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The effects of water supply on infant and childhood mortality: a review of historical evidence*

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Abstract

The provision of clean water is mentioned as an important factor in many studies dealing with the decline of mortality in Europe during the late nineteenth and early twentieth centuries. In developing countries too, improved water supply is assumed to have a strong impact on mortality. When studying the effect of water supply on public health, researchers are confronted with many methodological problems. Most of these also apply to historical studies of the subject. We review the evidence from this historical research, taking into account the methodological problems observed in contemporary impact evaluation studies, and we use more refined data from the Dutch city of Tilburg, enabling us to overcome many of these shortcomings. Finally, we discuss some factors which may explain why we failed to discover an effect of the availability of piped water on the level of childhood mortality.

In November 1980 the United Nations General Assembly declared the 1980s the International Drinking Water Supply and Sanitation Decade. It was anticipated that guaranteeing 'reasonable access to safe water' to all inhabitants of the world by 1990 would be followed by significant improvements in health and social conditions (WHO 1992:106). This ambitious task has not only increased the pace of the provision of improved methods of water supply, it has also stimulated research in which the effect of water supply on public health is evaluated (Lindskog and Lundqvist 1989:10).

Students of medical and demographic history have long discussed the influence of sanitary reforms, in particular improved water supply, on the reduction in mortality in the nineteenth and early twentieth centuries. According to McKeown (1976:121-123), the decline in airborne diseases, due to nutritional improvements, had been much stronger than the decrease in water- and foodborne diseases, due to advances in the purification of water and

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sewerage disposal. The role played by municipal sanitation thus has only been a secondary, reinforcing one. Demographers, economic historians and historical geographers have tried to put McKeown's hypothesis to a rigorous test by turning their attention from the national level to the local level of the municipality, the location of the principal agencies responsible for preventive health measures such as improved water supply and sanitation. Their research was based on large sets of local data relating to a variety of factors which could, in principle, have an impact on mortality. In evaluating the effect of water supply, they made use of much more refined statistical techniques than McKeown did. Moreover, their studies were no longer restricted to the special case of Britain but focused on other European countries as well.

In our paper we try to provide a synthesis of this more recent, scattered historical literature dealing with the impact of water supply on infant and child mortality. Our review evaluates only those studies which have attempted to quantify the resulting differences in mortality. As Briscoe, Feachem and Mujibur Rahaman (1986) have argued, this historical literature 'remains largely unknown to those concerned with evaluating the health impacts of water supply and sanitation programs and, yet, is of surprising relevance' to many of the questions plaguing them. Exploring the effects of water supply on health in different historical contexts may not only provide lessons for the present; reviewing the mechanisms through which water supply affects health in a contemporary context may be very relevant to historical research into the determinants of mortality decline in nineteenth and twentieth-century Europe. Understanding the factors which determine the effect of water supply on child mortality in today's developing countries will make it easier to identify the factors which have helped to reduce the excessive levels of child mortality in the Western world. By placing the problem in this wider perspective, historians may become aware of the complexity of the relationship between water supply and health.

We present an overview of studies which have tried to measure the mortality effect of improved water supply (that is improved quality and/or increased quantity) in France, the Netherlands, Germany, Sweden and the USA. We merely repeat the conclusions reached by the authors of these studies, and shortcomings of the studies are only explicitly mentioned when they are discussed by the authors themselves. In a following chapter, the results of our evaluation are discussed in the context of more recent studies dealing with the health effects of water supply in the developing world. Next, some of the methodological problems encountered in these 'impact studies' are discussed and we express our views about the earlier-reviewed historical studies. In a final paragraph, we present the results of a study in which the effects of water supply could be measured in a more refined way.

Estimating the impact of water supply on mortality in historical populations

Four groups of diseases may in principle be affected by improved water supply. Clean water may prevent the spread of waterborne diseases, like cholera and typhoid, which are directly transmitted when water contaminated by faeces or urine is drunk or used in the preparation of food; increased quantity and access to water may reduce the impact of water-washed diseases, like the common diarrhoeal diseases, which are transmitted by faecal-oral routes other than the ingestion of contaminated water, by providing the opportunity to improve hygiene: washing hands, face, eyes, cooking utensils, and cleaning floors. Improved water may lead to decreased contact with unsafe water sources, which decreases the impact of water-based - diseases, where water provides the habitat for intermediate host organisms in which some parasites spend part of their life cycle; and of water-related diseases, like malaria, where water provides a habitat for insect vectors of disease (Bradley 1977). Improvements in water may improve child health through other mechanisms as well. A number of studies have reported that improved water supplies have saved women time and energy, this additional time being spent on food-related activities, including feeding their children more frequently (Esrey 1994).

Although the advantages of improved water supply seem clear, the connection between water supply and mortality could not be convincingly demonstrated in many historical studies. This is due in part to serious shortcomings in data and research methods.

Most historical studies dealing with the effect of water supply are rather simple and superficial. Researchers concluded that there was a causal relationship between the improvement of water supplies or sewerage systems on the one hand and the decrease in mortality on the other hand because, at the subnational level, both developments took place simultaneously. The most common approach was rather intuitive: judgements were made by examining graphs or tables that gave annual total or cause-specific mortality rates, and more refined forms of time-series analysis (e.g. Hemminki and Paakkulainen 1976; Mackenbach and Looman 1988) were never used. Another approach has been to use data for cities or districts within cities in a cross-sectional analysis indicating, on one hand, the availability of sanitary provisions or expenditures on the construction of waterworks, and mortality rates on the other hand. Sometimes both approaches were followed in the same publication.

One of the earliest examples of research along the first line is the work of Meeker (1972). He inferred the impact of water supply improvements on mortality in American cities from the reduction in typhoid fever death rates following the filtration of their public water supplies. It appeared that the filtration of water dramatically reduced typhoid fever mortality. To measure the effects of public health reform in Boston on mortality levels over time, Meckel (1985) took the introduction of a central water supply and the construction of a city-wide sewerage system as the chief indices of that reform. Both were relatively easy to date and plot as they were expanded to reach all areas of the city. By comparing the time trends of water supply and mortality, Meckel concluded that the effect of the new water supply systems seemed to have been a pronounced reduction in deaths from waterborne diseases. At the same time, he stressed that water supply constituted only one of several developments which combined to make the Boston environment radically different from what it had been before.

Early examples of cross-sectional analysis include the work of Higgs and Booth (1979). They used typhoid fever death rates as a proxy for sanitary conditions. Using data on childhood mortality (ages 1-4) for 335 wards in 17 cities in the USA around 1890, and applying multiple regression techniques, they observed that these typhoid fever death rates were positively and significantly related to the child death rate.

Condran (1987; see also Condran and Cheney 1982) studied the effect of water supply in Philadelphia in the period 1890-1920. At the time when the filtration of water was introduced, the city was divided into six water districts that received water from different waterworks. The timing of the introduction of water filters differed across the six districts, stretching from 1902 to 1909. This enabled the authors to examine the relationship between the conditions of the water supplied to the city's residents and their cause-specific death rates. Only deaths from typhoid fever declined following water filtration. Even diseases like infant and childhood diarrhoea, dysentery and enteritis, which are believed to be waterborne, did not decline immediately following filtration.

Condran and Crimmins-Gardner (1978) examined the mortality decline for some specific causes of death in the last decade of the nineteenth century across US cities in relation to several specific public health measures. Data at specific points in time on miles of sewers, gallons of water consumed and expenditures on construction of waterworks were used in a cross-sectional analysis of the relationship between public health measures and mortality rates. Expenditures on the construction of waterworks and the miles of waterworks per person showed significant negative correlations with mortality rates in 1900 in the middle and old age groups. At ages below one and 1-4, correlations were very low. The relationship varied by region. Deaths from diarrhoeal diseases and typhoid, two disease categories which should have been directly affected by the construction of sewers and waterworks, showed no correlation with the expenditures on the construction of waterworks and the miles of waterworks per person.

Gaspari and Woolf (1985) also applied a cross-sectional approach, using information for 122 US cities with over 30,000 residents in 1910. The authors used the crude death rate as dependent variable and as independent variables the availability of a filtered water supply in 1910, population density, length of the sewer system, the number of physicians per capita, the wage level, the percentage of immigrants and the level of literacy. Whereas cities which invested in sewer systems experienced significant decreases in mortality, no impact of an urban water filtration system on mortality was found. Filtered water did reduce typhoid death rates in the cities but dirty drinking water was not the sole cause of typhoid, and typhoid death rates were usually a small percentage of the total deaths in cities. The filtration systems thus may have had some disease-specific benefits, but a decrease in crude mortality could not be attributed to them.

Preston and Van de Walle (1978) in their study of mortality in the French *départements* of Seine, Rhône and Bouches-du-Rhône in the period 1816-1906 had no measurable indicators of the quality of water supply and sewage disposal that could be used to gauge their importance in regional differentials in mortality. The authors therefore could use only impressionistic evidence left by contemporaries and historians' accounts of public works. They nonetheless explained the differences in trends between regions by the quality of the water supply and techniques of sewage disposal. Goubert (1984) was able to discern for the French city of Rennes a period before and after 1833 when public water supplies were introduced and when suddenly the impact of typhoid on the general mortality rate decreased by more than half. Refined quantitative estimates of the effect of public water works and the control of faecal dangers on the mortality decline were not presented, however.

Swartsenburg (1981) used data on 28 Dutch cities in the period 1869-1909 to study the influence of the drinking water situation on mortality. The dependent variables used were the proportional mortality ratios (PMR: the percentage of all deaths at ages 50 and above) for airborne, waterborne and foodborne diseases, and for all causes combined, and IMRs and Crude Death Rates (CDR) for the same three cause-of-death categories and for total mortality. Univariate correlations between the period in which piped water became available and (total) PMR and CDR were weak, and the correlation with total IMR was even non-existent. For airborne and foodborne PMRs, water supply did not have an effect either. In 1895-99 waterborne disease mortality was lower in cities where piped water had become available at an earlier date. Decreases in PMR and IMR between 1869-71 and 1907-09 were larger in cities where piped water was in use relatively early.

Vögele (1993) studied the situation in the ten largest German towns in 1910. The decrease in mortality in these cities was mainly caused by a strong decrease in mortality due to diseases of the digestive system. Yet as early as the 1870s and 1880s mortality from typhoid fever in these cities had considerably decreased whereas the extension of the water supply did not take place before the 1870s. The sanitary reforms thus did not set the pace for the mortality decrease but they can be interpreted as a catalytic agent, with the help of which typhoid fever could be eliminated more quickly. In addition, they protected the population from further epidemics.

