The Effects of Access to Health Care on Infant Mortality in Indonesia

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Draft--February, 1993

Governments in developed and developing countries alike have long promoted the health of their populations by subsidizing health care services. Common reasons for the subsidies are the belief that externalities arise from a healthy population, or that government is the guarantor of the right to good health. In the past two decades national governments, multilateral development agencies, and private voluntary organizations have focused on bolstering developing countries' institutional capacities to provide health care (Jamison and Mosley, 1991; Grant, 1990, 532).

Efforts to evaluate the impact of interventions on health outcomes have accompanied initiatives to provide services. Evaluation research is important: resources available for subsidizing health care have shriveled since the worldwide recession of the early 1980s and the ensuing international debt crisis. Accordingly, policy makers have increasingly emphasized the importance of choosing from among alternative programs and interventions those with the greatest potential to affect health outcomes. The concern with prioritizing is visible in several major policy initiatives, such as UNICEF's GOBI-FFF strategy and the World Bank's recent health sector priorities review (Grant, 1990, 532; The World Bank, 1990).

In this paper we examine the impact of access to health facilities and personnel on infant and child mortality in Indonesia. We explore the processes by which the spatial

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1 The recession and debt crisis have caused resources available for health to diminish for several reasons. First, growth in GDP slowed during the mid-1980s. Second, interest payments on public debt have drained national budgets and diverted money away from subsidies to public services. Third, at an individual level, inflation and unemployment have reduced purchasing power (The Commission on Health Research for Development, 1990).
distribution of health services arises and account for these processes in a model relating the mortality risks of individuals to access to health services.

The results of the analysis should inform policy-makers as to the effectiveness of various health interventions, as well as contribute to an understanding of the determinants of mortality. While researchers have devoted considerable attention to the impact of individual-level biomedical, demographic, and socioeconomic characteristics on health outcomes, considerably less is known about how community institutions affect health, particularly at a national level (though such institutions are included in theoretical models of health).\textsuperscript{2} Reasons for this dearth in knowledge include inadequate data and the methodological difficulties of multilevel analysis (Hobcraft, 1985). In particular, few analyses have controlled for the fact that institutions may be targeted to certain types of communities.

The data for this analysis are drawn from two sources. Community-level data on access to health services come from two censuses of village infrastructure implemented by the Indonesian Central Bureau of Statistics in 1983 and 1986. Information on levels of socioeconomic development was collected from the village leader and other village officials. The questionnaires vary little across the two years. These data are unique in that they are available from two points in time for each of Indonesia’s 63,000 villages. The information about health services, however, is rudimentary. Individual-level data on infant mortality are from the 1987 Demographic and Health Survey for Indonesia (a nationally representative sample of 11,884 women). Retrospective birth histories are available from this data set, as is information on individual and family-level determinants of mortality. Calculations from the 1987 DHS yield an infant mortality rate of 75.2 per 1000 for the period 1977-1987 (DHS, 1991). Women surveyed in the 1987 DHS are matched by block to census data on

\footnote{See, for example, Mosley and Chen (1984), Schultz (1984), Schultz, 1985; Boulier and Paqueo (1988).}
characteristics of the villages and subdistricts in which they reside. The matching process
was tedious but successful. All but ten DHS villages were matched to the community data.

Six sections make up the remainder of the paper: a discussion of approaches and
findings of related studies, a description of health services in Indonesia, an explanation of the
methods used in the analysis, presentation of the results, and a discussion and conclusions
section.

The Effects of Access to Services on
Infant and Child Health Outcomes

A number of studies of the determinants of infant and child mortality include
measures of access to health facilities or personnel. We briefly review results from studies of
individual-level mortality risks.

Al-Kabir (1984) assesses the effects of distances to hospitals, government dispensaries,
family planning clinics, qualified doctors, other doctors, and traditional birth attendants on
neonatal, post-neonatal, and child mortality in Bangladesh (Bangladesh Fertility Survey Data,
1975-76). Bivariate associations between these variables and mortality are consistent with the
hypothesis that proximity to care decreases mortality.

The association between distance to health facilities and mortality of under fives
weakens considerably with the inclusion of controls for individual and household variables
(eg. length of previous birth interval and maternal age and education). Even with controls,
however, increasing distance to a hospital is accompanied by a rise in mortality risks of
children: mortality rates for children living further than ten miles from a hospital are 40%
higher than those for children living within three miles. Children also fare poorly when they
live far from a qualified doctor. Mortality rates are 54% higher when children live further
than five miles from a doctor than when a doctor resides in their village.
Proximity to a family planning clinic reduces the mortality of both children and neonates. Mortality rates are 30% higher for newborns living further than 10 miles from a clinic than for their counterparts living within three miles, while children ten miles away have 60% higher mortality than their closer counterparts.\(^3\)

Al-Kabir’s results are generally confirmed by Hossain’s analysis of data from the Bangladesh Institute for Development Studies (Hossain, 1989). Presence within a village of family planning clinics lowers the child mortality experience of women, as does presence of a dispensary.\(^4\) Presence of hospitals does not lower mortality. Controls include maternal education and childhood residence, and paternal education, income, and occupation.

DaVanzo (1984) tests for a relationship between distance to facilities and infant mortality with data collected as part of the Malaysian Family Life Survey. She finds no evidence that such a relationship exists, possibly because the mortality data dates as far back as to the late 1940s while the facility data refers to the time of the survey.

In an analysis of data from the 1973 Colombia census, Rosenzweig and Schultz (1982) find that clinics and hospital beds are associated with lower child mortality ratios (see footnote four), particularly in urban areas, but the effects of these variables decline dramatically with the addition of controls for municipality-level average women’s schooling attainment. Family planning expenditures exert only a small effect on the mortality ratios of younger urban women, while rural health posts have no significant effects on mortality ratios in rural areas. Rosenzweig and Schultz also include information on the municipality-level

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\(^3\) This relationship may occur because at the time of the survey family planning clinics participated in supplementary feeding projects and were also sources of maternal and child health care.

\(^4\) The dependent variable in Hossain’s analysis is the ratio of the observed proportion of a woman’s children that have died relative to the expected proportion, where the expected proportion is calculated from a standard mortality schedule. The method is described in detail in Trussell and Preston (1982).
availability of public and private clinics, dispensaries, and mobile care units, but find no impacts on mortality ratios.

In India (1971 census data) availability of family planning clinics and dispensaries significantly reduce women’s child mortality ratios, while hospitals have a negative but insignificant effect on mortality (Rosenzweig and Wolpin, 1982). The presence of other health facilities (health centers, subcenters, and maternal child welfare clinics) significantly raises child mortality. In simulations of the effect of increasing facility availability, Rosenzweig and Wolpin find that doubling the number of villages with family planning programs would reduce child mortality ratios by about 10%, while doubling the number of villages with dispensaries would lower the ratios by 25%.

