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The effects of access to health care on infant mortality in Indonesia*

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Abstract

This paper examines the impact of access to health facilities and personnel on infant and child mortality in Indonesia. Demographic and Health Survey data are combined with village-level censuses of infrastructure collected by the Central Bureau of Statistics. Because the village-level data are available from two points in time, it is possible to analyse the effects on mortality risks within the village of changes in access to health care. Factors about villages that might affect both access to health care and mortality risks are held constant. Adding a maternity clinic to a village decreases the odds of infant mortality by almost 15 per cent, in comparison to the risk before the clinic was added. An additional doctor reduces the odds by about 1.7 per cent.

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 (Grant 1990: 532; Jamison and Mosley 1991).

Efforts to evaluate the impact of interventions on health outcomes have accompanied initiatives to provide services. Evaluation research is important, as resources available for subsidizing health care have shrivelled since the worldwide recession of the early 1980s and the ensuing international debt crisis, for three reasons: growth in GDP slowed during the mid-1980s; interest payments on public debt have drained national budgets and diverted money away from subsidies to public services; and at an individual level, inflation and unemployment have reduced purchasing power (Commission on Health Research for Development 1990). 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 priorities 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; World Bank 1990).

This paper examines the impact of access to health facilities and personnel on infant and child mortality in Indonesia; it explores the spatial distribution of health services and accounts

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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 socio-economic 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.[1] Reasons for this dearth of 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 censuses of village infrastructure conducted by the Indonesian Central Bureau of Statistics in 1983 and 1986. Information on levels of socio-economic development was collected from the village leaders 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 thousand for the period 1977-1987 (DHS 1991). Women surveyed in the 1987 DHS are matched by block to census data on characteristics of the villages (*desa*) and subdistricts (*kecamatan*) in which they reside. The matching process was tedious but successful: all but ten DHS villages were matched to the community data.

The remainder of this paper discusses related studies, describes health services in Indonesia, explains the methods used in the analysis, presents the results, and discusses them.

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. A brief review follows of studies of individual-level mortality risks, focusing on studies that use data from large, national-level household surveys.

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 children under five weakens considerably with the inclusion of controls for individual and household variables, such as 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 per cent 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 per cent 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 per cent higher for newborns living further than ten miles from a clinic

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[1] See, for example, Mosley and Chen 1984, Schultz 1984, 1985; Boulier and Paqueo 1988.

than for newborns living within three miles, while children ten miles away have 60 per cent higher mortality than children living closer. This relationship may have occurred 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.

Al-Kabir's results are generally confirmed by Hossain's (1989) analysis of data from the Bangladesh Institute for Development Studies. Presence within a village of family planning clinics lowers the child-mortality experience of women, as does presence of a dispensary.[2] Presence of hospitals does not lower mortality. Controls include maternal education and childhood residence, and paternal education, income, and occupation.

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

In an analysis of data from the 1973 Colombia census, Rosenzweig and Schultz (1982) found that clinics and hospital beds are associated with lower child mortality ratios (see Footnote 2), 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 included information on the municipality-level availability of public and private clinics, dispensaries, and mobile care units, but found no effect on mortality ratios.

In India (1971 census data) availability of family planning clinics and dispensaries significantly reduces 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, such as health centres, subcentres, and maternal child welfare clinics, significantly reduces child mortality. In simulations of the effect of increasing facility availability, Rosenzweig and Wolpin found that doubling the number of villages with family planning programs would reduce child mortality ratios by about 10 per cent, while doubling the number of villages with dispensaries would lower the ratios by 25 per cent.

Several studies examine the effect 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 areas where health outcomes are poor.

In the C™te d'Ivoire, 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 (Rosenzweig and Wolpin 1982; Al-Kabir 1984; Hossain 1989). In other settings access to services appears to have no effect on health outcomes (Rosenzweig and Schultz 1982; DaVanzo 1984; Strauss

[2] 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).

1990). 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, quality of care for example. Or perhaps measures of health status do not incorporate the aspects of health that are affected by access to facilities.

