Family Planning and Rural Fertility Decline in Iran: A Study in Program Evaluation*

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Abstract

During the first few years of the Islamic Revolution Iranian fertility was on the rise, in part because of the revolutionary government's pro-natal policies. In a policy reversal, in 1989 the government launched an ambitious and innovative family planning program specifically aimed at rural families. By 2005, the program had covered more than 90 percent of the rural population and the average number of births per rural woman had declined to replacement level from about 8 birth in the mid 1980s. In this paper we ask to what extent this decline was the result of a particular design feature of the family planning program, namely rural health houses. We use the timing of establishment of rural health houses to identify their effect on village-level fertility. Our results attribute about 7-18 percent of the fertility decline during 1986-96 to health houses. We discuss the role of other aspects of Iran's family planning program, such as its effective media campaign that took advantage of the revolutionary government's credibility with the rural poor, as well as the effect of other policies that increased the cost of high parity children and increased the returns to education for the rural poor in persuading rural families to change their behavior from high fertility and low investment in child education to low fertility and high investment in children.

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1 Introduction

The question of the role of state-provided family planning programs in fertility transition has long interested demographers, economists and policy makers. Economists stress the role of demand factors, such as infant mortality and education, as at least as important as the subsidized provision of family planning services (Schultz 1994; Schultz 2007). The few rigorous evaluations of family planning programs in developing countries that exist support only a moderate effect from family planning services. Joshi and Schultz (2007) exploit the experimental design of the Matlab program in Bangladesh and estimate that about 15 percent of the decline in fertility over more than a decade is attributable to the Matlab program. In a non-experimental setting, Angeles et al. (2005a) and Miller (2005) use geographic variation in program implementation to estimate the effect of the family planning programs in Peru and Columbia and find a similar moderate impact. This paper contributes to this literature by presenting an evaluation of the impact of Iran's rural family planning program on rural fertility.

During the last two decades Iran's fertility declined from one of the world's highest to replacement level. In a span of 15 years, from the mid 1980s to 2000, the total fertility rate (TFR) dropped by more than five births, from an average of 8 births per woman to nearly 2. By 2000, Iranian families were among the highest users of contraceptives in the developing world: the Contraceptive Prevalence Rates (CPR) reached 77 percent in urban and 67 percent rural areas, up from 54 percent and 20 percent in 1976 (Mehryar et al. (2001), Hosseini-Chavoshi (2007)).

The existing descriptive literature on Iran's fertility decline credits this spectacular decline to the innovative and ambitious family planning program, which was launched in 1989 as the Islamic government reversed the pro-natal position of its first ten years (Aghajanian 1995, Ladier-Fouladi 1996, Hoodfar and Assadpour 2000, Abbasi-Shavazi and McDonald 2006). Although early on the leader of the Islamic Revolution, Ayatollah Khomeini, had declared birth control legal, the government suspended the Shah's family planning program and adopted pro-natal policies. These policies seemed appropriate from the government's viewpoint as long as the war with Iraq was raging (1980-1988) and large numbers of young fighters were being recruited for the war effort. But by 1989 the situation had changed: the war had ended and the results of the 1986 census had shown that during the preceding decade population growth had accelerated to 3.9% per year, a fact that had not been lost to the Ministry of Education, whose primary schools in the meantime had moved to two and three shifts to accommodate the baby boom of the early 1980s. The government reconsidered its pro-natal stance, and family planning once again became official policy.

The effect of this policy reversal was greatest on rural families because after the revolution in urban areas the private sector continued to supply birth control devices, though on a more limited scale and at higher cost (Hoodfar 2008; Hoodfar and Assadpour 2000; Mirzaie 2005). In rural areas, where the government had been the only supplier of birth control devices, access dropped off sharply once the national family planning program was deactivated (Mehryar et al. 2001; Hosseini-Chavoshi 2007).

The revived program focused on rural areas, where fertility had shown little sign of decline in response to family planning under the Shah (Moore 2007). The new, post-revolution program differed in important respects from its predecessor: it had a more effective information campaign, in part because it was backed by the clergy both nationally and at the local level. Its operation relied on a rapidly expanding health infrastructure, focused on mothers and child health, that had started before the revolution but, reflecting the revolutionary government's commitment to rural reconstruction, had accelerated in the 1980s (Shadpour 2001).

At about the same time, fertility in rural areas began to fall rapidly, so that by 2006 the rural-urban gap of in fertility been all but eliminated (see Figure 1).

Interestingly, in 2000 rural women were more likely to use modern contraceptives than urban women, 57 percent compared to 54 percent (Hosseini-Chavoshi 2007). The spectacular decline in rural fertility has drawn international attention to Iran's family planning program, with praise for its innovative structure and its success in transforming behavior in a conservative Islamic society. The fact that the program was implemented by an Islamic government with strong pro-natal and conservative Islamic ideology added to the fascination (Obermeyer 1995). The program has been called an "Iranian miracle" (Mehryar et al. 2001) and offered as a model for developing countries (Boonstra 2001).

Despite the international acclaim, the literature on Iran's family planning program offers surprisingly little evidence of a causal link between the program and the decline in rural fertility. Accounts of the program's effectiveness in service delivery fall well short of establishing causal impact. Evidence of impact is only convincing when it approaches a counterfactual comparison between the actual outcomes in terms of fertility decline for women who were exposed to the program and what they would have experienced had they not been exposed. We propose a method of identifying impact which we believe comes close to such a counterfactual analysis. We aims to evaluate the impact of a particular, arguably the most important, part of the overall program, namely rural health house construction, using variation in its geographic implementation.

The rural component of Iran's family planning program is of special interest for several reasons. First, as just noted, the decline in fertility was most spectacular in rural areas where the pre-revolution program had not succeeded (Aghajanian 1995). Second, the program was most innovative in its rural component. Finally, because of near monopoly by the government in supply of birth control services in rural areas, the link between program placement and impact is more easily established.

The construction of rural health houses started before the revolution, but accelerated in the mid 1980s. Initially, family planning was not part of the government's primary health care system. In the 1980s, rural health houses offered primary child and maternal health care only, and in the 1990s, following the policy reversal, they actively delivered family planning services. Because of the change in function, the administrative decision to build health houses was not directly based on the level of fertility. If anything, as we show below, areas with higher fertility received their health houses later. According to interviews with the officials of the Ministry of Health and Medical Education (MOHME) who were actively involved in the decision making at the time, the criteria for early placement of health houses were: (i) the presence of capable local administrators willing to help, (ii) a minimum level of infrastructure (electricity and schools), and (iii) the availability of educated young men and women in the local area who could be trained as health workers. Selection based on the latter two criteria is confirmed by our analysis of placement data. We show below that the timing of health house establishments are positively correlated with the availability of basic infrastructure (electricity and water) and schools, but only weakly and *negatively* correlated with fertility. Since we are able to control for the most important village characteristics that affect placement, we believe that our identification strategy delivers consistent estimates of program impact.

We choose average fertility at the level of the village, measured by the ratio of children 0-4 to women 15-49, the child-woman ratio (CWR), as our outcome variable, and compare the outcomes in census years 1986 (before program) and 1996 (after program) for two groups of villages. We choose these census years because we can measure village CWR from the census data. The two groups of villages are those without a health house in 1986 and 1996 (comparison or control group) and those without a village in 1986 but with one in 1996 (program or treatment group). We find that while both groups experienced relatively sharp declines in fertility during 1986-96, the average decline in program villages was between 7 to 18 percent greater than in comparison villages. These findings are consistent with those from other countries, such as Bangladesh (Joshi and Schultz 2007), Columbia (Miller

2005), and Peru (Angeles, Guilkey, and Mroz 2005a), which attribute roughly 10-15 percent of the total decline in fertility to family planning.

Bearing in mind that we are only evaluating the impact of the active service delivery component of Iran's family program (health houses), the size of impact we estimate is by no means small. Nevertheless it does raise the question of what explains the remaining 85 percent of the decline. We note several factors that help set the context for interpreting our impact evaluation results.

First, the reversal in family planning policy and changes in fertility behavior of families should not be viewed entirely as cause and effect. The same pressures generated by the fertility boom of the early years of the revolution that manifested themselves in, among others, school overcrowding and the rising cost of education, may have prompted both actors–the government and the family–to reverse their position on fertility and family planning.