A much more refined study of the German situation was conducted by Brown (1990) and was related to the decline in urban mortality in the period 1876-1912. Brown's main purpose was to test whether urban mortality was reduced by sanitary improvements irrespective of changes in income. Detailed data were available for the period 1888 to 1912 for a large number of German medium-sized and large cities, initially 55, by 1908 over 90. Data included the extent of the provision of water (the number of properties connected to the system and the length of mains), sewerage systems (population served, the number of properties, and the length of mains), and the dominant type of waste disposal system used for human waste (sewerage system or privies). The data on water supply were further improved by a measure for water quality: dummy variable of groundwater sources or springs versus surface water. Information was also available on real wages, the number of hospital beds, the availability of medical personnel, density of settlement, etc. Dependent variables included overall crude mortality, infant mortality, mortality from typhoid fever and diarrhoeal diseases, and age-specific mortality. Apart from the data on age-specific mortality, which were available at five-year intervals from 1890 to 1910, all other data were supplied on an annual basis for the period 1888 to 1912. Statistical analysis of the relationship focused on two separate issues: the effectiveness of the interventions and their effect on the decline in mortality.

For infant mortality, mortality under ten, and typhoid and diarrhoeal disease mortality, sanitary practices did make a difference although some anomalous results stood out, one being a positive effect of sewerage systems on both mortality from diarrhoeal diseases and infant mortality. The importance of the improvement in the urban environment for the mortality decline was expressed as a percentage reduction of mortality in relation to the annual change in sanitary services. The decline in typhoid mortality resulting from sanitary reform measures was significant, accounting for about one-half of the actual drop of 80 per cent. The impact of reform on infant mortality and diarrhoeal disease was from one-quarter to one-third of the actual decline. For those under ten, mortality fell from one-third to almost one-half between 1890 and 1910, and changes in sanitation accounted for 30 to 40 per cent of the decline. Brown concluded that improved water supplies, sewers and abandoning the use of the privy cut typhoid fever substantially. These measures were effective and had an important impact. Although the evidence was more mixed for infant mortality and intestinal diseases, it still pointed to the effectiveness of improvements in water supplies and the removal of privies in contributing to their decline. The evidence for age-specific mortality supported these conclusions as sanitary measures accounted for at least one-third of the actual fall in mortality for those aged 0 to 10.

Three studies were published on the Swedish situation. Castensson, Löwgren and Sundin (1988) compared series of death rates with dates for the introduction of piped water supply facilities in the Swedish cities of Malmö, Jönköping, Linköping and Norrköping, and concluded that piped water did not seem to be a very important factor in the reduction in mortality during the period 1860-1920. There were no changes that were noticeably connected to the year of construction of water pipes or to the year of construction of sewerage systems. A more recent Swedish study used information only on the city of Linköping (Nilsson 1994:154-159:206-207). By 1876 Linköping had water and drainage systems and from that year onwards there were considerable differences in infant mortality between different parts of the town. Three specific quarters stood out as having an especially high rate of infant mortality. These quarters had in common that their water and drainage systems were poor in scope, or indeed non-existent. By reconstructing the introduction of water and drainage systems and their properties year by year, it was demonstrated that infant mortality declined fastest in those areas which were first connected to the network. Yet infant mortality also declined steeply in other parts of the town. It is feasible that through synergy, an improvement in one part of town resulted in improvements in neighbouring parts of the town.

A more detailed analysis, based on individual records from the Demographic Data Base in Umeå, was used to study the effect of sanitary improvements in the town of Sundsvall in northern Sweden in the 1880s (Edvinsson 1993, 1995). In 1879 the town was provided with piped water and a sewerage system with free access to water for all households connected to the system. Deaths from gastro-intestinal diseases at earlier ages, although considered the most important indicator of sanitary standards, increased in Sundsvall after the introduction of the water supply. The expected relationship between sanitary measures like improved access to water, and lower mortality, thus did not seem to apply to the city as a whole. The sanitary improvements were, however, unevenly spread throughout the town. Only the old quarters were connected to the piped water system, which meant that about half the population had to rely on water from wells. As the unregulated area was mainly populated by working-class households, sanitary conditions largely became socially determined and these discrepancies prevailed well into the twentieth century. Looking at the mortality patterns in different parts of the town, depending on whether or not they were connected to the piped water system, made it possible to study the effects of sanitation in more detail. The analysis was based on data for 1319 infants born between 1880 and 1887, distinguished according to place of birth. It was assumed that this was also their place of residence during the observed period. Other variables included were social status, sex and birth order, legitimacy and age of the mother.

It appeared that mortality was lower among infants born in areas equipped with piped water where the sanitary improvements had been put into action. This difference could, however, not be explained by deaths caused by gastro-intestinal diseases, which were the main group of diseases responsible for high mortality. Instead, the decisive impact was found in deaths from airborne epidemic diseases. The high mortality in the unregulated areas from diseases like smallpox, scarlet fever and diphtheria was attributed by contemporary physicians to overcrowded living conditions in working-class families. A Cox model showed no significant differences between both areas in the level of infant mortality. Among children (1-14 years), place of residence had a statistically not significant effect on the mortality pattern. Working-class children had a much higher mortality, regardless of where they lived in the town. Compared to infants, gastro-intestinal diseases played a minor role in this age group. As airborne epidemic diseases and respiratory diseases dominated, the analysis of these causes resembled the results from the overall analysis.

The results of this study thus show that the sanitary improvements did not favour the population to any great extent. Those diseases that could be expected to have been most sensitive to access to water and food control struck infants from different parts of the town alike. In line with this, the summer peak in mortality from gastro-intestinal diseases grew more and more dominant after the introduction of piped water until the late 1880s and early 1890s (Edvinsson 1993). Edvinsson argued that the fact that breastfeeding was uncommon could override any differences that could be caused by social conditions and spatial variables. As infections of the digestive system could also be sensitive to other influences, one should be cautious when measuring the success of sanitary improvements by the development of this group of diseases. Furthermore, the advantages of living in the town centre could have been offset by other circumstances, such as problems with drainage in the low-lying parts of the town. It might also be indicated that sanitary measures could not keep pace with the problems created by the fast-growing population. It is questionable whether the measures taken really succeeded in improving the sanitary conditions. It was surely an advantage that access to water was easier and it was probably more plentiful, thereby facilitating the possibilities of keeping the homes clean and hygienic; but in the early stage of the construction process, the quality of the water was not satisfactory. An analysis in 1887 proved that the water was not suitable for drinking, was too warm and contained too much organic material.

We may conclude that in historical studies strong differences were observed in the effect of water supply, ranging from no significant relationship to a clear positive effect on the decrease in mortality. In many studies dealing with the health effects of water supply in contemporary Third World countries more-or-less comparable results were reached.

The effect of water supply on mortality in contemporary populations

During the past decades, several attempts have been made to evaluate the conclusions of studies describing the effectiveness of water and sanitation interventions on the incidence of morbidity and childhood mortality in developing countries (Esrey and Habicht 1986; Esrey et al. 1991; Pisani 1994). Although the evidence was in favour of positive effects of one or more components of water supply and sanitation, these reviews have also provided contradictory and often confusing results and conclusions. The beneficial health effects following improvements in water and sanitation appeared to be dependent on the type of intervention, the level of environmental contamination, the presence or absence of certain risk factors and the health indicator used.

A study by the United Nations (1985:237-243), based mainly on World Fertility Survey data and relating to six countries (Ghana, Nigeria, Sierra Leone, Sudan, Sri Lanka and Jamaica), revealed that households with more modern facilities of water supply exhibited the lowest mortality ratio: ratio of observed to expected deaths during infancy and early childhood. In some areas, however, piped water was more susceptible to the risk of contamination than water from wells or streams. In those cases, the net effects of being located in a dwelling with modern facilities like running water, controlling for other socio-economic variables, were reduced, often considerably, and were reversed in some cases. Furthermore, there was very little consistency in the relative position of the water-supply variables across the different countries, with positive and negative coefficients for the same variables appearing nearly at random. The observed sharp reduction of differentials according to dwelling characteristics like water supply in multivariate analysis suggests, according to the authors, that attitudes and behaviour related to health practices and personal hygiene, which are considered to be greatly affected by education, may be more important than physical facilities. At the multivariate stage, this could explain the greater differentials by socio-economic factors, which are considered to be closely related to attitude and behaviour, than differentials by dwelling characteristics.

Type of intervention, as well as the presence of risk factors, was explicitly taken into account by Esrey and Habicht (1988) who investigated whether the availability of piped water and the presence of toilets provided different protection to infants of literate mothers compared with those of illiterate mothers. Providing piped water could either improve the quality of drinking water or increase the quantity of water used. A difference in the quality of water would benefit only the illiterate, since the literate would already be taking steps to decontaminate drinking water in some manner, such as by boiling, to protect their infants. Providing good-quality water would thus close the gap and result in an antagonistic interaction with literacy. On the other hand, if piped water increased water availability and use for domestic hygiene, infants of literate mothers would benefit more than infants of illiterate mothers if literate mothers knew that they could use the water to practise better domestic hygiene. Literacy would thus be synergistic with water quantity in lowering infant mortality rates of the literate and widening the gap. It appeared that among the illiterate group an infant was 1.36 times more likely to die if no piped water was available compared with having piped water, while among the literate group the relative odds of an infant dying was more than doubled if no piped water was available. Having piped water and a literate mother was associated with a larger drop in IMR than would be expected by adding each effect separately. The fact that the effect on IMR was largest for the literate group suggests that improvements in the quantity of water, and therefore its use, were more prevalent, or produced larger health effects than did improvements in water quality.