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 behaviour and outcome. For example, public health care may be targeted to the poor, who have relatively poor health. If the analysis does not control for this non-random element in service allocation, access to public health care may appear to produce poor health.[3]

Rosenzweig and Wolpin (1986) illustrated this problem in an analysis of the effects of health and family planning programs on the standardized height and weight of Filipino children. They experimented 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 *barangay* 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 the time when programs became available.[4]

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 seven per cent taller and twelve per cent heavier than children who have never lived in an area with a program. Children who have always lived in an area with a health clinic are five per cent taller and nine per cent heavier than children who have never lived in such an area. Rosenzweig and Wolpin (1986) attributed the change across the models in the estimated coefficients to the non-random placement and timing of placement of clinic services.

Two other studies using analogous methods with Indonesian data make similar points: that of Gertler and Molyneaux (1994) with respect to fertility and Pitt, Rosenzweig, and Gibbons (1993) on child mortality. Pitt et al. show that estimates of the effects of schools, family planning, and health clinics on subdistrict mortality rates are considerably different from the results obtained when changes in subdistrict mortality rates over time are explained as a function of changes in access to care within that subdistrict: an analysis of covariance

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[3] 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.

[4] 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.

with dummy variables approach. The results from the analysis of the covariance model indicate that none of the program variables affect child survival.

Health services in Indonesia

The idea is intuitively credible that regression results could be biased by unobserved processes generating a distribution of health services that is non-random with respect either to health status or to its determinants. Within the public sector planners surely allocate health services according to some design, whether it is to target high morbidity or 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 centres (*puskesmas*), and health subcentres (*puskesmas pembantu*) according to a set of general guidelines. Government hospitals are located in district capitals (*kabupaten*), limiting their accessibility to rural residents: a district consists of 300 to 500 villages (USAID 1988). Health centres are the basic source of primary health care, particularly in rural areas. Health centres 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 centre; densely populated subdistricts in urban and suburban areas have more than one. In addition to health centres, health subcentres are located in the more peripheral villages of remote subdistricts in which travel is difficult. Subcentres are either staffed by resident paramedical workers or are opened only once or twice a week by workers travelling out from the health centre (Berman, Ormand and Gani 1987; MOH 1990).

In Indonesia private practitioners, including paramedics, midwives, and doctors, are an important source of health care (Haliman and Williams 1983; Berman, Sisler, and Habicht 1989; Linnan 1990; Streatfield, Tampubulon and Surjadi 1990). The distribution of private practitioners partly 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 (Berman et al. 1987; USAID 1988). Villages with a health centre are likely to have a doctor’s practice and midwife’s delivery clinic as well, and sometimes health centre staff may also open practices in nearby villages. On the other hand, some private practitioners have no connection to government facilities. Generally private services are far more available in urban than in rural areas; this suggests that private practitioners prefer more developed areas where residents demand private services and can pay for them (Brotowasisto et al. 1988; World Bank 1990b).

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 socio-economic development, integration into transport 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 centres, health subcentres, 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. The community data are available at two levels of aggregation: the village and the subdistrict; Table 1 analyses service availability in the subdistricts within which the DHS villages were located. 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.

Defining the terms that refer to various types of health services involves first linking the names of health facilities that appear in the Central Bureau of Statistics village infrastructure questionnaire both to Ministry of Health descriptions of health services in Indonesia and to other descriptions in the general literature; then thinking about how village leaders unfamiliar with the terms or their Ministry of Health meanings might interpret them. This process is inevitably imprecise, particularly with respect to maternity clinics and health workers. I use the term maternity clinic to refer to a public or private health service that provides prenatal care, in-patient delivery service, postnatal care and well-baby care, and is staffed by a doctor or a midwife, or both. Facilities encompassed by the term maternity clinic are likely to range from sophisticated hospitals specializing in obstetrics to small delivery clinics operated by midwives in their own homes.[5] I use the term 'health worker' to refer to people the village leader has identified as having some health expertise. Health workers potentially include everyone from unlicensed injectors and graduates of the Health Worker Training Schools of the 1960s to recently trained nurses, midwives, and paramedics such as nutritionists and sanitarians.