Several studies examine the impact of access to health care on childhood nutritional status. In Brazil higher per capita numbers of nurses (within municipalities) are associated with being short for one’s age among children of literate women living in urban areas (Thomas and Strauss, 1990). Increasing numbers of hospital beds per capita are also associated with shorter children. The authors speculate that these perplexing results arise from a failure to control for quality of services, or because larger facilities may be disproportionately located in in areas where health outcomes are poor.

In the Ivory Coast increasing distance to health facilities is associated with lower height and weight for height, but the coefficients are small and statistically insignificant (Strauss, 1990). Children are significantly lighter and shorter when they live in communities where village leaders report problems with medical facilities, such as lack of medicine or congestion. Apparently, the absence of a traditional practitioner improves nutrition outcomes.
No clear picture of the effects of access to services on health outcomes emerges from these studies. Some health services improve health outcomes in some settings (Al-Kabir, 1984; Hossain, 1989; Rosenzweig and Wolpin, 1982). In other settings access to services appears to have no effect on health outcomes (DaVanzo, 1984; Strauss, 1990; Rosenzweig and Schultz, 1982). Occasionally certain services appear to worsen health outcomes (Rosenzweig and Wolpin, 1982, Thomas and Strauss, 1990).

While it is unreasonable to expect perfect consistency across studies from a multitude of countries and time periods relating disparate health services to various health outcomes, the review fails to build a compelling body of evidence regarding the importance of health services. Possibly access to health services is not a crucial determinant of health status, or perhaps measures of access to health services do not capture the dimensions of services that affect health outcomes (e.g. quality of care).¹

It is also possible that the approaches adopted by the analyses reviewed here are statistically flawed by unobserved (and hence omitted) factors. The processes that produce the distribution of health services relative to the characteristics of individuals are rarely known to researchers but can bias the measured effects of services on behaviors and outcomes. For example, public health care may be targeted to the poor, who have relatively poor health. If the analysis does not control for this nonrandom element in service allocation, access to public health care may appear to produce poor health.²

Rosenzweig and Wolpin (1986) illustrate this problem in an analysis of the effects of health and family planning programs on the (standardized) height and weight of Filipino

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¹ Or perhaps measures of health status do not incorporate the aspects of health that access to facilities affects.

² The location of private facilities is also susceptible to manipulation by processes the researcher cannot observe, such as a profit motive on the part of private practitioners.
children. They experiment with three specifications of the relationship. The first specification is a simple OLS cross-sectional regression of height and weight of children as a function of the existence of health and family planning programs within the barangays in which the children reside. The second specification is a fixed effects model that removes the effect of the barangay on nutritional status. The third specification uses data from two points in time to control for differences among children in exposure to the program arising from differences in age of the children and differences among barangay in when programs became available.⁷

The first two specifications produce statistically insignificant relationships between nutritional status and the existence of health and family planning programs. Both programs appear to decrease height, and health clinics appear to decrease weight. The first-differenced regressions produce completely different results. Exposure to health or family planning programs has a positive and significant effect on both height and weight. Children living in areas where (and time periods when) a family planning program has always been available are 7% taller and 12% heavier than their counterparts who have never lived in an area with a program. Children who have always lived in an area with a health clinic are 5% taller and 9% heavier than children who have never lived in such an area. The authors attribute the change across the models in the estimated coefficients to the nonrandom placement (and timing of placement) of clinic services.⁸

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⁷ The third specification is a first-differenced regression using matched samples from 1975 and 1979, so that differences in nutritional status between the two time periods are related to changes in the duration of exposure to the program.

⁸ Other papers that employ this technique include Gertler and Molyneaux (1993), and Pitt, Rosenzweig, and Gibbons (1992).
Health Services in Indonesia

The idea that regression results could be biased by unobserved processes generating a
distribution of health services that is nonrandom with respect either to health status or to its
determinants is intuitively credible. Within the public sector planners surely allocate health
services according to some design—whether it is to target high morbidity/mortality areas for
receipt of services, or to place facilities in villages most accessible to large numbers of people.
Within the private sector, practitioners almost certainly locate services in areas where clients
want private care and can afford to pay for it.

In the context of structuring an evaluation of health services in Indonesia, it is useful
to consider evidence that mechanisms such as those described above indeed operate.
Evidence emerges both from stated policies of the Ministry of Health and from empirical
analyses.

The Ministry of Health allocates hospitals, health centers, and health subcenters
according to a set of general guidelines. Government hospitals are located only in district
capitals and so are relatively inaccessible to much of the population (a district consists of
three to five hundred villages) (USAID, 1988). Health centers are the basic source of primary
health care, particularly in rural areas. Health centers are generally located in the subdistrict
capital and headed by a doctor, who oversees a midwife, one or more nurses, and various
paramedical workers (MOH, 1990). Each subdistrict (consisting of 20-40 villages) claims at
least one health center. Densely populated subdistricts (in urban and suburban areas) have
more than one health center. In addition to health centers, health subcenters are located in
the more peripheral villages of remote subdistricts in which travel is difficult. Subcenters are
staffed either by resident paramedical workers or are opened only once or twice a week by
workers travelling out from the health center (MOH, 1990; Berman, Ormand, and Gani, 1987).
In Indonesia private practitioners, including paramedics, midwives, and doctors, are an important source of health care (Berman, Sisler, and Habicht, 1989; Haliman and Williams, 1983; Linnan, 1990; Streatfield, Tanpubulon, and Surjadi, 1990). The distribution of private practitioners partially reflects facility allocation policies of the Ministry of Health, because staff of public facilities almost always operate private clinics and practices after public facilities have closed for the day (USAID, 1987; Berman, Ormand, and Gani, 1987). Villages with a health center are likely to claim a doctor's practice and midwife's delivery clinic as well. Generally private services are far more available in urban than in rural areas, implying that private practitioners locate in more developed areas where residents have a demand (and are able to pay) for private services (Brotowasisto et al., 1988; World Bank, 1990).

Ministry of Health policies and other studies of Indonesian health services suggest that the distribution of health facilities, both public and private, is related to factors such as population density, level of socioeconomic development, integration into transportation networks, and administrative rank, all of which may be related to morbidity and mortality. Data from the 1986 census of village infrastructure confirm these patterns.

Table 1 presents correlation coefficients among various community institutions: hospitals, health centers, health subcenters, doctors' practices, maternity clinics, health workers, high schools, and traditional midwives. Levels of community institutions are measured as the proportion of villages in a subdistrict with the attribute in question. Land area and population of each subdistrict are included, as is the proportion of villages receiving INPRES development money (presidential grants targeted to underdeveloped areas).

