes the percentage of households with piped water from the network, the percentage of households with sewerage connection, the income variable, the education variable, and the interaction between income and the water supply variable.[35]

α_i denotes the (unobserved) county effect, which controls for time-invariant sources of unobserved heterogeneity. Without this control, we would expect piped water to be correlated with the error in (1), leading to biased

[34]Victora et al. (1996) report than ORT played a larger role than income, education, and access to water in the sharp decline in infant deaths due to diarrhea in the 1980s. Nevertheless, Sastry and Burgard (2005) raise questions about this conclusion. “There is considerable uncertainty regarding trends in mortality by cause, because death registration is not complete and information on death certificates that are filed is often missing or inaccurate” (Sastry and Burgard, 2005).
[35] Colinearity in these covariates makes it difficult to estimate their distinct effects when they are included within the same regression model.

estimates. Indeed, we show this to be the case with a series of cross-sectional regressions below. If unobserved determinants of IMR do not vary in a county over the course of a decade, the county effect will control for them non-parametrically. Similarly, measurement error in infant mortality may vary by county. As long as the rate of measurement error is stable over the course of a decade, α_i will control for its impact on the reported IMR.

With a constant coefficient vector β and a mean zero restriction on the error term, the typical approach to identifying β with panel data is to estimate the first-differenced model:

(2) $$y_{i,2} - y_{i,1} = (x_{i,2} - x_{i,1})'\beta + (\varepsilon_{i,2} - \varepsilon_{i,1})$$

by simply regressing the differenced dependent variable on the differenced covariates. Unfortunately, such an approach will not be valid in the quantile regression setting. To see why, we return to the basic model introduced by Koenker and Bassett (1978) and Koenker and Hallock (2001), which allowed marginal effects to vary by quantile. They considered a (cross-sectional) linearly heteroskedastic model of the form:

(3) $$y_i = \alpha_i + x_i'\beta + (x_i'\psi)\varepsilon_i$$

which implies that the θ^{th} conditional quantile of the dependent variable has the following form:

(4) $$\begin{aligned} q_\theta &= \alpha_i + x_i'\beta + x_i'\psi\rho_\theta \\ &= \alpha_i + x_i'(\beta + \psi\rho_\theta) \\ &= \alpha_i + x_i'\beta_\theta \end{aligned}$$

where ρ_θ denotes the θ^{th} quantile of the distribution of ε_i. We now demonstrate that this model cannot carry through to the panel data model by first-differencing. In the linear heteroskedastic framework, differencing equation (3) yields:

(5) $$y_{i,2} - y_{i,1} = (x_{i,2} - x_{i,1})'\beta + (x'_{i,2}\psi\varepsilon_{i,2} - x'_{i,1}\psi\varepsilon_{i,1})$$

Taking conditional quantiles of both sides of equation (5) yields:

(6) $$q_\theta(y_{i,2} - y_{i,1} \mid x_{i,1}, x_{i,2}) = (x_{i,2} - x_{i,1})'\beta + q_\theta(x'_{i,2}\psi\varepsilon_{i,2} - x'_{i,1}\psi\varepsilon_{i,1})$$

Since the quantile and difference operators cannot typically be interchanged (unlike the mean and difference operators), the last term in the above expression is not equal to $(x_{i,2} - x_{i,1})'\psi\rho_\theta$.[36]

We therefore apply the approach described in Chen and Khan (2007). In particular, we impose non-parametric structure on the county effect:

(7) $$\alpha_i = \phi(x_{i,1}, x_{i,2})$$

Where $\phi(\cdot)$ is an unknown function that allows for arbitrary dependence on the covariates.[37] In particular, $\phi(\cdot)$ expresses α_i as a function of *i*'s covariates in both years *t* = 1, 2. This structure generalizes the typical random effects approach, which does not permit α_i to depend upon covariates. It also generalizes approaches which impose parametric specification on α_i, such as

[36] We also note that if we did not allow for the heteroskedastic component, $x_{i,t}\psi$, then the quantile difference function would be a linear function of β plus an additive constant that varied with the quantile. In this restricted setting, marginal effects would not be allowed to vary across quantiles.

[37] In practice, data limitations (in particular, a high degree of correlation between many of our regressors) will restrict us to using a second-order polynomial in this stage of the estimation. Depending upon the specifics of the application, this could be expanded to a higher-order polynomial or even a non-parametric bin estimator.

Chamberlain (1982), and Abrevaya and Dahl (2006).[38] Consequently, we have the following functional form for the conditional quantile functions:[39]

(8) $$q_\theta(y_{i,t} \mid x_{i,1}, x_{i,2}) = \phi(x_{i,1}, x_{i,2}) + x'_{i,t}\beta + x'_{i,t}\psi\rho_\theta$$

This implies that the first differences in the conditional quantile functions are of the form:

(9) $$\begin{aligned} &q_\theta(y_{i,2} \mid x_{i,1}, x_{i,2}) - q_\theta(y_{i,1} \mid x_{i,1}, x_{i,2}) = \\ &\qquad \phi(x_{i,1}, x_{i,2}) - \phi(x_{i,1}, x_{i,2}) + (x_{i,2} - x_{i,1})'\beta + (x_{i,2} - x_{i,1})'\psi\rho_\theta \end{aligned}$$

which, with some simplification, yields:

(10) $$\begin{aligned} q_\theta(y_{i,2} \mid x_{i,1}, x_{i,2}) - q_\theta(y_{i,1} \mid x_{i,1}, x_{i,2}) &= (x_{i,2} - x_{i,1})'(\beta + \psi\rho_\theta) \\ &= (x_{i,2} - x_{i,1})'\beta_\theta \end{aligned}$$

This implies an ability to estimate quantile-varying marginal effects. Of course, the above equations do not translate directly into a feasible estimation procedure since the conditional quantile functions, $q_\theta(y_{i,1} \mid x_{i,1}, x_{i,2})$ and $q_\theta(y_{i,2} \mid x_{i,1}, x_{i,2})$, are unknown. The approach can be implemented, however, by following a simple

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[38] Chen and Khan (2007) show that, despite this generalization, there is no curse-of-dimensionality associated with estimating *β*.

[39] We can also allow for an additive unobserved term in our structure if we impose certain conditions such as independence of the regressors for both this term and $\varepsilon_{i,t}$ as well as stationarity on $\varepsilon_{i,t}$ on this term. Specifically, we can define α_i

$$\alpha_i = \phi(x_{i,1}, x_{i,2}) + \eta_i + \varepsilon_{i,t}$$

if we assume the "composite" error term, $\eta_i + \varepsilon_{i,t}$ is distributed independently of the regressors and identically across individuals, as well as strongly stationary. In this way, the quantiles of the composite error are constants that vary across quantiles but do not depend on *i* nor *t*. Equation (8) would then be of the form

$$q_\theta(y_{i,t} \mid x_{i,1}, x_{i,2}) = \phi(x_{i,2}, x_{i,1})' + c_\theta + x'_{i,t}\beta + x'_{i,t}\psi\rho_\theta$$

where c_θ is a constant that gets differenced out, maintaining the form of equation (9).

two-step procedure. First, non-parametrically estimate the conditional quantile functions in (8), $q_\theta(y_{i,t} \mid x_{i,1}, x_{i,2})$ for t = 1, 2. It is important that the function $\phi(\cdot)$, which controls non-parametrically for the county fixed effect α_i, include data from both time periods. When estimating $q_\theta(y_{i,t} \mid x_{i,1}, x_{i,2})$, the equation also includes observables from period t in linear form. Denote these fitted values from each of these quantile regressions as $\hat{q}_\theta(y_{i,t} \mid x_{i,1}, x_{i,2})$. In the second step, we regress the differenced fitted values, $\hat{q}_\theta(y_{i,2} \mid x_{i,1}, x_{i,2}) - \hat{q}_\theta(y_{i,1} \mid x_{i,1}, x_{i,2})$ on the differenced regressors, $(x_{i,2} - x_{i,1})$. As seen in equations (9) and (10), the proxies for the county effects difference out, yielding an estimate of β_θ – i.e., the marginal effect for the θ^{th} quantile. As discussed in Chen and Khan (2007), this procedure is very simple to implement, requiring little more than STATA or comparable statistical software.[40]

We implement this panel data procedure separately for three time periods: 1970-1980, 1980-1991, and 1991-2000. For the weighted regressions, we weight the observations by the average county-level population over the two years. We also estimate unweighted regressions in order to check the robustness of our results. Finally, we use 2,000 bootstrap simulations to recover standard errors for our estimates.

5.2 Interpreting the coefficients from the quantile regression

The marginal impact of piped water (resulting from a one percentage point increase in the number of households receiving piped water in each county) at the θ^{th} quantile is given by the following expression, as estimated by the regression at that quantile: $\beta_{\theta,water}$ + ($\beta_{\theta,water\ x\ income}$ x INCOME) + ($\beta_{\theta,water\ x}$

[40] Since this approach attains identification off of variation in the regressors without varying the individual specific effect, it cannot be applied to estimate coefficients of time invariant regressors. This is also the case with standard fixed effect estimation. Nonetheless, the change in coefficients on time invariant variables (e.g., climate) can be estimated using our procedure by interacting them with time dummies.

$_{sewage}$ x SEWAGE), where INCOME and SEWAGE denotes the mean intra-group value of each variable.

