The short-run fertility impact of a disruption in publicly-provided contraceptive supply in the Philippines

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Abstract

I take advantage of the gradual phase out of USAID's contraceptive donations to the Philippines from 2004 to 2008 as a natural experiment that exogenously disrupted publicly-provided contraceptive supply. Prior to this, more than two-thirds of the country's contraceptive users relied on free supplies from the public sector, which in turn relied entirely on donations from international aid agencies for its contraceptive supply (of which USAID was the primary donor). Because it succumbed to pressure from the Catholic church, the national government did not fill the shortage that occurred. While it devised a progressive allocation schedule for the distribution of the declining contraceptive supply received from donors at the central office and delivered to the different provincial and city offices, the actual distribution of contraceptives was erratic, intermittent, and unanticipated due to shipment delays, inventory miscues at the central office, and lumpy deliveries induced by round lot sizes.

Utilizing substantial geographic and temporal variation in the proportion of women of reproductive age that had provisions for contraceptive supply from the public sector at the province-quarter level, this research looks into the short-run fertility impact of the contraceptive supply disruption. This panel fixed effects identification strategy isolates the contribution of an important supply-side determinant in reducing fertility and allows estimation of the causal impact of taking funding away from the contraceptive supply component of publicly-managed family planning programs.

Results using vital statistics data show that a demonstrable linkage exists between diminishing contraceptive supply from the public sector and higher general fertility rates, which suggests that couples' compensating behavior may be limited or incomplete. Furthermore, a woman-level analysis of pregnancy risk shows that this impact was more severe for rural residents, the poor, and the less educated, so that reducing or eliminating publicly-provided contraceptive supply has important welfare consequences.

1 Introduction

In the developing world, family planning (FP) programs have been an important component of development plans since the 1960s because of the belief that the availability and affordability of modern contraceptives will allow couples to regulate their fertility, which in turn may improve their families' socio-economic outcomes and increase their investment per child.[1] This belief, together with an interest in promoting the reproductive health of women and children,[2] underpin the substantial amount of international aid given to developing countries in the form of donated contraceptives (pills, IUDs, condoms, and injectables).[3]

However, clear evidence that supports or negates the belief that contraceptive subsidies matter for achieving fertility reduction goals has been difficult to obtain. Estimating the causal impact on fertility of the supply-side components of FP programs has faced two important challenges: (1) the establishment, management, and utilization of FP programs are likely to be related to the demand-side characteristics of the communities where they are placed, i.e. subsidies may be targeted, and this produces endogeneity that may bias estimates of program impact in either direction,[4] and (2) FP programs usually incorporate demand-side components that try to stimulate FP adoption or induce the revelation of "latent demand" for FP,[5] so that the estimated program impacts may commingle the effects of both components.

While several studies in the economics literature have confronted the problem of non-random FP program placement using a variety of approaches,[6] none yet have tried to distinguish the separate fertility effects of supply inputs from demand-side interventions. The available evidence from careful program evaluations for different countries confirm the effectiveness of FP programs in reducing fertility.[7] Unpacking this result is important because it will better inform policy discussions on budget allocations between the two FP program components, including the relative desirability of various financing and implementation options (e.g. full subsidy, cost recovery, insurance coverage, or public provision targeted to the poor alongside development of the private market for contraceptives geared for the non-poor) that are increasingly being explored as countries' FP programs mature and are weaned from

[1]In addition, there are also social benefits from a reduction in the population of young dependents relative to the population of working-age adults; this is usually referred to as the "demographic dividend."

[2]Contraceptives allow for safe birth spacing and limiting practices that help reduce the risk of maternal and infant mortality and low birth weight.

[3]While most international aid is channeled to governments, support is also given to non-government organizations (NGOs) and other private sector entities.

[4]The direction of the bias introduced by this non-random program placement is not clear since governments might have various motivations for its actions: for example, it may try to place FP programs where fertility is high, so that the estimated program impact might be weaker if high-fertility areas are relatively slower or more reluctant to adopt modern contraceptives, or it may place FP programs where other resources are already present because of administrative convenience, in which case the estimated program impact might be stronger because more developed areas are likely to be more receptive to using modern contraceptives.

[5]These demand-side components are relatively harder to measure than, and likely collinear with, supply inputs.

[6]These include classical experiments (Freedman and Takeshita 1969, Foster and Roy 1997, Sinha 2005, Joshi and Schultz 2007), quasi-experiments using panel fixed effects (Pitt, Rosenzweig, and Gibbons 1993; Gertler and Molyneaux 1994; Salehi-Isfahani, Abbasi-Shavazi, and Hosseini- Chavoshi 2010) and instrumental variables (Molyneaux and Gertler 2000), and structural model estimation (Angeles, Guilkey, and Mroz 1998, 2005a, 2005b). More recent evidence used geographic and temporal variation in FP program rollout that was argued to be as good as random (Miller 2009, Bailey 2011).

[7]However, there is a need to be cautious with this conclusion since the magnitudes involved in some cases were economically small, if not insignificant, and explain only a small portion of the actual fertility declines observed.

reliance on external support.

It is noteworthy that all these previous research traced out the impact on fertility of the introduction or intensified implementation of an FP program, but not much has been studied about the effects of a discontinuation or weakened implementation of the same.[8] In the case of a decline in the contraceptive supply of a publicly-managed FP program, because there is scope for couples' compensating behavior, such as the purchase of contraceptives from commercial sources (if available) and substitution into other contraceptive methods,[9] it is not likely that the estimated impact would simply correspond to a reversal. Looking at program rollbacks may also permit estimation of the "value" that couples attach to having contraceptives, which is not well-defined during a program rollout since its value may be hard to ascertain before usage and if given for free.

In this paper, I take advantage of the gradual phase out of USAID's contraceptive donations to the Philippines from 2004 to 2008 as a natural experiment that exogenously disrupted publicly-provided contraceptive supply.[10] Prior to this, more than two-thirds of the country's contraceptive users relied on free supplies from the public sector, which in turn relied entirely on donations for its contraceptive supply. Because it succumbed to pressure from the Catholic church, the national government did not take up the shortfall that occurred (it originally intended to do so), and instead devised a progressive allocation schedule[11] for the distribution of the declining contraceptive supply received at the health department's central office and delivered to the different provincial and city health offices. However, because of shipment delays, inventory miscues at the central office, and lumpy deliveries induced by round lot sizes, actual distribution was erratic and intermittent (although the progressive character of the original schedule was largely intact).

Accounting for the transparently progressive nature of the allocation scheme helps reduce confounding due to feedback effects,[12] which may arise if the government behaved strategically and incorporated emerging fertility trends in its supply-setting decision. Since knowledge of FP methods was already universal in this country, and the national government only promoted the use of "natural" contraceptive methods[13] during this period, these circumstances permit the isolation of the effect of the supply-side component of the FP program. Both these features, together with the unanticipated and idiosyncratic nature of the distribution scheme that materialized during the phase out period, help secure strong identification of the causal impact on fertility of taking funding away from the contraceptive supply component of publicly-managed family planning programs.

Estimates from a (balanced) panel linear regression with province and year-quarter fixed

[8]While there are some published reports about the "graduation" experiences of some countries (see USAID 2005 for a review), formal impact evaluations have been more limited (see discussion in Cromer, et al. 2004 for example).

[9]Alternatives include cheaper but less reliable traditional methods (calendar/rhythm and withdrawal) and more effective but irreversible long-acting/permanent methods (ligation or vasectomy).

[10]Conditions that influence the demand for children, like average income and educational attainment, were relatively stable during this short period.

[11]The *planned* distribution schedule was based on poverty incidence in year 2000 such that full phase out would happen sooner and faster for richer provinces and later and slower for poorer ones.

[12]This is the same issue of non-random program placement in the context of actively managing FP programs in different communities to address supply imbalances (stockouts and surpluses).

[13]These were variants of the rhythm method, the only form of contraception the Catholic church allows.

effects using natality vital statistics data show that birth rates significantly went up by about 4%[14] due to the average decline in contraceptive supply rate[15] of about 6 percentage points. Consistent with this, micro-level analysis using DHS data and employing women fixed effects show that the likelihood of a pregnancy went up for rural residents, the poor and the less educated.

2 Background

The birth rate in the Philippines, a predominantly Catholic country, continues to be high compared to other countries in Asia. It is considered one of the main reasons for its poor socioeconomic development (Pernia 2007). While its government has adopted an objective of reducing family size in the early 1970s, persistent opposition from the country's Catholic church hierarchy on the promotion and use of modern methods of contraception has brought about wavering policy thrusts[16] (Herrin 2002) and a weak institutional environment for the family planning program that is heavily reliant on international agencies for funding and support.

In 2004, USAID started phasing out its contraceptive donations to the Philippines, which accounted for 80% of the country's requirements in 1990-2001 (USAID 2003), as part of a broader effort to encourage country ownership of family planning programs. Instead of compensating for the shortfall in supply, as the government initially set out to do in the contraceptive self-reliance strategy that it devised, the president at the time, a devout Catholic, banned the purchase of contraceptives by the national government[17] and directed local governments to take responsibility for it. However, because of limited resources and concerted opposition coming from the country's Catholic cardinals, bishops, and parish priests, most local governments did not carry this out.[18]

Because more than two-thirds of modern contraceptive users obtained their supply for free from public health facilities, the phase out had the potential to significantly affect childbearing risk through a drop in contraceptive usage or a shift towards the use of less reliable methods (e.g. calendar/rhythm and withdrawal), especially for the poor, the young,

[14]This is the cumulative impact estimated after four years, taking into account a normal gestation period of three quarters and a (subsequent) distribution pipeline of contraceptive supplies to lower-level administrative units and village health facilities of up to 13 quarters.

[15]This is the proportion of women of childbearing age (15-44 years old) that had provisions for a quarterly supply of contraceptives. There was substantial cross-sectional and time-series variation in this variable: the average was 6% on a range of 0-30%.

[16]To highlight the influence of religious groups on government policy, two previous administrations focused resources on the promotion of traditional methods of family planning, i.e. variants of the rhythm method, the only form of contraception the Catholic church allows.

[17]This was viewed by many as a political accommodation to the domestic Catholic church during an election year. The Catholic Bishops Conference of the Philippines has issued in the past pastoral statements, read during masses in all parishes, condemning the government's promotion of modern methods of family planning and discouraging the faithful from voting for candidates whose beliefs do not align with the church's view.

[18]This occurred despite the fact that public opinion surveys have consistently shown that an overwhelming majority of the adult population (upwards of 70% nationally and across broad regions and socioeconomic classes) think that the government should provide budgetary support for modern methods of family planning (Pernia, 2007).

and the less educated who are least capable of paying for modern family planning methods. Evidence from the most recent nationally-representative family planning surveys in 2005, 2006, and 2008 indicate that the usage of pills dropped while the usage of condom and withdrawal has gone up (see Figure 1). Likewise, these surveys provide an indication of the bite of the phase out given drops in the share of modern contraceptive users obtaining their supply of pills, injectables, and condoms from public health facilities[19] (see Figure 2), leading to a sharp decline in total public sector reliance for contraceptive supply from 9% to just 5.5% of women age 15-49 in a span of just two years (see Figure 3).