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9 The community data are available at two levels of aggregation— the village and the subdistrict. In Table 1 we analyze service availability in the subdistricts within which the DHS villages were located.
Several interesting patterns emerge from the table. First, population size is positively correlated with levels of the more "modern" community institutions: hospitals, health centers, maternity clinics, doctors' practices, and secondary schools. In contrast, population size is negatively correlated with levels of health subcenters and receipt of INPRES funds. Land area, on the other hand, exhibits an opposite pattern. Land area is positively correlated with levels of health subcenters and INPRES money, but negatively correlated with levels of the more modern institutions.

Correlations among hospitals, health centers, maternity clinics, doctors' practices, and secondary schools are generally high (.6 and above). These institutions are much less strongly correlated with health subcenters (coefficients are all less than .10). Modern institutions are negatively correlated with INPRES money (coefficients vary from -.25 to -.53). The correlation between INPRES and health subcenters, however, is positive (.21). In sum, the more modern institutions tend to cluster together in areas of population concentrations. Health subcenters (and to a lesser extent health workers), are less likely to be in populated areas and their presence is only weakly related to the presence of other, more technology-intensive institutions. INPRES funds do appear to be successfully targeted toward underdeveloped subdistricts: underpopulated areas with relatively few hospitals, high schools, health centers, or doctors.

Taken together, stated policies of the Ministry of Health, results of other research on health services in Indonesia, and patterns within the village census data provide fairly firm evidence that the distribution of health facilities (as well as other institutions) is not random. The distribution partially reflects government allocation policies and partially reflects a tendency of modern institutions to cluster together in developed areas of population concentration. This finding is not surprising, but it indicates the need to design a statistical
analysis free from the assumption that facilities are distributed randomly with respect to mortality or its determinants.

Statistical Methods

The problem of a nonrandom distribution of health facilities can be conceptualized as an issue of omitted variables. Features of a village make it attractive to some institutions and unattractive to others. If these factors are related to mortality but are excluded from the model, the estimated parameters will be biased. Including the factors directly is problematic in that they are potentially numerous, may differ across the institutions of interest, and may be difficult to conceptualize, let alone measure empirically.

The estimation strategy used in this analysis was first formulated by Chamberlain (1980) as a means of eliminating bias from omitted variables in probability models. Chamberlain develops his argument with the linear regression case:

Eq. 1:

\[ y_{it} = \beta' x_{it} + a_{it} + \epsilon_{it} \]

where each of N groups contributes T observations. The \( a_{it} \) are group-specific effects. If an individual contributes multiple observations, each "group" is an individual and the \( a_{it} \) are individual effects. If the \( a_{it} \) are correlated with the \( x_{it} \)'s and the \( a_{it} \) are not included in the equation then the parameters will be biased. In this case a regression of \( y \) on \( x \), with dummy variables indicating membership group provides maximum likelihood estimates of the parameters in the equation (Chamberlain, 1980). The dummy variables solution does not extend to probability models.\(^{10}\) Instead, Chamberlain suggests maximizing a conditional likelihood function. The likelihood function is conditioned on the sum of the \( y_{it} \)’s:

\(^{10}\) Chamberlain (1980) shows that for several probability models, including the logit model, maximum likelihood estimates of parameters are biased when group-specific dummy variables are included.
Eq. 2:

\[ L^c = \prod_i \text{Prob}\{Y_{i1} = y_{i1}, Y_{i2} = y_{i2}, \ldots, Y_{iT} = y_{iT} | \sum_i y_{it}\} \]

When \( T = 2 \) and the \( y_{it} \)'s are binary one can maximize the likelihood with a logit specification. The sum over \( T \) of the \( y_{it} \)'s must equal 0, 1, or 2. When the sum equals 0 or 2, \( y_{i1} \) and \( y_{i2} \) are identified given their sum. Within the likelihood function any term where the sum of the \( y_{it} \)'s equals 0 or 2, will itself equal one and so will not contribute any information (Greene, 1990, 687).

The cases of interest, then, are when the \( y_{it} \)'s sum to one, so that the sum does not identify the values of \( y_{i1} \) and \( y_{i2} \). For these pairs of observations one estimates the probability that:

Eq. 3:

\[ \text{Prob}\{Y_{i1} = 1, Y_{i2} = 0 | \sum_i y_{it} = 1\} = \frac{\text{Prob}\{Y_{i1} = 1, Y_{i2} = 0\}}{\text{Prob}\{Y_{i1} = 1, Y_{i2} = 0\} \ast \text{Prob}\{Y_{i1} = 0, Y_{i2} = 1\}} \]

Substituting the formula for logistic regression, the right side of the equation becomes:

Eq. 4:

\[ \frac{1}{1 + e^{a_1 + b_1 x_{1i}}} \ast \frac{e^{a_1 + b_1 x_{1i}}}{1 + e^{a_1 + b_1 x_{1i}}} \]

\[ \frac{1}{1 + e^{a_1 + b_2 x_{2i}}} \ast \frac{e^{a_1 + b_2 x_{2i}}}{1 + e^{a_1 + b_2 x_{2i}}} \]

where the \( a_i \)'s are the effects of being one of the two members of a particular group. The equation simplifies to:

Eq. 5:
\[ P(y_{ij} = 1, y_{i2} = 0 | \sum_t y_{ijt} = 1) = \frac{e^{x_{ij}^T \beta}}{1 + e^{x_{ij}^T \beta}} \]

Since \( a_i = a_i^1 = a_i^2 \) for any group of two, the \( a_i \)'s disappear from the equation when one is subtracted from the other and the parameter estimates are no longer biased. The alternative is to estimate a standard, unconditional logistic regression:

Eq. 6:

\[ P(y_{it} = 1) = \frac{e^{x_{it}^T \beta}}{1 + e^{x_{it}^T \beta}} \]

where the \( a_i \)'s are not subtracted out and the B's are biased.

One can test the hypothesis that the parameters obtained from maximizing the unconditional likelihood are inconsistent relative to the parameters obtained from maximizing the conditional likelihood (Maddala, 1988, 435). The test statistic, which follows a \( \chi^2 \) distribution, is a Hausman statistic, calculated as:

Eq. 7:

\[ x^2 = (B_{ml} - B_{cm})' (Var_{cm} - Var_{m})^{-1} (B_{ml} - B_{cm}) \]

Chamberlain's approach provides a means of estimating the effects of independent variables on a dichotomous outcome variable when observations are grouped and group membership affects the outcome. The conditions for using Chamberlain's approach with logistic regression are that each group contains two members, that the outcomes of the members differ, and that explanatory variables vary between the members.