Butz et al. (1984) showed that the effects of breastfeeding and water supply were strongly interactive and that they changed systematically during the course of infancy. Infants born to households with piped water experienced significantly lower mortality in days 8-28, compared with infants in houses using other water sources. It appeared that the presence of piped water was associated significantly with lower mortality only for infants who were breastfed little or not at all. It appeared furthermore that in nearly all instances the presence of toilets was more important than the presence of piped water. As a consequence, breastfeeding declines were less harmful in households with either toilets or piped water and much less harmful in households with both. It also appeared that breastfeeding effectiveness fell rapidly during the course of infancy. Three explanations may be given for the declining association of breastfeeding with mortality during infancy: decreasing exposure to fatal pathogens with age; sample heterogeneity: infants who are most susceptible to a fatal factor (feeding other than breastfeeding) die early, leaving the less susceptible infants to survive; and increasing resistance to the detrimental factor with age. This last explanation was considered the correct one: the infant matures and can adequately ingest and digest other foods and is less susceptible to enteric pathogens. Infant maturation may also explain why in spite of increasing exposure with age to the environment and its pathogens, improved water and sanitation prevent fewer deaths in later months.

Merrick (1985), who studied the role of water supply in child mortality differentials in Brazil in 1970 and 1976, stressed the need to take into account variables associated with the real consumption of this particular service by households. A household may not consume a service such as water because the supply network is not geographically accessible or because that household chooses not to do so for reasons of price or income limitation.

The fact that the studies assessing the child health effects of water supply have resulted in contradictory outcomes is also partly due to the methodological deficiencies of many of these studies. Lindskog, Lindskog and Wall (1987) argued that the design and method of data collection had an effect on the results. They concluded that positive effects of water supply were shown in those studies which were based on 'weaker' methodologies. Several well-designed studies failed to show clear-cut results, probably because a change was multifactorial: an intervention was only one of several influential factors. Excellent reviews of the methodological problems are given by Briscoe et al. (1986), Esrey and Habicht (1985, 1986) and Blum and Feachem (1983).

Methodological problems in studying the effect of water supply on health

Several designs for the evaluation of the effect of water supply on health are possible. Following Briscoe et al. (1986) we distinguish between studies requiring observations at more than one point in time (quasi-experimental and cohort studies) and studies in which measurements of exposure and health status are made at a single, common point in time (cross-sectional studies).

Many historical studies can be considered to be based on a quasi-experimental design, in which water supply is applied to a community and withheld from (similar) control communities. Implementing such a study in a historical context causes a lot of problems.

First, there is the difficulty of predicting the sign of the correlation between waterworks and mortality rates (Meeker 1972). On the one hand, if waterworks reduce the number of deaths the mortality rates should have a negative relationship with these public health measures. On the other hand, if high mortality leads cities to make major expenditures on waterworks and sewers, the relationship with mortality may well be positive.

A second problem is which dependent variable has to be used. Different aspects of the public health programs were likely to affect some diseases and not others. Some causes of death like cholera infantum, diarrhoea and typhoid should have been directly affected by public health measures such as sewers and waterworks. A breakdown of deaths into categories reflecting their relationship to public health practices may provide a way of assessing the importance of these practices. Yet although other infectious diseases such as tuberculosis, diphtheria, measles, influenza, scarlet fever, and whooping cough may have been more strongly affected by general improvements in hygiene and by quarantine and disinfection practices, there is reason to believe that not only mortality from waterborne but also mortality from airborne diseases is influenced by water quality and quantity. Bouts of diarrhoea leave the host in a depleted and weakened condition and hence more susceptible to death from respiratory infection.[1]

In most countries data limitations preclude a sophisticated study of the effect of improved water supply on mortality. Year-on-year comparative statistics for the late nineteenth and early twentieth centuries on the construction of waterworks or on changes in consumption of water in individual cities on one hand, and on health indicators on the other hand, are almost always lacking. A further complication is the necessity to apply time-lags. The introduction of central water systems does not always have an immediate effect on disease. Even when a city has begun filtering its water, not everyone benefits immediately. Water mains do not reach the entire population; some still drink from polluted wells or unfiltered city-supplied water.

Quasi-experimental designs can be based on 'internal' comparisons, in which case mortality before the intervention is compared with mortality after the intervention, or on 'external' comparisons, in which health in the treatment group is compared with that in the control group some time after the intervention. The most fundamental problem of historical studies is the lack of comparability of 'treatment' and 'control' groups.

Historical studies sometimes completely lack adequate control. The absence of an external control group which did not undergo a health intervention makes it impossible to distinguish between health improvements resulting from water supply and health improvements that would have occurred in any case through other social, economic and environmental changes (Blum and Feachem 1983). Moreover, even if no health changes appear to have occurred, no conclusions can be made because in the absence of health or sanitation improvements, health status may have declined. Alternatively it is possible that without the water and sanitation facilities health may have improved, but these facilities could have resulted in the spread of infection (Sorkin 1988:81). In historical studies the practice is sometimes to select a single control community and compare it to a single community in which a health intervention took place, the one-to-one comparison. In this case too, it is impossible to draw statistically valid conclusions because the sample size in each category is one. A way to avoid the one-to-one comparison problem is to study one community and identify one or more internal control groups, for example districts, defined by their use of facilities.

As it is impossible to identify all the possible differences between comparison groups which could influence the results, one could choose to apply a design that randomly allocates groups either to receive the intervention or to serve as the control, as this can transform the uncertainty about these differences into a statistical probability statement (Esrey and Habicht 1985). It is clear that this solution is impossible in historical research. What is more, the number of observations will usually be much too small to test health differences in a statistically adequate way because the number of units (communities) should be in the order of 10 to 20 divided equally into improved and unimproved groups to adequately test health differences. There are many other problems affecting the lack of comparability between units in historical research.

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[1] A new term, 'the Mills-Reincke phenomenon' was even coined to express the relation between changes in mortality from all causes and from typhoid fever that were induced by water filtration in the first decade of this century. This multiplier was variously estimated to be in the range of 1.5:1 (Binghamton NY) to 1.8:1 (Lawrence MA). The principal causes of death through which the multiplier is typically found to operate are diarrhoea and gastro-intestinal diseases but respiratory diseases also play a significant role (Preston and Van der Walle 1978).

Experience with these designs has shown that the treatment and control groups are seldom comparable and that it is extremely difficult to adjust for this lack of comparability using statistical methods (Briscoe et al. 1986:33-34). As is the case with many contemporary studies, it is usually impossible to determine how much of the difference in effects observed between communities was a result of the different interventions, the general secular trends that were different between the communities, or the sudden unexpected events, such as epidemics, that affected only certain communities.

Behavioural differences associated with the intervention may have arisen which could not be controlled. Communities which received water supply may have come to act differently in other health-related fields as well because they 'received' the intervention and this could bias the results (Esrey and Habicht 1985). In the historical context, it is particularly important to realize that the public health movement included not only the provision of central water supplies and sewerage systems but also plans for street cleaning, improvements in slum housing, inspection of food and milk etc. (Condran and Crimmins-Gardner 1978). Water or sanitation interventions may be accompanied by additional inputs such as health education. All these factors which could have had an influence on mortality rates changed simultaneously. In this case the assumed intervention (water or sanitation) was not the true intervention (water or sanitation and health education) and the true benefit of water or sanitation cannot be estimated (Esrey and Habicht 1985).

The conditions prevailing before the adoption of a specific public health improvement are often implicitly assumed to be equal; differences between cities in climate, drainage conditi-ons, and the amount of crowding render this assumption untenable. Some cities may have had water supplies that did not require filtering. It would therefore follow that the omission of a variable specifying the initial conditions in a time-series analysis would bias the coefficients for the public health variables (Meeker 1972).

Blum and Feachem (1983) referred to two other shortcomings of research in developing countries which also apply to historical studies. First of all, there is the failure to analyse mortality by age. This can be a serious problem as diseases considered in environmental impact studies are unevenly distributed among various age groups (Esrey and Habicht 1986). A second factor influencing the validity of the studies relates to the fact that diarrhoeal diseases, and their associated infections, are markedly seasonal. Few studies analyse their data in such a way that possible differences in impact in different seasons can be measured.

Cross-sectional studies examine the concurrent presence or absence of good water or sanitation facilities in relation to health, enabling researchers to make probability statements of association between both factors. In theory, because of the settings, cross-sectional studies are restricted to the generation of hypotheses and cannot be used for testing hypotheses. However, where the exposure status of an individual is more-or-less permanent, as is generally the case with exposure to inadequate water supply and sanitation conditions, then an individual's current exposure status may be an adequate measure of previous exposure status, and a cross-sectional study may be used to test causal hypotheses (Briscoe et al. 1986).

Many of the problems mentioned above also play a role in this design. Data indicating the extent of sewers and waterworks in individual cities or districts within cities are rarely available and most data are unusable for comparisons across cities. What is more, a necessary condition for a satisfactory study is the use of a large sample of cities, each with a different sanitary situation (Vögele 1993). An important problem in using city-level data is the level of aggregation. The more city-wide measures conceal a large variance in both mortality and in the provision of a sanitary environment, or the level of exposure to infectious disease, the more statistical analysis will be faced with problems of errors in variables, which bias estimates of impacts downward (Brown 1990).

Furthermore, there may be confounding differences between the comparison groups which are reflected in different disease rates unrelated to the intervention. Baseline studies before the intervention are necessary to assess these differences. Inadequate control of confounding variables is a major problem in all but a few of the studies. The factors most likely to confound results in studies of water or sanitation are housing (pathogen survival), crowding (hand-to-hand contamination), age (acquired immunity), sex (activity and contact with environment), breastfeeding (exposure to pathogens and pathogen viability), health education (use of services), seasonality (availability of other water sources), rural-urban (exposure to pathogens), migration (exposure to new or more pathogens), income (better food and medical care), diet (quality and quantity), other infections (e.g. malaria causing poor nutritional status), and distance to medical care (whether proper attention is sought) (Esrey and Habicht 1985:73-74). Completely controlling for the large number of confounding variables that might influence the various health indicators is an impossible task in historical research.

Quasi-experimental as well as cross-sectional studies of the effect of water supply must be particularly aware that improved services may not always be used. There are many possible reasons, including lack of knowledge on how to use the services, and easy access to other sources of water, such as streams or ponds (Esrey and Habicht 1985). Failure to record facility use is a main disadvantage even of many contemporary studies (Blum and Feachem 1983). All health improvements depend on how new facilities are used and by whom they are used. Many studies assume that the presence of a particular water supply is synonymous with use of that facility. If no health benefit was recorded, it may have been because the new facility was not used; if a health improvement is recorded, one cannot with confidence say why if use was not carefully studied. It is essential to record use activity in detail especially for the age group which has the highest incidence of disease and thus serves as the main source of infection for others.