This paper looks into the demographic consequences of the policy reversal in terms of an increase in area birth rates and a woman's likelihood of conceiving (which may lead to a live birth or be terminated for some reason). To my knowledge, a thorough and systematic evaluation of the impact of such a policy reversal has not yet been done and it will provide crucial input to a longstanding debate in the Philippines on the importance of providing supply inputs in promoting family planning. The results will also be instructive to other developing countries that are facing or will face challenges in securing the availability and adequacy of their contraceptive supply.

3 Related literature

[need to cut down to size; mention that review focuses on experimental and quasi- experimental research designs and just point to references that use structural estimation]

Family planning (FP) programs have been a key component of economic development plans since the 1960s with the belief that access to and availability of modern contraceptives will allow couples to reduce their fertility, which in turn may improve their socio-economic outcomes. Finding empirical support for the causal impact of the supply-side components of FP programs on fertility have been difficult because the establishment, management, and utilization of FP programs are also likely to be related to the demand-side characteristics of the communities where they are placed. In particular, there might be endogeneity problems in program placement if governments[20] deliberately target particular areas for FP programs and contraceptive supplies. The direction of the bias introduced by this program placement is not clear since governments might have various motivations for its actions; e.g. it may try to place programs where fertility is high, so that the estimated impact might be weaker if high-fertility areas are relatively slower to adopt modern contraceptives (have low "latent demand"), or it may place programs where it is convenient to do so since other resources are already present, in which case the estimated impact might be stronger because "latent demand" is likely to be higher in more developed areas.

The fundamental problem of causal inference here is that we only observe the actual outcome in a particular area given the assignment (non-assignment) of the FP program and not the counterfactual for the same area had the FP program not been assigned (assigned). Under the potential outcomes framework, we rely on mean outcome comparisons between the

[19] There was no drop in the share for IUD users because this contraceptive was exempted from the phase out.

[20] In other settings, non-government organizations (NGOs) and/or other private sector entities introduce/manage FP programs.

two groups to estimate the average treatment effect, and it is necessary that the treatment assignment be not related to area characteristics for the selection bias to be zero. The "ideal" experiment would thus involve random placement of the FP program in a subset of communities.

This experimental approach was implemented in Matlab, an isolated rural region in Bangladesh that is frequently flooded and has been persistently poor, where 70 out of 142 villages, purportedly selected at random, were provided with intensive FP services in 1978. Sinha (2005) uses the 1996 Matlab Health and Socioeconomic Survey (MHSS) of 4,124 ever-married women to look at the fertility impact of the FP intervention. She checks that the treatment and control areas were largely similar in the characteristics of the pre-program cohort of women (40 or older in 1978), and then proceeds to show that the number of children ever born by the post-program cohort of women (39 or younger in 1978) was 9% lower for those in the treatment area compared to the control. In a regression context that includes other covariates controlling for woman's age and education (which is important given that cumulative fertility is age-dependent), the computed fertility reduction attributed to treatment residence was higher at 14%.

While running such an experiment provides strong evidence for the importance of supply-side FP inputs, there are several concerns with this project's implementation and external validity. The main concern is whether randomization was truly achieved. Even just looking at the map of treatment and control areas, there is an indication of clustering, which was possibly intended for logistical convenience and to temper spillover effects. Another concern is the introduction of maternal and child health services that were integrated to the FP program in subsequent years, so that the program effect cannot be attributed to the FP program alone. Lastly, the amount of resources poured into this program has been considered too high to be sustainable and replicable in other settings.

However, even though there are doubts about whether random assignment was fully accomplished, most analysis show that the treatment and control areas were indeed similar in most respects, and that some of the differences actually go in favor of finding no program effect or an underestimate (pre-program fertility rates in the treatment area were higher). And because Matlab is an isolated region that is relatively backward, it is likely that there were no substantial demand-side changes that affected the demand for children that could confound the supply-side intervention.

Miller (2009) utilizes what he argues as the haphazard geographic spread since 1965 of branches of Profamilia, the pioneering private provider of FP services in Colombia. It was the main provider of FP services in Colombia for a long time because conservative elements (i.e. Catholic church) restricted the government's involvement in FP. Using the sample of women born only in treatment counties and aged 10-54 in 1965 from the 1993 population census, he links the location and timing of the opening of the branches to the exposure of women in different five-year age cohorts to the treatment. He finds that conditional on ever having a child, the availability of the FP program when women were younger than 35 is associated with about 4% fewer births compared to peers who were not yet exposed to it at the time (estimation included birth county and cohort fixed effects), and the youngest cohort (15-19) exposed also had more education and was more likely to have formal sector employment.

The main concern in this paper is whether the geographic and temporal introduction of the county-level FP programs of Profamilia was indeed as good as random. Miller cites the Profamilia administrator during that time who said that the placement of FP programs was largely arbitrary, and shows that there was no discernible pattern of consecutive county branch openings spatially (e.g. "domino" pattern) using a map. He also shows validity tests of program placement not being correlated with pre-existing age- and county-specific trends in fertility or socio-economic status (i.e. outcome measures dated before 1965), among other tests. However, because he exclusively looks at treated counties, he is identifying the program effect mostly from temporal differences in program introduction, of which there was few annual variation. Couple this with five-year age cohorts, the "treatment" doesn't seem to be that finely measured, so that other coincident events may provide a completing explanation, such as other government programs (e.g. school construction) that happened in the same counties in roughly the same time (+/-2 years) as the FP branch openings.

Bailey (Forthcoming) conducts a generalized event-study analysis of the rollout of county-level federally-funded FP programs in the U.S. from 1964 to 1973 and its impact on general fertility rates (births per 1,000 women of reproductive age). Because the establishment of the FP programs were disorganized based on oral history of the grant application and approval process, the introduction of FP programs were considered as good as random, and validity tests largely show that other similar government programs were not coincident with it. Estimated impacts on birth rates were a 1.4%-2.7% reduction six to ten years after the program began.

Turning to more specific FP program inputs that go beyond program introduction, Gertler and Molyneaux (1994) looked at the falling Indonesian fertility level in the 1980s that were presumably driven by a sustained rise in contraceptive usage and an increase in marrying age ("proximate determinants"). Using the individual birth history of women age 15-49 from the 1987 Contraceptive Prevalence Survey (NICPS), they aggregate it at the subdistrict level into a quarterly panel of birth hazards (proportion who gave birth within the sample block of 30 households over a three-month period) between 1982 and 1987. They relate this (appropriately shifted 9 months behind to correspond with conception) to the following FP program inputs from the BKKBN's (government agency that administers FP program) service statistics system: no. of visits by mobile FP teams, no. of village contraceptive distribution centers, no. of health clinics, and no. of FP fieldworkers, all measured at the subdistrict level per 1,000 population. They proposed to remedy the nonrandom program placement issue by estimating a community fixed effects specification which sweeps away time-invariant community characteristics that may affect the allocation of FP program resources. They find that some of the FP program inputs are significantly related to birth hazards under OLS, but none are under the FE specification (regressions also control for women's age and education distribution, male and female wages, and infrastructure features, all at the subdistrict level, most of which were statistically significant). They interpret this as supporting the view that demand-side factors, such as economic development and improved education and economic opportunities for women, and not supply-side interventions were largely responsible for the fertility decline.

The main critique about this paper is that the BKKBN has been documented to be a very active manager in tuning its programmatic efforts to meet its fertility goals, which means that it is likely to be responsive to emerging supply issues that community fixed effects will fail to capture. In this regard, Molyneaux and Gertler (2000) build up on their previous work on Indonesia and used an institutional feature of the BKKBN to better address time-varying program resource allocation concerns. In particular, because contraceptive budgets are flexible at the district level but fixed at the (higher) provincial level, a district's contraceptive budget can be tweaked depending on the relative contraceptive demand among competing districts within the province. Thus, they instrument each FP program input with the age and education distribution in other districts which are plausibly related to contraceptive demand in the competing districts. They implemented a similar fixed effects regression on a 1985-1994 quarterly panel of districts, with data now coming from the 1991 and 1994 Demographic and Health Surveys, and FP program inputs used were no. of village contraceptive distribution centers, no. of clinics, and amount of subsidized contraceptive expenditure per eligible couple (this last one was not available for the previous study). They found that while the signs for all three FP inputs were negative in the IV-FE specification (signs mixed under OLS and FE), it was not statistically significant, although if any of the three variables is dropped the remaining two become significantly negative (so that there is substantial collinearity among the three variables and it is hard to figure out their relative importance). Even so, the magnitudes involved are small and again swamped by the effect of the demand-side factors.

An issue with the IV strategy is that while the first-stage regression had strong instruments, it is not conceivable that age and education distributions would change drastically for any quarter, so that the kind of programmatic tuning that responds to unexpected supply stockouts or surpluses in some district but not in others within the same province could still be uncontrolled for. Moreover, it is possible that both papers might be plagued by confounding simultaneity issues since significantly large demand-side changes that may affect the demand for children (booming economy; reaping fruits of substantial school expansion in the previous decade) were happening side-by-side with the supply-side improvements, and thus it would be hard to isolate the impact of supply-side FP inputs. Another concern is with the aggregation used (subdistrict- or district-level birth hazard), which might be subject to considerable measurement error since the sampling blocks used were quite small; it would be more convincing if the analysis was supplemented by actual population-level data, such as crude birth rate or general fertility rate based on vital statistics.

While the Matlab experiment provided evidence for a negative causal impact of the FP program on fertility, the challenge remains for determining the relative impact of each FP program input, as was attempted for the papers on Indonesia. The approach adopted in the papers on Indonesia are promising and it is unfortunate that the period analyzed may just be inhospitable to finding any significant program impacts. While I find the papers on the program rollout more convincing because identification is cleaner and more transparent than in the papers on Indonesia, it is difficult to muster unassailable evidence to support the identifying assumption, and it is challenging to limit the scope for competing explanations.

All these studies and many others involved tracing out the effects of the introduction or intensified implementation of the FP program on fertility, but not much has been written about the effects of a discontinuation or weakened implementation of the supply component of the FP program. Because there is scope for compensating behavior and prior learning (substitution into other methods, including permanent methods, and purchase from commercial sources), it is likely that the effects would be asymmetric (be weaker than anticipated). This paper takes advantage of the gradual phase out of USAID's contraceptive donations to the Philippines from 2004 to 2008 as a natural experiment that exogenously disrupted publicly-provided contraceptive supply.