The problem in this analysis is to estimate the impact of measured group-level variables (access to health care within villages) on individual outcomes. Omitted group-level variables (such as wealth or degree of remoteness) are a potential source of bias. The data and analysis problem are similar to the scenario under which Chamberlain recommends
conditional maximum likelihood estimation with logistic regression. The outcome variable (death) is binary, the data are grouped at the village level, and independent variables vary within villages over time. Villages, however, have more than two members. If infants are grouped within villages into pairs of observations, Chamberlain's approach provides a means of eliminating bias from omitted variables.

In the first step we separated births into two cohorts: births before mid-1983 and births after mid-1983 (see below). Infants from the earlier cohort were matched to the 1983 community-level data, while infants from the later cohorts were matched to the 1986 data.

To generate pairs of infants within each village we randomly paired infants from the early cohort with infants from the later cohort. Because the factors that produce or prevent a neonatal death may be quite different from those associated with a post-neonatal death, we performed the matching procedure twice to avoid matching neonatal deaths to post-neonatal deaths. First we coded each child according to whether he or she had died within the first month of life and, within villages, randomly matched children born between 1983 and mid-1984 with children born after mid-1984. We kept the pairs when one child died and the other survived. We repeated the steps with the children who survived the neonatal period, coding them according to whether they survived to thirteen months of age.\textsuperscript{11} Within each village we randomly matched infants born between 1980 and mid-1983 to infants born

\textsuperscript{11} In choosing a cutoff for mortality after the neonatal period, one must consider the problem of censoring. The individual level data were collected in September of 1987. One must restrict the analysis to children born long enough before the survey so as to have had a chance to survive for the entire period one is considering. I chose to analyze survival to thirteen months rather than to a later age or to twelve months because I can consider children born after the 1986 community data were collected and because a number of children were reported to have died at in the thirteenth month of life (had I considered infant mortality they would have been excluded). If I analyzed survival over a longer period, say from months one to eighteen, I could not consider children born after March of 1986.
between mid-1983 and thirteen months before September of 1987 (the interview date). Again, we kept the pairs when one infant died and the other survived.\footnote{We emphasize that the matching was done without replacement, to guarantee that the resulting pairs were obtained randomly.}

Infants should be matched to the community data so that the period for which they are exposed to the risk of death coincides (roughly) with the period to which the community data refers. The neonatal exposure period is only a month long, while the other period is one year long. Accordingly, the matching rules differ by exposure period. The diagram below depicts the matching rules.

**Matching Rules: Individual Observations to Community-Level Data**

**Month 0:**

<table>
<thead>
<tr>
<th>1/80</th>
<th>6/84</th>
<th>8/87</th>
<th>9/87</th>
</tr>
</thead>
<tbody>
<tr>
<td>xxxxxxxx xxxxxxxxxx xxxxxxxxxx</td>
<td>ooooooooooo ooooooooooo</td>
<td>cccccccc</td>
<td></td>
</tr>
</tbody>
</table>

**Months 1-13:**

<table>
<thead>
<tr>
<th>1/80</th>
<th>6/83</th>
<th>8/86</th>
<th>9/87</th>
</tr>
</thead>
<tbody>
<tr>
<td>xxxxxxxx xxxxxxxxxx</td>
<td>ooooooooooo ooooooooooo</td>
<td>ccccccccccc</td>
<td></td>
</tr>
</tbody>
</table>

xxx = births in this period matched to 1983 data  
ooo = births in this period matched to 1986 data  
ccc = censored

To summarize, we constructed two files of pairs of infants— one for neonates, and one for postneonates plus the thirteenth month of life (hereafter referred to as the postneonatal file). Each pair of infants serves as an observation, with the following characteristics: both infants were born in the same village, but in different time periods, and one infant had died in the specific period of exposure. The earlier infant was assigned the 1983 village.
characteristics, while the later infant was assigned the 1986 village characteristics.\textsuperscript{13} For each observation (pair) we created a dependent variable measuring whether the early child died (zero) or the later child died (one). The independent variables are the differences in characteristics across the two pairs.

The model is a logistic regression of the odds that the late child died, yielding the log odds that the later child died rather than the earlier child, as a function of change in access to health facilities, personnel, and sanitation infrastructure between time periods. If access to facilities improves survival chances, the chance that later child died rather than the earlier child should decrease as access to facilities improves over time. The advantage of this approach is that the parameters are not biased by omitted fixed characteristics at the village level.\textsuperscript{14}

A disadvantage of the approach is that the conditions on the data set severely limit the number of observations. The women in the DHS sample gave birth to almost thirteen thousand children between 1980 and September, 1987. Only 652 pairs of children (298 neonatal observations, 354 postneonatal observations) from 269 villages fit the criteria for entering the data set. The small number of pairs limits the precision of the parameter estimates.

The pairs provide a means of estimating the effect of community features on mortality risks, free of bias from omitted village-level characteristics. The alternative approach is to estimate the parameters of the community features from a data set of cross-sectional

\textsuperscript{13} The community data are available for two years but I analyze births from over an almost eight year period. The community data measure inexacty the levels of facilities in existence during infant’s survival experiences.

\textsuperscript{14} The approach does not yield unbiased estimates, however, if changes in community characteristics are correlated with omitted village characteristics that are changing over time. For example, if changes in the number of government health clinics in a village between 1983 and 1986 are a response to planners’ observations that mortality in the village is increasing, then the fixed effects parameter estimates for health clinics will be biased.
observations. The parameter estimates from the cross-sectional data set may be inconsistent because of the omitted village effects. If the data set of pairs is a subset of the cross-sectional data set one can construct a Hausman statistic to test formally whether the cross-sectional estimates are inconsistent.

We analyze the effects of individual-, family-, and community-level variables. Among individual and family-level variables we include those by which survival risks generally differ: first birth, sex, birth order, length of the preceding birth interval, and maternal education. We included the infants’ birthdates to capture the time trend in mortality. Each variable’s values are calculated as the difference in values between the two pairs of children.\textsuperscript{15}

We focus on five community variables. The numbers of health workers and doctors capture access to trained health personnel. The presence of a government health center and the numbers of maternity clinics capture access to health services. We also include a variable measuring whether or not the majority of villagers use a private toilet.

The four health services variables are of interest to policy. The Government of Indonesia invested considerable resources in the mid-1980s training health workers—doctors, nurses, midwives, and paramedical workers (USAID, 1988). Between 1984 and 1988 these workers’ numbers increased dramatically. The size and direction of the effects of doctors, health workers, and maternity clinics indicate whether increases in manpower have translated into reductions in mortality risk.