An important restriction of much of the historical research reviewed above is that it usually dealt with the characteristics of areas rather than with individuals. This has the important disadvantage that the researcher is dependent on published data sources and that satisfying the conditions for a satisfactory study design is very difficult. Where data on both health indicators and water supply are available at an individual level for historical populations, the study of the relationship between water supply and mortality may result in a more satisfying study design. The Dutch municipality of Tilburg presents itself as a good candidate for such a study of the effect of water supply. This has to do with the availability of individual medical certificates of death and with the presence of the archives of the Tilburg water company, giving information on the time at which individual households were connected to the piped water system. That is not to say that Tilburg offers the ideal study object; as Tilburg was located in the south of the country, it may be assumed that it was not influenced as strongly by the poor quality of drinking water as the cities in the west of the country, where most households were much worse off because of the salinization of surface and ground water (Hofstee 1978).

A case study

The research area

Tilburg is situated in the southern Dutch province of Noord-Brabant. In 1899, it had 40,628 inhabitants, a number that grew to 50,405 in 1910.

The economy of Tilburg was heavily dominated by the textile industry. In 1899, 29 per cent of the total working population were employed in textiles. Some of the other industries, such as the wool washeries and the engineering works, were highly dependent on the textile sector. The national railroad construction yard, opened in 1869, employed as many as eight per cent of the Tilburg working population. The industrial growth that had taken place was small-scale and massive concentrations of workers in huge industrial plants did not exist.

Although according to national standards Tilburg could be considered a town, it differed in several respects from the classical concept of a nineteenth-century industrial city (Janssens 1993). It lacked any degree of compactness and density, and the pattern of the many dispersed little hamlets from which Tilburg had developed was still visible. Owing to this special urban genesis the town preserved a kind of rural atmosphere until the first decades of the twentieth century. Overcrowding and appalling sanitary and health conditions were non-existent or much less extreme than in other industrial cities. The housing situation of the Tilburg working classes was relatively favourable compared with the deplorable conditions which usually characterized industrial towns in this period. The town's spacious layout provided ample space for building; many houses had a small strip of land used for the cultivation of potatoes or the raising of some cattle. Only a small proportion of the population was reported to be living in single-room houses and the sharing of houses by more than one family was very uncommon (Janssens 1993).

Another element distinguishing Tilburg from other industrial towns was the minor importance of female factory work. The textile industry has traditionally been one of the major employers of female labour, and this was also the case in Tilburg, yet girls were expected to leave the mill when they got married. This is often related to the influence of the local clergy who required that the mills did not admit married women. The priests had a powerful influence in the community which was almost entirely Catholic.

The rural mentality of the population, the large proportion of the population born and bred in the city, and the strong influence of Catholicism, all led to a continuation of traditional paternalistic relationships, and a general reserve towards innovations. This also prevented improvements in hygienic conditions of infant care and the integration of the modern medical sector into society.

Nonetheless, during the largest part of nineteenth century the level of infant mortality in the city was much lower than that of the country as a whole. But whereas in the last quarter of the nineteenth century the national IMR was decreasing, the rate for Tilburg and for the province as a whole was increasing: from 116 per 1000 live born in 1851-60 (the same level as in 1821-30) to a maximum of 177 per 1000 in the period 1901-10. It was only after the first decade of the twentieth century that the IMR in Tilburg started to decline (Van der Heijden 1995b).

Of all the health problems caused by the poor environmental situation in which the population was living, the quality of drinking water was considered as the most important one by the Municipal Health Committee of the city.

Till the moment that piped water became available to the city, there were three principal sources of Tilburg's water supply. If the worst came to the worst, the city could make use of *brandkuilen*: large pits which mainly served to supply large amounts of water to extinguish fires. They were also used as washing places for sheep or watering places for cattle. As a consequence of the enormous pollution, the quality of this water left much to be desired (Van der Heijden 1995a). Several streams constituted the second supplier of drinking water. The pollution of this water had increased strongly in the last quarter of the nineteenth century as a result of the growth of the city and the extension of its industries. Dwelling water, street water and manufacturing water were all carried to the surface water. The Public Health Inspectorate concluded in 1885 'that the water is unsuitable as drinking water for humans and cattle' (Van der Heijden 1995b:138). The third and most important source of drinking water consisted of wells and pumps. Many households did not have a water supply of their own and had to make do with a communal well or pump, which often was not even close to the dwelling. According to a report, published in 1905 by two members of the Municipal Health Committee, the majority of the wells were at a distance from the dwellings they served. Manure heaps, toilets and cesspools, and stables for goats or pigs could be found on the same premises. As a consequence, the well water, which was originally clean, became contaminated by the sinking surface water which was mixed with faecal matter and water used for domestic purposes. Dwellings which had a pump or access to one were only seemingly better-off: here too privies and cesspools were in the vicinity of the well. The Municipal Health Committee were of the opinion that bad-quality drinking water in Tilburg was no exception and this situation called for urgent improvement. They even saw a direct connection between the level of infant mortality and the quality of the drinking water: ‘It is our conviction that the high infant mortality in our town might partly be blamed for the contamination of milk with bad quality water, while water purification could not be applied’ (Van der Heijden 1995b:140).

After a long period of negotiations, piped water became available in the city from 1898 on. The extension of the piped water system took place very unevenly over the suburbs of the town: the installation of company water was mainly concentrated in those parts of town where the wool industry was concentrated. The length of the water mains only increased considerably after a period of several years. During the first decades only a minority of the households were supplied with piped water. In 1906, the percentage of dwellings supplied by the water company varied from 0 to 27 depending on the district. In 1900 only nine per cent of the total volume of piped water was supplied to private households; in 1910 this had increased to 14 per cent and in 1920 to 29 per cent. Financial considerations were given as the main reason not to install company water. In 1905 a breakthrough took place because the municipal council and the water company found a way to compel individual property owners to connect their premises to the mains system. In that year, the Municipal Health Committee closed a large number of wells and pumps and the local authorities compelled the proprietors to improve the situation, on penalty of condemning the house as unfit to live in. In 1906 the most densely populated districts had the highest proportion of dwellings with a connection to company water but the correlation between the level of income, social class and access to company water was rather low.

The construction of sewers ran more or less parallel with the construction of waterworks. Till the end of the 1860s, no need at all was felt to take measures directed towards the construction of a sewer system for the transport of rain, rinse and waste water and for the removal of faeces (Van Doremalen 1993). Around 1870, most privies were still in a very primitive condition. Only rarely were brick secretion pits available within easy reach of people’s homes. In many places the excreta disposal facilities were confined to a hole in the ground in which the faeces fell. Those who could not use or dispose of the human excreta near

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AA?eds of metres. Around 1900 most streets in the central part of town were equipped with some form of sewerage whereas some roads running from the centre to the other districts were provided with sewers. The paving and cleaning of streets and the emptying of cesspits were still restricted to the central part of the town. From the second decade of the twentieth century on, lavatories in the house were included in plans for new housing and dry closets were replaced by water closets. In 1907 no more than 2150 of the 9500 dwellings were connected to the sewerage system. The basket system for the removal of faeces remained in use until World War I.

Data-collection strategy

The collection of information on infant and child mortality started with the birth certificates. All 5256 children born in the city of Tilburg in the years 1904, 1905 and 1906 were selected for this study. Excluded from analysis were 37 children who were born out of wedlock, 84 twins, and 271 children whose parents left the city within two years after the birth of the child. This left us with 4864 children.

From the birth certificates of these children, we took information on month and year of birth, first name and surname of the parents, first names of the child, sex, occupation of the father and neighbourhood in which the parents lived. Stillborn children were not included in our analysis. For all births in our study, the death certificates were checked to see whether the children had died before they had reached age five. For each deceased child, age and month and year of death were recorded. For persons younger than two at the time of death, ages were expressed in months; for persons younger than two months, in weeks; for those younger than two weeks, in days; and for children under two days old ages were expressed in hours.

In addition to the death certificates, the original individual medical death certificates had survived. Under Article 5 of the Medical Practitioners Act (*Wet op de uitoefening der geneeskunst*), dating from 1 June 1865, doctors were required, in the event of the death of one of their patients, to issue a medical certificate to be given to the Registrar, in which they were to ‘state as accurately as possible what, in their opinion, but with due regard to their oath of confidentiality, was the cause of death’. They were not to issue this certificate until they had examined the deceased in person and satisfied themselves that the person was dead (Van Poppel and Van Dijk in press). For all children who had died we recorded the cause of death, as mentioned on the death certificate, and classified them in one of the 14 main categories of the First International Classification of Diseases (ICD). In addition, mention was made of whether or not a person had died without a doctor in attendance. As over 25 per cent of deaths were classified in the category ‘ill-defined cause of death’, the reliability of this classification is not optimal.

In the next step, information was collected on the socio-economic characteristics of the households. For that purpose, we first consulted the population register of Tilburg for the period 1900-1910. Continuous population registers recorded the population residing within the municipality. In most municipalities, population registers remained in use until 1910 or 1920. The filled-in forms of the census of 1899 were copied into the population register, and from then on all changes occurring in the population in the next decade were recorded in the register. Each household was entered on a double page, with the head of the household first; he was followed by his wife (if the head was a married male), children, relatives, and other members of the household. For each individual, date and place of birth, relation to the head of household, sex, marital status, occupation, and religion were recorded. New household members arriving after the registration had started were added to the list of individuals already recorded, and those moving out by death or migration were deleted with reference to place and date of migration or date of death. In fact, the population register combined census listings with vital registration in an already linked format for the entire population.

In our study, the population register was used to supply information on the address at which the family lived during the first year of life of the child, the occupation of the father of the child, the age of the mother at the time of birth of the child, the parity of the child (excluding children notified as stillborn), and the interval in months between the index child and the following child, this time taking into account children notified as stillborn in the death register. The calculation of the duration of that interval, and that of the parity of the child were all based on information from the population register for the period 1900-1910. This implies that children born after 1910, and children born before 1900 who had died or moved before 1900, were not included in our calculations.

The occupations were coded on a six-point prestige scale, originally developed by Van Tulder (1962) and frequently used in Dutch historical research. The following categories were distinguished:

I. Upper upper class, including scientists, managers of larger firms, senior civil servants, university and high school teachers;
II. Lower upper class, including senior employees, managers of smaller firms, farmers with a large farm;
III. Upper middle class, including big storekeepers, senior employees, farmers with a middle-sized farm;
IV. Lower middle class: including small dealers and middle employees, farmers with a small farm, craftsmen;
V. Upper lower class: including skilled labour and lower employees;
VI. Lower lower class: unskilled and farm labour.