4 Estimation and identification strategy

This paper's analysis will rely on linking the timing of changes in different fertility outcomes across the different provinces, including outcomes for the women living in them, to the timing of changes in publicly-provided contraceptive supplies distributed to the same. In particular, for the aggregate-level analysis, a provincial-level quarterly panel data was assembled to estimate equation 1:

$$GFR_{j,t+3} = \alpha + \sum_{s=t}^{t-12} \beta_s CSR_{js} + X'_{jt}\theta + \delta_j + \delta_t + \epsilon_{jt}, \tag{1}$$

where j indexes provinces and t indexes each quarter. The dependent variable is the general fertility rate (GFR) three quarters forward, which is the the number of births within a quarter per 1,000 women of childbearing age (I use 15-44 years old). The independent variable of interest is the contraceptive supply rate (CSR), which I define as the proportion of women of childbearing age that had provisions for a quarterly supply of contraceptives, where quantities of different contraceptive commodities (pills, injectables, IUDs, and condoms) distributed to the different provinces were converted into a common unit using its couple years of protection (CYP)-equivalent.[21] Note that while the data I have captures the distribution of the contraceptive supply from the central office to the different provinces, actual information on the subsequent distribution pipeline to lower-level administrative units up to the village health facilities on the frontline is not available. To account for varying distribution speeds further downstream, I include lags in the contraceptive supply rate of up to three years in order to be conservative.[22]

I include time-varying provincial-level controls such as women's age and education distribution, poverty and subsistence rates, and local government spending on population, health, and nutrition programs (lumped together). I rely on time fixed effects to sweep out broad-based secular trends in the GFR, and rely on province fixed effects to soak up time-invariant province characteristics that might be related to the contraceptive supply the province receives. For my purposes, this includes the fixed allocation scheme that was based on the 2000 poverty incidence in each province. This rule was in effect during the phase out period and was not revised with the release of new data on poverty incidence.

For the micro-level analysis, I use a similar strategy augmented by woman fixed effects

[21] The treatment for IUDs was different since it provides protection for a period longer than a quarter, so I constructed a moving-average of the proportion of women who had provisions for an IUD over the duration of its typical use (3.5 years).

[22] This is not too long a period; indeed, while the phase out started in the second half of 2004, we can see in Figure 2 that a dip in the share of public facilities as source of pills only happened in April 2006 (the survey was undertaken during this time). Since data on provincial-level contraceptive distribution is only available starting 2000, this choice of lag length will push the start of the estimation period to the first quarter of 2003.

on a quarterly pregnancy history panel of women 15-44 years old from the 2008 DHS:

$$Conceive_{ijt} = \alpha + \sum_{s=t}^{t-12} \beta_s CSR_{js} + X'_{jt}\theta + W'_{it}\lambda + \delta_i + \delta_j + \delta_t + \epsilon_{ijt}, \quad (2)$$

where i indexes individuals, j refers to i's residence in the area served by the local government above, and t continues to index each quarter. The outcome of interest I look at is the probability of conceiving in a given quarter conditional on being fertile (since this probability is trivial otherwise), which I define as not currently being pregnant or concluding pregnancy. Since I have information on when a woman was ligated, I also exclude periods after this procedure was done since it becomes impossible for a woman to conceive afterwards.[23] Future work will look at the probability of having induced abortions.[24] Similar to the specification above, this woman-level analysis includes time and province fixed effects and lags in the CSR of up to three years.

The main advantage of this specification is that it includes woman fixed effects, so that time-invariant unobservables at the individual level that affect fertility and other life choices are accounted for. I also control for time-varying woman-specific characteristics that predict fecundity such as dummies for single-year age and parity and an indicator that turns on when a woman has her first marriage or union. The main limitation, however, with the dataset used is that residence data in the DHS is limited to current residence at the time of the interview and the date when a woman started residing there, so that it is only possible to estimate a restricted version of equation 2:

$$Conceive_{i\bar{j}t,k} = \alpha + \sum_{k=0}^{1} \left(\sum_{s=t}^{t-12} \beta_{s,k} CSR_{\bar{j}s,k} + X'_{\bar{j}t,k}\theta + \delta_{\bar{j},k} \right) + W'_{it}\lambda + \delta_i + \delta_t + \epsilon_{i\bar{j}t,k}, \quad (3)$$

where $\bar{j}$ refers to the latest residence of woman i (it is the same for each t), and k is an indicator that turns on when a woman started residing in $\bar{j}$. In this context, only the estimates of $\beta_{s,1}$ are meaningful in interpreting the effects on fertility of changing contraceptive supplies from the public sector. However, because of concerns that migration may bias our estimates, I also estimate a simplified form of equation 3 on the subsample of women who settled on their latest residence before 2003, which I label as "stayers":

$$Conceive_{i\bar{j}t,1} = \alpha + \sum_{s=t}^{t-12} \beta_{s,1} CSR_{\bar{j}s,1} + X'_{\bar{j}t,1}\theta + W'_{it}\lambda + \delta_i + \delta_t + \epsilon_{i\bar{j}t,1}, \quad (4)$$

where $\delta_{\bar{j},1}$ is subsumed under δ_i since it is fixed for each woman.

[Will work in a regional analysis (probability of conception collapsed by region to create regional conception rates) in the micro part using the subsample of stayers to tie the results better with the macro part (a regional version of equation 1).]

All the standard errors were clustered at the provincial level since the identifying variation is also at the same level.[25] In all the analyses, observations were weighted by the number

[23] Results are very similar even when women continue to be included in the sample after they are ligated.

[24] The DHS contains information on pregnancy terminations but it does not distinguish between induced abortion, miscarriage, and still birth. Bendavid et al. (2011) employ an algorithm to infer which ones are likely to be induced abortions– I plan on extending this work to look at this outcome too.

[25] For equation 3, the clustering of standard errors is adjusted so that only standard errors from *actual* residents are pooled together.

of women of reproductive age for each province in 2007 for the aggregate-level analysis, and the survey sample weights for each woman in the micro-level analysis.

5 Data and descriptive statistics

Before moving on to the estimation results, we provide a discussion of the case for why the change in the public sector contraceptive supply could be argued as plausibly exogenous. We also take a look at summary statistics of the data and visually inspect the trends for our variables of interest before and during the phase out.

5.1 Public sector contraceptive supply

While the total quantity of donated contraceptives [26] from USAID was predetermined with the intent of having a gradual phase out from 2003 to 2007, it seems that this was pushed back to 2004 to 2008 because the Department of Health only came up with the implementing guidelines for the distribution of the diminishing supply in mid-2004. This timing gap created some lumping in the subsequent distribution to local governments: in 2003 before the phase out details were threshed out, and in 2005 just after things were cleared up.

All throughout, the receipt of donated contraceptives at the central office was sometimes delayed (shipment issues), and there is also the possibility of inventory mismanagement/miscues since quarterly distributions to the local governments were erratic/intermittent (see Figure 8) even before the phase out. In fact, the planned gradual phase out was not actually implemented (see Figure 4 which shows the story for pills; while not shown, a similar story could be said for injectables).[27]

While all this was operating at the aggregate/topmost level (so identifying variation is temporal), there is also the issue of how contraceptives were allocated among the local governments. To accommodate varying local conditions, the national government tried to implement a staggered allocation scheme for the distribution of the declining public sector contraceptive supply. The phase out plan classified local governments as poor, middle income, or rich based on their poverty incidence rate in 2000, and allocations (relative to average quarterly consumption in 2003) were tailored such that full phase out would happen sooner for richer local governments and later for poorer ones. In general, this progressive setup was complied with in terms of relatively higher allocations for poorer local governments each period, but the temporal dimension was not followed (see Figure 4), so that much of the variation was unanticipated.

Note that these general trends mask individual local government variation such that within each local government grouping, there is substantial variation in actual distribution: some local governments received more or less (even none) than the average for the group, and this also varies in each period (so identifying variation is both temporal and geographic). Sometimes this was due to lot indivisibilities (supplies were in certain lot sizes, so round-off

[26] It was always in commodity form, not fund donations for purchase of contraceptives.

[27] Although IUDs were not subject to phase out due to the absence of a private market for it, it still saw a reduction in donations; condoms were fully phased out in 2004 because of the presence of a large private market for it.

adjustments had to be made and); other times this could be due to non-random targeting. I try to address this targeting issue by employing province fixed effects that take into account all time-invariant local government-specific observables and unobservables that determine fertility, which in this case includes the imposed grouping (it was not revised with the release of new poverty incidence statistics), and the capability of local governments to influence the allocation scheme (at least the part that was fixed all throughout the study period).

To facilitate the analysis, I utilize a standardized measure of distributed contraceptive supply: I take the quarterly supply numbers and convert it to couple years of protection (CYP) values. The following quantities were assumed to be the required contraceptive supply for a couple for a year using each particular method (so divided by 4 to arrive at a quarterly requirement): 15 cycles for pills, 4 vials for injectables, and 120 pieces for condoms. Since IUDs typically provide 3.5 years of protection, I separately estimate a 14-quarter moving average of the no. of distributed IUD units. I add up the quarterly values across all four items and then divide it by the number of women of reproductive age (15 to 44 years old) in the population to obtain the contraceptive supply rate (CSR).

Figure 5 shows the changes in the components of the average contraceptive supply rate over time (annually), while Figure 6 shows the rich quarterly variation in the average contraceptive supply rate from 2000 to 2008 that we will take advantage of.[28] We can see in both that there was a substantial reduction in the contraceptive supply rate starting in 2004. In Table 1, we see that over the whole period, the contraceptive supply rate was 6.3 percent, but if we divide the period into before and during the phase out implementation, it was 9.6 percent in 2000-2003 and only 3.6 percent in 2004-2008.

[think of highlighting two representative/interesting provinces side-by-side]

5.2 Outcome variables

The outcome variable for the aggregate-level analysis is the general fertility rate, which is the total number of births each quarter per 1,000 women of reproductive age (15-44 years old). In the vital statistics data that we use, place of birth refers to the mother's reported usual residence.[29]

Figure 7 shows the variation in the GFR across the different provinces from 2000 to 2008 (and highlights the importance of including province fixed effects in the estimation), while Figure 8 shows the quarterly movement in the average general fertility rate. Note that while the general fertility rate has generally been showing a pattern of decline (with quarterly cycles), an upward trend has set in starting in the latter part of 2006.[30]

For the micro-level analysis, the outcome variable is a woman's probability of conceiving conditional on being fertile. We see in the first panel of Figure 9 the mean values for this

[28] 114 local government units, composed of all provinces and chartered cities, receive contraceptive supplies directly from the national government. Because my unit of analysis is at the provincial level, I integrate the data for chartered cities into the province where they are situated. This approach has the advantage of mitigating concerns about spillovers; i.e. women in the provinces living near the city may be getting their supply from the public health facilities located in the city.

[29] One advantage of using vital statistics data over the census is that selective mortality is less of a concern since births are captured in a flow measure. Since the census measures the population stock at a given time, no information can be gleaned on children who died after birth and before the census date.