Second, the onset of fertility decline in Iran preceded the turnabout in government policy. Estimates by Abbasi-Shavazi et al. (2009) using the own-children method and presented in Figure 1 show that TFR in both urban and rural areas started to fall a few years *before* the policy reversal went into effect in 1989, suggesting that factors other than family planning may have been at work throughout.

Third, the Islamic government's message extolling the virtues of smaller families traveled widely and effectively across the country. This message was credible because before pushing family planning the government had shown its commitment to rural development by heavily investing in rural infrastructure, such as electricity, piped water and health. In addition the government was able to use mosques and enlist the help of the local clergy to advocate family planning. In this sense, the ideation of the post-revolution program was probably much stronger than the Shah's program. Raftery, Lewis, and Aghajanian (1995) who examined the latter program concluded that reduced demand for children rather than ideation explained its (small) impact on fertility.

Fourth, the government's push to reduce family size did not stop with persuasion and ideation. It actively encouraged the notion that it would back its message by policies to increase the cost to families of going beyond two children by, among other measures, withholding health and education subsidies for higher parity children.[1] In 1989, the government had already begun to dismantle the system of rationing of basic consumer goods, which had been in effect during the war and had favored larger families.

Fifth, social and economic change affected demand for children independently of government family planning policy. Economists emphasize the role of household characteristics, such as returns to child education and women's opportunity cost of time, in determination of fertility (Becker 1992; Schultz 1988; Schultz 1994). In Iran, women's education had been increasing rapidly, especially for rural women. Rural women born around 1960, who reached adulthood before the revolution, had on average only about 2 years of schooling, about half the schooling of rural men and one-third of urban women; those born twenty years later had increased their average years of schooling beyond primary education, to about 5.6 years, and narrowed the gap with rural men to 90 percent and with urban women to 65 percent. Such a rapid increase in the education of rural women is consistent with increased demand for contraception and lower fertility (Abbasi-Shavazi et al. 2008). Estimated reduced form models of children ever born for Iran also confirm that, as in other countries, fertility is negatively influenced by women's education and positively by family income (Salehi-Isfahani 2001). Thus the increase in education and falling income in the 1980s could potentially explain why families were more eager to adopt family planning in 1989 than when it was first offered in the 1970s. Infant mortality is another important determinant of demand for children fell by two-thirds after the revolution, from 92 per 1000 live births in 1980 to 32 in 2004. A similar argument

[1] The government had actually intended to do this for its own employees only, and eventually decided against it, but public perceptions were of a more general policy shift (Hosseini-Chavoshi 2007).

has been made in the case of Taiwan where fertility decline appears to have preceded family planning and was more closely correlated with mortality decline (Li 1973).

Finally, greater equality in income and access to basic services may have changed the perceptions of returns to child education among the rural poor and thus altered their quality-quantity tradeoff. The inequality of income declined substantially after the revolution and access to basic services by the poor substantially increased (Salehi-Isfahani 2009). Statements by the founder of the Islamic Republic, Ayatollah Khomeini, that the revolution belonged to the poor and the disinherited (*mostazafin*), many of whom lived in rural areas, contributed to the perception that a rural child had as much chance in getting ahead as an urban child, provided he or she was educated. By thus raising the expected returns to quality relative to quantity of children, the pro-poor policies of the revolutionary governments may have induced rural families to reduce fertility.

Rigorous impact evaluation of these factors requires different data than we have assembled for this study. If they are at all doable, they must await future research. The contribution of this paper is to provide a rigorous evaluation of just one component of Iran's program. The value of this evaluation is enhanced by the fact that this particular component, health houses, is not only its most innovative part, but also its most replicable. It is very difficult to imagine how Iran's entire program can be replicated elsewhere since, as we have argued, its operation was closely tied with the nature of Iran's Islamic Revolution. But, fortunately, the part which should interest policy makers in developing countries is active service delivery, about which we are able to present tangible evidence in what follows.

2 Program description

Health houses are at the center of primary health service delivery in rural Iran. When the rural Health Network System (HNS) was extended to a rural district, a villages with a central location would be selected as the "Main" village to receive the health house. Each health house would serve about 1500 individuals residing in that village and a few smaller "Satellite" villages in the surrounding area. Health houses are typically staffed with two health workers (*behvarz*) at least one of whom is a woman. They are recruited locally and trained for two years in primary health care delivery. Families in small hamlets located farther away are served by mobile units that operate directly out of rural Health Centers. Health Centers connect health houses to the larger network and oversee their operation. Figure 2 shows the organizational structure of the rural health delivery system. The comprehensive service offered by the HNS includes a hospital established in the main urban center of each district, a polyclinic for providing professional health care including specialist services, and training facilities for health workers.

Figure 3 shows the distribution of health houses by year of establishment. This is the administrative information we use to identify program effect. As this figure shows, the construction of health houses began before the revolution and continued at a slow pace in the early 1980s. In 1985, when the law mandating the establishment of the rural health infrastructure went into effect, the pace of construction of health houses accelerated, reaching its peak in 1989, the year family planning was added to the mandate of the HNS. By then roughly half of all health houses were already in existence. By 2005, the year our administrative data end, the program had covered about 90 percent of the country's rural population (20.4 million individuals living in 4.2 million households); there were 16,560 health houses staffed by 26,403 health workers and linked to 2,321 rural Health Centers that served about 55,000 villages.

As already noted, the initial focus of the HNS was child and maternal health. This feature of the program is important because it helped establish trust among rural families about the government's intention to improve their lives rather than merely limit their numbers. We believe that such credibility is important in the success of family planning policy. Recruitment and training of local health workers for service delivery was another feature that added to this trust. As a result, by the time the national communications campaign for family planning was in full swing, most rural residents had come to take seriously the second part of the campaign's main slogan, namely fewer children, better life.

Our data show rapid fertility decline in villages outside the HNS. The question arises as to how the women in villages without a health house planned their pregnancies. This question goes to the heart of the difference between active and passive delivery of family planning services and the role of health houses which we evaluate. Active delivery means that, for example, if a woman resident of a village which is served by a health house fails to visit it within a year, a health worker would pay her a visit to collect new information and provide needed services. Health workers not only supplied maternal and child health services on a regular basis, they kept close track of birth control needs of individual women. For example, if a woman was on the pill, the health worker would make sure that she had her monthly supply on time. All married women have a file which is annually updated. The data is stored in a massive database, known as *zeej*, which contains individual health histories of rural woman (Naghavi 2007).

We do not have direct information on contraceptive use in comparison villages, but we know that all married women had access to free birth control devices through mobile units that operated in rural areas from even before the revolution (Moore 2007), and through government health facilities in nearby towns. This type of access is called passive because delivery depends on initiative from the individual. Mobile units had delivered family planning services from before the revolution. They carried on their services after the revolution, focusing more on child and maternal health until 1989, and later added family planning to their services once again. The level of service they offered was very different from that offered by health houses because they did not visit villages regularly so that, for example, a woman on the pill could run out of her supply in one month before the next visit by the mobile unit, increasing the risk of pregnancy (Moore 2007). Women in villages with active delivery received the same minimum amount of service, whereas those without had to be willing to seek them. To get a perspective on the relative need for infrastructure of passive versus active delivery, consider that in 1972 the program before the revolution had estimated that to "provide good access" a total of 2,450 clinics would be needed across the country (Moore 2007), whereas the post-revolution program aimed at, and later achieved, about 18,000 clinics.

Families in program villages supported with active delivery naturally also received better information about the government's family planning effort compared to families in non-program villages. Although after 1989 the message that government policy with respect to support for larger families had ceased was effectively communicated through the media –radio and television –as well as sermons at mosques, the rate of diffusion of the message still depended on ownership of radio and television and presence of mosques, etc. The incentives to change behavior may not have been as great for families in non-program villages because they received less direct information about the change in government policy with respect to family size.

An important issue in program evaluation is spillovers. Spillovers arise when delivery of service to treated units–individuals or villages–generates externalities that affect the behavior of control units. In our case, this would mean that services delivered to one rural district, villages in nearby districts become more aware of, or have easier access, to family planning and therefore begin to limit their fertility. However, spillovers do not apply to the active delivery part of the services provided by health houses, for those requited being resident in a village designated in HNS. Therefore, the more accurate interpretation of our results is the impact on fertility of active versus passive family planning services.