Several interesting patterns emerge from Table 1. First, population size is positively correlated with levels of the more 'modern' community institutions: hospitals, health centres, maternity clinics, doctors' practices, and secondary schools. In contrast, population size is negatively correlated with levels of health subcentres and receipt of INPRES funds. Land area, on the other hand, exhibits an opposite pattern. Land area is positively correlated with levels of health subcentres and INPRES money, but negatively correlated with levels of the more modern institutions.

Correlations among hospitals, health centres, maternity clinics, doctors' practices, and secondary schools are generally high, 0.6 and above. These institutions are much less strongly correlated with health subcentres: coefficients are all less than 0.10. Modern institutions are negatively correlated with INPRES money: coefficients vary from -0.25 to -0.53. The correlation between INPRES and health subcentres, however, is positive, 0.21. In sum, the more modern institutions tend to cluster together in areas of population concentration. Health

5 The community questionnaire asks about the number of *Rumah Sakit Bersalin/BKIA* in each village and in the 1986 explanatory notes defines these to be facilities where 'women can receive prenatal care, give birth, and receive postnatal and well-baby care, typically staffed by a doctor and midwife.' The term *Rumah Sakit Bersalin* (*RSB*) suggests a public or private multi-practitioner hospital specializing in obstetric care: a rare entity in Indonesian villages. The term BKIA refers to the precursors to health centres, which were clinics headed by a midwife and specializing in maternal and child health (Hugo et al. 1987:110; MOH 1990). During the 1970s and 1980s the BKIA were generally converted to government health centres headed by doctors, and the term is no longer in common use by the Ministry of Health, although it appears in the censuses of village infrastructure conducted by the Central Bureau of Statistics. Because the censuses also ask about the presence of government health centres into which the BKIA were converted, it is not clear that many government clinics would be reported as RSB/BKIA. Meanwhile, another source of obstetric care and delivery assistance is becoming increasingly common in Indonesia: small-scale private clinics operated by midwives who have connections to doctors and hospitals where they can refer complicated cases (Soh-Sanu 1989). These facilities are not RSBs by the strict Ministry of Health definition of the term; however, they more closely fit the RSB/BKIA category and description than any other category in the community questionnaire, so village leaders would probably report them here: far more villages report these facilities than could be expected to have RSBs by the strict Ministry of Health definition.

subcentres, 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 centres, or doctors.

Table 1
Correlations among community facilities, subdistrict level Indonesian Census of Village Infrastructure (1986)

	LA	Pop	Hosp	MC	HC	HSC	Dr.	HW	TM	SS	INPR
Land area	1	-.16	-.15	-.21	-.17	.03	-.24	-.26	-.02	-.22	.08
Population		1	.56	.69	.77	-.20	.64	.47	.08	.55	-.25
Hospitals			1	.66	.59	.05	.69	.51	-.08	.61	-.53
Maternity clinics				1	.76	.07	.79	.65	-.02	.74	-.30
Health centres					1	0	.74	.54	.15	.70	-.26
Health subcentres						1	.04	.32	.09	.29	.21
Doctors							1	.72	-.19	.80	-.32
Health workers								1	-.07	.78	-.07
Trad. midwife									1	-.03	.03
Second. school										1	-.23
INPRES funds											1

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 partly reflects government allocation policies and partly 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 non-random 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.

The general problem of unobserved confounding factors is well-known in the epidemiological literature. A common solution with retrospective data is the use of matched pairs, where one member of the pair serves as the case and one member of the pair is the control (Fleiss 1981). I use this approach, comparing survival outcomes for pairs of children from the same village. Because each member is from the same village, they are matched on village characteristics and so differences in their survival outcomes will not reflect unobserved characteristics that vary across villages but are constant within villages.