Importantly, the comparison of the impact of piped water across quantiles of the conditional IMR distribution must be made within *conditionally similar groups of counties*. To understand this concept, imagine that counties have only two characteristics and that each characteristic is binomial. The two characteristics are high or low water supply, and high or low income. Therefore, counties can be grouped into four groups i.e. (1) low water supply and low income; (2) low water supply and high income; (3) high water supply and low income; and (4) high water supply and high income. In evaluating the variation, if any, of the impact of water across quantiles of the distribution, we focus our attention on one group of counties, for example, Group 1 with high water supply and high income. Using the intra-group mean value of income for Group 1, we examine the value of the expression above for the 10th through the 90th quantile of the conditional distribution. If the value of the expression were negative and smallest in magnitude at the 10th quantile, and negative and largest in magnitude at the 90th quantile, we would conclude that *within* the group of counties with low water supply and low income, piped water has the strongest impact in reducing infant mortality rate at the higher quantiles of the conditional infant mortality rate. We use the analogous method to examine the impact of water across the conditional IMR distribution for counties in Group 2, Group 3 and Group 4.

Next, consider our actual estimation model with four covariates.[41] In order to generate a group of counties that are conditionally similar, we categorize counties into groups sharing the same covariates or development indicators. We have four covariates – piped water, piped sewage, income and education. A county can measure either high or low on each covariate, with the cutoff for high/low at the median of these variables.[42] Consequently, we have 16 groups of

[41] We limit the number of covariates in part because of the need to create group of counties that are similar in their covariates. With four covariates, we generate 16 groups of counties.

[42] In 1970 and 1980, we use the cutoff of sewage coverage at the 55th and 65th percentile of observations. In those years, a substantial number of counties did not have any households connected to the sewage network.

counties, arising from 2^4 combination of covariates. In order to capture the local characteristic of each group of counties, we calculate the intra-group mean of each characteristic for each of these groups of counties.

Finally, to uncover the marginal impact of water across quantiles of the conditional IMR distribution, *within conditionally similar groups of counties*, we do the following. For Group 1, i.e. counties that score low in all four covariates, we calculate the expression for the θ^{th} quantile using estimates from the appropriate quantile regression. Next, for Group 1, we present the estimates from the 10^{th} to the 90^{th} quantile in Figure 1. We repeat this exercise for other groups of counties which share similar covariates. The marginal impact of a 0.01 increase in income is calculated using an analogous method.

5.3 Simulation – Marginal effects of piped water and averted infant deaths

After conducting the estimations described above, we simulate the policymaker's expectation of averted infant deaths resulting from the additional provision of piped water. We make this calculation using the estimates from both the mean and quantile regression specifications. We apply these estimates to a simulated change of one percentage point in the number of households receiving piped water in each county.

Recall that counties are grouped as being high or low in each of these four covariates or development indicators: piped water, piped sewage, income and education. We therefore have 16 groups of counties corresponding to 16 possible combinations of high or low values for the four indicators. We calculate for each group of counties their intra-group mean income~~.~~ Next, for each group of counties, we calculate the intra-group distribution of the infant mortality rate. A county will therefore occupy the θ^{th} percentile of the conditional infant mortality rate distribution (i.e., within a group of counties that are similar in their four development indicators).

For each county in a given group, we calculate the marginal effect of piped water on its infant mortality rate (measured as deaths per 1000 live births) using estimates from the appropriate quantile regression and accounting for local conditions as captured by income. We then simulate the effect of an increase of one percentage point in the number of households with piped water.[43]

6 Regression results

Table 2 reports the results from the regressions weighted by county-level population.[44] Results from the mean regression are in column 1, while those from the quantile regressions are in columns 2 to 10. Panels A, B and C present results from the regressions for 1970-1980, 1980-1991, and 1991-2000, respectively. To calculate the marginal impact of piped water, we use the coefficients from the weighted quantile panel data regressions for 1970-1980, 1980-1991 and 1991-2000 and intra-group means from 1970, 1980, and 1991, respectively.[45]

6.1 Marginal impact of piped water

We report the impact of a one percentage point increase in the number of households with piped water supply.[46] As seen in Table 2, the coefficients for water and the interaction term between water and income for the years 1970-1980 and 1980-1991 are generally statistically significant at or below the 10% level. For the estimates in 1991-2000, these coefficients are statistically significant only in the regressions at the upper quantiles.

[43] For counties whose intra-group IMR is below the 10th percentile, we use the estimates from the 10th quantile regression. For counties whose intra-group IMR is between the 10th and 20th percentile, we use the estimates from the 20th percentile regression, and so on. We use the estimates from the 90th percentile for counties whose intra-group IMR is between the 80th and 90th percentiles, as well as for counties whose intra-group IMR is above the 90th percentile.

[44] The results from the un-weighted regressions are similar to those from the weighted regressions and are available on request from the authors.

[45] Results using the intra-group mean from 1980, 1991, and 2000 are qualitatively similar.

[46] We describe reductions in infant mortality resulting from a one percentage point increase in households with piped water. In practice, the mean increase in households with piped water is 8.9 percentage points between 1970 and 1980, 18.2 percentage points between 1980 and 1991, and 20.2 percentage points between 1991 and 2000.

We find four main results for piped water.[47] First, our results are consistent with Shuval et al.'s (1981) theory. Piped water has a sizable impact in reducing IMR after counties exceed a minimal threshold of socioeconomic development, but it has little impact after counties cross a saturation threshold. Consider Figure 1 which plots these impacts in counties that measure low in all their development indicators. In 1970-1980, the increased piped water supply reduced IMR by 0.61 to 0.82 deaths per 1000 live births. By 1980-1991, the counties at the upper tail of the conditional IMR distribution have exceeded the threshold of development, and we see piped water having a very strong impact. At the 80th to the 90th conditional quantiles of the IMR distribution, piped water reduced IMR by 1.25 to 1.28 deaths per 1000 live births. These reductions are sizable when compared to the mean of 86.8 and 49.2 deaths per 1000 live births in 1980 and 1991, respectively. In that same time period, we see some evidence of counties in the lower tails of the conditional IMR distribution moving towards the saturation point for the impact of water. At the 10th and 20th quantiles of the conditional IMR distribution, increased piped water reduced IMR by 0.54 to 0.55 deaths per 1000 live births. Finally, corresponding to the saturation threshold, by 1991-2000 we see that piped water has very little impact at any point across the conditional IMR distribution. By this point, increased by piped water reduced IMR by only 0.03 to 0.21 deaths per 1000 live births.

Second, we find that piped water has a larger impact in reducing IMR in counties that rank low in their development indicators than in counties that rank high, particularly in 1980-1991. Consider Figures 1 and 2, which plot these impacts in counties that measure low in all of their development indicators and high in at least two of their indicators, respectively. In the first set of counties, a one percentage point increase in piped water reduces IMR by 0.54 to 1.28 deaths per 1000 live births. In contrast, in the second set of counties, the increase in piped water reduces IMR by only 0.36 to 0.72 deaths per 1000 live

[47] As discussed above, each year we separate counties into sixteen groups that rank similarly in their development indicators. For brevity, we present two graphs only, the first, from the group of counties that rank low in their development indicators and the second, from the group of counties that rank high in at least two of their development indicators.

births. This pattern of piped water having a stronger protective effect in counties that measure low in observable development indicators than in those that measure high hold for most of the other group of counties as well.[48]

Third, we find evidence that the impact of piped water varies across the conditional IMR distribution. In particular, in the group of counties that measure similarly low or similarly high in their development indicators, piped water exerts a stronger protective effect at the upper tail of the conditional IMR distribution (particularly in 1980-1991). Consider counties that measure low in all of their development indicators. As seen in Figure 1, additional piped water reduces IMR by 1.25 deaths per 1000 at the 90th percentile but by only by 0.55 deaths per 1000 live births at the 10th percentile of the conditional IMR. This pattern of a stronger protective impact of water in the upper quantiles of the conditional IMR distribution in 1980-1991 is also evident in the group of counties that measure high in their development indicators. As seen in Figure 2, the additional piped water reduces IMR by 0.68 deaths per 1000 at the 90th percentile, but by 0.36 deaths per 1000 live births at the 10th percentile of the conditional IMR.

Fourth, we find that (particularly in 1981-1990) the estimates from the mean panel regression model severely understate the protective impact of piped water for the populations occupying the upper quantiles of the conditional IMR distribution. The mean estimates suggest that a one percentage point increase in the number of households with piped water reduces IMR by 0.72 deaths per 1000 live births. In contrast, for the group of counties that measure low in their development indicators, the quantile panel model finds that an increase in piped water reduces IMR by 1.25 deaths per 1000 live births at the 90th conditional quantile of the IMR.

6.2 Other variables: Income

The marginal impact of income is given by the following expression: $\beta_{\theta,\text{income}} + \beta_{\theta,\text{water x income}}$ x WATER + $\beta_{\theta,\text{sewage x income}}$ X SEWAGE, where WATER and SEWAGE represent the intra-group mean percentage of households with

[48] Figures for these other groups of counties are available on request from the authors.

piped water supply and networked sewage, respectively. As seen in Table 2, the coefficients for income and the interaction term between income and water for the years 1970-1980 and 1980-1991 are generally statistically significant at or below the 10% level. For the estimates in 1991-2000, these coefficients are statistically significant only in some of the quantile regressions. We report the impact of a 0.01 increase in income.[49]

Our results for income closely track the four patterns that emerged for piped water. First, income has a sizable impact in reducing IMR after counties exceed a minimal socioeconomic development threshold, but that impact dies off after counties cross a saturation threshold. Consider Figure 3, which plots these impacts in counties that measure low in all of their development indicators. In 1970-1980, increased income reduced IMR by 0.43 to 0.74 deaths per 1000 live births. By 1980-1991, the counties at the upper tail of the conditional IMR distribution exceeded the threshold of development, and we see income having a very strong impact. At the 80th to the 90th conditional quantiles of the IMR, income reduced IMR by 1.17 to 1.35 deaths per 1000 live births. In that same time period, however, we see some evidence that counties in the lower tails of the conditional IMR distribution were moving towards the saturation point for the impact of income. At the 10th and 20th conditional quantiles of the IMR distribution, increased income reduced IMR by only 0.26 and 0.36 deaths per 1000 live births, respectively. Finally, corresponding to the saturation threshold, we see that income has very little impact across the conditional IMR distribution by 1991-2000 when a 0.01 increase reduced IMR by only 0.24 deaths and 0.21 deaths at the 10th and 90th percentiles, respectively.