To the extent that we go beyond this interpretation, program impact might go beyond the difference in fertility decline between program and comparison villages. If we include the parts of the family planning program that have spillover effect, such as its information campaign, we should expect a larger program impact. To get an idea of how large these effects might be, in Section 6 we will consider the effect of program coverage on fertility. The idea is that since non-active part of family planning services depend on distance, fertility in more densely covered districts would be lower after we control for the presence of a health house in each village. We find a negative effect which indicates the existence of spillover effects.

Another important question relates to heterogeneity of service quality. The Health Ministry in Iran conducts routine evaluation of the performance of health houses and requires regular retraining of health workers. Our assessment of "program fidelity" based on our examination of the Ministry's procedures and visits to health houses around the country is that it is high. However, to the extent that service quality depends on the level of development of the village, it must vary. A random visit by one of the authors to a health house (a program village) in a remote area of the province of Khorasan at the edge of Iran's largest desert revealed the importance of infrastructure. Two women who had waited for hours could not receive services because the health house did not have water that day. This type of heterogeneity due to infrastructure and other observable factors is less of a problem because we condition our evaluation results on them.

3 Program placement

An important feature of our identification strategy is the variation in the timing of health house construction across the country. The usual concern with non-random placement of a program is that selection might be compensatory in the sense that villages with higher fertility would receive programs earlier. This would bias the results of program impact downward because program villages would have higher fertility to begin with. According to interviews with MOHME officials in charge of the program, the 1984 law which mandated the establishment of the HNS required implementation to begin in one district in each of the 25 provinces (now 30 provinces) and then expand to other districts within each province, eventually covering all districts (about 180 then and 360 now). As noted earlier, according to these officials, placement was mainly influenced by the capability of local administrators (provincial governor, mayors and town councils), and the availability of educated potential health workers (initially minimum of primary education, later lower secondary required).

There was a fair amount of variation in fertility between villages in 1986 and 1996 that potentially could have influenced program placement. About a quarter of villages in our sample have child-woman ratios in excess of 1,110 children per 1000 women (roughly equivalent to TFR of 8.5) while a quarter had CWR of less than 800 (TFR of about 6). In 1996, seven years after the family planning program had been adopted nationally, the birth rate in the more developed Esfahan province was still less than half that of the least developed Sistan and Baluchestan (Abbasi-Shavazi and McDonald 2006).

Several factors that affected program placement helped reduce the degree of its endogeneity. First, half of the health houses had been constructed, and more were under construction, by the time family planning became official policy. It is likely that villages with higher fertility would have been selected first had the initial program focus been on family planning rather than child and maternal health. But this was not the case, and we see below that the presence of a health house before 1989 is negatively related to fertility. Second, a practical administrative rule rather than level of fertility guided village selection. Districts with better infrastructure, especially electricity, and with higher education were selected first because it was easier to build, maintain, and staff health houses in such places. Finally, another administrative rule which further reduced endogeneity of placement was the requirement that the HNS start work in all provinces immediately after the 1984 law had gone into effect. By 1989, when family planning was added to the

HNS mandate, all provinces and all districts within each province had received at least one health house. So the geographic variation that we utilize for identification arises mainly from the speed with which villages within each district were brought into the HNS. Since there is much greater heterogeneity between villages in different districts than between village in the same district, the fact that all districts had HNS villages reduces endogenous placement.

We examine the placement of health houses by modeling the probability of a village having a health house in 1986 and in 1989. These years are good choices for understanding the factors that affected the decision to build a health house in a particular village because they are prior to policy reversal. Because health houses did not offer family planning services in these years, we do not expect a direct effect from being in HNS on fertility, so any relationship between having a health house and fertility can be reasonable interpreted as fertility affecting placement. Table 1 reports the probit estimates of being in the HNS (marginal probabilities computed at the mean of the independent variables). We use the entire sample for this purpose rather than the sample consisting of program and comparison villages only. The estimated marginal probability of being in the HNS before 1986 or 1989 is *negatively* related to fertility both before (column 1) and after (columns 2 and 3) we condition on village characteristics. The size of the negative association drops by two-thirds when we condition on education, village size, and religion variables. These results show that, at the very least, Iran program did not target high fertility areas. They also confirm the relevance of several village characteristics that appear to have affected placement, and which we can control for in our impact evaluation analysis below.

The effect of village size, schooling, and infrastructure generally increase the probability of having a health house early. Population size does not matter, but the presence of middle school, electricity, and piped water are important, and make perfect sense in light of the anecdotal account of the factors that influenced priority for placement as told to us by Ministry of Health officials, namely that areas with better administrations, infrastructure, and education received priority. By controlling for these variables in our impact evaluation regressions, we bring our results closer to those that would have obtained if assignment of villages to HNS were random.

4 Data

Our unit of observation is a village designated as a "Main village" by MOHME. We obtained information on the year of establishment for 16,715 Main villages that had a health house by 2005. More than 90% of the rural population live in these villages, which comprise about 30% of all villages in Iran. Smaller villages and hamlets are served by the health houses in our sample as "Satellite" villages or by mobile units operated by Health Centers in nearby towns. Our test of program effect concerns changes in fertility in the "Main" villages only.

We were able to match about 14,176 villages with village-level information on demography and facilities, such as schools, electricity, piped water, etc, from censuses of 1986 and 1996. For each census year we are able to tell whether the villages in our sample had schools (primary, lower- or upper-secondary), electricity, piped water, mosque, public bath, and the like. We eliminated the following outliers which in each case made minimal changes to our estimated results: 667 villages were dropped because they exhibited change in total population or the number of women ages 15-49 greater than plus or minus 100 percent during 1986-96, and 544 villages dropped because either their CWR was unrealistically low (less than 200 per 1000, roughly with TFR less than 1) or too high (greater than 1500 per 1000, roughly TFR exceeding 12 births). Of the remaining 13,053 villages we pick 9,287 as our sample for program evaluation. Our comparison group consists of 2,360 villages that did not have a health house in 1986 or 1996, and our program group consists of the 6,927 that did not have a health house in 1986 but acquired one in 1996. Therefore, 3,766 villages with complete data that had a health house before 1986 could not be part of our evaluation.

We measure the fertility outcome as the ratio of children 0-4 to women 15-49, the child-woman ratio (CWR), for each village using census data. CWR measures fertility in the previous five years, so in 1986 it actually measures fertility during 1982-86 and in 1996 fertility during 1992-96. This does not present a problem for us since the change in fertility from 1982-96 to 1992-96 still provides a good measure of change in fertility during a period of rapid expansion of the rural health network. CWR is not the ideal measure of fertility because it is a measure of surviving children and is therefore affected by infant mortality.[2]

In populations with the same life expectancy TFR can be derived from CWR, but not if mortality varies (Rele 1967). Since we are comparing CWR's at two points in time, if the distribution of the error induced by differences in child mortality across the country remains constant during 1986-96, the errors will cancel out. If, on the other hand, changes in child mortality are systematic we should expect a bias. For example, villages integrated into the health network may experience a more rapid decline in child mortality, which cause more of their births survive to be counted. In this case CWR would underestimate the extent of decline in fertility in program, but not comparison, villages. This would induce a positive correlation between family planning services and CWR, which is due to reduced mortality. This would cause a downward bias in the estimated program impact. We do sensitivity analysis by assuming a range of reductions in child mortality rates. For this purpose, in section 6 we experiment with reducing CWR for villages that received a health house during 1986-96 by 5-10 percent. These experiments indicate that the estimated program effects may be twice as large as our base estimates indicate.