The presence of maternity clinics reflects both government policy of encouraging trained nurse-midwives to open small private clinics for prenatal, delivery, and postnatal

\textsuperscript{15} For example, maternal education is measured as the difference between the education of the mother of the early child and the mother of the late child.
care, and increases in the numbers of women trained to provide such services (USAID, 1988; Soh-Sanu, 1989). Some of these women probably run clinics in addition to serving at health centers, while others operate clinics after retiring from government health centers.

Health centers are the keystone of Indonesian government efforts to provide health services. The number of centers more than doubled in the 1970s (Brotowasisto et al., 1988). Growth in facilities slumped in the 1980s, partly because of macroeconomic crises and partly because basic goals of service provision had been achieved in the 1970s (Dichter et al., 1990). We include health centers in the analysis because their presence reflects substantial earlier investments in health infrastructure.

We include access to private toilets in the models because their presence reflects the state of sanitation infrastructure in the village. In a previous analysis of aggregate mortality levels with this data set, access to private toilets was strongly associated with mortality levels (Frankenberg, 1992).

We measure changes in access to facilities as changes in the number of facilities or personnel within a village. We experimented with other measures of accessibility, such as per capita availability, an indirect estimate of proximity, and measurements at various levels of aggregation (village versus subdistrict). We rejected these measures because they are more difficult to interpret in the context of a model of differences and because the incorporation of population and land area into the measure of accessibility substantially increases the level of correlation across the facilities of interest. Frequency distributions for both levels of and changes in access to facilities are presented in Tables 2 and 3. Table 2 shows that levels of

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16 For government health centers we measured the presence of any health center (a dichotomy of some versus none) rather than the absolute numbers. Questions about the presence of health centers were asked slightly differently in the two years of the census. Treating availability as a dichotomy minimizes the chance that change in availability is a product of a reporting differences rather than of an actual increase in facilities.
health services increased between 1983 and 1986 for each of the services we are considering. Table 3 shows that a considerable number of villages experienced a change in access to facilities between 1983 and 1986.

Researchers have often suggested that the impact of access to health facilities varies by level of education (Caldwell, 1979; Caldwell, Reddy, and Caldwell, 1983; Al Kabir, 1984; Rosenzweig and Schultz, 1982). One argument suggests that education and access to services are substitutes: access to services is more beneficial to the infants of poorly-educated women than to the infants of well-educated women. Another argument posits that education and access to health services are complements: infants of educated women have more to gain from access to services than do the infants of uneducated women (Barrera, 1990).

Both arguments are logical, and each may hold in different settings. The relationship between maternal education and access to facilities depends on how education affects use of health services, how use of services lowers the risk of death, and how education affects the ability of mothers to translate health service resources into lower risks for their babies. Education facilitates service use if educated women feel more competent interacting with health service personnel (producing a positive interaction between maternal levels of education and access to health services) (Lindenbaum and Elias, 1983). If health services lower mortality risks by changing women’s behavior (e.g. convincing mothers to use oral rehydration therapy) and educated women are better learners, the interaction between access to services and education will be positive. On the other hand, increasing service availability may benefit uneducated women more if these are the women for whom distance or congestion discourage service use. Similarly, if educated women already follow healthy child care practices, increasing service availability may benefit uneducated women more because the scope for changes in their behavior as services become more available is greater.
One can explore the relationship between access to services and levels of education by interacting educational level with access to services. We estimated models that included interactions between education and the health infrastructure variables. We distinguish between women who have completed at least a primary school education (seven or more years) and women who have less than a completed primary school education.¹⁷

For each pair, we interacted the education of the early child’s mother with the level of facilities in 1983 and the education of the later child’s mother with the level of facilities in 1986. The two products were differenced. The interaction term is zero when facilities were not available in either year, when neither woman was educated, or when the number of facilities and levels of education do not change over time. The coefficient of the interaction term multiplied by the value of the interaction term for a pair of children, yields a value that adjusts the main effect of a change in access to facilities for the absolute and relative levels of maternal education within the pair (so that the effect of adding a health center is different when both mothers are educated than when neither is educated).

Results

The first issue to resolve is whether neonatal and postneonatal observations can be pooled. The issue can be explicitly tested by comparing the likelihood statistics from a model applied to a pooled data set with the sum of the likelihood statistics from separate models (Table 4).¹⁸ The test statistic is insignificant: one cannot reject the null hypothesis that the effects of changes in the variables on the neonatal mortality risks are the same as for

---

¹⁷ We estimated four separate models, each of which included an interaction term between maternal education and one of the health service variables and so avoided estimating a large model in which the variables were highly correlated. Nevertheless, correlations between levels of facilities and the interaction of levels of facilities with maternal education are fairly high (between .4 and .8).

¹⁸ Summing these two likelihoods is equivalent to estimating a model with the pooled data set that includes an interaction term between the age interval and each of the independent variables, so that the effect of each independent variable can vary with the age of the child.
post-neonatal risks. This result contrasts with the general wisdom that the determinants of mortality change over the first year of life. Possibly the number of observations in this analysis is too small for differences in the magnitudes of effects across age intervals to translate into statistically significant differences in the comparative fit of the two models.

Some of the coefficients do exhibit different effects across the two age intervals. The differences are broadly consistent with other analyses suggesting that as a child ages, socioeconomic factors gradually replace biomedical factors as the most important determinants of mortality (DaVanzo, Butz, and Habicht, 1983). Specifically, the coefficient on sex of the child is much larger for the neonatal pairs than for the postneonatal pairs, the opposite pattern holds for maternal education, and the community variables have larger (and more nearly significant) effects for postneonatal than neonatal mortality.

The preferred model is the one in which neonatal and postneonatal observations are pooled (Table 5). The parameters of the model reveal the effects of access to health infrastructure and personnel on the relative risk of mortality. By comparing the fixed effects model with the alternative (cross-sectional) model, one can determine the degree to which results are affected by failure to control for omitted variable effects.

To test formally for differences between a fixed effects model and a cross-sectional model, one must estimate a standard logit model, with explanatory variables identical to those included in the fixed effects model (Maddala, 1988, 435). The construction of the data set for the cross-sectional model should parallel that for the fixed effects model. Accordingly, we constructed a pooled file of neonatal and postneonatal observations, in which infants
were matched to the community data as depicted in Figure 1. This file is equivalent to the file of pairs constructed for the fixed effects models.\textsuperscript{19}

The results from the fixed effects model and the cross-sectional model are presented in Table 5. In both models individual and family-level mortality determinants exert relatively predictable effects on the odds of death, and the effects are of comparable magnitude.