[30] Since the data is available only until 2008, it is not clear whether this upward trend will continue on.

variable coming from the pregnancy history of the sample of women interviewed in the 2008 DHS. We find a similar pattern of a downward trend in this probability from 2000, for both the full sample of women and those who were residing in their current residence since 2003[31], and a stalling, even a bit of an uptick, sometime near the start of 2006. It is reassuring that this is roughly three quarters ahead of the upturn seen in births from the general fertility rate. To further convey the consistency with vital statistics data, we also plot the average conception rates from the sample women (so that we ignore information on whether a woman is fertile or not) in the second panel of Figure 9. The patterns look similar nothwithstanding a change in scale due to an adjustment in the denominator.

From Table 1, we see that over the whole period, there were 22 births per 1,000 women on average for each quarter, while the probability of conceiving (conditional on being fertile) over The general fertility rate was 4 percent in the full sample and a little lower at 3.6 percent in the stayers subsample.

The estimation results below will help us systematically tease out specific cross-sectional variation and trends over time in fertility behavior from changes that are plausibly related to a decline in publicly-provided contraceptive supply.

6 Results and discussion

In this section, we look at the effects on short-term fertility of changes in contraceptive supply from the public sector. We start with an aggregate-level analysis that looks at the impact on the general fertility rate, and then move on to a micro-level analysis that looks at any given woman to see if she is more likely to conceive at a time when the province she resides in has received little contraceptive supplies from the central office (with lag from varying distribution speeds accounted for).

6.1 Aggregate-level analysis

We run several regressions for the aggregate-level analysis in order to check if the coefficient estimates are stable given different sets of regressors. The first column of Table 2 shows the estimates from a basic specification that only includes province and time fixed effects, while the second column adds in province by calendar quarter fixed effects (Buckles and Hungerman show that in the United States, there are differential patterns in mother characteristics based on the quarter when they gave birth). The third column of Table 2 adds in some time-varying controls at the provincial level such as poverty and subsistence incidence, local government spending on population, nutrition, and health (lumped together in the data), and local government revenues (as a proxy for local economic activity), while the last column includes leads in the contraceptive supply rate of up to 3 quarters. This If the public sector contraceptive supply rate was indeed plausibly exogenous, these added variables shouldn't alter the results much.

As expected, the sign on most of the coefficients was negative, and it was statistically significant at 1% on the second to the sixth quarter lags. This was consistent across all the

[31] In the actual estimation, we only utilize information on the outcome variables starting from 2003 because data on contraceptive supply is only available starting in 2000 and we have adopted a 3-year lag structure.

different specifications. No leads were statistically significant in the last column, so that preexisting trends were nonexistent.

These estimates show that it takes from two to six quarters (sometimes even the tenth quarter) for a change in the contraceptive rate to have a significant impact on fertility. Because of the 3-quarter gestation period, any impact on birth rates would likely be seen only after 5-9 quarters. Since the phase out started in 2004, birth rate increases responding to the phase out were only seen sometime in late 2005 and in early 2006. Looking back at Figure 8, we do find such an increase in the GFR around that period.

Figure 10 shows the cumulative impact over 12 quarters of a decline in the contraceptive supply rate, scaled to correspond to a 6-percentage point dip, the average change in the CSR between 2000-2003 and 2004-2008. After 12 quarters, the mean estimate is that such a disruption resulted in an additional 0.9 birth per 1,000 women of reproductive age, for an effect size of 4%. Note that this short-run impact is significantly different from zero from quarter 2 onwards and continues to increase until the 12th quarter.

6.2 Micro-level analysis

Turning to the within-woman estimation of the short-run fertility effects of the contraceptive supply disruption, we run a regression that is similar to the specification in the third column of Table 2. As a robustness check, we do this for the full sample of women, considering only the period since they moved to their latest residence, and also restrict the sample to those who have lived in their latest residence throughout the period that we study. We again see in Table 3 the expected sign for the coefficients, which are mostly negative for the different lags. However, the estimates are only statistically significant for the previous third quarter, so this might lead one to think that a rather different picture emerges compared to the aggregate-level analysis.

However, if we plot out the cumulative impact on the probability of conceiving per quarter, we do find that the mean estimate also has a similar increasing pattern up to the 12th quarter, and that while it is not as precise as to rule out no impact, the confidence band suggests that negative impacts are still more likely to occur. The final mean cumulative impact estimate is about a 0.005 increase in the probability of conceiving, which translates to an effect size of around 12% on an average probability that is nearly 4%.

We explore the possibility that a negative impact is masked by substantial subgroup heterogeneity. We interact the contraceptive supply rate with indicators for various characteristics of a woman at the time of the survey interview: her poverty status (defined as belonging or not belonging to the lowest wealth quintile using the DHS' asset index), educational attainment (has or has not attended high school, which should be age-accessible even for the youngest women in the sample since high school education starts at age 13), or residence type (living in an area classified as either rural or urban).

The results for these subgroup analyses are presented in Tables 4 to 6. We now find that many estimates are negative and statistically significant for various lags, but only for the subgroups with a disadvantaged background (poor, less educated, rural). The mean cumulative impact is also much higher for these disadvantaged subgroups, with effect sizes around 22% for rural residents, 60% for the less educated, and 90% for the poor. Note that while zero cumulative impacts could still not be ruled out for rural residents in Figure 12, it is comfortably rejected for the less educated and the poor.

[need to formally test if there is any difference between the two subgroups]

7 Concluding remarks

In this paper, I find support for the causal negative impact of taking funding away from the supply component of family planning programs. The phase out of USAID's contraceptive donations to the Philippines was a plausibly exogenous event that severely disrupted the country's public sector contraceptive supply. I find that it led to an increase in birth rates with a two to three year lag, and part of this increase in births could be attributed to an increase in the pregnancy risk experienced by disadvantaged groups like rural residents, the poor, and the less educated. Future work should delve deeper into the reasons for incomplete compensating behavior exhibited by these groups.

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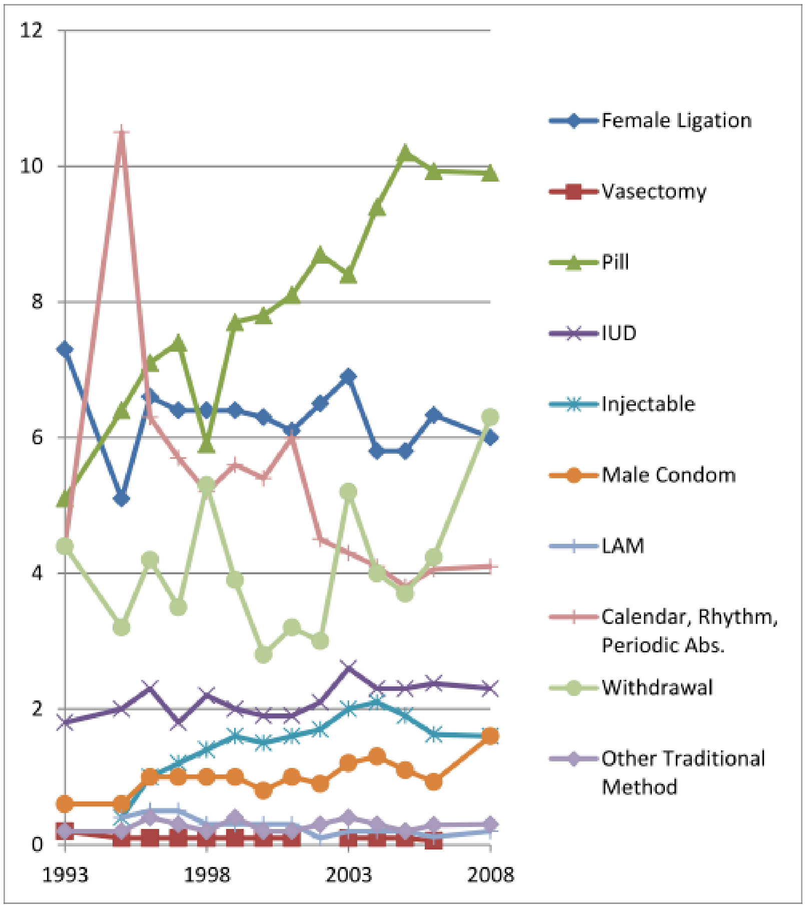
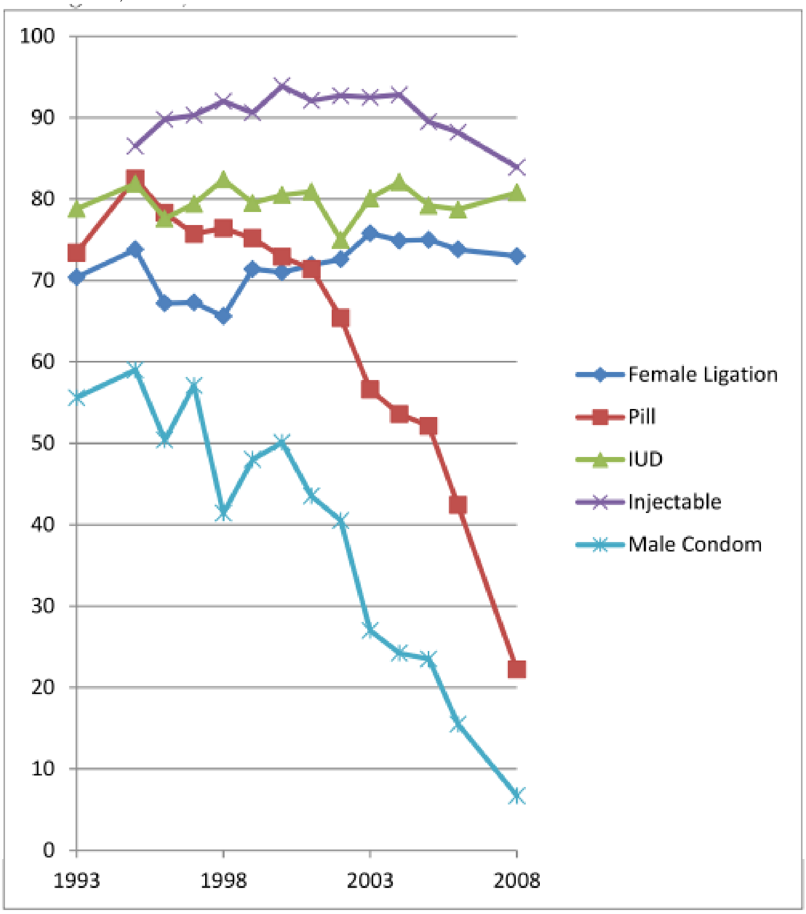
Figure 1: Contraceptive prevalence rate among women age 15-49 ("current" usage at time of interview), in %