Information was also used from the Income Tax records (*Kohier van de Hoofdelijke omslag*) for the year 1906, to determine the income of the father of the children. Individuals were classified in categories according to the income level recorded. Those earning less than a certain minimum were exempted from this tax and were classified as such.

The number of rooms in the house in which the child lived was established on the basis of lists which had to be filled in since 1903, in the implementation of the Housing Act (*Woningwet*) of 1901. In principle, a form had to be filled in for each rented house with three rooms or less used for habitation. On this form, the number of residents and the number of rooms was given. The address given in the population register could be used to determine whether this concerned a rented house and if so, whether this dwelling had one, two, three or more rooms. All other dwellings were classified as owner-occupied houses or houses for which it was not known whether they were rented.

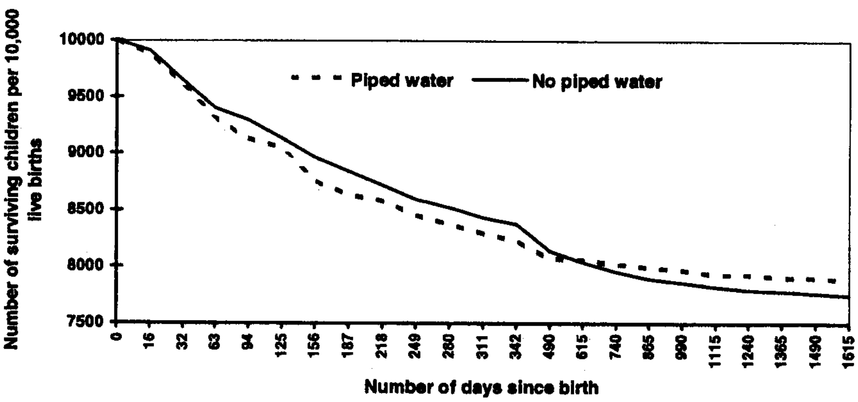
Finally, the connection registers of the Tilburg Water Company were consulted to check whether the house in which the child had lived during its first year of life had been connected to the water mains. If this was not the case, it was assumed that the need for water was met in another way.

The data file constructed for this analysis is a child-level file, that is, each record is a child with the mother's and father's information attached. Since the observations are live births, there is frequently more than one observation per family. This creates the possibility that influences on different infants born to the same mother may be correlated. Unmeasured mother-specific influences may thus play a role. A total of 3232 families were studied, 1797 of whom had only one child in our study population, 1238 had two children and 197 had three.

Results of the analysis

Figure 1 presents the life table for all infants born during the period 1904-1906. The survivorship function is calculated by grouping the ages at death into fixed intervals of less than 7 and 7-31 days, whereas for higher ages one-month intervals were used.2 The figure shows estimates of the proportions of children who had survived up to a specific age. A separate life table was calculated for children born in households which were connected to the piped water system and for all other children.

Figure 1
Number of surviving children during the first five years of life according to availability of piped water in the household, Tilburg 1904-1906.



It is clear that during the first one-and-a-half years the proportion of surviving children born in households which had piped water at their disposal was lower than in households for which that was not the case. After this period, the differences found were in the expected direction. Several test statistics, Log-Rank (Savage) statistic, Wilcoxon (Breslow) test statistic, Wilcoxon (Tarone-Ware) test statistic and the Wilcoxon (Prentice) statistic, were used to compare the two survivorship functions. All these statistics are based on the null hypothesis that the survivorship functions of both groups of children do not differ. All

[2]The original data only included detailed information for the first month of death. By combining the information from the year and month of birth, the year and month of death, and the age at death we could recode the age at death in monthly intervals. For this purpose, we have first of all recalculated each age at death as the average number of days between the month of birth and month of death. By combining this information with age at death a more-or-less exact age at death in months could be deduced.

statistics are chi-square-distributed with m-1 degrees of freedom, m being the number of groups being compared. None of these statistics was significant, indicating that the null hypothesis, namely that survivor functions were independent of the household's water supply, could not be rejected.[3]

Although the result of the comparison of the group-specific survivor functions is clear, this does not imply that further analysis of the effect of water supply is useless. First of all, it is possible in principle that the availability of piped water is correlated with a number of other factors, a correlation which can neutralize the effect of water supply. One might, for example, think of the fact that priority in the supply of clean drinking water was given to those areas which were, in general, characterized by the worst sanitary situation. Such an association between piped water supply and other factors affecting the health situation could result in the disappearance of the effect of water supply on mortality. By inclusion of these other factors in the analysis, the bias of some of these, confounding variables may be eliminated. At the same time, including these other factors can lead to an improvement of the confidence intervals and tests.

To analyse the effect of water supply on infant and childhood mortality in such a sophisticated way, use was made of event history modelling techniques, also known as hazard rate models.[4] In these models, the rate at which people experience an event (the transition rate) such as death is analysed dependent on a time-related factor (age or another duration) and other covariates. Alternative models differ in the manner in which the age dependence of the process is handled. Hazard models are elaborations of the more basic life-table methodology, combining the strength of life-table and regression analysis. They allow the formulation of equations relating independent variables to the hazard function, in our case the risk of dying, as in conventional least-squares regression (Yamaguchi 1991; Blossfeld and Rohwer 1995).

The Cox model was used to estimate the effects of covariates on the death rate of children. The model is also called a proportional hazards model because the effect of a variable on the baseline is assumed to be the same at each time t, so there are no interactions between covariates and age intervals. There is an underlying hazard function which varies

This model can be expressed as

$$r_{(t)} = h_{(t)} * \exp(b\mathbf{X}),$$

where
$r_{(t)}$ = the rate of death at time t,
$h_{(t)}$ = the base-line hazard at time t, and
$\mathbf{X}$, a vector of covariates.

The relationship between the hazard function and the independent variables is written as an equation in which a set of regression-like coefficients indicates the effect of the independent variables in shifting a time-varying baseline hazard function, associated with a baseline group of individuals, upward or downward. The baseline hazard function is defined when all independent variables in the model take the value of zero.

The dependent variable is the hazard rate at any given time t. The rate is modelled by a baseline hazard rate $h_{(t)}$, which is roughly analogous to the constant term in a standard

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[3]The t-values were 0.4749, 0.2142, 0.3309 and 0.2370 respectively with 1 degree of freedom.

[4]The computer program used was Transition Data Analysis (TDA), written by Götz Rohwer. See Rohwer, G. (1994) TDA working papers. Bremen University.

by age, but does not depend on the values of the covariates, and is therefore the same for all individuals.

regression model, with the important difference that h(0 denotes values that change over time rather than a single value.

The following variables were included in the analysis.

Water supply. A difference was made between children who during their first year of life lived in a dwelling which was connected to the piped water system, and all other children. Children from this last group include children who had access to abundant and clean water from pumps or wells, as well as children living in houses with a totally insufficient water supply. The heterogeneity of this group may of course lead to a lessening of the differences between the two groups and to less convincing results.

Socio-economic status. The six status groups which were distinguished were grouped into three categories: upper class (groups I, II and Ill of the Van Tulder classification), middle class (group IV), and lower class (groups V and VI).

Income level of the father of the child. It might be expected that it is the poor who suffer most from lack of water and sanitation because they reside in crowded unhygienic housing that facilitates the spread of faecal-oral pathogens (Esrey and Habicht 1986), because they lack the income to provide the necessary facilities, and because they are unaware of how to minimize the deleterious effects of the unsanitary conditions in which they live. In our study, income level was split into four groups: household heads whose income was below the minimum assessment level of Dfl 500, designated as Very low income level; households with incomes between Dfl 500 and Dfl 750 (Low level); households with incomes between Dfl 750 and Dfl 1000 and households with incomes above Dfl 1000 (High level) .[5]

Age of the mother at birth. Higher mortality risks are generally associated with late or very early childbearing. To take into account the parabolic relationship between age of the mother and mortality of the child, age was entered into the equation first as the difference between age of the mother and the average age of the mother at birth (31.49 years), and secondly as the squared value of this difference.

Birth order of child. The birth order of each child from the sample could be determined by making use of the population register for the period 1900-1910. In this register only those children were mentioned who belonged to the household during the period concerned. This implies that children who had died before the year in which the register was made up, were not mentioned. In addition the population register did not include data on stillborn children. As a consequence the birth order of the child as deduced from the register may be lower than the real birth order. Three categories were distinguished: birth order one; two or three; and four or higher.

Interval to next child. The effect of the length of the subsequent birth interval on the survival of the index child may be due to early and abrupt cessation of breastfeeding, competition for mother’s care and attention as well as competition for scarce resources (De Sweemer 1984). For all children from the sample, we verified whether the birth of the index child was followed by a succeeding birth, and how long the interval was between these two births, expressed in months. Intervals of less than 16 months were distinguished from those of between 16 and 24 months and from intervals of 25 months or more. In 955 cases the index child was the last child in the family. *All* these cases were included in the group with an interval of 25 months or more.

Season of birth of the child. In the first weeks of their lives, children born during the winter period are particularly at risk to respiratory diseases, risks which can be reduced by providing adequate protection: clothes, heating, less exposure to infection. Water quality

[5]To test whether income levels and socio-economic status were measuring the same factor, a factor analysis was carried out. This showed that socio-economic group and income were measuring two different dimensions of the social and economic status of the families concerned.

both at the water source and at home varies tremendously with the seasons. These seasonal variations naturally affect the pattern of diseases. The combined effects of child age and season may make children born during certain months of the year more vulnerable to diarrhoeal disease than children born during other months (Lindskog and Lundqvist 1989:93-95). This pattern may be attributed to both the changing environmental conditions and changes in water quality and availability. In addition, there is a variation between the seasons in the amount of time people spend indoors and in different parts of the environment, which may be important in terms of human exposure to contamination. In the summer children are at risk of infections of the digestive tract. This risk depends on the age of the child in the summer. The period in which weaning takes place is particularly important (Breschi and Livi-Bacci 1992). To take the seasonal factor into account, children born in July and August, designated as Summer, were distinguished from those born in the winter, November to February, and children born in other months (Other).