This strategy has also been developed in the econometric literature. Chamberlain (1980) formalized a method for eliminating bias from omitted variables in probability models. Chamberlain develops his argument with the linear regression case:

[1]

where each of N groups contributes T observations. The a_i are group-specific effects. If an individual contributes multiple observations, each 'group' is an individual and the a_is are individual effects. If the a_is are correlated with the x_{it}s and the a_is 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.[6] Instead, Chamberlain suggested maximizing a conditional likelihood function. The likelihood function is conditioned on the sum of the y_{it}s:

[2]

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_is 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:

[3]

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

[4]

[6] Chamberlain (1980) showed that for several probability models, including the logit model, maximum likelihood estimates of parameters are biased when group-specific dummy variables are included.

where the a_is are the effects of being one of the two members of a particular group. The equation simplifies to:

[5]

Since $a_i = a_{i1} = a_{i2}$ 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:

[6]

where the a_is are not subtracted out and the Bs 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 χ^2 distribution, is a Hausman statistic, calculated as:

[7]

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 problems are similar to the circumstances 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 born in the same village are grouped into pairs of observations, Chamberlain's approach provides a means of eliminating bias from omitted variables. The use of pairs, matched on village residence, corresponds to the matched case-control designs common in the epidemiological literature.

The process of creating matched pairs required several steps. In the first step births were separated into two cohorts: births before mid-1983 and births after mid-1983. 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 provide pairs of infants within each village infants from the early cohort were randomly paired 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, the matching procedure was performed twice to avoid matching neonatal deaths to post-neonatal deaths. First I coded each child according to whether it 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. The pairs were kept when one child died and the other survived. I repeated the steps with the children who survived the neonatal period, coding them according to whether they survived to 13 months of age.[7] Within each village infants born between 1980 and mid-1983 were randomly matched to infants born between mid-1983 and 13 months before September 1987, the interview date. Again, the pairs were kept when one infant died and the other survived. 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.

Figure 1
Matching rules: individual observations to community-level data

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Month 0:
          1/80                                6/84                      8/87   9/87
          |xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx | oooooooooooooooooooooo | ccccc |

Months 1-13:
          1/80                   6/83                         8/86         9/87
          |xxxxxxxxxxxxxxxxxxxxxxxx | oooooooooooooooooooooooo | ccccccccccc |
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xxx = births in this period matched to 1983 data
ooo = births in this period matched to 1986 data
ccc = censored

Two files of pairs of infants were constructed: one for neonates, and one for post-neonates plus the thirteenth month of life, hereafter referred to as the post-neonatal 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.[8] For each observation (pair) I created a dependent variable measuring whether the early child died (zero) or the later child

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[7] In choosing a cutoff for mortality after the neonatal period, it is necessary to consider the problem of censoring. The individual-level data were collected in September 1987. The analysis must be restricted to children born long enough before the survey to have had a chance to survive for the entire period under consideration. I chose to analyse survival to 13 months rather than to a later age or to 12 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 in the thirteenth month of life: had I considered infant mortality they would have been excluded. If I analysed survival over a longer period, say from months one to 18, I could not consider children born after March 1986.

[8] The community data are available for two years but I analyse births from over an almost eight-year period. The community data measure inexactly the levels of facilities in existence during infants' survival experiences.

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 later 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 services between time periods. If access to facilities improves survival chances, the chance that the 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. 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.

A disadvantage of the approach is that the conditions on the data set limit the number of observations. For each village the size of the early cohort differs from the size of the later cohort, so that some infants are not paired and cannot be analysed. Additionally, pairs of which both members survived or both members died are dropped because they do not contribute to the estimation process (Fleiss 1980: 114; Chamberlain 1980). Because death is a relatively rare event, most of the dropped pairs were ones in which both children survived. However, 50 post-neonatal pairs and 43 neonatal pairs were rejected because both infants died. The process also means that certain villages do not contribute any pairs to the analysis file. This happens most frequently because there are some villages in which none of the 20 or so women interviewed in the DHS had lost an infant since 1980. Thus, villages with particularly low fertility or infant mortality are likely to be underrepresented. The women in the DHS sample gave birth to almost 13,000 children between 1980 and September, 1987. Using the pairing process described above we constructed an analysis file of 652 pairs of children (298 neonatal observations, 354 post-neonatal observations) from 269 villages. The small number of pairs limits the precision of the parameter estimates, but not their consistency.

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 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 a Hausman statistic can be constructed to test formally whether the cross-sectional estimates are inconsistent.

The effects of individual-level, family-level, and community-level variables were analysed, including those by which survival risks generally differ: first birth, sex, birth order, length of the preceding birth interval, and maternal education. The infants' birthdates were included to capture the time trend in mortality. Each variable's values are calculated as the difference in values between the two pairs of children: 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.