The fixed effects estimates indicate that a decline in mortality risks has occurred over time in Indonesia. Of children born one month apart, the later child’s odds of death are .5% lower than the earlier child’s. Male children have about 29% higher odds of death than female children.\textsuperscript{20} The coefficients on previous interval length, first births, and birth order must be interpreted together (first births must be compared to a child of a certain birth order, with a certain preceding interval length). First births face mortality risks comparable to those of second order births with interval lengths of 30 months or more, or to those of third order births with interval lengths of 34 months or more. Each year of maternal education decreases the risk of death by about 7% (comparable to the effects documented by Cleland and van Ginnekin (1988) in a major review of the maternal education and infant mortality).

If increases in the availability of the community institutions decrease mortality, the parameters of the community-level variables should be negative. The coefficients on maternity clinics, health centers, and doctors are negative, as is the variable indicating that a majority of village residents have access to a private toilet.

\textsuperscript{19} Most children appear in this pooled cross-sectional file twice, once as a neonatal observation and once as a postneonatal observation. This repetition of observation parallels discrete time hazards models, which are commonly used to study demographic phenomenon (Trussell and Hammarslough, 1984; Martin et al., 1983; Foster et al., 1986). To the extent that the mortality risks of a child are independent across age intervals, the repeated observations are not a problem. If mortality risks are not independent the estimated standard errors will be incorrect.

\textsuperscript{20} This excess risk is consistent with the ratio of the male to female infant mortality rate calculated for all births between 1977 and 1987, which is 1.28 (Sullivan, Bicego, and Rutstein, 1990). Males were also found to have considerably higher mortality risks than females in the World Fertility Survey of Java and Bali (Martin et al., 1983).
The coefficients on maternity clinics and doctors are significant (p<.03 and p<.06, respectively). Within a village an increase of one maternity clinic increases the survival chances of a child with access to that clinic by about 16%, relative to the child born before the clinic existed. The positive impact of additional doctors is much smaller. An additional doctors increases an infant's survival chances by a little less than 2%. The coefficients on private toilets and health centers are not significant, but are somewhat larger than the coefficient on doctors.

Contrary to expectations, the effect of additional health workers on relative mortality risks is slightly positive. An infant born after health workers are added to a village has about a 1.3% greater odds of death than an infant born before the addition of health workers. The effect is significant at the 10% level.

The effects of the community variables in the model estimated with the pooled cross-sectional data set are very different. In this model, access to private toilets exerts a dramatic, negative impact on mortality risks, lowering the odds of death by about 20%. The coefficient is significant at the 3% confidence level. None of the other community variables significantly affect mortality risks. The coefficients on maternity clinics and doctors are negative, but much smaller than the estimated coefficients in the fixed effects model. In the cross-sectional model the coefficient on doctors shrinks to about one quarter of its size in the model of differences. The cross-sectional estimate of the coefficient on maternity clinics is about one tenth its size in the model of differences. The coefficient on health centers is slightly larger in the pooled cross-sectional estimates than in the difference model.

One can test the hypothesis that the parameters from the pooled cross-section are consistent with a Hausman statistic. The Hausman statistic follows a X² distribution. The
statistic is 23.6, which is significant at the 2.5% level of confidence. The hypothesis that the estimates from the pooled cross-sectional data are consistent is rejected.

One can also determine which parameters are particularly inconsistent by constructing individual Hausman statistics for each parameter. The variables for which the individual Hausman tests are significant are birth interval lengths, maternity clinics, health workers, and doctors. The significance of the Hausman statistic for these variables implies that fixed, omitted characteristics of the village are correlated both with mortality and with the distributions of birth interval lengths, maternity clinics, health workers, and doctors. Eliminating these fixed factors from the model produces markedly more consistent estimates of the effects of these characteristics on mortality risks.

The significance of the overall Hausman statistic means that the effects of community characteristics on infants’ survival chances as estimated in a standard logit analysis are biased. To what extent would interpretation of the pooled cross-sectional results generate misleading conclusions? With the exception of previous interval lengths, the effects of individual and family level determinants are consistent across the two models, as is the effect of health centers. The lack of change for health centers is surprising given the strong pattern of correlations presented earlier, where health centers were positively correlated with doctors and maternity clinics, but subcenters were negatively correlated with other modern institutions (suggesting that centers and subcenters are not distributed randomly). This result may arise from combining centers and subcenters into one variable (necessary because the two were not distinguished from each other in the 1983 data). Possibly, by lumping the centers and subcenters into one measure, biases associated with their nonrandom distributions, which produced opposite patterns of correlations between the two, cancel out.
The coefficients on health workers, private toilets, maternity clinics, and doctors from the pooled cross-sectional results do generate misleading interpretations. The positive effects of toilets are massively overstated (probably because private toilets are correlated with wealth). The positive effects of doctors and maternity clinics are understated. If one accepted the cross-sectional results one might consider abandoning health centers, maternity clinics, and doctors in favor of mass construction of private toilets. The fixed effects estimates indicate that such a strategy would be a mistake.

None of the interaction terms between education and health services or personnel are significant, probably because correlations between the interaction terms and the main effects are somewhat high, even after dichotomizing educational levels. Including the interaction terms does not improve the fit of the models (statistical results not shown).

Although the interaction terms are not significant, the signs of the terms are interesting. The positive impact of maternity clinics on infant survival chances is considerably stronger for women with at least a primary level of education than for women with lower levels of education. Results are similar for access to doctors’ services. Increasing access to doctors’ services benefits infants of women with at least a primary school education more than it benefits their less-educated counterparts, but the difference is quite small.

In contrast to private facilities, government health centers benefit women with low levels of education more than women with high levels of education. Establishing a health center decreases the odds of death of infants born to women with low levels of education by about 6.5%. For the infants of better-educated women, the reduction is only 2%. For health workers, who seem to have a positive impact on mortality, the deleterious effect of their presence is lessened for women of higher educational attainments.
Discussion

The potential for a positive impact of maternity clinics on mortality risks is clear from a theoretical standpoint. Maternity clinics provide services to women while they are pregnant, when they deliver, and after the birth in the form of well-baby care. These clinics concentrate on services that directly affect fetal development, birth, and infant health, so they are well-positioned to affect infant mortality. The services provided by health workers, doctors, and at health centers are much more general. Nevertheless, the large, positive effect of maternity clinics is somewhat of a surprise. Maternity clinics have received little attention in the literature on health services, public or private, in Indonesia (although a recent article documents their emergence as a source of care) (Soh-Sanu, 1988). Recent policies of the Ministry of Health suggest this situation has changed in the 1990s. A pilot program testing a strategy of assigning midwives directly to villages (rather than to health centers) was implemented in 1991 (Oetomo, 1992).