Number of rooms in the dwelling. The level of crowding of a house is not only a proxy for general social standing[6]; it may also have a direct effect on mortality through the transmission of infectious diseases (Bernhardt 1992). To measure this variable, information was collected on the dwelling in which the child had lived during its first year of life. Dwellings with one, two or three rooms were differentiated from all other dwellings. This last group included houses with four or more rooms and houses occupied by the owner.

District of birth. To capture the aspects of the sanitary situation in which the children were born and lived which did not refer directly to the individual household, we used the level of infant and childhood mortality of the district in which the child was born. The original data set distinguished fourteen districts. We grouped them into two categories, entitled Unhealthy districts and Other. In the first-mentioned group of districts, Groeseind, Hasselt, Hoeven, Oerle and Stokhasselt,.with the lowest housing density, domestic industry and agricultural activities were overrepresented. Mortality in the first five years of life ranged from 25.5 to 34.0 per 100 live births, in the second .group the highest level was 22.9 per 100. In addition to the above-mentioned variables, the *sex of the child* was also included.

The results of the .analysis are presented in Table 1. The table gives relative risks of death, the risk (or hazard) of dying relative to a baseline hazard for each category of the selected covariates. A relative risk of 1.00 indicates that the estimated hazard is equivalent to the baseline hazard; a relative risk greater than 1.00 indicates mortality higher than the baseline hazard; and a relative risk less than 1.00 indicates mortality lower than the baseline hazard. The quantitative importance of one covariate relative to another may be judged by the relative sizes of the largest differences between the relative risks. The statistical significance of individual coefficients can be determined by t-tests, where t-values of 1.92 or more will be considered to point to statistically significant differences.

Table 1 shows that when other factors are taken into account, water supply still did not have an impact on the mortality of children. The sex of the child did, however, as did birth order, interval to the next child, income level, season of birth, district and age of the. mother. In all these cases, the effects were in the expected direction. Thus, male children had higher mortality risks, as did children with high birth order, children who were quickly followed by the birth of another child, children born in the summer period, children born to very young and old mothers, and children born in districts characterized by high mortality levels.

[6]Although factor analysis shoved that this variable was hardly correlated with variables such as income and social group. Although the housing situation in Tilburg was relatively favourable, overcrowding did exist, partly because of the high fertility levels of the population. This might have caused a high mortality from diseases like diphtheria and smallpox for which access to water was less crucial. The number of rooms per dwelling is not a perfect indicator of such overcrowding.

Mortality was significantly lower among firstborn children, children with a long subsequent

Table 1
Proportional hazards coefficients for infant and childhood death, based on Cox-regression model (N=4863)

Variable		Relative risk
Water supply	Piped water	1.01
	Other	1.00
Sex	Male	1.16*
	Female	1.00
Birth order of child	One	0.73**
	Two or three	1.00
	Four or higher	1.37***
Interval to next child in months	Less than 16	1.65***
	16-24	1.00
	25 or more	0.68*
Socio-economic group	Upper class	0.94
	Middle class	0.98
	Lower class	1.00
Income	High	0.74*
	Middle	0.90
	Low	0.93
	Very low	1.00
Season of birth	Summer	1.24***
	Winter	0.95
	Other	1.00
Number of rooms in dwelling	Rented dwelling with two or three rooms	1.06
	Four or more rooms and own dwelling	1.00
District	Unhealthy	1.25***
	Other	1.00
Age of mother		1.00
Age of mother squared		1.00*

*Significant at 0.05 level; ** significant at 0.01 level; *** significant at 0.001 level.

Log likelihood - 9046.88

interval, and children from high-income families.

A complication arises from the fact that children can die of a large variety of causes. One might thus argue not only that there is a transition from the state of being alive to the state of being deceased, but that several destinations may be distinguished, depending on the cause of death of the child. One might expect that water supply had a stronger effect on some transitions (causes of death) than on others. In particular, one might assume that diseases of the digestive system were more sensitive to the effect of piped water than other causes of death. Models for the situation with a single origin state (alive) but two or more destination states are called *models with competing* risks. These competing risks can be considered within one analysis. In this way it is possible to test whether the effect of water supply varies according to the cause of death. Four causes of death were distinguished here: diseases of the respiratory system, diseases of the digestive system, ill-defined and unspecified causes, and all other causes. These causes were responsible for 17.5, 35.8, 25.5 and 21.2 per cent respectively of all deaths in the cohort studied.

The results of the analysis of this competing risk model are given in Table 2.

The results of the more refined model demonstrate that children who were living in a house which had access to piped water had a statistically significant lower risk of dying of respiratory diseases. The expected negative effect on mortality due to diseases of the digestive system was absent, however. For ill-defined causes of death and other causes, piped water supply had no effect.

As far as the other variables are concerned, the picture is not always consistent. Male infants had statistically significant higher mortality for diseases of the digestive tract, and for ill-defined causes, but not for respiratory diseases and other causes of death. Although first-order children had lower mortality for all causes of death and high-order children had excess mortality, differences were not always significant. Short intervals to a subsequent child were associated with excess mortality for all causes of death, and long intervals with lower mortality risks. High income groups had significantly lower mortality for diseases of the digestive system and for ill-defined causes of death, whereas the other socio-economic variable, the socio-economic group, had no effect at all. As might be expected, children born in the summer period had much higher mortality for diseases of the digestive system. Children born in the winter period on the other hand did not have statistically significant higher mortality due to diseases of the respiratory system. The districts which were characterized as unhealthy had excess mortality for all causes of death; the differences were statistically significant for diseases of the digestive system and for ill-defined causes. The age of the mother had a parabolic relationship with infant mortality due to digestive diseases: children of young and older women thus had higher mortality.

Up to this point we examined the effects of piped water and other confounding and mediating variables on mortality in the age range 0 to 5. However, the effect of piped water supply on death risks may not be the same at all ages. For instance, a well-established idea is that water supply affects mortality at higher ages more than immediately after the birth of the child since environmental effects are generally thought to become more prominent after the first few months of life. There are several ways to test this hypothesis. Here, separate Cox models are estimated for different age intervals. If the effects of piped water depend on the age of the child, one would expect no effect of piped water at some ages (e.g. during the first couple of months after birth), but a significant effect at other ages. In our analysis, the hazard models were estimated separately for three different periods in the life of the newborn. Somewhat arbitrarily, but partly also forced by the given age classification of the deceased, these episodes were split into the first six months after birth, from six elapsed months to 18 months, and from 18 months to five elapsed years. By performing our analysis for these three age groups, we can observe whether water supply has a strong influence of the period in which weaning takes place whereas it has an attenuated effect at other ages of the child.

Table 2
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks)

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Water supply								
Piped water	1.26	1.73	0.50	-2.60	0.94	-0.30	1.15	0.56
Other	1.00		1.00		1.00		1.00	
Sex								
Male	1.25	2.17	0.89	-0.82	1.28	2.02	1.13	0.65
Female	1.00		1.00		1.00		1.00	
Birth order								
One	0.62	-2.70	0.69	-1.27	0.80	-1.01	0.91	-0.43
Two or three	1.00		1.00		1.00		1.00	
Four or higher	1.25	0.91	1.48	2.08	1.37	1.98	1.48	2.28
Interval to next child in months								
Less than 16	1.67	4.18	1.01	0.07	2.09	4.97	1.73	3.38
16-24	1.00		1.00		1.00		1.00	
25 or more	0.65	-3.35	0.60	-2.99	0.74	-1.99	0.78	-1.47
Socio-economic group								
Upper class	0.77	-0.70	1.46	0.68	0.64	-0.81	0.98	-0.05
Middle class	1.01	0.12	1.20	0.74	0.85	-1.21	0.95	-0.38
Lower class	1.00		1.00		1.00		1.00	
Income								
High	0.53	-2.18	0.84	-0.57	0.26	-2.74	1.51	1.62
Middle	0.84	-0.76	0.98	-0.09	0.65	-1.46	1.28	0.97
Low	0.91	-0.51	0.77	-0.95	0.95	-0.24	1.08	0.34
Very low	1.00		1.00		1.00		1.00	

Table 2 (cont.)
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks)

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Season of birth								
Summer	1.33	2.08	1.03	0.12	1.25	1.36	1.25	1.20
Winter	0.88	-1.06	1.12	0.72	0.96	-0.31	0.93	-0.47
Other	1.00		1.00		1.00		1.00	
Number of rooms in dwelling								
Two or three	1.04	0.36	0.96	-0.25	1.27	1.89	0.93	-0.48
Four or more	1.00		1.00		1.00		1.00	
District								
Unhealthy	1.28	2.19	1.07	0.38	1.48	3.06	1.08	0.49
Other	1.00		1.00		1.00		1.00	
Age of mother	0.99	-0.92	1.03	1.41	1.03	2.49	0.99	-0.86
Age of mother squared	1.00	2.77	0.99	-0.71	1.00	0.53	1.00	0.38
Number of events	391		191		278		232	
Log-likelihood	-8995.88							

Table 3
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with split episodes (relative risks)

		Between birth and age 6 months		Between 6 and 18 months		Between 18 and 60 months	
		Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Water supply	Piped water	1.22	1.81	0.86	-0.89	0.60	-1.84
	Other	1.00		1.00		1.00	
Sex	Male	1.31	3.27	1.04	0.35	0.93	-0.44
	Female	1.00		1.00		1.00	
Birth order	One	0.94	-0.42	0.51	-3.17	0.45	-2.36
	Two or three	1.00		1.00		1.00	
	Four or higher	1.30	2.40	1.43	2.55	1.46	1.91
Interval to next child	Less than 16	2.01	7.00	1.28	1.83	1.18	0.78
	16-24	1.00		1.00		1.00	
	25 or more	0.67	-3.64	0.61	-3.76	0.88	-0.72
Socio-economic group	Upper class	0.85	-0.63	1.30	0.82	0.67	-0.71
	Middle class	0.97	-0.34	0.98	-0.13	1.02	0.13
	Lower class	1.00		1.00		1.00	
Income	High	0.85	-0.77	0.67	-1.55	0.53	-1.49
	Middle	1.01	0.06	0.64	-1.80	1.16	0.49
	Low	1.14	0.94	0.65	-2.01	0.79	-0.80
	Very low	1.00		1.00		1.00	
Season of birth	Summer	1.43	3.39	0.98	-0.14	1.00	0.01
	Winter	0.71	-3.30	1.46	3.29	0.88	-0.70
	Other	1.00		1.00		1.00	
Number of rooms	Two or three	1.19	1.97	0.96	-0.37	0.85	-0.94
	Four or more	1.00		1.00		1.00	
District	Unhealthy	1.13	1.28	1.36	0.99	1.49	2.31
	Other	1.00		1.00		1.00	
Age of mother		1.01	1.40	1.00	-0.14	0.99	-0.41
Age of mother squared		1.00	1.37	1.00	3.00	1.00	-1.38
Number of events		581		348		163	
Log-likelihood		-4814.95		-2852.79		-1324.37	

The results of the analysis of this multi-episode model are given in Table 3, for all causes of death combined. It is clear that the selected variables have a much stronger impact on mortality during the first half-year of life than on mortality in the second period (stretching from six to 18 months) and on mortality at ages one-and-a-half to five years. Again, however, the availability of piped water did not have a decreasing effect on mortality risks. In later episodes, mortality was indeed lower in households which had piped water at their disposal but differences were not significant. Male children had a much higher mortality, in particular during the first half-year of life. Birth order of the child and interval to a subsequent child had an effect on mortality, in particular during the first two episodes. The season of birth also had an important effect during the first and second periods of the life of the child. As expected, the crowding variable, the number of rooms per dwelling, showed excess mortality during the first half-year. Age of the mother (squared values) had an effect on mortality after age six months.