The focus is on three community variables. The numbers of health workers and doctors in each village capture access to trained health personnel. The numbers of maternity clinics capture access to fixed-site health services. A measure of access to government health clinics is not included in the analysis. Unlike the 1986 data, the 1983 community-level data on availability of government clinics did not distinguish between health centres and subcentres; however, the 1986 data presented in Table 1 suggest that these facilities are located in different types of villages, and we know that the two types of facilities differ in terms of quality and staffing levels. Because I did not want to combine the two types of facilities, I did not include measures of change in access to government clinics in the models.

The health services variables are of interest from a policy perspective. The Government of Indonesia invested considerable resources in the mid-1980s training health 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 these investments have translated into reductions in mortality risk.

Changes in access to facilities were measured as changes in the number of facilities or personnel within a village. I experimented with other measures of accessibility, such as per capita availability, an indirect estimate of proximity, and measurements at various levels of aggregation (village or subdistrict). I 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. Unfortunately, no information was collected about the quality of the health services, so we are limited to analysing measures of availability. Frequency distributions for both levels of access and changes in access to facilities are presented in Tables 2 and 3. Table 2 shows that in the villages analysed, levels of maternity clinics increased between 1983 and 1986, while levels of health workers changed little and levels of doctors decreased. Table 3 shows that a considerable number of villages experienced a change in access to facilities and to personnel between 1983 and 1986.

Table 2
Levels of health infrastructure, 1983 and 1986 percentage distribution and mean

Variable	1983	1986
Maternity Clinics	%	%
0	71	71
1	21	16
2+	8	13
Mean	0.41	0.59
Doctor		
0	59	69
1-10	29	23
11+	12	8
Mean	3.9	3.0
Health worker		
0	32	42
1-10	55	47
11+	13	11
Mean	5.2	5.1

Based on the 269 villages that contributed observations to the pairs file

Table 3
Frequencies, change in community variables (percentage distribution)

Change 1983 to 1986	Doctors	Maternity clinics	Health workers
+3 or more	15.6	3.7	26.4
+2	4.5	4.5	8.2
+1	12.7	9.7	10.4

0	51.3	65.8	25.3
- 1	6.1	13.4	5.2
- 2	3.4	1.8	7.1
- 3 or more	6.4	1.1	17.4

Based on the 269 villages that contributed observations to the pairs file

Possibly the addition of the first health care provider to a village has a stronger impact on mortality than the addition of subsequent providers of that type. I tested for a differential impact by estimating a model that includes terms for whether any maternity clinics were available in the baseline period, 1983, and whether villages in which clinics were added already had established clinics. There were no villages in which the number of health workers or doctors increased from zero in 1983, so it was not possible to repeat the analysis for these provider types.

Researchers have often suggested that the impact of access to health facilities varies by level of education (Caldwell 1979; Rosenzweig and Schultz 1982; Caldwell, Reddy and Caldwell 1983; Al Kabir 1984). 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, Chakraborty, and Elias 1985). If health services lower mortality risks by changing women’s behaviour, for example by persuading mothers to use oral rehydration therapy, and if 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 discourages service use. Similarly, if educated women already follow healthy child care practices, increasing service availability may benefit uneducated women more because there is greater scope for changes in their behaviour as services become more available.

The relationship between access to services and levels of education can be explored by interacting educational level with access to services. Models were estimated that included interactions between education and the health infrastructure variables, distinguishing 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.[9]

For each pair, I 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, then differenced the two products. 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

[9] I 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 0.4 and 0.8.

education within the pair, so that the effect of adding a health centre is different when both mothers are educated from 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). Adding 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. 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 post-neonatal risks. This result contrasts with the accepted 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 magnitude of effects across age intervals to translate into statistically significant differences in the comparative fit of the two models.