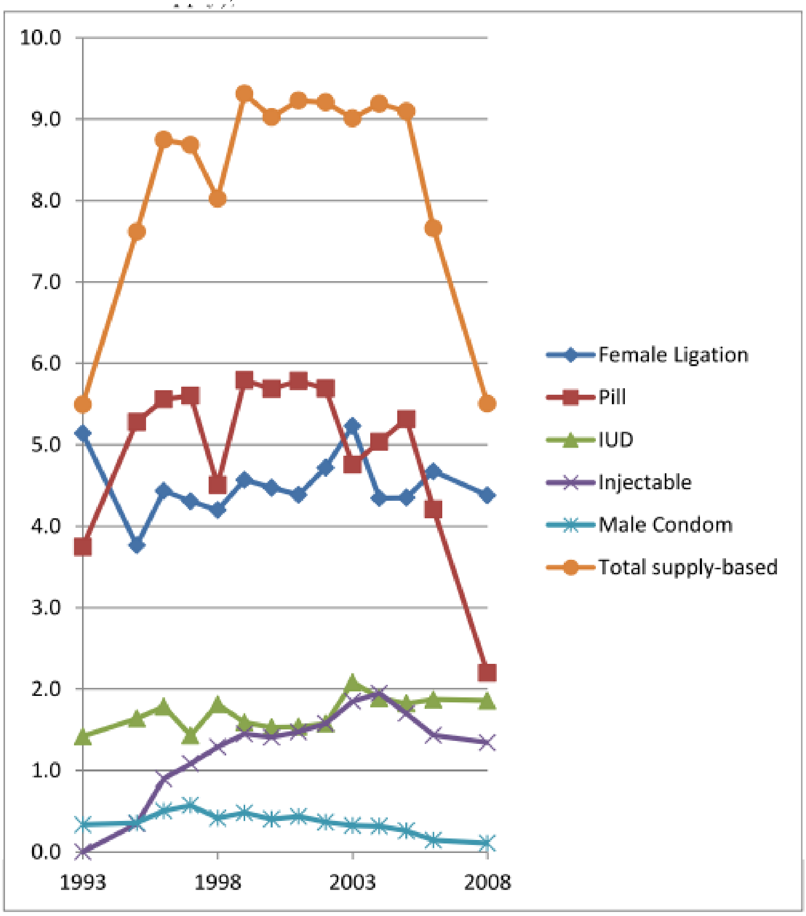
Note: Data from DHS 1993, 1998, 2003, 2008 and Family Planning Survey (FPS) in the intervening years.

Figure 2: Public health facilities as most recent source of supply among users of modern family planning methods, in %



Note: Data from DHS 1993, 1998, 2003, 2008 and Family Planning Survey (FPS) in the intervening years.

Figure 3: Public sector reliance among women age 15-49 ("current" usage at time of interview and most recent source of supply), in %



Note: Data from DHS 1993, 1998, 2003, 2008 and Family Planning Survey (FPS) in the intervening years.

Figure 4: Planned and actual distribution schedule for pills during the phase out period, semestral, indexed to 2003 actual consumption levels

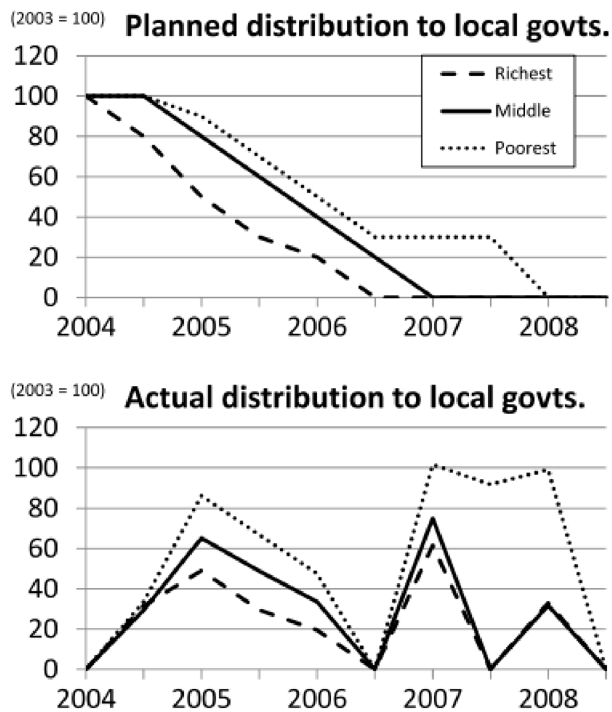


Figure 5: Trends in the contraceptive supply rate before and during the phase out period

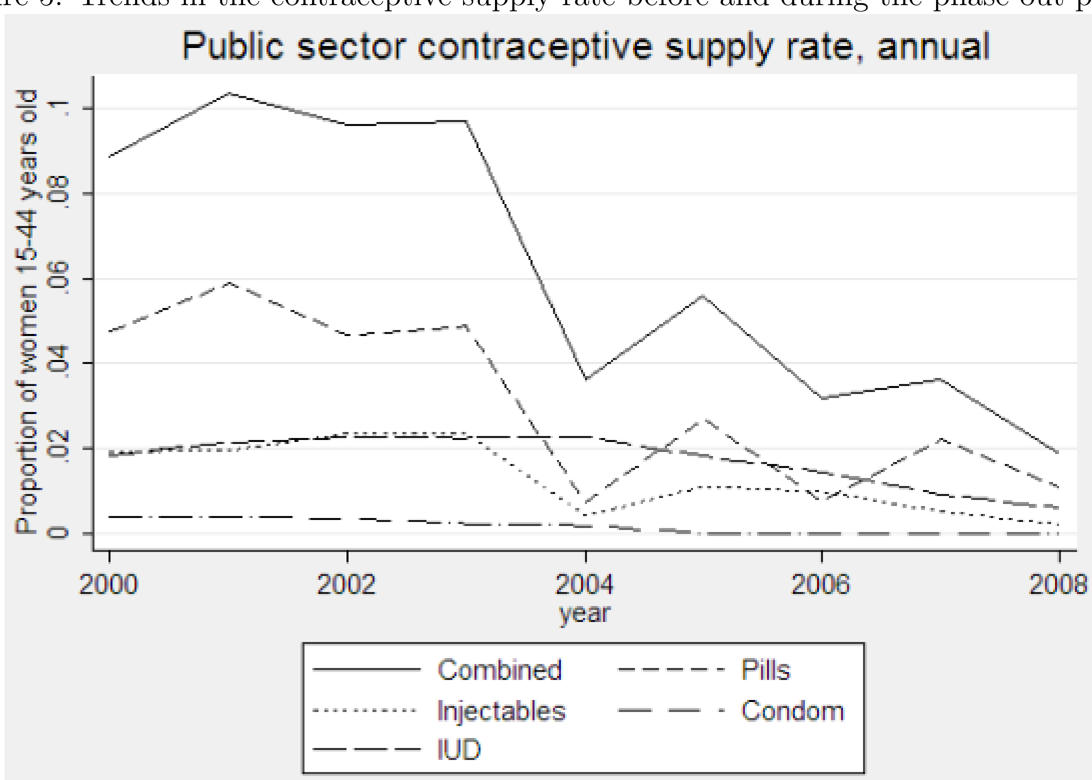
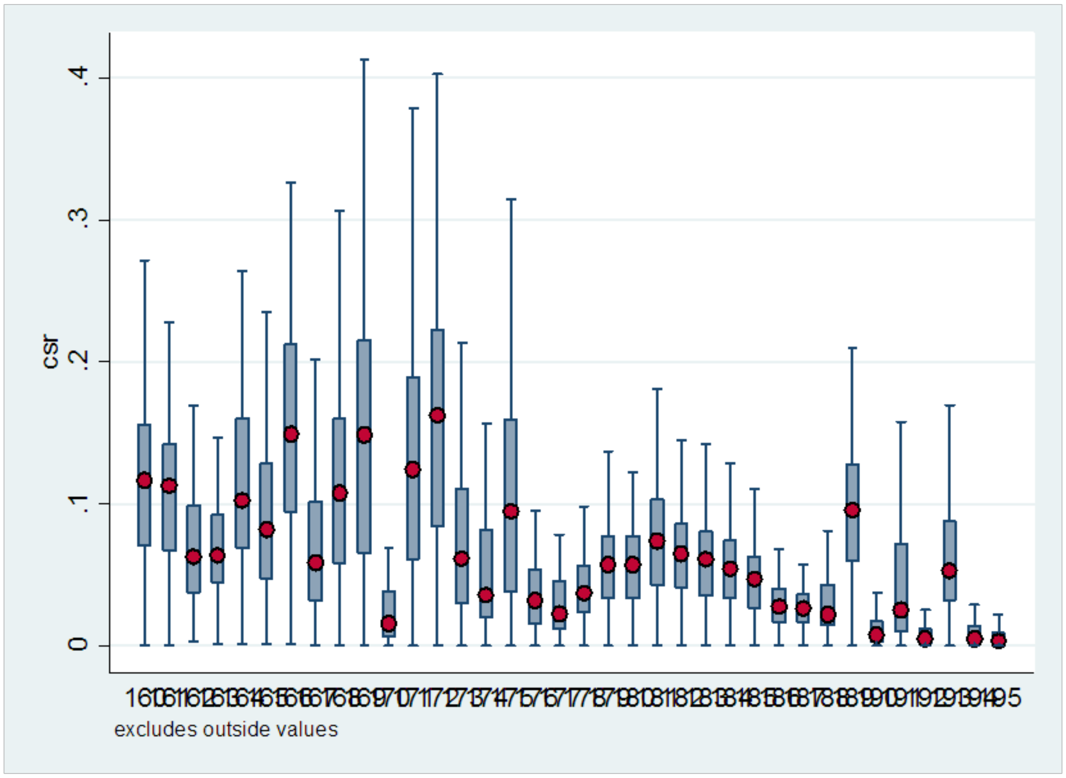
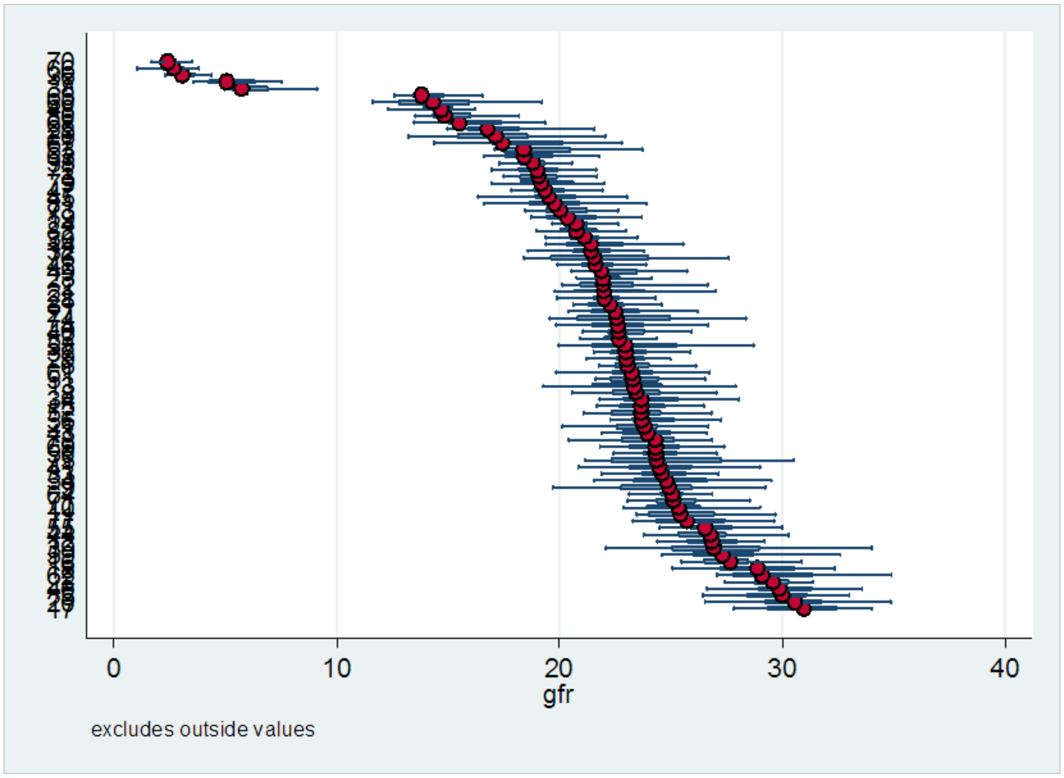


Figure 6: Quarterly variation in the contraceptive supply rate before and during the phase out period



Note: Dots show the mean, while the bars give the interquartile range. The line extensions ("whiskers") show the next adjacent values. Outside values have been omitted.