In the next stage, both approaches were combined, that is to say, a competing risk model was estimated and age groups were split. The results of this procedure are given in Table 4, for ages between birth and six months, and Table 5, between six and 18 months.

Again it appeared that during the first six months of the life of a child, piped water did not have the expected effect on mortality. The only variable that had a significant and consistent effect on mortality for more than one cause of death was the interval to the subsequent child. For deaths among children aged between six and 18 months at death the situation was different. The diseases of the digestive system were particularly heavily influenced by the selected variables. More important however, is the finding that once again piped water did not have a statistically significant effect on mortality due to respiratory diseases.

Low birth order, long intervals to a subsequent child, average and high incomes all led to decreased mortality risks for digestive diseases. Children born in the winter had excess mortality due to this cause of death, an outcome which is related to the fact that these children were, during the selected stage in their lives, at a high risk of contracting digestive diseases in the summer period.

Finally, we tried to detect whether there were any interaction effects between water supply and one or more of the selected variables. We were particularly interested in the question whether we could detect effects of piped water supply in those districts which we considered to be less healthy, among children born in the summer period, and among children born to mothers who had a subsequent child after a relatively short interval. The relative risks showed that the combination of unhealthy districts with piped water was characterized by lower mortality but the combination of piped water with short intervals and with the summer season led to increased mortality rates. Yet the effects were not statistically significant in any of these cases. By comparing the log-likelihood values of the old model and the new model with interaction effects, we could test the null hypothesis that the death risk for water supply did not vary over the districts. To compare the fit of the two models, twice the positive difference between their log-likelihoods is calculated. Under the null hypothesis of no difference, this statistic has a chi-square distribution with the difference between the number of variables in the two models being the number of degrees of freedom. All the observed chi-square values were far below the critical value for the 0.05 level of significance, the evidence thus being low that the effect of water supply on the death rate varied by district, season of birth and interval to subsequent child.

Table 4
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks), between birth and age 6 months

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Water supply								
Piped water	1.33	1.75	1.01	0.02	1.00	-0.00	1.38	1.39
Other	1.00		1.00		1.00		1.00	
Sex								
Male	1.25	1.72	1.02	0.04	1.48	2.59	1.31	1.45
Female	1.00		1.00		1.00		1.00	
Birth order								
One	0.73	-1.55	0.73	-0.45	1.03	0.13	1.53	1.46
Two or three	1.00		1.00		1.00		1.00	
Four or higher	1.12	0.69	1.39	0.71	1.34	1.49	1.72	2.11
Interval to next child in months								
Less than 16	1.89	4.06	1.69	1.30	2.26	4.64	2.01	3.17
16-24	1.00		1.00		1.00		1.00	
25 or more	0.78	-1.49	0.38	-1.98	0.61	-2.48	0.64	-1.77
Socio-economic group								
Upper class	0.55	-1.27	0.69	-0.32	0.62	-0.75	1.65	1.18
Lower middle class	0.94	-0.46	1.06	0.15	0.89	-0.71	1.20	0.87
Lower class	1.00		1.00		1.00		1.00	
Income								
High	0.87	-0.42	1.63	-0.18	0.35	-1.94	1.40	0.95
Middle	0.97	-0.10	1.24	0.33	0.87	-0.43	1.23	0.61
Low	1.24	1.08	0.86	0.93	1.13	0.50	0.86	-0.47
Very low	1.00		1.00		1.00		1.00	

Table 4 (cont.)
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks), between birth and age 6 months

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Season of birth								
Summer	1.57	1.00	0.96	-0.08	1.38	1.61	1.33	1.12
Winter	0.42	1.00	1.06	0.14	0.97	-0.16	0.92	-0.37
Other	1.00		1.00		1.00		1.00	
Number of rooms in dwelling								
Two or three	1.21	1.35	1.18	0.44	1.35	1.89	0.95	-0.24
Four or more	1.00		1.00		1.00		1.00	
District								
Unhealthy	1.08	0.53	0.88	-0.28	1.35	1.83	0.96	-0.16
Other	1.00		1.00		1.00		1.00	
Age of mother	1.00	-0.05	1.07	1.38	1.04	2.05	1.00	0.10
Age of mother squared	1.00	1.89	0.99	-0.94	1.00	-0.12	1.00	0.73
Number of events	248		32		184		117	
Log-likelihood	-4779.50							

Table 4 (cont.)
Parameter estimat
with competing ri
Season of birth
Summer
Winter
Other
Number of rooms i
Two or three
Four or more
District
Unhealthy
Other
Age of mother
Age of mother squ
Number of events
Log-likelihood

Table 5
Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks), between ages 6 and 18 months

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Water supply								
Piped water	1.04	0.13	0.53	-1.69	1.00	0.01	0.94	-0.14
Other	1.00		1.00		1.00		1.00	
Sex								
Male	1.39	1.86	0.71	-1.57	0.95	-0.22	1.07	0.24
Female	1.00		1.00		1.00		1.00	
Birth order								
One	0.49	-2.12	0.78	-0.59	0.36	-1.82	0.41	-1.74
Two or three	1.00		1.00		1.00		1.00	
Four or higher	1.53	1.90	1.48	1.38	1.40	1.11	1.17	0.44
Interval to next child in months								
Less than 16	1.43	1.74	0.57	-1.68	2.19	2.58	1.29	0.74
16-24	1.00		1.00		1.00		1.00	
25 or more	0.44	-3.55	0.63	-1.90	0.98	-0.07	0.60	-1.52
Socio-economic group								
Upper class	1.79	0.98	2.36	1.80	0.76	-0.25	0.31	-1.40
Middle class	1.25	1.17	1.24	0.89	0.74	-1.16	0.54	-1.87
Lower class	1.00		1.00		1.00		1.00	
Income								
High	0.17	-2.75	1.52	1.06	0.16	-1.68	2.00	1.30
Middle	0.59	-1.32	0.56	-1.07	0.32	-1.54	1.67	0.97
Low	0.41	-2.25	0.89	-0.29	0.73	-0.72	0.86	-0.27
Very low	1.00		1.00		1.00		1.00	

Table 5 (cont.)

Parameter estimates for the effects of independent variables on the rate of mortality among children, based on Cox-regression model with competing risks (relative risks), between ages 6 and 18 months

	Digestive system		Respiratory diseases		Ill-defined		Other causes	
	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value	Relative risk	t-value
Season of birth								
Summer	0.82	-0.60	1.00	0.01	1.12	0.35	1.03	0.07
Winter	2.19	4.21	1.12	0.49	1.09	0.34	1.24	0.72
Other	1.00		1.00		1.00		1.00	
Number of rooms in dwelling								
Two or three	0.82	-1.08	1.24	0.90	1.20	0.76	0.68	-1.27
Four or more	1.00		1.00		1.00		1.00	
District								
Unhealthy	1.47	1.98	1.12	0.45	1.59	1.92	1.19	0.54
Other	1.00		1.00		1.00		1.00	
Age of mother	0.98	-1.24	1.03	0.99	1.03	1.30	0.97	-1.15
Age of mother squared	1.01	2.37	1.00	0.10	1.00	1.47	1.00	1.37
Number of events	130		87		78		53	
Log-likelihood	-2808.18							

Conclusion

In our study we were able to use a research design which in principle could adequately measure the health effect of the new water supply system. We could use rather refined information on factors considered relevant to evaluating the effect of water supply on health. We were able to use rather detailed information on the availability of improved water supply. We could take into account the fact that an improvement of the water supply system may not influence all diseases but only infectious water-related diseases. We could relate the effect of water supply to the period of birth of the child and could take into account mortality variations among age groups. And finally, we were able to control for the effect of several confounding variables which in earlier research had proved to be relevant. Still, the degree of association between water supply and mortality variables appeared to be nonexistent.

Esrey and Habicht (1985) used three criteria to evaluate studies that reported a negative association or lack of association between water and health. The first relates to the question whether the outcome was likely to be changed by an improvement in water and sanitation. That is, were conditions such that an outcome could respond to the intervention? For example, in an area where most children are breastfed, the provision of high-quality water is not likely to reduce infant mortality rates because breastfeeding reduces exposure to pathogens and confers immunity to ingested pathogens. The second criterion deals with sample size. The question here is whether the number of replications of the unit of analysis was so high that the power of the test was sufficiently large to find differences in the groups compared. The third criterion is control for possible negative confounding. A negative confounder is a factor related to both the intervention and the outcome which would wipe out any true benefit from the intervention. For example, improved water supplies may draw many people to a particular area, creating a crowding problem. Overcrowding, which enhances the spread of pathogens directly from one person to another, will mask the true effects of improved water. Negative confounding may prevent an outcome from occurring (Esrey and Habicht 1985). Examination of contemporary studies reporting negative associations or reporting a change in outcome of less than 50 per cent revealed that all suffered from one or more of these flaws. Lindskog and Lundqvist (1989:11) also proposed a scheme to analyse the reasons why the expectation of an effect of improved methods of water supply on health was not met. Their scheme partly overlaps with the one first mentioned. First of all, the problem is what they call one of social documentation. Since the effect may be fairly slight, if noticeable at all, and since changes in the health situation may have other causes than improved methods of water supply, the proper design of the study is extremely important. Secondly, there is a failure to comprehend and to collect information on the complex interrelationships between water supply, the physical and human environment in which the improved water supply is provided, human behaviour, and the implications for health and social conditions.