Table 4
Fixed-effects analysis of mortality determinants, neonatal and post-neonatal age intervals, Indonesia, 1987 DHS data

Age interval	Variable	Coefficient	t Statistic
Neonates	Birthdate	-.00359	0.534
N=298	Female	-.47301	2.399
	Previous interval	-.01258	2.679
	Birth order	.04587	1.002
	First birth	-.26009	0.856
	Maternal education	-.04986	1.302
	Maternity clinic	.08521	0.728
	Health worker	-.00045	0.026
LL= -181.09	Doctor	-.01714	0.965
Post-neonates	Birthdate	-.00099	0.147
N=354	Female	-.09315	0.594
	Previous interval	-.01178	2.651
	Birth order	.07331	1.726
	First birth	-.32389	1.198
	Maternal education	-.13602	3.637
	Maternity clinic	-.15820	1.805
	Health worker	.01972	1.876
LL= -227.71	Doctor	-.01628	1.446
Sum LL= -408.79	Pooled LL= -416.93	Difference in LL= 8.14, 10 DF	

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, socio-economic 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 post-neonatal pairs, the opposite pattern holds for maternal education, and the community variables have larger and more significant effects for post-neonatal than neonatal mortality.

The preferred model is the one in which neonatal and post-neonatal 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, I constructed a pooled file of neonatal and post-neonatal 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.[10]

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.

Table 5
Fixed-effects and cross-sectional estimates of mortality determinants, pooled neonatal and post-neonatal observations, Indonesia, 1980-1987

Variable	Fixed Effects		Cross Section		Hausman
	Coefficient	t	Coefficient	t	
Birth-date	-.00537	2.99	-.00500	3.44	0.09
Female	-.25504	2.13	-.24458	3.46	0.01
Previous interval	-.01278	4.00	-.01857	7.47	8.34
Birth order	.05312	1.74	.05159	3.01	0.11
First birth	-.32562	1.64	-.37717	3.02	0.004
Maternal education	-.07083	3.33	-.06738	6.46	0.04
Maternity clinic	-.15762	2.31	-.04403	1.08	4.32
Health worker	.01358	1.66	-.00415	1.08	6.14
Doctor	-.01695	1.90	.00281	0.56	6.92
Constant			2.8212	1.92	
Log likelihood	-416.93		-3594.77		23.4
N	652		24190		9 DF

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 0.5 per cent lower than the earlier child's. Male children have about 29 per cent higher odds of death than female children.[11] The coefficients on previous interval length, first births, and

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10 Most children appear in this pooled cross-sectional file twice, once as a neonatal observation and once as a post-neonatal observation. This repetition of observation parallels discrete time hazards models, which are commonly used to study demographic phenomena (Trussell and Hammerslough 1983; 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.

11 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).

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 prior-interval lengths of 34 months or more. Each year of maternal education decreases the odds of death by about seven per cent, comparable to the effects documented by Cleland and van Ginneken (1988) in a major review of the relationship between maternal education and infant mortality.

If increases in the availability of health care decrease mortality, the parameters of the community-level variables should be negative. The coefficients on maternity clinics and doctors are negative and significant ($p<.03$ and $p<.06$, respectively). Within a village an increase of one maternity clinic decreases the odds of death of an infant with access to that clinic by about 15 per cent, relative to the infant born before the clinic existed. The impact of additional doctors is much smaller. An additional doctor decreases an infant's odds of death by around 1.7 per cent.

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 1.3 per cent greater odds of death than an infant born before the addition of health workers. The effect is significant at the 10 per cent level.

The effects of the community variables in the model estimated with the pooled cross-sectional data set are very different: none of the health service variables significantly affects mortality risks. In the cross-sectional model the coefficient on maternity clinics shrinks to about one quarter of its size in the model of differences. The cross-sectional estimate of the coefficient on health workers is negative, whereas it is positive in the model of differences; the reverse is true of the coefficient on doctors.

It is possible to test the hypothesis that the parameters from the pooled cross-section are consistent, with a Hausman statistic. The Hausman statistic follows a χ^2 distribution; the statistic is 23.4, which is significant at the one per cent level of confidence. The hypothesis that the estimates from the pooled cross-sectional data are consistent is rejected.

It is possible also to 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. The coefficients on all the health service variables from the pooled cross-sectional results do generate misleading interpretations.