Second, we find that income has a larger impact in reducing IMR in counties that measure low in their development indicators than in counties that measure high. Consider Figures 3 and 4, which plot these impacts for these two groups of counties, respectively. In the first set of counties, a 0.01 increase in piped water reduces IMR by 0.26 to 1.34 deaths per 1000 live births at the 10th

49 In practice, the mean increase in income (measured by the HDI_income variable) between 1970 and 1980 is about 0.32. Income increased by 0.02 between 1980 and 1991, and it increased by 0.05 between 1991 and 2000.

and 90th percentiles. In contrast, in the second set of counties, the increase in income reduces IMR by only 0.08 to 0.97 deaths per 1000 live births at the same percentiles. This pattern of income having a stronger protective effect in counties that measure low in development indicators than in those that measure high also holds for most of the other county groups.

Third, we find evidence that the impact of income varies across the conditional IMR distribution. In particular, in the group of counties that measure similarly low or similarly high in their development indicators, income exerts a stronger protective effect in the upper tail of the conditional IMR distribution (particularly in 1980-1991). Consider counties that measure low in their development indicators. As seen in Figure 3, additional income reduces IMR by 1.34 deaths per 1000 at the 90th percentile but by only by 0.26 deaths per 1000 live births at the 10th percentile of the conditional IMR. This pattern of stronger protective impact of income in the upper quantiles of the conditional IMR in 1980-1991 is also evident in the group of counties that measure high in their development indicators. As seen in Figure 4, additional income reduces IMR by 0.97 deaths per 1000 live births at the 90th percentile but by only 0.08 deaths per 1000 at the 10th percentile.

Fourth, we find that, particularly in 1980-1991, the estimates from the mean panel regression model severely understate the protective impact of income for the populations occupying the upper quantiles of the conditional IMR distribution. The mean estimates suggest that a 0.01 increase in income reduces IMR by 0.41 deaths per 1000 live births. In contrast, for the group of counties that measure low in their development indicators, the quantile panel model finds that the increase in income reduces IMR by 1.34 deaths per 1000 live births at the 90th conditional quantile of the IMR.

The total fertility rate may be associated with IMR. As this variable is available only for 1991 and 2000, we compare 1991-2000 panel regressions with and without it. Results are tabulated in Panel C1 and Panel C2 in Table 2. A calculation of the marginal impact of water using the coefficients from Panel C1 and Panel C2 indicate that the regression results are robust to the inclusion of the total fertility rate variable.

6.3 Other variables: Interaction of piped water and income

The interaction effects between piped water and income at the θth quantile is given by $B_{\theta \text{ water x income}}$. This coefficient appears to be statistically significant for all quantiles in 1970-1980, 1980-1991, and for the upper-quantiles in 1991-2000. For a given panel model, the size of the estimates are larger at the upper-quantiles relative to the lower quantiles, and this gap in size appears largest in 1980-1991.

To recall, our results on water indicate that, holding income constant, piped water appears most effective at the upper-quantiles of the conditional IMR distribution, where unobserved determinants are worse. The positive interaction effect between piped water and income suggest that these two inputs into infant health are substitutes. In other words, income reduces the effectiveness of piped water, suggesting that households are able to translate income into purchases that can substitute for piped water as an input into producing better health outcomes. The larger size of the coefficients at the upper quantiles suggests that income can be more effectively used as a substitute for water at those upper-quantiles. Plausibly, in counties occupying the upper-quantiles of the conditional IMR distribution, which suffer numerous unobserved poor circumstances, income can be used to address the ‘simpler’ pathways that lead to infant mortality. In the lower-quantiles, fewer unobserved circumstances remain that can be addressed with income, and ultimately, the provision piped water is necessary to attain the desired IMR reduction.

6.4 Other variables: Sewage

The marginal impact of sewage at the θth quantile is given by $\beta_{\theta,\text{sewage}} + \beta_{\theta,\text{water x sewage}}$ X WATER + $\beta_{\theta,\text{water x income}}$ X INCOME, where WATER and INCOME represent the intra-group mean percentage of households with piped water supply and the intragroup mean income. Table 2 indicates that this impact is not statistically significant. These results stand in contrast with our earlier results on piped water.

Our results are consistent with Victora et al (1988), in a population-based study in Porto Alegro and Pelotas, Brazil, which finds that piped water supply is associated with reduced infant mortality, but no association is detected for measures of sanitation facilities (flush toilets or pit latrine). In contrast, Barreto et al. (2007) find that a city-wide sanitation program in Salvador, Brazil, reduced diarrheal prevalence by 22% on average and by 43% in the poorest neighborhoods.[50] [51]

Nevertheless, our results should be treated with caution in light of two limitations in our study. First, as our variable measures networked sewage (i.e., only one type of sanitation facilities), our study provides limited information on the potential impact of sanitation facilities on IMR. Second, we have little cross-sectional variation in our explanatory variable ($sewage_{t2}$-$sewage_{t1}$), so we have limited ability to discern related variation in our dependent variable, ($IMR_{t2} – IMR_{t1}$). Thus, the corresponding estimated coefficient has a large standard error.

6.5 Other variables: Education[52]

Our results indicate that education is more effective at reducing IMR where the overall circumstances are worse. The marginal impact of education is indeed larger in 1970-1980 and 1980-1991 and its asymmetry is more pronounced in these years than in the latter years. In 1970-1980, a 0.01 increase in education reduces infant mortality by 1.8 deaths in the 10th percentile and by almost one-and-a-half times that amount (i.e. 2.8 deaths) at the 90th percentile. In 1980-1991, the gap is larger with 0.74 avoided deaths at the 10th percentile and 2.0 deaths at the 90th percentile. In 1991-2000, the gap declines to

[50] The sanitation program aimed to increase the number of households with an adequate sewer system from 26% to 80%. The program focused on sewage connection, but included some improvements in water supply (Barreto et al, 2007).

[51] The contrasting results between diarrheal prevalence and mortality may be caused by care-seeking behavior, case management and nutritional status (Caincross, pers. comm. May 21 2008).

0.77 avoided deaths at the 10th percentile and 1.2 deaths at the 90th percentile. In all three decades, the mean estimate falls between the estimates from the 10th and 90th quantile regressions.

6.6 Other models: Urban counties only

As described earlier, our basic model includes only one type of sanitation (i.e., network sewage), which is considered the only adequate sanitation method for the urban areas. Our model therefore omits other types of sanitation, such as pit latrines, that are considered to be adequate in rural areas. To address this limitation, we re-run our analysis restricting our sample to only urban counties. We define urban counties to be those with 50% or more of their households living in urban areas.[53]

Table 3 tabulates the results from the urban only sample. Our results (particularly from 1980-1991) indicate that additional piped water supply has a sizable impact in reducing the infant mortality rate. Figures 5 and 6 show a comparison of the marginal impact of water estimated using the full sample and that estimated using the urban only sample. As seen from these figures, the marginal impact of water remains sizable for the urban sample, though it is smaller in magnitude than in the full sample. It may well be that, in our earlier estimates, the failure to control adequately for latrines in rural areas led us to attribute too much of the impact in reduction of IMR to piped water supply. Nevertheless, our results from the restricted urban only sample continue to show the four patterns observed earlier. First, the marginal impact of water follows the pattern suggested by Shuval et al. (1981). Second, comparing Figure 5 and 6, we see that piped water has a stronger effect in the set of counties that measure low in their development indicators. Third, piped water has a stronger protective effect in the upper tails of the conditional IMR distribution. Fourth, we find that the

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[52] Gender-specific education information are not available at the MCA-level. Therefore, we are not able to examine the question of the interaction between maternal education and piped water (Barrera, 1990a).

[53] Urbanized counties in the 1970-80, 1980-1991 and 1991-2000 panels are defined based on 1970, 1980 and 1991 values. Results (available from the authors upon request) are similar using a 70% urbanization cutoff.

mean panel regression model underestimates the protective impact of income for the populations occupying the upper quantiles of the conditional IMR distribution. While the mean estimate suggests that a one percentage point increase in the number of households with piped water reduces IMR by 0.63 deaths, the quantile estimates at the 90th percentile suggest a reduction of 0.87 deaths per 1000 live births.

6.7 Other models: Under-5 child mortality rate

We extend our analysis to the under-5 child mortality rate.[54] Our analysis is limited to one panel model (i.e., 1991-2000) because the under-5 child mortality rates at the MCA level are available only for those two census years. Our results, presented in Table 4, indicate that piped water did not have a statistically significant impact on under-5 child mortality. Recall that piped water has a strong protective impact on under-1 mortality in 1980-1991 but its effects are attenuated by 1991-2000.

Our results showing that piped water has a stronger protective impact on infants than on children under the age of five are consistent with those in the previous literature. Several studies document a negative association between the presence of piped water and infant mortality, but no statistically significant association between that presence and child mortality (Rutstein, 2000; Abou-Ali, 2003; Woldemicael, 2000).[55] Butz el al. (1984) suggests the following explanation for this age pattern. “As infants mature (i.e., their immunity systems mature), they become less susceptible to enteric pathogens. Infant maturation

[54] A large number of studies reviewed in Fewtrell et al. (2005) examine infant and children aged 0-60 months.