Another possible source of bias is the sensitivity of CWR to migration. As with infant mortality, migration may cause CWR to measure fertility with error. Migra-

[2]It is also affected by mortality of women 15-49, but the variation in the mortality of this demographic group is much less than infant mortality.

tion can affect CWR in the absence of any change in fertility if its age selectivity affects the number of children 0-4 and women 15-49 at different rates. The problem can be serious if this error is correlated with the unobservables that influence program impact. During 1986-96, on average about one percent of the rural population migrated to cities. Moreover, the rate of migration differed between provinces and even by district within the same province, generally depending on economic conditions. So, potentially migration can be a source of variation in CWR that is not related to fertility and may thus bias our impact evaluation results. In particular, if migration affected CWR in program villages differently than in comparison villages, our estimated program effect would be biased. This can arise if, for example, program villages, which we know are better endowed with educational facilities, also send more unmarried women thus lowering the denominator and raising CWR. This would increase CWR in program villages relative to comparison leading to underestimation of the program effect. It is important to remember that because we use the difference-in-difference method of identification, any effect of migration on program and comparison villages at a point in time will cancel out. We will return to this issue and discuss the direction of potential bias from migration in Section 6 where we present our estimation results.

Table 2 presents the summary statistics for the sample as a whole. Mean village population was 828 in 1986, rising to 879 in 1996 and 968 in 2006. Average literacy rate for sample villages in 1986 was 37 percent, rising to 58 and 66 percent in 1996 and 2006. Child woman ratio declined from 940 children per 1000 women in 1986 to 510 in 1996 to 320 in 2006. We have information on village infrastructure only for 1986, when 94 percent of villages had a primary school, 29 percent a middle school, and only 2 percent a high school. Infrastructure ws more widely available: 85 percent of villages had electricity and 54 percent piped water. We have information on two variables related to religion, where or not there was a mosque in a village and whether the village had a majority Shia population. The presence of a mosque can signal the level of wealth of a village or it religiosity. Given the level of infrastructure and schools, it may be a better signal for the latter. Shia is the sect of Islam to which a majority of Iranian belong. Accordingly, 85 percent of sample villages are classified as Shia majority. We include these variables because the family planning program might be more effective in villages with a mosque and a Shia majority.

The distributions of CWRs for the full sample are shown in Figure 4 by census year. There is a dramatic shift to the left in the distribution of CWR in each ten year period as well as a reduction in the variance. The mean CWR declined from about one child per woman in 1986 to 0.4 in 1996 to about 0.25 in 2006. The declining mean and variance in village level CWR are indications of the depth and breadth of fertility decline in Iran. Figure 5 shows the shifts in the distributions for the program and comparison groups of villages that we use in our estimation. Both groups experienced sharp shifts in distribution to the left, indicating substantial fertility decline in both program and comparison villages, but the shift in the former was greater. Below we quantify the difference in CWR for the two groups as out estimate of program impact.

5 Identification

The geographic spread of health houses across Iran during 1986-96 is the basis for our strategy to identify their impact on fertility. Several studies have taken advantage of geographic variation in program placement to evaluate their impact (Duflo 2000, 2001, Breierova and Duflo 2004, Angeles, Guilkey, and Mroz 2005a and 2005b, and Miller 2005). As noted earlier, we construct a comparison group consisting of villages that did not have a health house in 1986 or 1996, and a program group consisting of those that did not have a health house in 1986 but did in 1996. We estimate the average treatment effect (ATE) by comparing change in fertility between these two groups.

The binary use of health house construction, which occurred each year during 1986-96, is an approximation forced on us by the fact that we observe village level fertility only in census years 1986 and 1996. It may be argued that villages with a health house in 1995 should be counted in the comparison group. We examine the sensitivity of our results to how we define being treated by taking out the villages with short duration from the program group. The results do not change significantly. We also try a non-binary approach in section 7, where we estimate the effect of the number of years that a health house is present on fertility. Again, our results do not change when we define treatment differently.

More formally, the expected or average change in fertility in comparison villages is given by

$$E(Y^0_{86} - Y^0_{96}|D = 0),$$

where Y^0_{86} is CWR in 1986 for comparison villages (the superscript 0 denotes comparison and 1 program), and D is the program dummy which is equal to 0 if not treated (comparison). Similarly, the expected change for program villages is given by the difference in fertility over time of that group given treatment:

$$E(Y^1_{86} - Y^1_{96}|D = 1).$$

Average treatment effect is the difference between these differences:

$$E(Y^1_{86} - Y^1_{96}|D = 1) - E(Y^0_{86} - Y^0_{96}|D = 0). \qquad (1)$$

The theoretical (counterfactual) difference, which is the true program impact, is the change in the outcome variable in program villages had they not been treated (Heckman, LaLonde, and Smith 1999):

$$E(Y^1_{86} - Y^1_{96} | D = 1) - E(Y^1_{86} - Y^1_{96} | D = 0). \tag{2}$$

Because program villages cannot be observed in the no-program state, we cannot observe (2). However, under certain assumptions the average treatment effect (1) is a good estimate of (2). The most straightforward case is when we can assume random assignment of villages to comparison and program groups, this would work. When selection is not random by design, good approximations can be obtained by matching or accounting for the differences in outcomes that are due to observable factors that affect assignment. As we have shown, in our case we cannot claim random assignment, but we are able to account for observables that we know affected placement, such as infrastructure and education. By conditioning on these variables we are in essence comparing CWR's for the two groups as if they had the same distribution of these attributes. The more thorny problem of *unobservables* that follow different trends over time is much harder to deal with. For the DID to accurately identify the program effect we must assume that these trends are the same for the two groups. Unfortunately, as with (2), this assumption cannot be tested.

6 Difference-in-difference estimation

We begin with the summary statistics for DID estimation presented in Table 3. Recall that comparison villages are those without a health house in 1986 and 1996 and program villages are those without a health house in 1986 but with one in 1996. Approximately 25% of the villages in our sample belong to comparison group (2,360 villages) and the rest (6,927 villages) to program. The two groups of villages are similar in average population size before and after treatment; they are very similar in CWR in 1986 (1.00 for comparison vs, 0.96 for program villages) but become different in 1996. Consistent with their lower CWR, program villages are on average more literate and more likely to have schools.

A simple comparison of fertility change during 1986-96 in comparison and program villages shows what to expect from a more full fledged comparison from a DID regression. Table 4 shows that while both groups of villages experienced decline in CWR, the rate of decline in program villages was faster. CWR fell in program villages by an average of 460 per 1000 women and in comparison villages by 425, resulting in the difference in difference of 35 children per 1000 women (roughly one birth per four women). Under the assumption of random assignment, this decline can be attributed to health houses.

To improve on this estimate we condition CWR on a host of variables that can potentially affect program placement and fertility. The conditional DID estimates are obtained from this regression, which is used often in DID estimation (Todd 2008):

$$Y_{it} = \alpha + \beta D_{it} + \gamma Year + \delta(D_{it} * Year) + X_{it}\psi + \epsilon_{it} \tag{3}$$

where Y_{it} is the child-woman ratio of village i in year t, D is a dummy variable which takes the value of one if the village has a health house in year t, $Year = 1$ if 1996 and zero otherwise, and X is a vector of controls that affect Y_{it}. The value of β is the estimate of the difference between program and comparison villages, γ is the common time trend, and δ is the program effect, which is the DID estimator.

As noted in Section 3, the main concern with our identification strategy is that program placement may be endogenous, that is, villages covered earlier are systematically different from those that received services later. If these differences are not observed and therefore not included in X, the dummy variable D would be correlated with the error term ϵ and the estimates of program effect would not be consistent. This can happen, for example, when program placement is compensatory in the sense that villages with lower health status or higher fertility are selected first, or if people migrated to program areas to take advantage of the program (Rosenzweig and Wolpin 1986, 1988; Schultz 1988). Only studies that take advantage of an experimental design can claim true randomness in assigning women to treatment and control groups and thus offer a clean test of program effect (Sinha 2005). Others must rely on ways to reduce the bias resulting from endogenous program placement. As discussed earlier, placement of health houses in Iran was not based on the level of fertility, which removes one problem, and we alleviate the bias from other observable sources of village selection, such as education and infrastructure, by conditioning on them.

The regression results for equation (4) are presented in Table 5. The last column (5) presents the results of the district-level fixed effects, which intend to remove unobserved variables that vary between but not within district. Although fixed effects results are very close to those of the full model in column 4, we consider them to be our most complete set of results. The average difference between program and comparison villages in first row is rather small and gets much smaller and insignificant when we add controls for schools, infrastructure, religion, and population size, and when we estimate using fixed effects. This is the kind of result one would like to see when CWR is conditioned on village characteristics that may have played a role in placement. This result suggests that the differences in fertility between program and comparison villages at the beginning of the period are mostly due to the variables that we can observe. The common time trend in row 2 shows a reduction of 427 children per 1000, which is a decline of about fifty percent in just 10 years.