Figure 7: Geographic variation in the general fertility rate



Note: Dots show the mean, while the bars give the interquartile range. The line extensions ("whiskers") show the next adjacent values. Outside values have been omitted.

Figure 8: Quarterly variation in the contraceptive supply rate and the general fertility rate before and during the phase out period

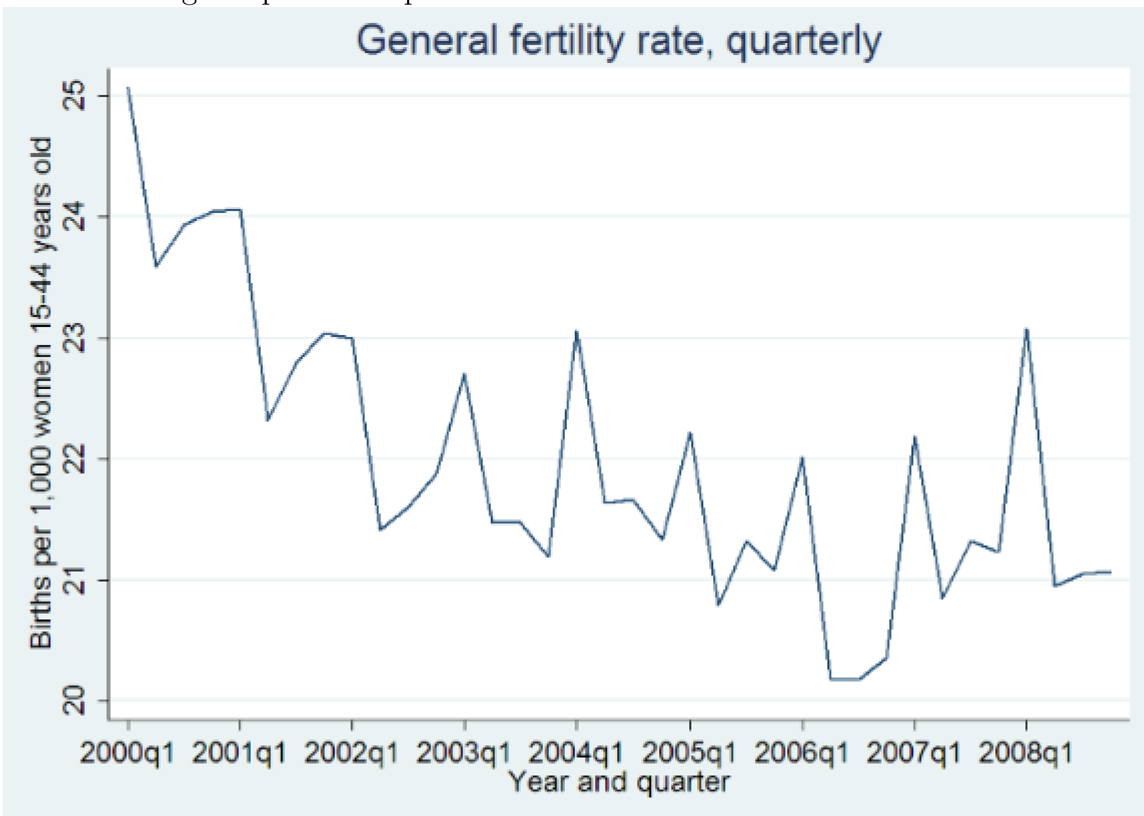


Figure 9: Average conception rates and probability of conceiving (conditional on being fertile) of sample women from the 2008 DHS, 2000q1-2008q2

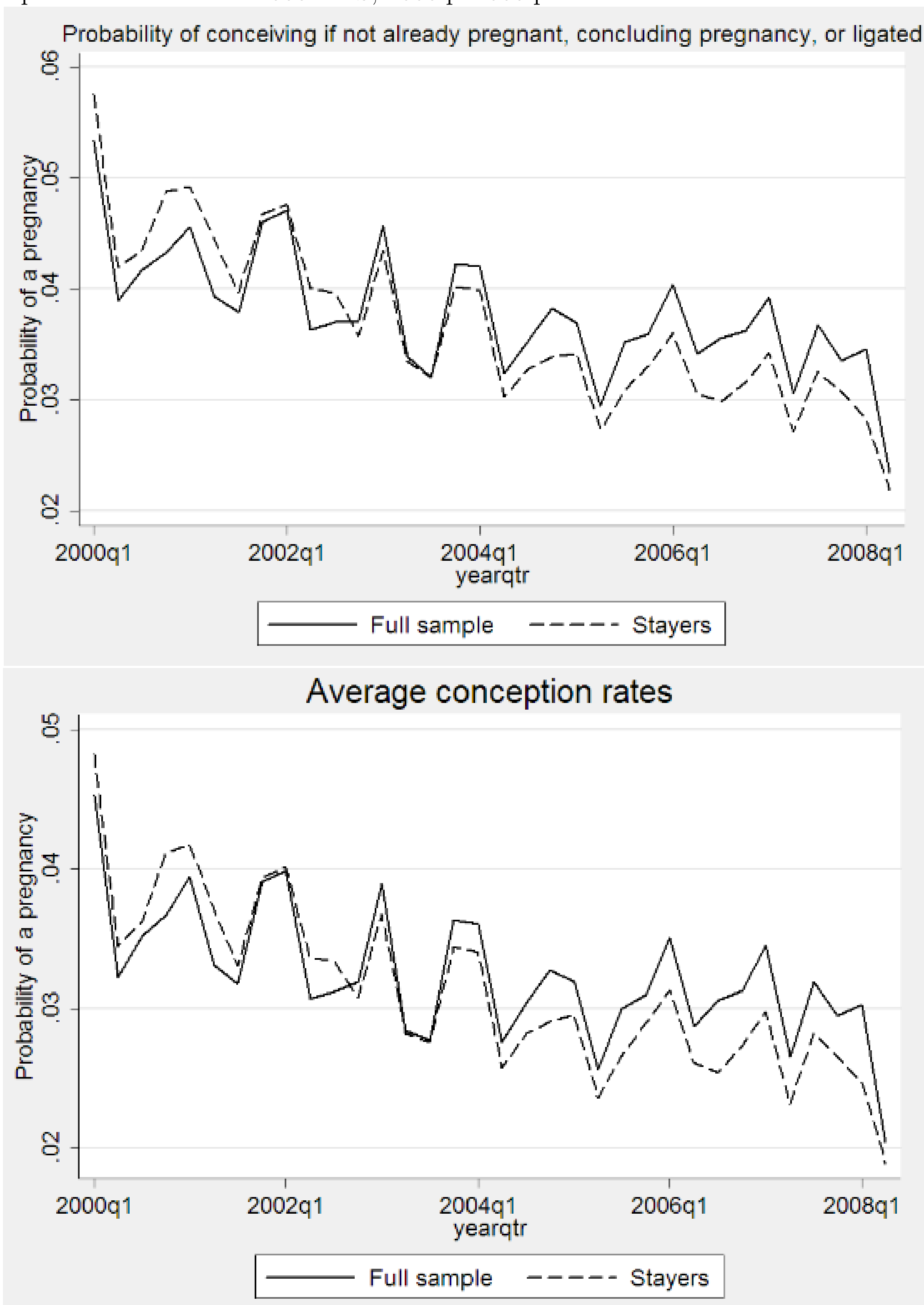


Figure 10: Cumulative impact on the general fertility rate per quarter of a 6-percentage point decline in the contraceptive supply rate, with 95% confidence band