[55] Rutstein (2000), in her meta-analysis of the 89 DHS surveys between 1986 and 1998, finds that a statistically significant negative association between piped water and the mortality of children aged 1, but no statistically significant association between piped water and the mortality of children aged 1-4. Abou-Ali (2003), examining the 1995-6 Egyptian Demographic and Health Survey (DHS), finds a statistically significant negative association between presence of piped water (whether in residence or in the neighborhood) and post-neonatal mortality (2-12 months) but no statistical significant association between piped water and child mortality (12-60 months). Woldemicael (2000) finds, in the 1995 Eritrean DHS, a statistically significant negative association between good household environment (neither piped water nor flush toilet or only one of the two) and neonatal mortality (1-13 months) but no statistically significant association between piped water and child mortality (14-60 months).

explains why, in spite of increasing exposure with age to the environment and its pathogens, improved water and sanitation prevent fewer deaths in the later months."

6.8 Averted deaths

Section 1 of Table 5 shows the number of averted deaths as a result of an increase of one percentage point in the number of households with piped water in each county. Section 2 shows the number of averted deaths as a result of an increase of 0.01 in income. Calculations using the quantile panel data procedure are tabulated in column (1) while those using the mean fixed effect regressions are tabulated in column (2). The difference between the two columns reflects the asymmetry of the distribution of marginal effects in each decade. There is a strong right-skew pulling up the mean marginal effect of both water and income estimated in the 1980-1991 regression. In contrast, the opposite skew pulls down the mean effect for water estimated in the 1970-1980 regression. By the period 1991-2000, aggregate effects (particularly for water) are very similar under both measures. These results emphasize the fact that it is difficult to predict which direction asymmetric marginal effects will pull mean estimates, highlighting the role played by quantile estimation in recovering the full distribution of effects.

6.9 Cross-section results

We examine the naïve cross-sectional estimates as seen in Table 6. The association between a one percentage point increase in the number of households with piped water and the infant mortality rate at the θth quantile is plotted in Figure 7. The cross sectional estimates, particularly in 1970 and 1980, indicate that greater provision of piped water is correlated with *larger* infant mortality rates. We see a particularly strong correlation between water supply and increased mortality at the higher conditional quantiles of the IMR, although the size of the bias diminishes in the latter years of the analysis. This counterintuitive result is likely to be an artifact from the systematic underreporting of infant mortality rates in areas on the upper tails of the IMR distribution that tend to receive less water.

7 Discussion and Policy Implication

For those populations at greatest risk, can the provision of piped water reduce the infant mortality rate or are complementary inputs such as income and other public health infrastructure required? Our results are consistent with Shuval et al.'s (1981) threshold-saturation hypothesis, in which the relationship between water supply and IMR varies with changing socioeconomic levels. Assuming that our differencing procedure can adequately control for correlated unobservables and measurement error, we find that water has a small effect in the most undeveloped places (i.e., when we look at the high conditional quantiles in 1970-80). As counties start to develop (i.e., the higher quantiles in 1980-91), the protective effect of water on IMR starts to rise rapidly. As counties become more developed (i.e., low quantiles in 1980-91) the protective effect of water declines. Finally, when very developed (i.e., low quantiles in 1991-00), the effect of water on IMR is very small.

In 1980-1991, the marginal impact of piped water is greatest in those counties with poorest performance in their observable development indicators. For counties with poor development indicators – averted deaths at the 90th percentile is 1.25 per 1000 live births, while for counties with good development indicators only 0.68 are averted. In addition, among those counties that share common development indicators, particularly in 1980-1991, we find that piped water exerts a stronger protective effect in those counties that occupy higher positions in the conditional IMR distribution (i.e., counties that are worse in unobservable development indicators).

Our results therefore show that (1) piped water provision can cause a significant reduction in the IMR (when accompanied by a basic level of other public health inputs); and (2) the impact of a piped water provision policy is determined in large part by how those piped water connections are distributed.

Ignoring costs of provision, our results suggest that, from the perspective of health outcomes, new piped water resources should be targeted to the most disadvantaged communities. [56,57,58]

What can policy-makers learn from our study? In addition to recognizing the role of particular observed characteristics that influence the effectiveness of piped water in reducing IMR, policymakers also need to take into account the role of unobserved characteristics – i.e., characteristics that cannot be easily summarized with available data. In practice, policymakers can control for these unobservables by implementing the following strategy, which allows one to recover their distribution up to a scale and location normalization. Reconsider equation (10), from which we know that the estimated value of β_θ is equal to $\beta + \psi\rho_\theta$. Making the location normalization that the median of $\varepsilon_{i,t}$ is zero (i.e., $\rho_{50} = 0$), we immediately identify β (i.e., $\beta = \beta_{50}$). Next, making a scale normalization (e.g., $\rho_{75} = 1$) we can further identify ψ (i.e., $\psi = \beta_{75} - \beta$). With estimates of β and ψ, we can then recover the distribution of $\varepsilon_{i,t}$ (i.e., different values of ρ_θ) from equation (9) simply by observing the conditional quantiles of $y_{i,t}$. The resulting ranking of residuals can then be used to help determine where to target piped water interventions. Our results suggest that there will be

[56] We do not have detailed data to allow a comprehensive cost-effectiveness or cost-benefit analysis on the impact of piped water. Program-specific costs of piped water provision will depend on numerous location conditions such as the geography and the existing piped water network. Dr. Marcos Thadeu Abicalil, Senior Water and Sanitation Specialist in The World Bank's Brasilia's Office, reports the following estimates for the per capita costs for the provision of piped water supply and sewage networks: BRL 428; and BRL 797 (pers. comm., 2008). Abiko (2007) provides some estimates on the costs of provision of piped water based on slum upgrading programs. Haller et al. (2007) and Hutton et al. (2007) provide a comprehensive study on the cost effectiveness and the costs-benefit analyses of various child health and water and sanitation improvements, respectively. Both Haller et al. (2007) and Hutton et al. (2007) add qualifications to their study, citing Briscoe's (1984b) and Okun's (1988) earlier critique of such analyses.

[57] Policymakers may consider other factors in piped water placement such as population density. We acknowledge that the provision of piped water may be cheaper in areas with good development indicators and/or low conditional IMR. These locations may already have a minimal level of existing infrastructure. New outlays of pipelines may have to be undertaken in disadvantaged areas.

[58] "It has been suggested that piped water disproportionately benefits the better-off people of a village" (Mohan, 2005). Further interventions would have to be undertaken to overcome social constraints and connection costs that prevent the vulnerable households from accessing the network.

statistically and economically significant, policy-relevant differences in the effectiveness of piped water over these indicators of unobservable determinants, with the biggest effects coming high in their distribution.

Methodologically, these results highlight the importance of applying the quantile regression framework to recover the marginal effects of water at various parts of the conditional distribution of the IMR. The marginal effects at various parts of the conditional IMR distribution differ substantially from those at the mean of the distribution. Indeed, focusing on the mean of the distribution can lead to an underestimate of the potential impact of piped water intervention in higher percentiles of the conditional IMR distribution. Our results for piped water intervention correspond with the growing literature on the heterogeneity of program impacts across the quantiles of the conditional distribution of the outcome variable and the insufficiency of mean estimates to represent this policy-relevant heterogeneity.

Quantile estimation for the evaluation of policy is, however, quite difficult. Policies are not often allocated randomly, and good instruments may not be available. Traditional quantile regression is not generally feasible in the panel data context. In contrast, our quantile panel data approach can be widely applied to the evaluation of other programs that provide health inputs or public goods in developing countries. This method allows policymakers to understand the impact of these programs on the subpopulations that are most at risk, and these subpopulations tend to occupy the tails in the conditional distribution. Amidst the scarcity of random assignment and viable instruments, but with the growing availability of panel data in developing countries, the panel data approach provides a promising strategy to address the issue of bias arising from unobservables (albeit only time invariant ones) within the context of quantile regressions.

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Table 1A: Provision of piped water by region (1970-2000)

Year		1970	1970	1980	1980	1990	1990	2000	2000
Location		Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural
Water	Northeast	15	2	29	8	39	19	62	42
IMR	Northeast	192	180	138	136	73	78	50	55
Water	North	17	4	22	8	27	13	50	31
IMR	North	118	113	72	70	54	59	40	42
Water	Center-West	20	4	21	7	44	25	69	48
IMR	Center-West	93	92	60	60	33	36	25	31
Water	South	27	8	43	16	62	32	74	44
IMR	South	84	85	55	53	32	31	19	19
Water	Southeast	51	20	52	23	66	33	77	47
IMR	Southeast	96	98	60	62	31	40	22	32

Notes: Water denotes the percentage of households in a county with piped water.
IMR denotes the under-1 infant mortality rate in deaths per 1000 live births.
Urban counties in a given year are those with 50% or more of their population living in urban areas.