The coefficients of interest are in row 3, where the estimated program effect is listed as 33-35 fewer children per 1000 women. These are nearly identical to the unconditional effects reported above in Table 4, or about 7 percent of the decline in fertility in program villages. The size of this estimate increases significantly when we try to account for the possible underestimation of CWR in 1996 in program areas due to lower child mortality, as discussed in section 4. Reducing CWR for these villages by 5 and 10 percent increases the estimated program effect to 58 and 83 children per 1000 women. These estimates indicate that the program effect can be as high as 20 percent of the decline in CWR. For one specific part of the program, active health and family planning delivery, even the lower bound of 7 percent is not a negligible effect.

We introduce our exogenous regressors in columns 2-4 and interact them with our treatment dummy. None of the interaction variables are significant, indicating that the effect of the control variables did not depend on whether villages did or did not have a health house. The presence of primary, middle, and high school in the village in 1986 are all significant and, as expected, negatively related to fertility. These variables may be endogenous to some degree (villages with lower fertility are more likely to acquire schools), so their estimated coefficients may overstate the true size of their effect. The most important schooling level is middle school, which is in part explained by the fact that the variation in the other two types of schools is low — in 1986, 94 percent of villages had a primary school and only 2 percent had a high school (Table 2). The interaction of schooling with the combined treatment-year dummies, which pick up differences in the effect of treatment in villages with and without schools were likewise insignificant (not reported).

Villages with mosques and with majority Shia populations also had, on average, lower fertility, indicating the possible greater credibility of the message of the government (led by the Shia clergy) in Shia areas. Villages with access to electricity had lower fertility but, surprisingly, program villages with piped water had *higher* fertility. This may be due to the fact that availability of clean water has a more direct effect on lowering mortality which, as discussed in Section 4, can raise CWR for a given level of fertility. The coefficient of piped water changes sign and is significant in the fixed effects regression. This is probably due to the fact that unobservable village health conditions vary between but not within districts, and therefore disappear when we take out the fixed effects.

Fixed effects regressions also control for cultural norms that affect individual responses to the family planning program as well as the quality of family planning services that we believe vary between districts rather than within districts. Since we do not have information about the quality of services offered in the HNS, these results offer a glimpse of how important quality variation may be in determining the size of the program effect. The fact that the size and sign of the coefficients of the common time trend and program effect remain unchanged in the fixed effects regressions indicates that the unobserved district-level variables are not important in the determination of these effects. Finally, we introduced province dummies, which pick up the fertility differences between provinces, but the results were unchanged (not reported).

To check for the existence of spillover effects from program to comparison villages, we add a control in the DID regression for the extent of HNS coverage in the district in which a village is located. If spillovers are important, this variable should negatively affect village fertility even after controlling for the existence of a health house and other village characteristics. We define coverage as the proportion of women in a district with HNS coverage in 1996. The mean value of this variable is 0.63, and its standard deviation is 0.38 (Table 3). Regressions results with this variable interacted with the treatment dummy in Table 6 show that the effect for district level coverage of health houses on CWR is negative and significant for both groups of villages. The coefficients of other variables in the regression are not reported because they do not change. The estimated effect of district-wide coverage on CWR is 107 children per 1000 women in comparison villages when we do not control for village characteristics (column 1), which is very large and is three times the effect on program villages. But with the addition of controls the former reduces to 44 and the latter increases to 51. Clearly, the variable measuring health house coverage at the district level is correlated with village characteristics, so its coefficient conditional on those characteristics is closer to the spillover effect. Taking the lower figure of 44 fewer children per 1000 women, each additional health house in a district with average population of 13,812 women raises the district coverage ratio by 0.016 and CWR in a comparison village by less than one child per 1000 women. This is less than 3 percent of the effect of a health house on a program village.

We also revisit here the potential bias due to rural out-migration that may cause errors in CWR as a measure of fertility. Recall from our discussion in Section 4 that the problem arises when the construction of a health house might raise or lower the migration rate and thus change CWR without change in fertility. Consider the case when villages with a health house are more likely to lose younger or older women who do not have children. In this case, CWR in these villages will be higher and it would appear as if fertility had not declined by as much as it did, causing the program effect to be underestimated. We believe that this is unlikely to increase the program effect much beyond the 7-20 percent range that we have reported here because, first, such pattern of age selectivity of out-migration is rare in Iran. Most migrants to urban areas are young unmarried men, or married men who migrate with their wife and children. Second, like child mortality, to the extent that out-migration is a district characteristic, the fixed effects regression results in column 5 should not be contaminated by it. Since the fixed effects results are virtually the same as OLS, the bias due to out-migration is very unlikely to change the estimated program impact beyond our estimated range.

The regression in this section define treatment as a binary variable, thus ignoring the effect of the length of exposure to fertility. An obvious question arises if those villages who received their health houses in 1995 and are grouped as treated are that different from the not treated group. Similarly, those that got theirs in 1987 and are therefore included in the sample should perhaps be dropped as are those with a health house in 1986. We changed the break points to allow for these changes and noticed no substantial change in the results. Nevertheless, we believe that exposure, especially before and after policy reversal may matter, so in the next section we introduce the number of years of exposure explicitly.

7 The effect of exposure to the program

In this section we report on a different way of using the administrative data on the timing of establishment of health houses. About 8,000 health houses that were in operation before 1989, the year of policy reversal, changed function that year from delivering health services for mother and child to family planning. Some of these health houses may have helped rural women with family planning, but at best those services would amount to passive delivery. It can be therefore argued that the level of family planning services in villages with a health house before 1989 is not that different from the passive services available to women in villages without a health house *after* 1989. So, one way of gauging the impact of health houses on family planning is to ask whether the effect of having one on fertility differed before and after 1989. To answer this question we move from a binary variable for presence of a health house to a more continuous one based on the length of exposure (number of years a health house has been present in the village). This allows us to measure exposure before and after 1989, and measure impact according to function.

To do this we work with the entire sample of 13,053 matched villages, and regress CWR in 1996 and 2006 on the number of years the health house has been in operation till then.

$$Y_{it'} = \alpha + \alpha' Y_{i86} + \beta E_{i86} + \beta' E_{i89} + X_{it}\psi + \epsilon_i \tag{4}$$

where t' is 1996 or 2006, and E_{i86} and E_{i96} are years of exposure before and after 1989. The results are presented in Table 7. In columns (1) and (4) we do not distinguish exposure based on function, and as a result exposure has a small but significant negative effect on CWR in 1996, and even smaller in 2006. The smaller effect by 2006 is understandable as most of the decline had occurred by 1996 and other aspects of the family planning program besides health houses had more time to influence fertility. The interesting results are when we distinguish between the years of program exposure before and after policy reversal (columns 2 and 5), where we see strong evidence that function mattered. Having a health house before 1989 that only offers child and maternal health care has a small positive (and significant) effect on CWR, presumably picking up the effect on CWR through child mortality, but having one after 1989, when they offered family planning, has a larger negative effect on fertility (also significant). Columns 3 and 6 report the results using district-level fixed effects. The estimated effect is quantitatively similar to the range we found treating the health house as binary variable. Each year of having a health house reduces CWR in 1996 by 8 or 9 births, which for the average village with 5 years of post 1989 HH service is about 40 children per 1000 women. This is about 10 percent of the decline, which is greater than the 7 percent lower bound estimate from the binary health house variable.

8 Conclusion

During the 1990s rural fertility in Iran declined sharply. At the same time an innovative family planning program, known as the rural Health Network System, was gradually extended across the country covering more than 60,000 villages and about 5 million rural families. This paper takes advantage of the timing of integration of villages into the Health Network System to identify the impact of health houses on fertility. We focus on health houses build between 1986 and 1996, which number about 8,000 and serve about 1.2 million households. We measure fertility at the village level in these census years for two groups of villages, those without a health house in either year, which we call comparison villages, and those that received one between 1986 and 1996, which we call the program group. We show that while both groups had substantial declines in their child woman ratios, the program group experienced a faster rate of decline. The average decline in CWR in the program group was 460 children 0-4 per 1000 women, compared to 425 in comparison villages, yielding a difference-in-difference (DID) estimate of about 35 children per 1000 women, or about 7 percent of the decline in program villages.