The addition of maternity clinics appears to have a greater impact on the infants of educated women than on the infants of uneducated women. Possibly modern delivery services may be more appealing to educated women, who view themselves as middle class, than to uneducated, more "traditional," women who prefer the services of traditional midwives (and the accompanying birth rituals traditional midwives perform).\(^{21}\) Additionally, to the extent that education and income are correlated, educated women are probably more able to pay for the services of maternity clinics than are uneducated women. After birth, educated women may be more able to convert the instructions received at a maternity clinics into healthy practices than uneducated women.

\[\text{\(^{21}\) This kind of argument was suggested by Lindenbaum and Elias (1983), who noted that in Bangladesh education tends to change women's ideas about their status and consequently about what behaviors are appropriate.}\]
The effects of increasing access to doctors are small, but encouraging. The presence of doctors does lower mortality risks. Taken together, the beneficial effects of living in a village where doctors and maternity clinics are available indicate that access to private sector services has a measurable impact on mortality risks, particularly for the better-educated. A review of available literature suggests that Indonesians frequently use private services as a source of health care (Berman, Sisler, and Habicht, 1989; Haliman and Williams, 1983; Linnan, 1990). The results of this analysis suggest that the services are effective.

Increases in the availability of health workers appear to raise mortality risks a small (but marginally significant) amount. Although we know of no reason why health workers should actually raise mortality risks, neither did we expect their presence to have a substantial negative impact. First, health workers are widely available as a source of care in Indonesia (over 70% of the DHS villages have at least one). Increasing the numbers of health workers probably does not greatly increase access to their services. Second, the category is vague and covers people with varied levels of training (probably ranging from graduates of the Health Worker Training Schools of the 1960s, to recent graduates of Paramedical Academies).22 While some health workers probably do positively affect survival chances, many other poorly-trained ones probably have no effect. Possibly their effect is harmful if their presence delays people from seeking better, but more expensive or more distant sources of care. Health workers increase mortality less for infants of educated women than uneducated women. Educated women may use health workers less, or less exclusively, than uneducated women. Educated women may also have the confidence to abandon use of health workers' services when they seem ineffectual and the cash to pay for care elsewhere).

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22 Berman, Ormond, and Gani (1987) describe the various sources of informal modern care available in Indonesia: volunteer health workers, unlicensed injectors, paramedical workers from health centers and subcenters, and others. All of these workers are probably encompassed by this variable.
The lack of an impact of health centers is somewhat disappointing, but not altogether surprising. For one thing, the simple presence-absence of health centers in no way captures variations in quality of services. At the grossest level one cannot even distinguish between health centers and subcenters. Because staffing patterns and hours of operation are much less standardized for subcenters than for centers, the effects of the two may be quite different (Berman, Ormond, and Gani, 1987; Streatfield and Singarimbun, 1990). Even if the two types of facilities could be distinguished, one can tell nothing about how well-staffed or supplied they are. Personal observation, as well as the Indonesian health service literature suggest that facilities vary a good deal on these dimensions (getting doctors and drugs to the middle of a rain forest is much more difficult than getting them to villages outside Jakarta).

Another potential explanation for the lack of effects of health centers is the shortness of the time period under analysis. We are considering change in access to health centers as a function of whether or not a health center was present in the village in both 1983 and 1986. In the villages where a new health center appeared in the three years between the censuses, it is not clear that it would be operating effectively by 1986. Considerable gaps separate a center’s construction, the arrival of furniture and supplies, the appearance of staff, and residents’ acceptance and use of it as a source of care (Berman, Sisler, and Habicht, 1989; Oetomo, 1992).

Additionally, the quality of data on changes in access to health centers may be problematic. The mid-1980s were a period of extreme austerity in public sector expenditures, particularly for the Ministry of Health (Dichter et al., 1990; Brotowasisto et al., 1988; GOI, 1988). Though most sources report only slow increases in the numbers of health centers and subcenters in the mid-1980s (USAID, 1988), the census data show a number of villages gaining a health center between 1983 and 1986. Possibly the total numbers of centers (of
either kind) was ascertained more accurately in 1986 than in 1983 (the questions were asked slightly differently in the two censuses). Another possibility is that health centers were underreported in 1983 because the census questionnaire used a term to describe them that had been only recently adopted by the Ministry of Health (which was in the process of renaming facilities) (Paramita, 1992).

It is encouraging that, unlike private sector services, the addition of a public health clinic appears to benefit uneducated women more than educated women. This result is consistent with studies that show that the poor and poorly-educated tend to rely on public sector facilities for a much larger proportion of their health care than do the rich and well-educated (Gertler et al., 1992). The finding may reflect the lesser ability of uneducated women to pay for private care (if education and income levels are correlated), or the fact that uneducated women may be less intimidated by government clinics than by private doctors or midwives (Berman, Sisler, and Habicht, 1989; Haliman and Williams, 1990; The World Bank, 1990; Linnan, 1990).

The results of the analysis suggest that access to private sector services has more of an impact on mortality than does access to public sector services. Such a conclusion should be tempered by recognition that public and private services in Indonesia blend together. Most health workers at government clinics or hospitals operate private practices after hours, while many private practitioners also hold a government job (USAID, 1987; Brotowasisto et al., 1988; Berman, Ormand, and Gani, 1987; WHO, 1982; Bair, 1987; Hugo et al., 1987, 115). Consequently, the distribution of public services partially determines the distribution of private services (if a village did not have a health center, it likely would not have a doctor's practice either). Additionally, public programs pay for the training of most health professionals in Indonesia, whether these people ultimately work for public or private
institutions (USAID, 1988). While access to private care may be the factor that ultimately affects mortality, the existence and distribution of private services must be credited partially to public programs.

Conclusions

Analysis of the effects of access to health care on mortality contributes both to policymakers’ perceptions of program impacts and to researchers’ understanding of the determinants of demographic outcomes. Use of national-level data is appealing in that representative data provides insights into the functioning of the existing health system. On the other hand, the nonexperimental nature of national-level data complicates the analysis considerably. If the distribution of health care is not random with respect to mortality or its determinants, standard estimates of program impacts will be biased. Policies of the Ministry of Health, literature on health services in Indonesia, and exploratory data analysis with censuses of village infrastructure all suggest that the distribution of health services in Indonesia is certainly not random. The processes that generate the distribution of health services, however, appear complex and difficult to capture with available data.

Rather than trying to construct and include variables that control for these processes, this analysis employed a fixed-effects approach that takes advantage of data from two points in time. In this approach changes within villages in infant mortality risks are related to changes within villages in access to public and private facilities. Fixed characteristics of villages that might affect both access to facilities and mortality risks are differenced out of the equation and so do not bias parameter estimates.