Table 1B: Summary Statistics

	Year	Mean	Std. Dev.
Infant mortality rate (in deaths per 1000 live births)	1970	125.3	52.7
	1980	86.8	45.2
	1991	49.2	24.4
	2000	33.7	18.1
Percentage households with piped water (%) (Water)	1970	15.1	19.5
	1980	24.0	21.8
	1991	42.2	23.9
	2000	62.4	20.5
Percentage households with sewage connections (%) (Sewage)	1970	5.3	12.3
	1980	10.6	19.1
	1991	18.0	26.6
	2000	29.4	30.4
Human Development Index Income (income)	1970	0.22	0.16
	1980	0.54	0.27
	1991	0.56	0.10
	2000	0.61	0.10
Human Development Index Education (education)	1970	0.40	0.14
	1980	0.47	0.14
	1991	0.65	0.13
	2000	0.78	0.09
Total fertility rate	1970	5.9	1.5
	1980	4.5	1.4
	1991	3.6	1.2
	2000	2.8	0.7
Female population	1970	12,796	68,110
	1980	16,366	93,898
	1991	20,322	110,204
	2000	23,542	123,214

Table 1C: Literacy rates by region

	1981		1991		2000	
	Male	Female	Male	Female	Male	Female
Northeast	42.3	40.9	38.9	34.1	26.3	22.4
North	14.2	16.7	11.8	13.1	11.5	11.0
Central-west	19.9	22.7	16.6	17.1	10.2	10.3
South	13.5	17.7	10.1	13.1	6.4	7.7
Southeast	12.5	17.6	9.6	13.0	6.7	8.3

Source: IPEA region-level data

Table 2 : The influence of piped water on infant mortality rates: panel regression weighted by county-level population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel A: 1970-1980										
water	-0.48 **	-0.81 **	-0.75 **	-0.69 **	-0.75 **	-0.83 **	-0.79 **	-0.94 **	-0.91 **	-0.95 **
	(0.06)	(0.17)	(0.17)	(0.15)	(0.14)	(0.15)	(0.16)	(0.16)	(0.19)	(0.28)
sewage	0.14	0.33	0.44	0.23	0.38	0.38	0.46 *	0.41	0.20	0.31
	(0.11)	(0.30)	(0.28)	(0.27)	(0.27)	(0.27)	(0.28)	(0.29)	(0.35)	(0.48)
income	-56 **	-47 **	-45 **	-49 **	-56 **	-56 **	-58 **	-66 **	-70 **	-77 **
	(2.99)	(6.52)	(6.20)	(6.42)	(6.40)	(6.62)	(7.08)	(7.51)	(8.39)	(11.3)
education	-242 **	-179 **	-212 **	-224 **	-221 **	-231 **	-243 **	-256 **	-270 **	-277 **
	(9.90)	(21)	(21.6)	(21)	(22)	(23)	(25.5)	(28.9)	(30.5)	(37.0)
water	0.46 **	0.90 **	0.68 **	0.68 **	0.76 **	0.80 **	0.77 **	1.15 **	1.04 **	1.08 **
x income	(0.06)	(0.22)	(0.21)	(0.18)	(0.18)	(0.19)	(0.20)	(0.22)	(0.26)	(0.39)
sewage	-0.08	-0.16	-0.48 *	-0.24	-0.37	-0.47 *	-0.44 *	-0.43	-0.21	-0.21
x income	(0.10)	(0.30)	(0.27)	(0.26)	(0.25)	(0.24)	(0.26)	(0.28)	(0.35)	(0.53)
water	-0.003 **	-0.012 *	-0.002	-0.002	-0.002	-0.001	-0.002	-0.008	-0.007	-0.008
x sewage	(0.001)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.009)

Notes: No. obs. 3568. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions.

Table 2 (continued) : The influence of piped water on infant mortality rates: panel regression weighted by county-level population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel B: 1980-1991										
water	-1.36 **	-0.76 **	-0.74 **	-0.93 **	-1.04 **	-1.25 **	-1.40 **	-1.74 **	-2.01 **	-2.00 **
	(0.10)	(0.15)	(0.13)	(0.15)	(0.15)	(0.16)	(0.17)	(0.18)	(0.19)	(0.23)
sewage	0.32 **	-0.31	-0.40	0.02	-0.05	0.09	0.20	0.28	0.28	0.37
	(0.14)	(0.31)	(0.31)	(0.32)	(0.30)	(0.30)	(0.32)	(0.34)	(0.37)	(0.39)
income	-69.27 **	-32 **	-41 **	-56 **	-67 **	-81 **	-93 **	-110 **	-136 **	-155 **
	(4.48)	(5.52)	(5.70)	(6.29)	(6.49)	(6.93)	(7.50)	(8.11)	(8.43)	(9.68)
education	-139.37 **	-74 **	-91 **	-107 **	-116 **	-124 **	-130 **	-146 **	-166 **	-203 **
	(5.51)	(7.57)	(7.30)	(7.50)	(8.14)	(7.86)	(8.55)	(9.44)	(11.0)	(13.0)
water	1.26 **	0.76 **	0.69 **	1.20 **	1.31 **	1.59 **	1.75 **	2.16 **	2.59 **	2.67 **
x income	(0.13)	(0.24)	(0.23)	(0.24)	(0.23)	(0.23)	(0.25)	(0.27)	(0.27)	(0.31)
sewage	-0.19	0.50 *	0.66 **	0.13	0.20	0.09	0.09	-0.04	-0.07	-0.19
x income	(0.12)	(0.30)	(0.29)	(0.29)	(0.27)	(0.27)	(0.29)	(0.30)	(0.33)	(0.33)
water	-0.004 **	0.0008	0.0002	-0.002	-0.003	-0.003	-0.005 **	-0.003	-0.003	-0.003
x sewage	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)

Notes: No. obs. 3568. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions.

Table 2 (continued) : The influence of piped water on infant mortality rates: panel regression weighted by county-level population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel C1: 1991-2000										
water	-0.20 **	-0.10	-0.22 **	-0.19	-0.10	-0.08	-0.21 *	-0.31 **	-0.40 **	-0.47 **
	(0.04)	(0.13)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.13)	(0.16)
sewage	0.05	0.15	0.20	0.22 *	0.11	0.16	0.17	0.13	0.27 *	-0.004
	(0.04)	(0.15)	(0.13)	(0.13)	(0.13)	(0.13)	(0.12)	(0.13)	(0.16)	(0.22)
income	-14.01 **	-27	-34 **	-32 **	-23	-18	-37 **	-51 **	-32	-32
	(6.87)	(16.6)	(14.3)	(15.3)	(15.2)	(16.2)	(16.7)	(18.4)	(19.7)	(22.7)
education	-99.50 **	-77 **	-77 **	-85 **	-95 **	-101 **	-107 **	-105 **	-112 **	-118 **
	(2.54)	(7.17)	(6.18)	(6.72)	(6.76)	(7.24)	(7.19)	(6.97)	(7.59)	(9.74)
water	0.17 **	0.15	0.33 *	0.30	0.15	0.10	0.31	0.43 **	0.54 **	0.59 **
x income	(0.08)	(0.22)	(0.19)	(0.19)	(0.19)	(0.21)	(0.21)	(0.21)	(0.23)	(0.28)
sewage	-0.07	0.01	-0.10	-0.21	-0.11	-0.33	-0.31	-0.13	-0.26	0.15
x income	(0.08)	(0.30)	(0.27)	(0.26)	(0.26)	(0.25)	(0.24)	(0.25)	(0.30)	(0.41)
water	-0.001 **	-0.002 **	-0.002 **	-0.002 **	-0.001	-0.00004	-0.0001	-0.001	-0.003 *	-0.003 *
x sewage	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)

Notes: No. obs. 3568. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions.

Table 2 (continued): The influence of piped water on infant mortality rates: panel regression weighted by county-level population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel C2: 1991-2000. Fertility rate is included as a regressor.										
water	-0.09 **	0.01	0.02	-0.01	-0.01	-0.07	-0.21 *	-0.16	-0.12	-0.28 *
	(0.04)	(0.13)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)	(0.13)	(0.17)
sewage	0.03	0.22	0.15	0.06	0.10	0.15	0.14	0.08	0.16	0.14
	(0.04)	(0.16)	(0.14)	(0.14)	(0.13)	(0.12)	(0.13)	(0.13)	(0.15)	(0.24)
income	-6.18	-9	-15	-28 **	-27 *	-30 *	-42 **	-28	-13	-19
	(6.82)	(17)	(14)	(14)	(15)	(16)	(17)	(18)	(19)	(24)
education	-84 **	-78 **	-75 **	-78 **	-81 **	-87 **	-87 **	-96 **	-102 **	-108 **
	(2.97)	(8.71)	(7.26)	(6.76)	(7.10)	(7.71)	(8.05)	(7.85)	(8.55)	(12)
fertility	-0.0009	1.60 **	1.80 **	2.15 **	2.27 **	2.34 **	2.70 **	2.45 **	3.20 **	3.62 **
rate	(0.08)	(0.64)	(0.58)	(0.57)	(0.59)	(0.59)	(0.66)	(0.69)	(0.78)	(0.94)
water	-0.05	0.01	-0.02	0.03	0.01	0.12	0.30	0.18	0.11	0.36
x income	(0.08)	(0.22)	(0.19)	(0.19)	(0.20)	(0.20)	(0.21)	(0.20)	(0.22)	(0.29)
sewage	-0.001 *	-0.05	-0.11	0.05	-0.06	-0.16	-0.26	-0.22	-0.16	-0.10
x income	(0.0004)	(0.31)	(0.27)	(0.26)	(0.25)	(0.24)	(0.24)	(0.26)	(0.28)	(0.44)
water	2.71 **	-0.002 **	-0.001	-0.002 *	-0.001	-0.001	-0.0002	0.0002	-0.002	-0.003 *
x sewage	(0.28)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

Notes: No obs. 3568. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for the quantile regressions are from 2000 bootstrap repetitions.