We discuss the possibility of endogenous placement effect and, using a probit equation, show that the probability of having a health house in 1986 and in 1989 was not positively related to fertility. We use the set of variables representing village characteristics — such as the availability of schools and basic infrastructure, population size, and religion — that do affect placement, as controls in the DID regression. The results show an average treatment effect of about 33 children per 1000 women, which is very close to the unconditional DID results.

We check the sensitivity of our estimates of program impact for possible bias due to measurement error in CWR; specifically, one caused by the effect of health houses on child mortality that would raise the measured CWR relative to the actual level of fertility. Allowing for a reduction of 10 percent in CWR in 1996 in program villages, we obtain an estimate of 83 children per 1000 women, or 18 percent of fertility decline, as the upper bound for program impact.

Our estimates of impact evaluation refer specifically to health house construction, which is just one component – albeit the most important one– of Iran's family planning program. In light of this, the estimated size of impact is not at all negligible.

The question of interest for policy makers is what, if anything, can be learned from Iran's family planning program. We believe that, by isolating the effect of an important component of the program, and one that is most likely to be replicable in other circumstances, this paper has contributed to the policy debate on the impact of government sponsored family planning programs. To replicate even this specific aspect of Iran's program, it is important to understand the larger context in which the health houses in Iran operated and succeeded in lowering fertility in rural areas. Two sets of factors define the Iranian context in our view. One set of factors belong to the family planning program itself, most importantly the credibility of the information campaign that accompanied the program. Another set are factors are the secular trends in economic development, education, and general modernization, which have independently contributed to fertility decline.

The government's information campaign in support of family planning and advocating the benefits of smaller families was particulary effective because in prior years the government had been busy building in rural areas infrastructure and health houses focused on child and maternal health care. The messages on billboards across the country seemed to echo the economist's view of a tradeoff between quantity and quality of children. The populist nature of the revolution and consistent messages from its leaders about empowering the rural poor in Iran's new Islamic society, raised the hopes of the poorer rural families that their children, once educated, could compete with the children of urban families on equal, if not better, terms for government jobs. While these considerations likely raised the value of child education in the eyes of the poor, other government actions, raised the cost of large families. Families were on notice that government support for health and education of higher parity children might soon cease. By 1989, the system of rationing of basic commodities that was in effect during the war with Iraq and had favored larger families was actively being dismantled.

Under these circumstances the effective delivery of health, and later family planning, services lowered the cost of controlling fertility to rural families, and helped persuade them to change their childbearing strategy from high fertility and low investment in children to low fertility and high investment, even before the actual services arrived.

At the same time, Iran had been on a steady path of growth and modernization going back at least to the 1950s. The secular trends in education and modernization that had been at work since the 1960s may have already put rural Iran on a path of fertility decline. However, even if secular forces were at work the whole time, it is difficult to deny the role of government policies after the revolution that empowered the lower social strata and supplied them with basic health and family planning services. Given the complexity of the interactions between various factors that we have outlined here, we have at least been able to isolate and quantify the impact of one component of Iran's program using causal empirical techniques.

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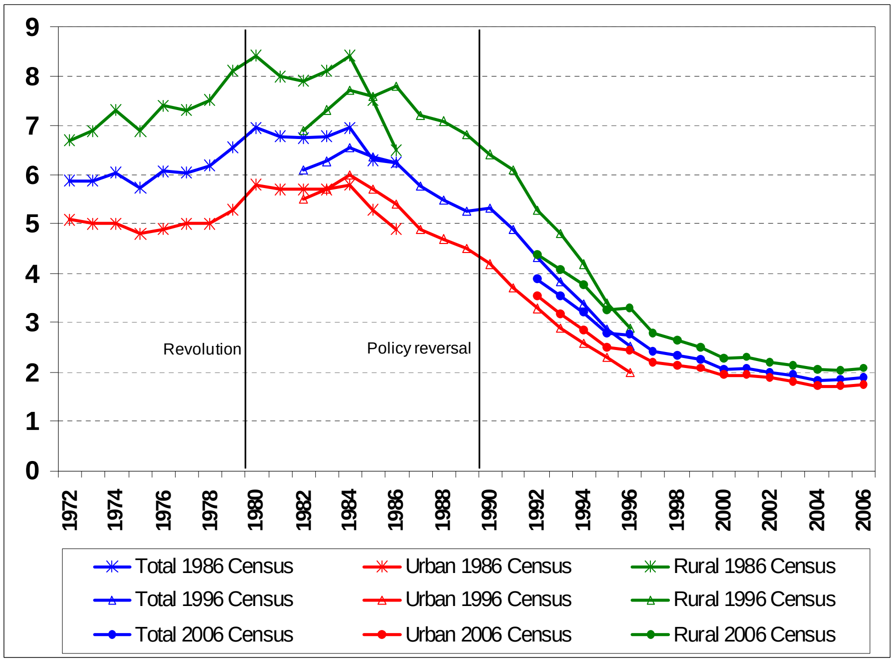
Figures

Figure 1: Estimates of the Total Fertility Rates Using the Own-Children Method, 1972-2006



Source: Abbasi-Shavazi, McDonald, and Hosseini-Chavoshi (2009)

Figure 2: The Administrative Structure of Iran's Rural Health Network System

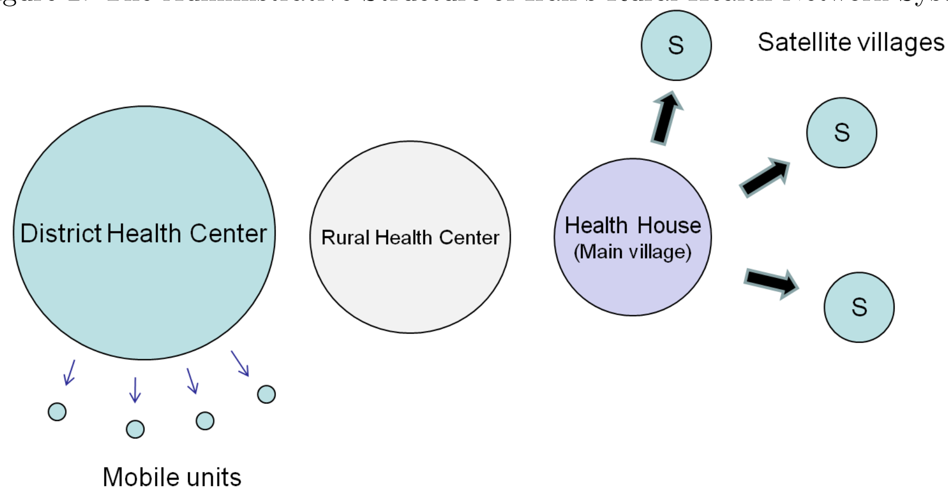


Figure 3: The Cumulative Distribution of Health Houses in Rural Iran by Year of Establishment