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23 Most graduates of Indonesian health training programs (medical schools, midwifery academies, etc.) are required to serve in a government post for several years, after which they can obtain a license to practice privately (Berman and Sakai, 1986; USAID, 1988).
The fixed effects approach yields considerably different results from those of a standard cross-sectional logit. A formal test for differences between the two models concludes that the parameter estimates of the cross-sectional model are biased. The cross-sectional estimates also turn out to be misleading. Comparison of the two models confirms the importance of designing the analysis so as to account for nonrandom allocation of health services.

The results of the fixed effects model suggest that health services do significantly alter infant mortality risks. In particular, access to maternity clinics and to doctors reduces the risk of infant mortality.

The results of this analysis should be of interest to several audiences. From a policy perspective the analysis indicates that efforts to improve health care have lowered individual risks of mortality. From a theoretical perspective, the results serve as an empirical justification for models of infant mortality that include community-level determinants. From a methodological perspective, the statistical approach developed here should be applicable to other data sets.
References


Table 1
Correlations Among Community Facilities, Subdistrict Level
Indonesian Census of Village Infrastructure (1986)

<table>
<thead>
<tr>
<th></th>
<th>LA</th>
<th>Pop</th>
<th>Hosp</th>
<th>MC</th>
<th>HC</th>
<th>HSC</th>
<th>Dr</th>
<th>HW</th>
<th>TM</th>
<th>SS</th>
<th>INPR</th>
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<tbody>
<tr>
<td>Land Area</td>
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<td>-.16</td>
<td>-.15</td>
<td>-.21</td>
<td>-.17</td>
<td>.03</td>
<td>-.24</td>
<td>-.26</td>
<td>-.02</td>
<td>-.22</td>
<td>.08</td>
</tr>
<tr>
<td>Population</td>
<td>1</td>
<td>.56</td>
<td>.69</td>
<td>.77</td>
<td>-.20</td>
<td>.64</td>
<td>.47</td>
<td>.08</td>
<td>.55</td>
<td>-.25</td>
<td></td>
</tr>
<tr>
<td>Hospitals</td>
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<td>.59</td>
<td>.05</td>
<td>.69</td>
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<td>-.08</td>
<td>.61</td>
<td>-.53</td>
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<td>Maternity Clinics</td>
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<td>.07</td>
<td>.79</td>
<td>.65</td>
<td>-.02</td>
<td>.74</td>
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<td></td>
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<td></td>
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<td>Health Centers</td>
<td>1</td>
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<td>.74</td>
<td>.54</td>
<td>.15</td>
<td>.70</td>
<td>-.26</td>
<td></td>
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<tr>
<td>Health Subcenters</td>
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<td>.32</td>
<td>.09</td>
<td>.29</td>
<td>.21</td>
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<td>Doctors</td>
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<td></td>
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<td>Health Workers</td>
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<td>.78</td>
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<td>.03</td>
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<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
</tbody>
</table>

Subdistrict availability measured as the proportion of villages within a subdistrict that have the attribute in question
Table 2
Levels of Health Infrastructure, 1983 and 1986
Percentage Distribution and Mean

<table>
<thead>
<tr>
<th>Variable</th>
<th>1983</th>
<th>1986</th>
</tr>
</thead>
<tbody>
<tr>
<td>Maternity Clinics</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>71%</td>
<td>71%</td>
</tr>
<tr>
<td>1</td>
<td>21%</td>
<td>16%</td>
</tr>
<tr>
<td>2+</td>
<td>8%</td>
<td>13%</td>
</tr>
<tr>
<td>Mean</td>
<td>.41</td>
<td>.59</td>
</tr>
<tr>
<td>Health Centers</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>65%</td>
<td>44%</td>
</tr>
<tr>
<td>1+</td>
<td>35%</td>
<td>56%</td>
</tr>
<tr>
<td>Mean</td>
<td>.41</td>
<td>.59</td>
</tr>
<tr>
<td>Doctor</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0</td>
<td>69%</td>
<td>59%</td>
</tr>
<tr>
<td>1-10</td>
<td>23%</td>
<td>29%</td>
</tr>
<tr>
<td>11+</td>
<td>7%</td>
<td>12%</td>
</tr>
<tr>
<td>Mean</td>
<td>3.0</td>
<td>3.9</td>
</tr>
<tr>
<td>Health Worker</td>
<td></td>
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<tr>
<td>0</td>
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<td>32%</td>
</tr>
<tr>
<td>1-10</td>
<td>47%</td>
<td>47%</td>
</tr>
<tr>
<td>11+</td>
<td>11%</td>
<td>21%</td>
</tr>
<tr>
<td>Mean</td>
<td>5.0</td>
<td>5.2</td>
</tr>
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</table>

Based on the 269 villages that contributed observations to the pairs file
<table>
<thead>
<tr>
<th>Change 1983 to 1986</th>
<th>Doctors</th>
<th>Maternity Clinics</th>
<th>Health Workers</th>
<th>Health Centers</th>
<th>Private Toilet¹</th>
</tr>
</thead>
<tbody>
<tr>
<td>+ 3 or more</td>
<td>15.6</td>
<td>3.7</td>
<td>26.4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>+ 2</td>
<td>4.5</td>
<td>4.5</td>
<td>8.2</td>
<td></td>
<td></td>
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<td>+ 1</td>
<td>12.7</td>
<td>9.7</td>
<td>10.4</td>
<td>36.0</td>
<td>8.4</td>
</tr>
<tr>
<td>0</td>
<td>51.3</td>
<td>65.8</td>
<td>25.3</td>
<td>54.7</td>
<td>70.7</td>
</tr>
<tr>
<td>- 1</td>
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<td>3.4</td>
<td>1.8</td>
<td>7.1</td>
<td></td>
<td></td>
</tr>
<tr>
<td>- 3 or more</td>
<td>6.4</td>
<td>1.1</td>
<td>17.4</td>
<td></td>
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</tr>
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</table>

¹ - villages where the majority of households did not have a private toilet/piped water in 1983, but did in 1986. -1 signifies the reverse, 0 signifies no change.
Table 4  
Fixed Effects Analysis of Mortality Determinants  
Neonatal and Postneonatal Age Intervals  
Indonesia, 1987 DHS Data

<table>
<thead>
<tr>
<th>Age Interval</th>
<th>Variable</th>
<th>Coefficient</th>
<th>t Stat.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neonates</td>
<td>Birthday</td>
<td>-.0100756</td>
<td>-3.73</td>
</tr>
<tr>
<td>N=298</td>
<td>Female</td>
<td>-.4961818</td>
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Sum LL= -408.80  
Pooled LL= -416.85  
Difference in LL= 8.05, 12 DF
Table 5
Fixed Effects and Cross-Sectional Estimates
Mortality Determinants
Pooled Neonatal and Postneonatal Observations
Indonesia, 1980-1987

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