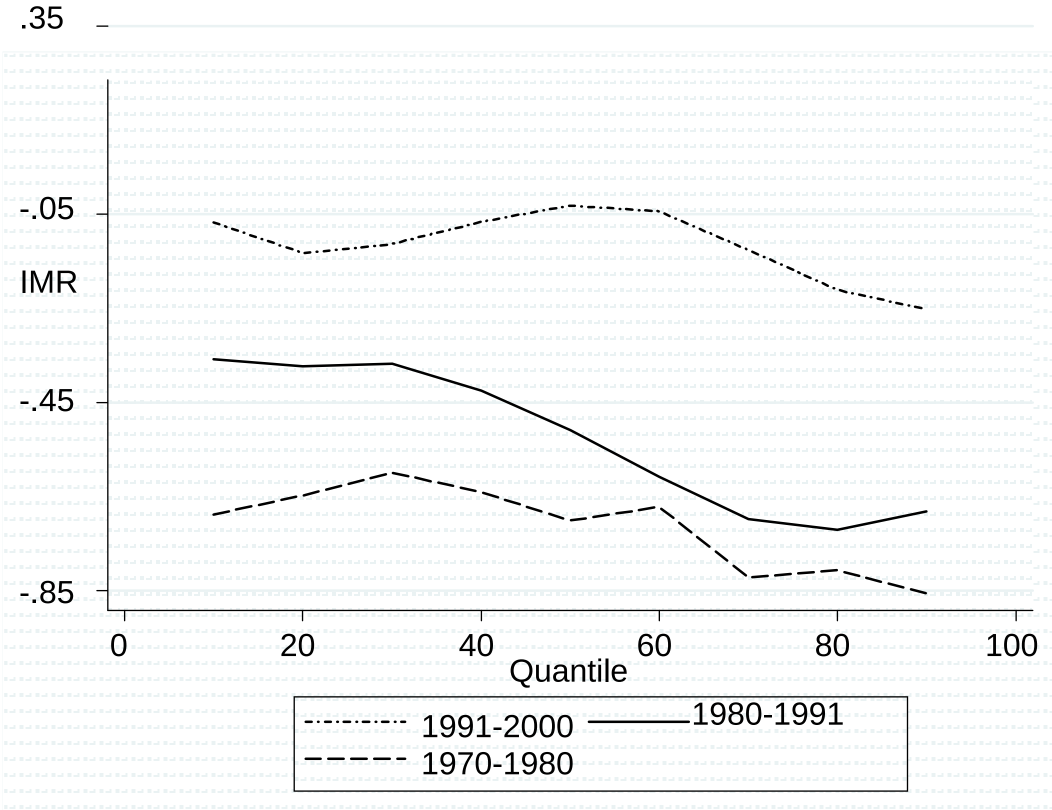
Figure 1 : Marginal Impact of One Percentage Point Increase in Households with Piped Water Supply on the Under-1 Infant Mortality Rates (IMR).

Group of counties that measure low in their development indicators.

Notes: Y-axis: Negative values indicate reduction in IMR.

Figure 2 : Marginal Impact of One Percentage Point Increase in Households with Piped Water Supply on the Under-1 Infant Mortality Rates (IMR).

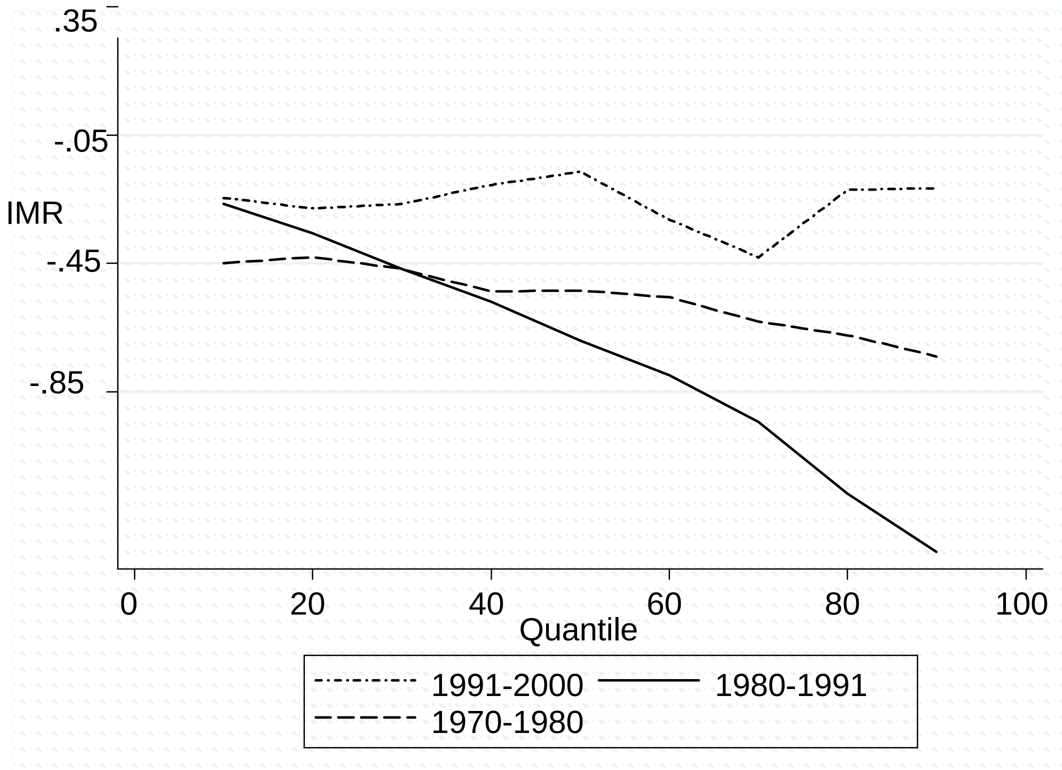
Group of counties that measure high in their development indicators.



Notes: Y-axis: Negative values indicate reduction in IMR.

Figure 3 : Marginal Impact of 0.01 Increase in Income-Related Human Development Index on the Under-1 Infant Mortality Rates (IMR).

Group of counties that measure low in their development indicators.



Notes: Y-axis: Negative values indicate reduction in IMR.

Figure 4 : Marginal Impact of 0.01 Increase in Income-Related Human Development Index on the Under-1 Infant Mortality Rates (IMR).

Group of counties that measure high in their development indicators.

Table 3: The influence of piped water on infant mortality rates: panel regression weighted by county-level population. Urban counties only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel A: 1970-1980 (Urban counties only)										
water	-0.14	-0.54 *	-0.44 *	-0.34	-0.43 **	-0.44 **	-0.37	-0.69 **	-0.70 **	-0.49
	(0.09)	(0.28)	(0.25)	(0.22)	(0.22)	(0.22)	(0.23)	(0.25)	(0.31)	(0.38)
sewage	0.02	-0.44	-0.57	-0.16	0.17	0.28	0.19	-0.41	-0.95	-0.47
	(0.17)	(0.59)	(0.50)	(0.49)	(0.45)	(0.46)	(0.51)	(0.54)	(0.65)	(0.82)
income	-51 **	-28	-45 **	-47 **	-46 **	-48 **	-53 **	-51 **	-39	-42
	(7.30)	(21)	(19)	(18)	(18)	(18)	(18)	(21)	(24)	(29)
education	-385 **	-322 **	-255 **	-285 **	-339 **	-340 **	-327 **	-326 **	-324 **	-366 **
	(23)	(72)	(63)	(64)	(67)	(69)	(73)	(76)	(87)	(105)
water	0.21 *	0.34	0.45	0.27	0.55 *	0.49	0.44	0.74 **	0.56	0.42
x income	(0.12)	(0.41)	(0.36)	(0.34)	(0.32)	(0.32)	(0.33)	(0.35)	(0.43)	(0.55)
sewage	0.47 **	-0.03	0.20	0.30	-0.10	-0.06	0.10	0.21	0.34	0.19
x income	(0.15)	(0.39)	(0.35)	(0.33)	(0.31)	(0.30)	(0.31)	(0.34)	(0.43)	(0.55)
water	-0.006 **	0.01	0.0002	-0.002	-0.002	-0.004	-0.01	-0.01	-0.005	-0.01
x sewage	(0.002)	(0.01)	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)	(0.008)	(0.01)

Notes: No. obs. 704. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions

Table 3 (con't): The influence of piped water on infant mortality rates: panel regression weighted by county-level population. Urban counties only										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel B: 1980-1991 (Urban counties only)										
water	-1.15 **	-0.28	-0.39	-0.65 **	-0.58 **	-0.79 **	-1.02 **	-1.29 **	-1.44 **	-1.35 **
	(0.20)	(0.27)	(0.27)	(0.26)	(0.25)	(0.26)	(0.27)	(0.30)	(0.35)	(0.45)
sewage	0.24	0.02	-0.37	0.28	0.05	0.12	0.34	-0.12	-0.17	-0.004
	(0.20)	(0.45)	(0.45)	(0.45)	(0.44)	(0.43)	(0.42)	(0.45)	(0.47)	(0.51)
income	-80 **	-28	-40 **	-66 **	-62 **	-74 **	-83 **	-106 **	-113 **	-133 **
	(11)	(17)	(16)	(16)	(16)	(16)	(17)	(18)	(22)	(27)
education	-192 **	-122 **	-134 **	-161 **	-160 **	-172 **	-179 **	-189 **	-190 **	-255 **
	(10)	(16)	(16)	(16)	(15)	(14)	(15)	(16)	(19)	(23)
water	1.00 **	0.51	0.48	1.13 **	0.84 **	1.18 **	1.45 **	1.70 **	1.76 **	1.72 **
x income	(0.24)	(0.39)	(0.38)	(0.38)	(0.36)	(0.35)	(0.37)	(0.39)	(0.45)	(0.56)
sewage	-0.25	0.32	0.51	-0.09	0.20	0.11	-0.12	0.27	0.25	-0.11
x income	(0.16)	(0.36)	(0.37)	(0.36)	(0.34)	(0.33)	(0.32)	(0.33)	(0.35)	(0.39)
water	-0.002	-0.003	0.0002	-0.005	-0.004	-0.004	-0.005	-0.001	-0.0003	0.0003
x sewage	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)

Notes: No. obs. 1275. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions

Table 3 (con't): The influence of piped water on infant mortality rates: panel regression weighted by county-level population. Urban counties only										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel C: 1991-2000 (urban counties only)										
water	-0.32 **	-0.02	0.03	-0.05	-0.14	-0.17	-0.34 *	-0.28	-0.30	-0.22
	(0.07)	(0.21)	(0.19)	(0.19)	(0.18)	(0.20)	(0.19)	(0.19)	(0.20)	(0.23)
sewage	0.09 *	0.10	0.01	0.11	0.09	0.10	0.07	-0.01	0.11	0.10
	(0.05)	(0.19)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.18)	(0.24)
income	-8.90	-0.91	-11	-18	-30	-31	-55 *	-41	-16	1.60
	(11)	(31)	(28)	(26)	(27)	(29)	(29)	(30)	(31)	(33)
education	-115 **	-91 **	-88 **	-104 **	-106 **	-117 **	-111 **	-117 **	-120 **	-125 **
	(3.88)	(12)	(11)	(11)	(12)	(12)	(12)	(12)	(12)	(14)
water	0.39 **	0.05	-0.07	0.13	0.26	0.38	0.60 *	0.42	0.35	0.21
x income	(0.11)	(0.35)	(0.32)	(0.30)	(0.30)	(0.33)	(0.32)	(0.32)	(0.34)	(0.40)
sewage	-0.20 **	-0.03	0.08	-0.13	-0.15	-0.05	0.01	-0.01	-0.31	-0.06
x income	(0.09)	(0.37)	(0.33)	(0.33)	(0.33)	(0.31)	(0.29)	(0.29)	(0.32)	(0.42)
water	-0.0003	-0.002	-0.001	-0.001	-0.001	-0.002	-0.002	-0.001	-0.0002	-0.002
x sewage	(0.0004)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)

Notes: No. obs. 2026. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for quantile regressions are from 2000 bootstrap repetitions

Figure 5. Marginal impact of one percentage point increase in households with piped water on the under-1 infant mortality rates (IMR): A comparison of the full sample with the urban only sample.