First of all data shortcomings may be the main reason for the observed negative results. We might safely assume that the quality of information on the dependent variable is rather good. In 1906, the death registration system in the Netherlands had already functioned for almost a century, and it is considered to be rather complete and reliable (Oomens 1989). The quality of the cause-of-death information was lower but the system had also functioned for quite some time and was based on medical certification. A problem in the analysis of the cause-of-death data is the large group with ‘Unknown cause’ or other vague classifications, groups which were especially common among infant deaths. Yet the large majority of infant deaths could be accurately classified. Since infant mortality is a relatively infrequent event, several authors have argued that to detect an effect of water supply on mortality rates a much larger sample size is generally required than for measures of morbidity or nutritional status (Briscoe et al. 1986; Esrey 1994:7). As we were able to show large effects of other variables on mortality, we may assume that in principle sample size was large enough to find differences in the groups being compared.

Next we have to discuss whether the conditions in the research area were such that no effect at all of water supply could be expected. Several factors may have played a role here.

First of all, the conditions prevailing before the introduction of the piped water system may have been such that only a minor or no effect could be expected. Two competing situations are possible. Esrey and Habicht (1986) argued that sometimes the population being studied is relatively healthy as a result of which the, water (and sanitation) interventions may be insufficient to produce health effects. The 'threshold-saturation theory' points out that there is not only an upper limit of saturation where. further investments in sanitation do not result in further improvement of health status, but also a threshold of socioeconomic and health status below which no health benefits can be achieved by investing in sanitation (Cvjetanovfc 1986). With a high level of environmental contamination, the beneficial effect on health of improved water supply may be greatly reduced or even nullified (Massee Bateman, Smith and Roark 1993:91). Given the level of mortality and the further characteristics of the socio-economic and ecological situation in Tilburg, neither of these explanations is very plausible.

As a second relevant factor Esrey and Habicht (1986) discussed the common feeding pattern of the newborn child. Breastfeeding is the most appropriate food for young infants because it is nutritious and sterile, may reduce the ingestion of other, often contaminated foods (reducing exposure), and confers immunity. Thus infants who are not breastfed would be expected to benefit more from improved water supply than breastfed infants. Similarly in areas where breastfeeding was not universal or of short duration, water and sanitation facilities could be expected to produce a large health effect. Breastfeeding *was* far from general in Tilburg. In the first decade of the twentieth century, the municipal health committee distributed thousands of copies of a circular containing rules for feeding children. It urged mothers to breastfeed their babies, to postpone weaning and to use clean nursing bottles if a child was not breastfed. Good and fresh cow's milk which was not diluted was advised. As the milk being fed to Tilburg's infants and children was often contaminated and watered, artificial feeding led to malnourishment of children. For that reason, a routine milk inspection was established in 1906. From this strong encouragement to mothers to breastfeed their children and from information from a survey held at the request of the Municipal Health Committee, we can infer that breastfeeding was by no means universal. Information relating to 1628 children born in Tilburg in 1912 showed that breastfeeding only was given to 62 per cent of the children, artificial feeding only was given to 22 per cent, and 15 per cent received mixed feeding. Given this low frequency of breastfeeding, effects of being connected to the water supply system could have been expected.

Another factor may also have played a role: the decrease in mortality among non-connected households may have been an unplanned side-effect of the creation of the 'experimental condition' in the city. There may have been an increased awareness of health-related issues among households which relied on traditional sources of water, supply. The construction of the water system may have 'activated' people's awareness of the importance of proper water handling for health and may have increased the motivation of people to modify their behaviour in such a way that health conditions were affected. In this case, the significant intervention was the exposure to new ideas about the importance of clean water (Lindskog and Lundqvist 1989:80-81).

A fourth factor leading to the unexpected outcome may be classified under the headings 'improper design of the study' and 'data quality', and relates to shortcomings in the quality of information on the intervention factor. Reductions in mortality can only be expected if the level of pathogen ingestion in the connected households really improved in comparison with that in the non-connected households. If it is assumed that households use improved water supplies when in fact they continue to use unimproved facilities, the consequence will be that the effects are underestimated (Briscoe et al. 1986). This misclassification of exposure (Esrey and Habicht 1986; Briscoe et al. 1986), an error in the classification of individuals as either 'exposed' or 'not exposed' to the intervention, may have an effect on the measured association between health and water supply. Several forms of misclassification can be imagined in our study. First of all, uncontaminated water, often the engineering goal, may become polluted during inappropriate transport and storage leading to the ingestion of contaminated water (Esrey and Habicht 1986). As might be concluded from the results of chemical, bacteriological and microscopic examinations, the quality of the water in Tilburg was considered to be very good.[7] Misclassification of the exposure factor may also have been caused by the fact that data on water supply related to the situation of the child during its first year of life. During the next four years some infants may have been exposed to a different environment from that described at the time of their birth (Esrey and Habicht 1985:64; Esrey 1994). Household characteristics too were measured at a time which did not always correspond with that of birth or death of the child. Spurious association due to misclassification of this kind may only have been relevant to childhood mortality. A more important form of misclassification relates to the fact that in our study we lacked information on the water supply of those households which were not connected to the piped water system. This group may have included a wide variety of situations: public stand-pipe, and non-piped, which included wells, streams, springs and rainwater. The most important problem, however, is the failure to record how the new facilities were used, and by whom they were used. Water consumption and water, handling are determined by a mixture of needs of the individuals, the social and cultural norms prevailing in that society and the resource situation. People may recognize the need for improvements in their water supply system but through lack of economic resources they may not be in a position to change their situation. It is also possible that the wish to reduce costs led some households to use piped water less frequently than was the case among the well-to-do. The significance of socio-economic conditions for water use patterns could be taken into account by using information on occupation and income. However, economic conditions may have been less important for the health of the child than the formal and informal education of the parents.

Finally, there are numerous factors which together determine the results of the improved method of water supply which we were not able to take into account. Lindskog and Lundqvist (1989:20) grouped these factors into three categories: knowledge, perception and motivation; the resource situation of the beneficiaries; and physical and environmental characteristics of the area where the intervention takes place and where the beneficiaries live.

Improvements in household water supply are a necessary but not a sufficient condition for improved health. Improvements in hygiene and sanitary conditions are also required, particularly to attain a reduction in diarrhoeal diseases. Research in developing countries has shown that improvements in hygiene and sanitation have an even greater impact upon water-related diseases than improvements in water quality. Whereas drinking water constitutes only one source of pathogen transmission, improvements in sanitation and personal hygiene influence the disease load in three different ways: better personal hygiene

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[7]Municipal Archive Tilburg. Archives of the Tilburg Water Company, Inv. No. 129, Incoming correspondence, section Water examinations 1896-1960. Examination reports were available for the years 1896-1898 and 1909.

reduces faecal-oral transmission through solid bodies, it reduces faecal-oral transmission through water and it reduces faecal-oral transmission through food. Excreta disposal in particular appears to consistently play a more important role in determining children's health in developing countries than does water supply, especially where the prevalence of diarrhoea is high (Esrey and Habicht 1985). Also if diarrhoeal and other infectious diseases are primarily water-washed, the provision and use of sufficient water, albeit of poor quality, for personal and domestic hygiene could prevent the contamination of food, utensils, and hands and thereby reduce the transmission of the major infectious agents of diarrhoea. Water quantity may thus be more important than water quality, and information on the type of water supply only may therefore be a poor guide to the overall effects of water supply on childhood mortality (Esrey and Habicht 1986).

Information on human behaviour, knowledge and practice in relation to hygiene and sanitation needed to provide a proper understanding of the effect of changing water supply, implies knowledge of the actual use of sanitation; the sanitary disposal of human faeces; personal hygiene habits such as frequency of hand-washing, where and how it is done and whether soap is used; water handling; bathing and clothes-washing; personal hygiene in relation to cooking; and when to feed children (Hardy 1993:187-188). Only very scanty information on these subjects is available for the study area. However, the data all point in the same direction: the personal hygiene situation in this area was much worse than in most other parts of the country. Changes took place only after the first decade of the twentieth century when a start was made with the education of mothers to promote breastfeeding, handwashing after defecation and before preparing meals, cleaning of the home environment, and provision of soap (Marland 1995, 1996).

In many respects, the households in our survey were living in comparable physical and environmental circumstances, but there were also important differences, about which we do not have much information. It is certain that there was a considerable degree of variation in the level of environmental contamination: it depended on the kind of activities that took place around the house in which the child lived, the presence of poultry and other livestock in the farms in the inner city, and the way streets were cleaned. Local factors relating to drainage and refuse disposal were very important in determining the prevalence of the enteric group of diseases. The seasonal incidence which was so marked a feature of typhoid, as of diarrhoea, indicates the potentially considerable role of flies as a source of infection. The seasonal peak of the annual typhoid curve coincided with that of the greatest fly activity, at the time of the year when harvesting and fruit cropping meant that farmers were often too busy to relieve the city of its manure (Hardy 1993:183-184).

It is a very difficult task to assess the contribution of the provision of clean water, to the mortality decline. The literature seems to be biased toward the link between the poor physical environment (of which water quality is an indicator) and mortality, rather than toward an analysis of the more complex socio-economic and psycho-social variables influencing mortality (Cvjetanovfc 1986; Bradley et al. 1992). Unidimensional explanations almost certainly represent a gross simplification of a complex set of circumstances ultimately leading to an improvement in life chances. A model suitable to the task of explaining mortality declines would have to recognize improvements in medicine and medical care, changes in virulence of disease-producing organisms, changes in the economic well-being of the population, and changes in sanitation and public health not as competing explanations of the mortality transition but as co-existing and complexly-interrelated causes of mortality decline. It is not likely that there will be many opportunities for studies of this sort. Yet we fully agree with a remark made by Briscoe and colleagues (Briscoe et al. 1986) when discussing earlier attempts to use historical cohort designs to . examine the determinants of mortality in developed countries in the nineteenth century. In their opinion, the major constraint for these studies is 'limited imagination and analytic skill rather than the absolute absence of reasonably reliable data'**.**

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