I also estimated a model that included terms testing for an additional impact from adding a maternity clinic to a village in which none had existed, over and above the effect of adding more clinics to villages that already had some. This model indicates that adding a maternity clinic to a village in which none was present before does have a stronger impact on mortality than adding clinics to villages in which they are already present, but the effect is not significant (results not shown).

None of the interaction terms between education and health services or personnel is 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 addition of maternity clinics reduces the risk of death more for infants of women with at least a primary level of education than for infants of women with lower levels of education. Results are similar for access to doctors' services, but the differential impact is quite small.

Discussion

The potential for maternity clinics to reduce mortality risks is clear from a theoretical standpoint. Maternity clinics provide services to women while they are pregnant, when they give birth, and after the birth in the form of baby care. These clinics concentrate on services that directly affect foetal development, birth, and infant health, so they are well-positioned to affect infant mortality. The services provided by health workers and doctors are much more general. The large effect of maternity clinics in reducing mortality risks suggests that Ministry of Health effort in the 1980s to increase access to maternal and child health services, in part by increasing the number of trained midwives, has paid off. In the 1990s major programs have been implemented to assign midwives directly to villages rather than to clinics. As data become available it will be interesting to evaluate the effect of these programs on infant mortality.

The effect of adding maternity clinics appears to be particularly strong in villages in which no clinics were present before, and for the infants of women with at least a primary school education. Possibly modern obstetric 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. Lindenbaum et al. (1985) noted that in Bangladesh education tends to change women's ideas about their status and consequently about what behaviour is appropriate. 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 childbirth, educated women may be more capable than uneducated women of converting the instructions received at maternity clinics into healthy practices.

The effects of increasing access to doctors are small, but encouraging: the presence of doctors does lower mortality risks. A review of available literature suggests that Indonesians frequently use private services as a source of health care (Haliman and Williams 1983; Berman et al. 1989; 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 there seems no reason why health workers should actually raise mortality risks, their presence was not expected to substantially lower them. First, health workers are widely available as a source of care in Indonesia: over 70 per cent of the DHS villages have at least one, and 21 per cent of DHS villages have more than ten. Increasing the numbers of health workers probably does not greatly increase access to their services. Secondly, the category is vague, encompassing 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.[12] While some health workers probably do positively affect survival chances, many other poorly-trained ones probably have no effect.

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[12] Berman et al. (1987) describe the various sources of informal modern care available in Indonesia: volunteer health workers, unlicensed injectors, paramedical workers from health centres and subcentres, and others. All of these workers are probably encompassed by this variable.

Possibly their effect is harmful if their presence delays people from seeking better, but more expensive or more distant sources of care.

The results presented above strongly suggest that increases in access to health care and particularly to maternal and child health services decrease mortality risks for Indonesian infants. Unfortunately, the community-level data on health care are not sufficiently detailed to support the formulation of specific policy recommendations.

The data have several limitations. First, it is impossible to isolate the roles of public and private services. It is likely that both public and private services are included in the maternity clinic variable (see Footnote 5). Although the measures of doctors and health workers pick up private practices, some (but not all) of these practices exist because the government stations newly trained health personnel in government clinics, often in areas where they would not otherwise choose to live. Most graduates of Indonesian medical schools, midwifery academies, etc. are required to serve in a government post for several years before they are allowed to practise privately as their sole job (Berman and Sakai 1986; USAID 1988). While in these posts, such personnel then open private practices in the areas as well. Consequently, the distribution of public services partly determines the distribution of private services: if a village did not have a health centre, it probably would not have a doctor's practice either. It does seem clear that government expenditures on training programs for health personnel have been beneficial and that the existence and distribution of private services must be credited partly to public programs.

A second limitation of data is that we know almost nothing about the training levels, services provided, or quality of the health personnel and facilities we are analysing. The lack of data on training levels makes it particularly difficult to interpret the finding that mortality risks increase with the addition of health workers. Also, while the addition of a maternity clinic substantially reduces infant mortality risks, it is not clear what levels of services or quality of care account for this impact.

Conclusions

Analysis of the effects of access to health care on mortality contributes both to policy makers' 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 provide insights into the functioning of the existing health system. On the other hand, the non-experimental 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 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.

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 non-random allocation of health services.

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

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