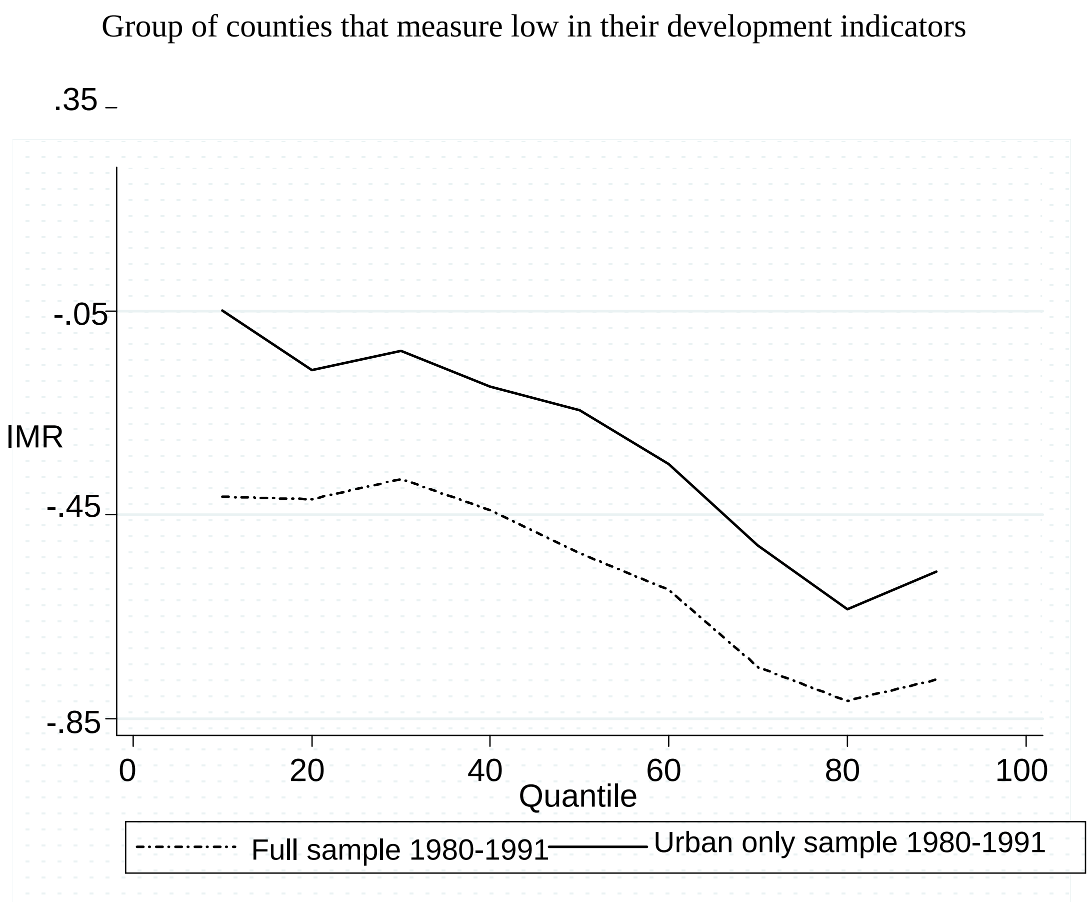


Figure 6. Marginal impact of one percentage point increase in households with piped water on the under-1 infant mortality rates (IMR): A comparison of the full sample with the urban only sample.

Group of counties that measure high in their development indicators

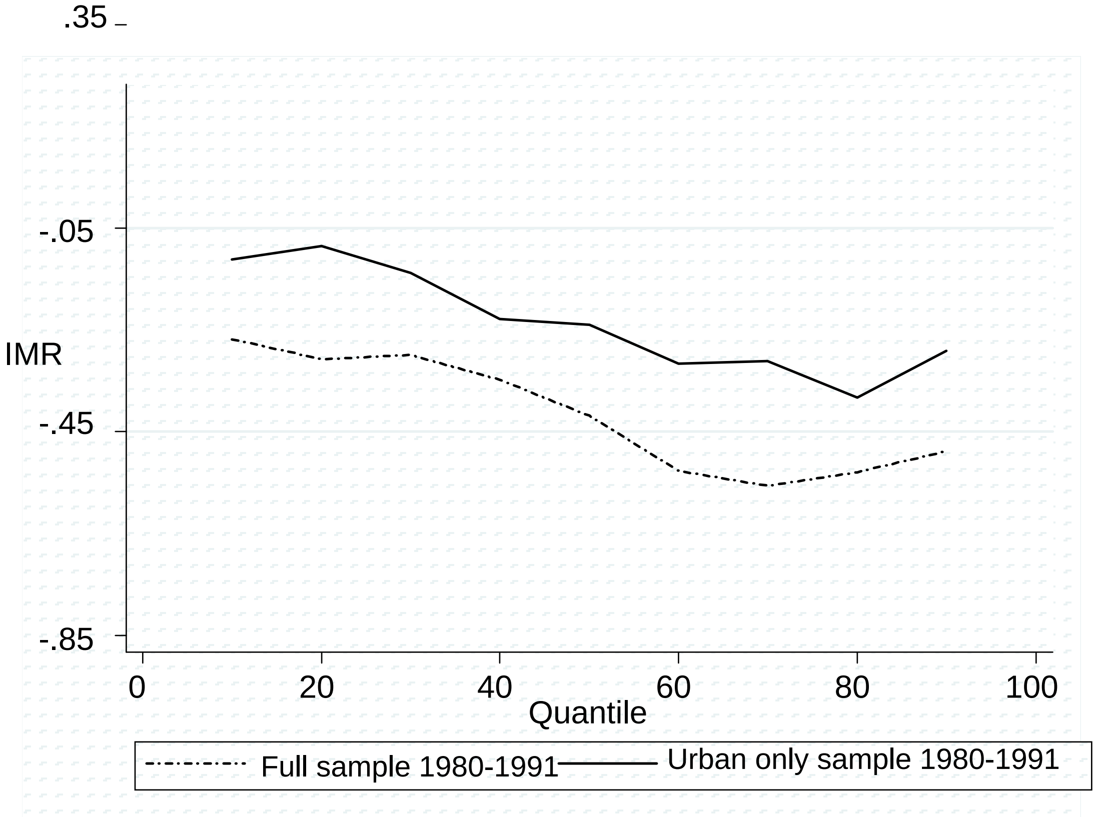


Table 4 : The influence of piped water on under-5 infant mortality rates:panel regression weighted by county-level population (1991-2000)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
water	0.20 **	-0.40 **	-0.02	0.04	-0.01	0.02	-0.02	-0.16	0.13	0.40
	(0.07)	(0.18)	(0.16)	(0.16)	(0.16)	(0.16)	(0.17)	(0.18)	(0.20)	(0.26)
sewage	-0.18 **	0.16	-0.03	-0.31 *	-0.25	-0.35 *	-0.35 *	-0.02	-0.28	-0.42
	(0.07)	(0.21)	(0.19)	(0.18)	(0.17)	(0.18)	(0.19)	(0.20)	(0.23)	(0.34)
income	-15	-45 *	-37 *	-60 **	-56 **	-68 **	-76 **	-82 **	-58 *	-24
	(11)	(23)	(20)	(19)	(20)	(23)	(24)	(26)	(29)	(38)
education	-107 **	-89 **	-94 **	-92 **	-100 **	-102 **	-110 **	-115 **	-104 **	-156 **
	(4.63)	(13)	(10)	(9.5)	(10)	(10)	(11)	(11)	(13)	(19)
fertility rate	4.97 **	3.55 **	4.24 **	5.15 **	4.84 **	5.45 **	5.13 **	4.80 **	4.97 **	4.58 **
	(0.44)	(0.90)	(0.83)	(0.79)	(0.84)	(0.88)	(0.99)	(1.06)	(1.16)	(1.4)
water	-0.45 **	0.65 **	0.02	-0.07	0.01	-0.06	0.01	0.23	-0.43	-0.67
x income	(0.12)	(0.30)	(0.26)	(0.26)	(0.27)	(0.27)	(0.29)	(0.30)	(0.34)	(0.45)
sewage	0.44 **	-0.13	0.15	0.72 **	0.58 *	0.66 **	0.58 *	0.04	0.47	1.28 *
x income	(0.12)	(0.41)	(0.36)	(0.33)	(0.31)	(0.33)	(0.35)	(0.37)	(0.44)	(0.68)
water	-0.002726 **	-0.002	-0.001	-0.003 **	-0.003 **	-0.002	-0.001	-0.001	-0.002	-0.01 **
x sewage	(0.0006)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)

Notes: No obs. 3568. ** statistically significant at the 5% level. * statistically significant at the 10% level.
Standard errors for the quantile regressions are from 2000 bootstrap repetititons.