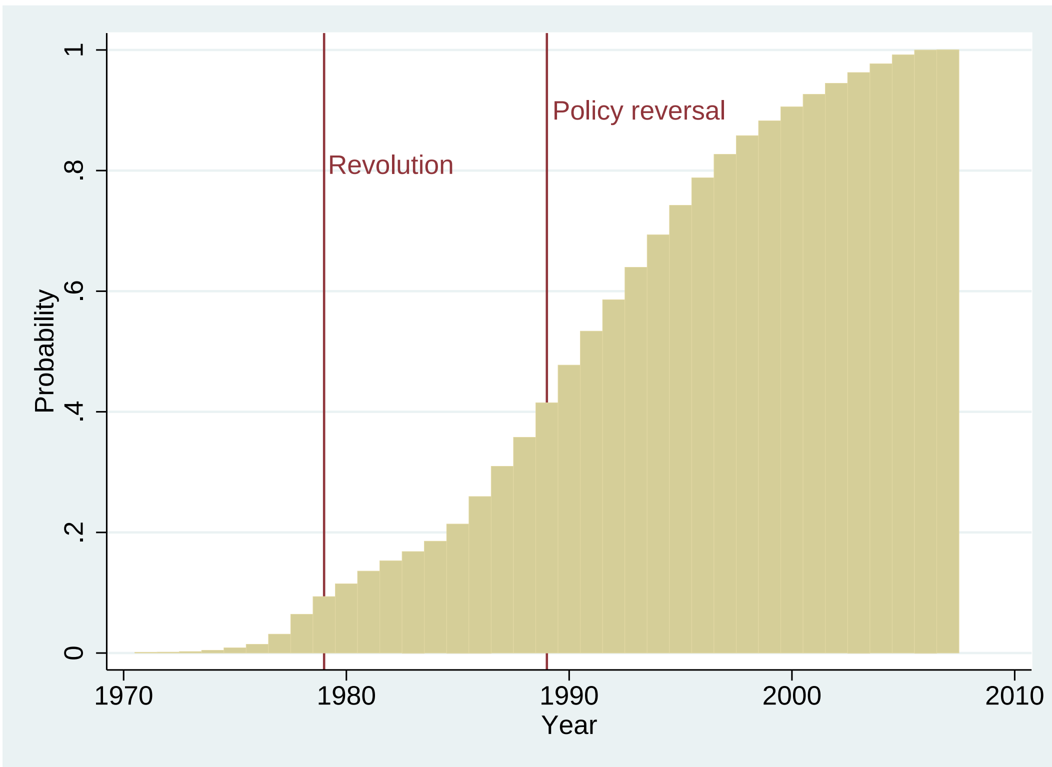


Figure 4: Changes in the Distribution of Village-Level Child Woman Ratios, Rural Iran, 1986-96

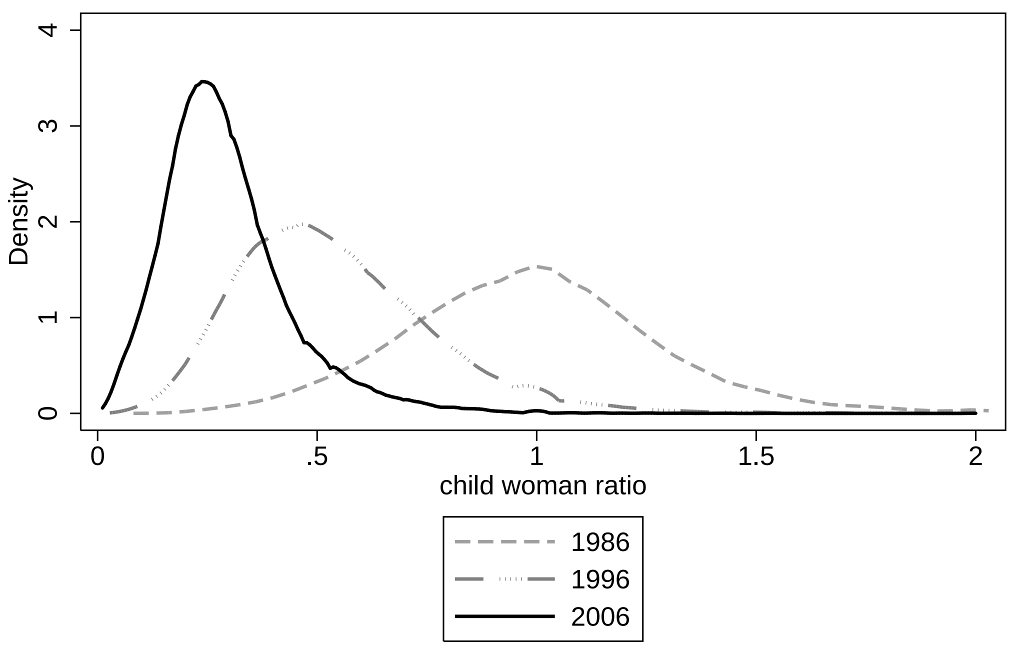
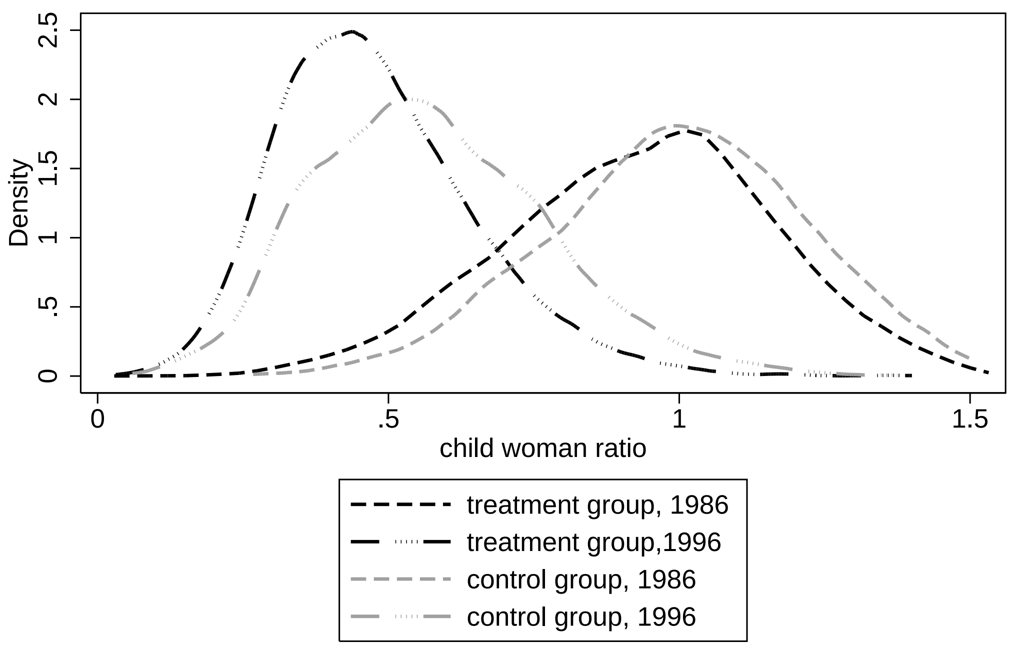


Figure 5: Change in the Distribution of Child Woman Ratios for Comparison and Program Groups, Rural Iran, 1986-96



Tables

Table 1: The Determinants of Program Placement: Probability of Having a Health House in in Rural Iran

	In 1986	In 1986	In 1989
Child Woman Ratio 1986	-0.302	-0.113	-0.115
	(0.018)**	(0.021)**	(0.023)**
Log of population 1986	-	0.012	0.010
		(0.007)	(0.008)
Proportion of women literate 1986	-	0.498	0.635
		(0.046)**	(0.050)**
In 1986 had:			
Primary school	-	0.082	0.105
		(0.020)**	(0.022)**
Middle school	-	0.186	0.243
		(0.011)**	(0.011)**
High school	-	-0.003	0.039
		(0.025)	(0.033)
Mosque	-	-0.000	-0.014
		(0.013)	(0.014)
Shia majority	-	0.048	-0.021
		(0.013)**	(0.015)
Electricity	-	0.035	0.039
		(0.009)**	(0.010)**
Piped water	-	0.030	0.036
		(0.008)**	(0.010)**
Observations	13053	13040	13040

Notes: The dependent variable is a binary variable for the presence of a health house in 1986 and 1989. Coefficients are marginal effects evaluated at the mean of the independent variables. Standard errors in parentheses: * significant at 5%; ** significant at 1%.