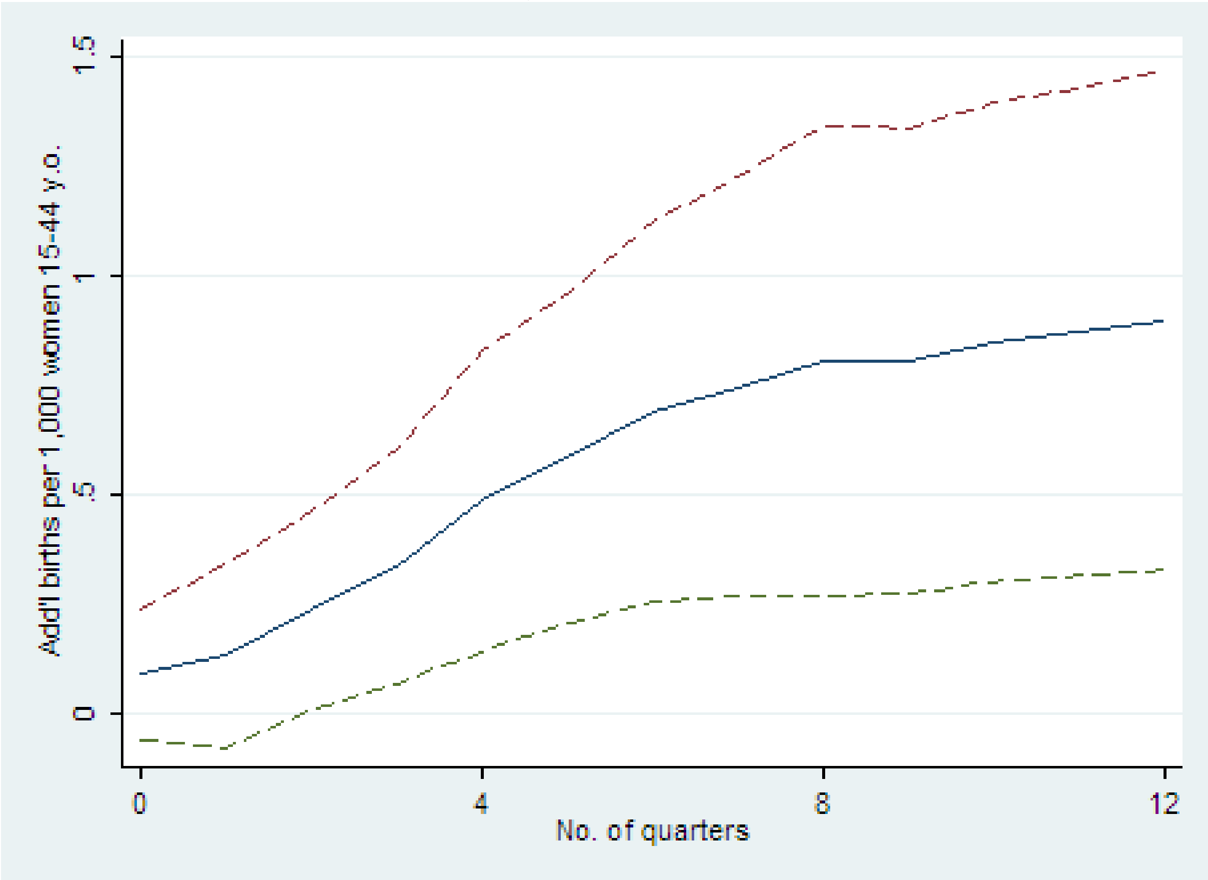
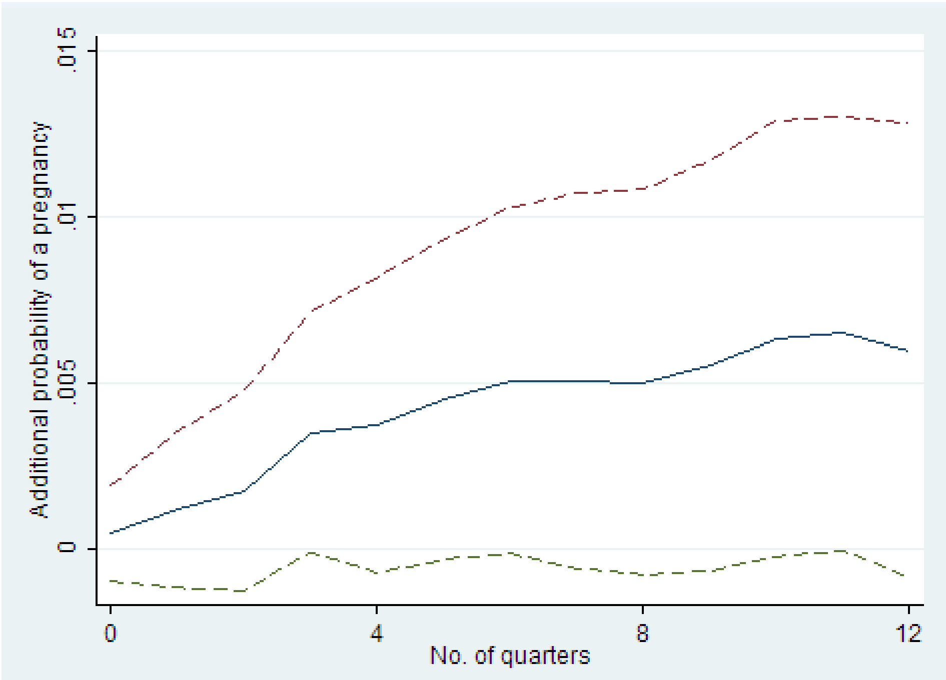


Figure 11: Cumulative impact on the probability of conceiving per quarter of a 6-percentage point decline in the contraceptive supply rate, with 95% confidence band

Full sample



Stayers

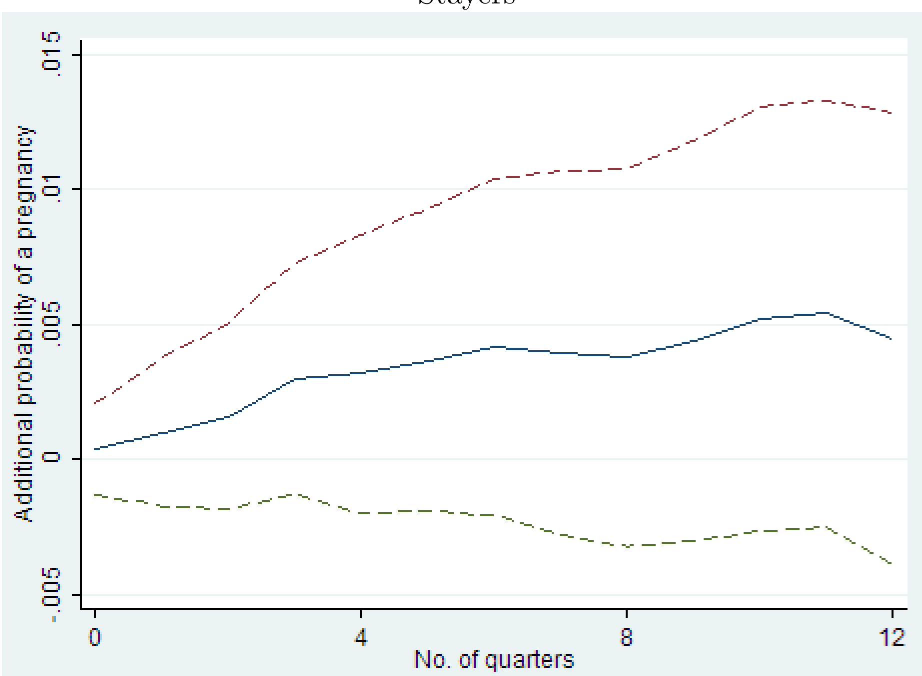


Figure 12: Cumulative impact on the probability of conceiving per quarter of a 6-percentage point decline in the contraceptive supply rate, various subgroups, with 95% confidence band

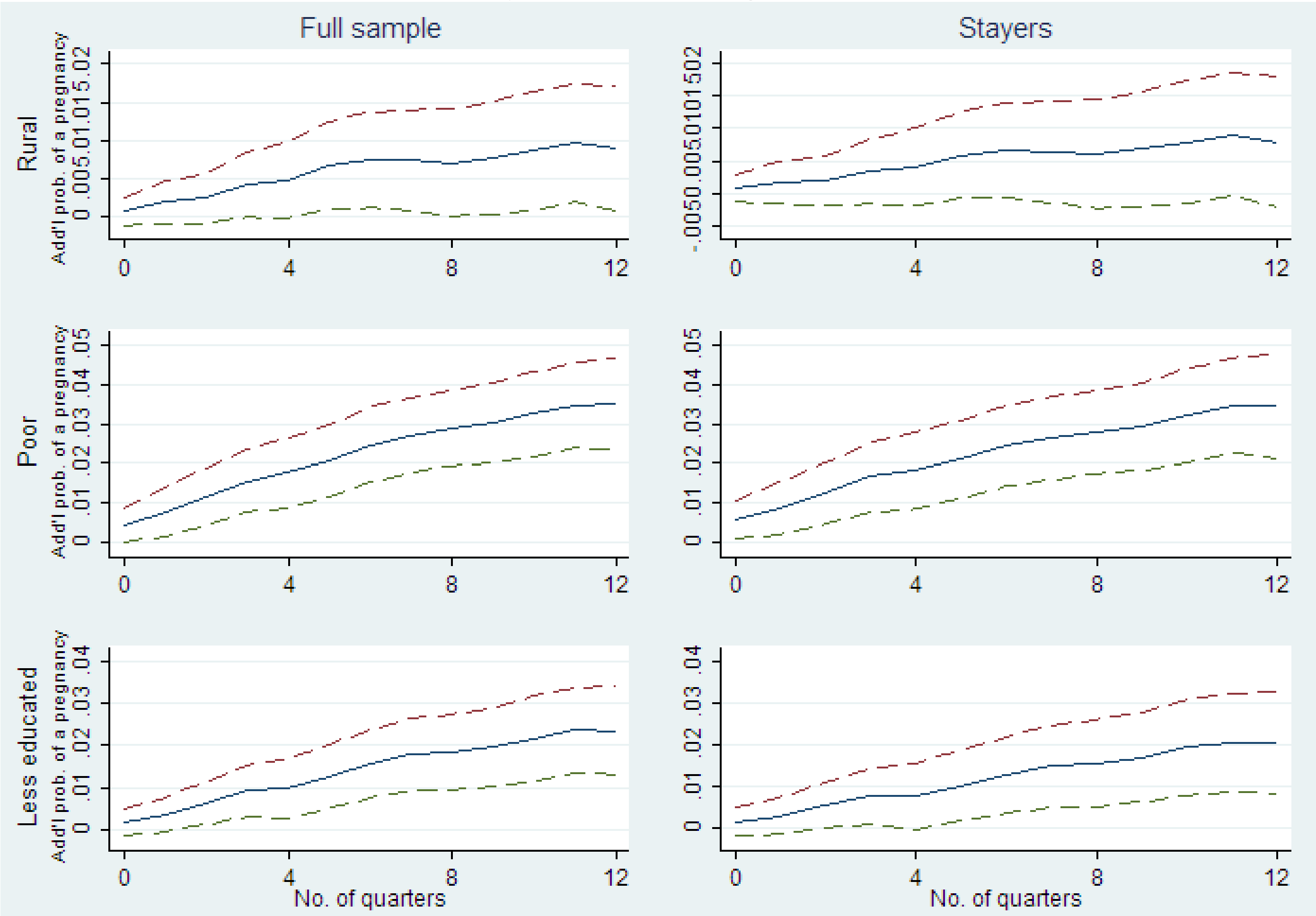


Table 1: Summary statistics, weighted by population of women of reproductive age in 2007

Variable	Period (quarterly)	N	Mean	Std. Dev.	Min	Max
GFR	2000-2008	2952	21.98	5.86	0.47	37.26
CSR	2000-2008	2952	0.0628	0.0674	0	1.3141
CSR	2000-2003	1312	0.0965	0.0811	0	1.3141
CSR	2004-2008	1640	0.0359	0.0361	0	0.4123
Conceive:						
Full sample	2000-2008	178408	0.0397	0.1952	0	1
Stayers	2000-2008	130026	0.0359	0.1860	0	1

Table 2: Public sector contraceptive supply and its impact on general fertility rate per quarter