Table 5: Estimated number of averted deaths from simulated changes in health inputs

	(1)	(2)
Regression:	Quantile	Mean
Section 1: Simulation: One percentage point increase in households with piped water		
Source of coefficients:		
Panel A: 1970-1980	95,000	67,000
Panel B: 1980-1991	104,000	166,000
Panel C1: 1991-2000	26,000	26,000
Section 2: Simulation: 0.01 increase in income-related Human Development Index		
Source of coefficients:		
Panel A: 1970-1980	97,000	119,000
Panel B: 1980-1991	14,000	45,000
Panel C1: 1991-2000	16,000	11,000

Notes: Coefficients are from Table 2

Table 6: Correlation of piped water supply and infant mortality rates: cross-section regression weighted by county-level population

Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel A: 1970										
water	0.91 **	0.12	0.15	0.21	0.39	0.62 **	0.99 **	1.22 **	1.68 **	3.11 **
	(0.10)	(0.19)	(0.23)	(0.23)	(0.26)	(0.29)	(0.42)	(0.49)	(0.78)	(0.94)
sewage	-0.54 **	-0.03	0.21	0.28	-0.02	-0.05	-0.48	-0.70	-1.18	-2.94 *
	(0.19)	(0.32)	(0.33)	(0.34)	(0.39)	(0.42)	(0.54)	(0.69)	(1.05)	(1.65)
income	14	3.49	2.67	-5.96	11	7	30	42	56	61
	(8.96)	(18)	(21)	(24)	(24)	(25)	(31)	(33)	(48)	(111)
education	-269 **	-176 **	-202 **	-217 **	-248 **	-268 **	-292 **	-311 **	-350 **	-395 **
	(8.13)	(12)	(14)	(17)	(18)	(17)	(21)	(25)	(24)	(44)
water	-0.47 **	0.69	0.62	0.61	0.20	0.01	-0.70	-1.21	-1.93	-3.60 *
x income	(0.16)	(0.43)	(0.51)	(0.51)	(0.55)	(0.57)	(0.76)	(0.89)	(1.27)	(1.93)
sewage	1.68 **	-0.23	0.28	1.03	1.16	1.60 *	2.21 **	2.32 **	2.87 *	4.30 **
x income	(0.23)	(0.61)	(0.61)	(0.64)	(0.74)	(0.85)	(1.10)	(1.18)	(1.56)	(1.83)
water	-0.01 **	-0.003	-0.01 *	-0.02 **	-0.01 **	-0.02 **	-0.02 **	-0.02 **	-0.02	-0.01
x sewage	(0.003)	(0.006)	(0.006)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.01)	(0.023)

No. obs. 3658. Constant included. ** and * indicate significance at the 5% and 10% level, respectively.
Standard errors in quantile reg are from 2000 bootstrap repetitions.

Table 6 (con't): Correlation of piped water supply and infant mortality rates: cross-section regression weighted by county-level population

Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel B:1980										
water	1.25 **	0.57 **	0.55 *	0.82 **	0.82 **	0.82 **	0.91 **	1.28 **	1.60 **	2.10 **
	(0.12)	(0.22)	(0.30)	(0.34)	(0.35)	(0.41)	(0.32)	(0.36)	(0.38)	(0.62)
sewage	-1.98 **	-1.05 **	-1.36 **	-1.94 **	-1.98 **	-2.08 **	-2.05 **	-2.47 **	-2.27 **	-1.29
	(0.23)	(0.26)	(0.30)	(0.37)	(0.36)	(0.43)	(0.46)	(0.67)	(1.03)	(1.71)
income	-40 **	-29 **	-43 **	-55 **	-61 **	-68 **	-66 **	-63 **	-53	-12
	(5.56)	(15)	(18)	(14)	(14)	(16)	(14)	(17)	(33)	(94)
education	-172 **	-93 **	-102 **	-117 **	-126 **	-146 **	-177 **	-179 **	-228 **	-291 **
	(8.38)	(16)	(18)	(16)	(16)	(17)	(20)	(25)	(32)	(51)
water	-0.57 **	-0.20	-0.07	-0.12	-0.01	0.12	0.18	-0.35	-0.74	-1.52
x income	(0.14)	(0.31)	(0.47)	(0.53)	(0.52)	(0.57)	(0.45)	(0.44)	(0.48)	(0.98)
sewage	2.51 **	1.40 **	1.71 **	2.54 **	2.68 **	2.84 **	2.72 **	3.44 **	3.15 **	2.35
x income	(0.30)	(0.35)	(0.43)	(0.52)	(0.51)	(0.55)	(0.65)	(0.88)	(1.3)	(1.85)
water	-0.008 **	-0.004	-0.003	-0.01 **	-0.01 **	-0.01 **	-0.01 **	-0.01 **	-0.01 **	-0.01
x sewage	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.01)

No. obs. 3658. Constant included. ** and * indicate significance at the 5% and 10% level, respectively.
Standard errors in quantile reg are from 2000 bootstrap repetitions.

Table 6 (con't): Correlation of piped water supply and infant mortality rates: cross-section regression weighted by county-level population

Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel C: 1991										
water	-0.29 **	-0.50 **	-0.40 **	-0.20	-0.35	-0.47	-0.40	-0.61 *	-0.22	-0.53 *
	(0.07)	(0.15)	(0.17)	(0.23)	(0.29)	(0.33)	(0.31)	(0.31)	(0.33)	(0.32)
sewage	-0.66 **	-0.51 *	-0.73 **	-1.00 **	-0.78 **	-0.67 **	-0.05	-0.46	-0.70 **	-0.59 *
	(0.08)	(0.26)	(0.26)	(0.26)	(0.30)	(0.34)	(0.35)	(0.32)	(0.32)	(0.31)
income	-127** **	-113 **	-124 **	-120 **	-117 **	-140 **	-88 **	-171 **	-139 **	-189 **
	(7.85)	(16)	(19)	(23)	(22)	(28)	(31)	(32)	(42)	(35)
education	-82** **	-69 **	-73 **	-77 **	-87 **	-85 **	-109 **	-84 **	-85 **	-65 **
	(3.86)	(6.51)	(9.70)	(11)	(12)	(16)	(17)	(16)	(15)	(16)
water	0.76 **	1.05 **	0.94 **	0.62 *	0.83 *	1.10 **	0.95 *	1.47 **	0.76	1.23 **
x income	(0.11)	(0.24)	(0.26)	(0.37)	(0.47)	(0.56)	(0.55)	(0.54)	(0.61)	(0.56)
sewage	1.00 **	1.09 **	1.43 **	1.71 **	1.24 **	0.99 *	0.04	0.67	0.98 *	0.86
x income	(0.13)	(0.52)	(0.50)	(0.44)	(0.46)	(0.51)	(0.55)	(0.51)	(0.50)	(0.54)
water	-0.002 **	-0.004 **	-0.004 **	-0.003 **	-0.002 *	-0.002	-0.001	-0.003 *	-0.003	-0.003
x sewage	(0.0006)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)

No. obs. 3658. Constant included. ** and * indicate significance at the 5% and 10% level, respectively.
Standard errors in quantile reg are from 2000 bootstrap repetitions.

Table 6 (con't): Correlation of piped water supply and infant mortality rates: cross-section regression weighted by county-level population

Method	Mean	Quantile								
Percentile		10	20	30	40	50	60	70	80	90
Panel D: 2000										
water	-0.42 **	-0.40 **	-0.56 **	-0.62 **	-0.50 **	-0.41 *	-0.51 **	-0.66 **	-0.28	-0.13
	(0.06)	(0.18)	(0.17)	(0.20)	(0.20)	(0.22)	(0.26)	(0.26)	(0.26)	(0.23)
sewage	-0.22** **	-0.37 **	-0.31	-0.40 *	-0.50 **	-0.44 **	-0.32	-0.07	-0.18	-0.26
	(0.05)	(0.15)	(0.20)	(0.23)	(0.20)	(0.18)	(0.22)	(0.27)	(0.26)	(0.25)
income	-132 **	-127 **	-146 **	-147 **	-149 **	-134 **	-141 **	-155 **	-110 **	-121 **
	(7.80)	(20)	(20)	(23)	(27)	(33)	(35)	(32)	(29)	(26)
education	-87 **	-43 **	-54 **	-72 **	-75 **	-87 **	-85 **	-85 **	-103 **	-110 **
	(4.24)	(11)	(13)	(14)	(14)	(15)	(18)	(21)	(20)	(27)
water	0.97 **	0.81 **	1.11 **	1.23 **	1.09 **	0.93 **	1.10 **	1.37 **	0.78 *	0.61
x income	(0.10)	(0.33)	(0.30)	(0.34)	(0.35)	(0.40)	(0.46)	(0.46)	(0.43)	(0.37)
sewage	0.47 **	0.79 **	0.64 **	0.76 **	1.03 **	0.89 **	0.63 *	0.20	0.18	0.34
x income	(0.08)	(0.23)	(0.30)	(0.36)	(0.33)	(0.32)	(0.36)	(0.44)	(0.46)	(0.52)
water	-0.002 **	-0.003 **	-0.002 **	-0.003 *	-0.004 **	-0.003	-0.003	-0.002	-0.001	-0.001
x sewage	(0.0004)	(0.0009)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)

No. obs. 3658. Constant included. ** and * indicate significance at the 5% and 10% level, respectively.
Standard errors in quantile reg are from 2000 bootstrap repetitions.

Figure 7. The association between one percentage point increase in households with piped water supply and the under-1 infant mortality rates (IMR), as estimated in the cross-sectional regressions.

Group 1: Counties that measure low in their development indicators.