Table 2: Summary Statistics for the Full Sample

	Mean			Std. Dev.		
Year	1986	1996	2006	1986	1996	2006
Village population	828	879	968	724	819	990
Children 0-4	155	101	85	142	98	93
Women 15-49	166	205	277	146	193	291
Child woman ratio	0.95	0.51	0.32	0.23	0.18	0.10
Literacy	0.37	0.58	0.66	0.13	0.11	0.09
Proportion of villages in 1986 with						
primary school	0.94	-	-	0.23	-	-
middle school	0.29	-	-	0.45	-	-
high school	0.02	-	-	0.16	-	-
mosque	0.84	-	-	0.36	-	-
shia majority	0.85	-	-	0.35	-	-
electricity	0.54	-	-	0.50	-	-
piped water	0.58	-	-	0.49	-	-
# of observations	14,158	14,225	11,011			

Table 3: Summary Statistics for Comparison and Program Groups

	Mean				Std. Dev.			
	Comparison		Program		Comparison		Program	
Year	1986	1996	1986	1996	1986	1996	1986	1996
Total population	700	758	816	839	767	880	658	709
Children 0-4	137	95	154	96	155	106	128	85
Women 15-49	139	176	163	197	154	208	132	168
Child woman ratio	1.00	0.58	0.96	0.50	0.22	0.19	0.21	0.17
Literacy	0.31	0.54	0.35	0.58	0.14	0.12	0.12	0.10
Proportion of villages in 1986 with								
primary school	0.91	-	0.95	-	0.29	-	0.21	-
middle school	0.12	-	0.24	-	0.32	-	0.43	-
high school	0.01	-	0.02	-	0.10	-	0.13	-
mosque	0.78	-	0.86	-	0.41	-	0.35	-
shia majority	0.80	-	0.85	-	0.40	-	0.36	-
electricity	0.43	-	0.51	-	0.50	-	0.50	-
piped water	0.53	-	0.57	-	0.50	-	0.49	-
District HH coverage in 1996		0.63				0.38		
# of observations	2,360	2,360	6,927	6,927				

Note: This is a sub-sample which excluded villages who received their health house before 1986 or after 1996. Program villages received their health house after 1986 and Comparison villages did not have a health house by 1996. District level health house coverage is the proportion of women in a district with HNS coverage.

Table 4: The (Unconditional) Difference-In-Difference Estimation of Program Effect

	CWR1986	CWR1996	Difference
Program	0.964	0.503	0.460
Comparison	1.001	0.576	0.425
Difference	0.038	0.073	0.035

Note: CWR is the average ratio of children 0-4 to women 15-49.

Table 5: The difference-in-difference estimation for change in fertility 1986-1996

	(1)	(2)	(3)	(4)	(5)
Difference between two groups	-0.038	-0.043	-0.028	-0.019	0.011
(Program dummy=1)	(0.005)**	(0.012)**	(0.014)*	(0.029)	(0.026)
Common time trend	-0.425	-0.425	-0.425	-0.427	-0.427
(Period dummy=1)	(0.006)**	(0.006)**	(0.005)**	(0.005)**	(0.005)**
Program effect	-0.035	-0.035	-0.035	-0.033	-0.033
(Program dummy*Period dummy=1)	(0.007)**	(0.006)**	(0.006)**	(0.006)**	(0.005)**
Primary school 86		-0.046	-0.008	-0.015	-0.038
		(0.010)**	(0.010)	(0.010)	(0.009)**
(treatment=1)*prim school 86		0.018	0.017	0.001	0.003
		(0.012)	(0.013)	(0.012)	(0.011)
Middle school 86		-0.081	-0.061	-0.077	-0.064
		(0.009)**	(0.009)**	(0.009)**	(0.008)**
(treatment=1)*middle school 86		0.003	-0.002	-0.003	-0.001
		(0.010)	(0.010)	(0.010)	(0.009)
High school 86		-0.094	-0.097	-0.117	-0.081
		(0.029)**	(0.028)**	(0.028)**	(0.025)**
(treatment=1)*high school 86		0.024	0.030	0.045	0.025
		(0.031)	(0.031)	(0.030)	(0.027)
Mosque 86			-0.070	-0.085	-0.010
			(0.007)**	(0.007)**	(0.007)
(treatment=1)*mosque 86			-0.013	-0.015	-0.018
			(0.008)	(0.009)	(0.008)*
Shia 86			-0.079	-0.076	-0.027
			(0.007)**	(0.007)**	(0.007)**
(treatment=1)*shia 86			0.004	0.004	-0.006
			(0.008)	(0.008)	(0.007)
Electricity 86				-0.080	-0.045
				(0.006)**	(0.005)**
(treatment=1)*electricity 86				0.012	0.001
				(0.007)	(0.006)
Pipedwater 86				-0.009	-0.019
				(0.006)	(0.005)**
(treatment=1)*pipedwater 86				0.019	0.009
				(0.006)**	(0.006)
Log population				0.044	0.040
				(0.004)**	(0.004)**
(treatment=1)*lpopulation				-0.001	-0.002
				(0.005)	(0.004)
Constant	1.001	1.053	1.134	0.915	0.845
	(0.004)**	(0.010)**	(0.011)**	(0.025)**	(0.023)**
Observations	18574	18574	18574	18574	18574
R-squared	0.57	0.59	0.61	0.62	0.72

Notes: Standard errors in parentheses; * significant at 5%; ** significant at 1%. Columns 1-4 are OLS, column 5 is district-level fixed effects.

Table 6: The DID estimation with health house coverage at the district level

	(1)	(2)	(3)	(4)
Program=1	-0.000	-0.008	0.003	0.018
	(0.012)	(0.016)	(0.017)	(0.032)
Common time trend	-0.425	-0.425	-0.425	-0.427
	(0.006)**	(0.006)**	(0.005)**	(0.005)**
Program effect	-0.035	-0.035	-0.035	-0.033
	(0.007)**	(0.006)**	(0.006)**	(0.006)**
HH coverage	-0.107	-0.104	-0.075	-0.044
	(0.016)**	(0.015)**	(0.015)**	(0.015)**
(treatment=1)*HH coverage	-0.035	-0.033	-0.036	-0.051
	(0.018)	(0.018)	(0.018)*	(0.018)**

Notes: Standard errors in parentheses; * significant at 5%; ** significant at 1%. The list of controls in each column are the same as in Table 5.

Table 7: The effect of years of exposure to family planning on CWR in 1996 and 2006

	CWR 1996			CWR 2006		
	(1)	(2)	(3)	(4)	(5)	(6)
CWR in 1986	0.319	0.318	0.241	0.066	0.066	0.035
	(0.006)**	(0.006)**	(0.006)**	(0.004)**	(0.004)**	(0.004)**
Exposure	-0.002			-0.001		
	(0.000)**			(0.000)**		
Exposure before 1989		0.001	0.001		0.001	0.001
		(0.000)**	(0.000)**		(0.000)**	(0.000)*
Exposure after 1989		-0.007	-0.008		-0.003	-0.002
		(0.000)**	(0.000)**		(0.000)**	(0.000)**
Primary school in 1986	-0.038	-0.035	-0.039	-0.030	-0.029	-0.022
	(0.006)**	(0.006)**	(0.005)**	(0.004)**	(0.004)**	(0.003)**
Middle school in 1986	-0.036	-0.035	-0.027	-0.012	-0.013	-0.014
	(0.003)**	(0.003)**	(0.003)**	(0.002)**	(0.002)**	(0.002)**
High school in 1986	-0.004	-0.005	0.003	-0.002	-0.004	-0.005
	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Mosque in 1986	-0.038	-0.038	-0.027	-0.017	-0.017	-0.022
	(0.004)**	(0.004)**	(0.004)**	(0.003)**	(0.002)**	(0.003)**
Shia majority in 1986	-0.090	-0.091	-0.042	-0.061	-0.061	-0.024
	(0.004)**	(0.004)**	(0.005)**	(0.002)**	(0.002)**	(0.003)**
Electricity in 1986	-0.057	-0.057	-0.038	-0.020	-0.021	-0.014
	(0.003)**	(0.003)**	(0.003)**	(0.002)**	(0.002)**	(0.002)**
Piped water in 1986	-0.012	-0.012	-0.012	-0.013	-0.013	-0.009
	(0.002)**	(0.002)**	(0.002)**	(0.002)**	(0.002)**	(0.002)**
Constant	0.407	0.420	0.434	0.377	0.393	0.378
	(0.009)**	(0.009)**	(0.009)**	(0.006)**	(0.006)**	(0.007)**
Observations	13051	13051	13051	10458	10458	10458
R-squared	0.37	0.38	0.52	0.18	0.18	0.41

Note: Standard errors in parentheses; Dependent variable in columns 1-3: child woman ratio in 1996; Dependent variable in columns 4-6: child woman ratio in 2006 columns 3 and 6 are district-level fixed effects; * significant at 5%; ** significant at 1%.