	(1) F3.gfr	(2) F3.gfr	(3) F3.gfr	(4) F3.gfr
F3.csr				-0.788
				[0.467]
F2.csr				0.931
				[0.417]
F.csr				0.585
				[0.650]
csr	-1.214	-1.768	-1.845	-1.938
	[0.334]	[0.260]	[0.228]	[0.173]
L.csr	-1.299	-0.827	-0.834	-1.051
	[0.114]	[0.332]	[0.320]	[0.214]
L2.csr	-1.492**	-2.062***	-2.068***	-2.092***
	[0.015]	[0.002]	[0.002]	[0.001]
L3.csr	-2.635***	-1.958***	-2.030***	-1.940**
	[0.001]	[0.009]	[0.009]	[0.012]
L4.csr	-2.589***	-2.875***	-2.963***	-2.937***
	[0.003]	[0.007]	[0.006]	[0.006]
L5.csr	-1.993***	-1.880**	-1.944***	-2.052***
	[0.005]	[0.012]	[0.009]	[0.007]
L6.csr	-1.849***	-2.091**	-2.120**	-2.037**
	[0.010]	[0.014]	[0.012]	[0.015]
L7.csr	-1.290*	-1.170	-1.149	-1.137
	[0.064]	[0.126]	[0.125]	[0.140]
L8.csr	-1.087	-1.171	-1.195	-1.233
	[0.108]	[0.126]	[0.111]	[0.126]
L9.csr	-0.0260	0.0366	0.0403	-0.0676
	[0.958]	[0.946]	[0.941]	[0.899]
L10.csr	-0.766*	-0.841*	-0.858*	-0.850*
	[0.068]	[0.067]	[0.060]	[0.071]
L11.csr	-0.586	-0.499	-0.487	-0.440
	[0.135]	[0.269]	[0.272]	[0.327]
L12.csr	-0.378	-0.466	-0.482	-0.489
	[0.306]	[0.344]	[0.335]	[0.320]
Time FE (21)	yes	yes	yes	yes
Province FE (82)	yes	yes	yes	yes
Prov x Qtr FE		yes	yes	yes
Other controls			yes	yes
N	1722	1722	1722	1722
adj. R-sq	0.977	0.977	0.977	0.977

Estimation period: 2003q1-2008q1
SEs clustered at provincial level. p-values in brackets:
* p<0.10, ** p<0.05, *** p<0.01

Note: Other controls include poverty and subsistence incidence.

Table 3: Public sector contraceptive supply and its impact on probability of conception per quarter

	Full sample	Stayers
	(1) conceive	(2) conceive
csr	-0.00955	-0.00757
	[0.518]	[0.659]
L.csr	-0.0144	-0.0126
	[0.364]	[0.463]
L2.csr	-0.0114	-0.0118
	[0.365]	[0.367]
L3.csr	-0.0350**	-0.0281*
	[0.019]	[0.088]
L4.csr	-0.00414	-0.00315
	[0.793]	[0.860]
L5.csr	-0.0155	-0.0103
	[0.243]	[0.460]
L6.csr	-0.0114	-0.00954
	[0.430]	[0.538]
L7.csr	0.0000643	0.00456
	[0.996]	[0.771]
L8.csr	0.000805	0.00339
	[0.939]	[0.760]
L9.csr	-0.0100	-0.0126
	[0.373]	[0.270]
L10.csr	-0.0161	-0.0159
	[0.159]	[0.198]
L11.csr	-0.00311	-0.00399
	[0.817]	[0.756]
L12.csr	0.0106	0.0184
	[0.317]	[0.135]
Time FE (21)	yes	yes
Province FE (82)	yes	yes
Woman FE	yes	yes
Other controls	yes	yes
N	178408	130026
No. of women		
adj. R-sq	0.131	0.125

Estimation period: 2003q1-2008q2
SEs clustered at provincial level. p-values in brackets:
* p<0.10, ** p<0.05, *** p<0.01

Table 4: Public sector contraceptive supply and its impact on probability of conception per quarter, heterogeneity by wealth status indicator

	Full sample		Stayers	
	Poor conceive (1)	Non-poor conceive (2)	Poor conceive (1)	Non-poor conceive (2)
csr	-0.0752**	0.00483	-0.0949**	0.0109
	[0.045]	[0.747]	[0.021]	[0.517]
L.csr	-0.0534	-0.00647	-0.0495	-0.00409
	[0.108]	[0.688]	[0.139]	[0.816]
L2.csr	-0.0652**	0.00219	-0.0615**	0.000245
	[0.027]	[0.864]	[0.035]	[0.986]
L3.csr	-0.0664**	-0.0232	-0.0708**	-0.0136
	[0.023]	[0.138]	[0.026]	[0.415]
L4.csr	-0.0331	0.00762	-0.0259	0.00673
	[0.271]	[0.632]	[0.407]	[0.708]
L5.csr	-0.0496*	-0.00568	-0.0473	-0.0000398
	[0.075]	[0.657]	[0.114]	[0.998]
L6.csr	-0.0687***	0.00428	-0.0580**	0.00420
	[0.003]	[0.783]	[0.015]	[0.802]
L7.csr	-0.0406	0.0114	-0.0307	0.0149
	[0.232]	[0.398]	[0.392]	[0.331]
L8.csr	-0.0283	0.0107	-0.0273	0.0133
	[0.268]	[0.353]	[0.263]	[0.291]
L9.csr	-0.0254	-0.00468	-0.0220	-0.00888
	[0.385]	[0.665]	[0.465]	[0.425]
L10.csr	-0.0341	-0.00909	-0.0451*	-0.00620
	[0.173]	[0.452]	[0.083]	[0.635]
L11.csr	-0.0377	0.00823	-0.0447	0.00856
	[0.174]	[0.545]	[0.112]	[0.512]
L12.csr	-0.00589	0.0166	0.000570	0.0244*
	[0.841]	[0.125]	[0.986]	[0.059]
Time FE (21)	yes	yes	yes	yes
Province FE (82)	yes	yes	yes	yes
Woman FE	yes	yes	yes	yes
Other controls	yes	yes	yes	yes
N	34089	144319	26631	103395
No. of women				
adj. R-sq	0.132	0.132	0.126	0.126

Estimation period: 2003q1-2008q2
SEs clustered at provincial level. p-values in brackets:
* p<0.10, ** p<0.05, *** p<0.01

Table 5: Public sector contraceptive supply and its impact on probability of conception per quarter, heterogeneity by educational attainment

	Full sample		Stayers	
	LessHS conceive (1)	HSplus conceive (2)	LessHS conceive (1)	HSplus conceive (2)
csr	-0.0294	-0.00454	-0.0271	-0.00249
	[0.294]	[0.757]	[0.352]	[0.883]
L.csr	-0.0306	-0.0104	-0.0239	-0.0102
	[0.233]	[0.556]	[0.349]	[0.600]
L2.csr	-0.0463**	-0.00140	-0.0435**	-0.00277
	[0.024]	[0.920]	[0.034]	[0.846]
L3.csr	-0.0487**	-0.0290*	-0.0330	-0.0248
	[0.039]	[0.059]	[0.203]	[0.136]
L4.csr	-0.00743	-0.00154	-0.00248	-0.00214
	[0.724]	[0.926]	[0.916]	[0.908]
L5.csr	-0.0502**	-0.00277	-0.0412*	0.00181
	[0.024]	[0.835]	[0.069]	[0.898]
L6.csr	-0.0482**	0.00215	-0.0438**	0.00363
	[0.019]	[0.891]	[0.044]	[0.829]
L7.csr	-0.0372*	0.0142	-0.0333	0.0197
	[0.063]	[0.351]	[0.141]	[0.238]
L8.csr	-0.00888	0.00599	-0.0143	0.0119
	[0.648]	[0.637]	[0.476]	[0.366]
L9.csr	-0.0211	-0.00493	-0.0226	-0.00731
	[0.277]	[0.708]	[0.266]	[0.593]
L10.csr	-0.0339	-0.00894	-0.0404*	-0.00575
	[0.130]	[0.468]	[0.072]	[0.656]
L11.csr	-0.0319*	0.00720	-0.0203	0.00248
	[0.068]	[0.628]	[0.307]	[0.864]
L12.csr	0.000367	0.0151	0.00457	0.0246*
	[0.983]	[0.179]	[0.805]	[0.071]
Time FE (21)	yes	yes	yes	yes
Province FE (82)	yes	yes	yes	yes
Woman FE	yes	yes	yes	yes
Other controls	yes	yes	yes	yes
N	41559	136849	33138	96888
No. of women				
adj. R-sq	0.132	0.132	0.125	0.125

Estimation period: 2003q1-2008q2
SEs clustered at provincial level. p-values in brackets:
* p<0.10, ** p<0.05, *** p<0.01

Table 6: Public sector contraceptive supply and its impact on probability of conception per quarter, heterogeneity by rural/urban residence

	Full sample		Stayers	
	Rural conceive (1)	Urban conceive (2)	Rural conceive (1)	Urban conceive (2)
csr	-0.0133	-0.000760	-0.0150	0.00193
	[0.398]	[0.976]	[0.408]	[0.944]
L.csr	-0.0211	-0.000625	-0.0134	-0.0108
	[0.173]	[0.980]	[0.439]	[0.664]
L2.csr	-0.00958	-0.0138	-0.00716	-0.0186
	[0.502]	[0.429]	[0.623]	[0.305]
L3.csr	-0.0311*	-0.0326	-0.0239	-0.0297
	[0.073]	[0.103]	[0.186]	[0.179]
L4.csr	-0.0118	0.0166	-0.0142	0.0185
	[0.494]	[0.507]	[0.458]	[0.482]
L5.csr	-0.0353**	0.0137	-0.0316*	0.0236
	[0.036]	[0.415]	[0.069]	[0.145]
L6.csr	-0.0135	-0.00598	-0.0132	-0.00121
	[0.401]	[0.762]	[0.443]	[0.953]
L7.csr	-0.000401	0.00806	0.00191	0.0134
	[0.980]	[0.638]	[0.912]	[0.488]
L8.csr	0.00295	0.00470	0.00488	0.00873
	[0.811]	[0.764]	[0.705]	[0.590]
L9.csr	-0.0112	-0.00762	-0.0138	-0.0109
	[0.400]	[0.610]	[0.325]	[0.467]
L10.csr	-0.0171	-0.00768	-0.0187	-0.0105
	[0.207]	[0.568]	[0.201]	[0.478]
L11.csr	-0.0184	0.0293*	-0.0198	0.0276
	[0.221]	[0.080]	[0.163]	[0.128]
L12.csr	0.0105	0.0160	0.0175	0.0216
	[0.337]	[0.413]	[0.181]	[0.272]
Time FE (21)	yes	yes	yes	yes
Province FE (82)	yes	yes	yes	yes
Woman FE	yes	yes	yes	yes
Other controls	yes	yes	yes	yes
N	88349	90059	69461	60565
No. of women				
adj. R-sq	0.132	0.132	0.125	0.125

Estimation period: 2003q1-2008q2
SEs clustered at provincial level. p-values in brackets:
* p<0.10, ** p<0.05, *** p<0.01