CONTRACEPTION AS DEVELOPMENT?
NEW EVIDENCE FROM FAMILY PLANNING IN COLOMBIA

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ABSTRACT

There has been considerable debate in the last decade about whether or not family planning programs in developing countries reduce fertility or improve socio-economic outcomes. Despite suggestive associations, disagreement persists because the availability and use of modern contraceptives are generally determined by both supply- and demand-side factors. This paper provides new evidence on the role of contraceptive supply by exploiting the surprisingly haphazard expansion of one of the world's oldest and largest family planning organizations ─ PROFAMILIA of Colombia. Its findings suggest that family planning allowed Colombian women to postpone their first birth and have approximately one-half fewer children in their lifetime. Delayed first births, in turn, seem to have enabled young women to obtain more education and to work more and live independently later in life. Although family planning explains only about 10% of Colombia's fertility decline, it appears to have reduced the otherwise substantial costs of fertility control and may be among the most effective development interventions.

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"The goal is not reducing, increasing, or stabilizing the numbers of people. It is helping make more possible a richer, fuller life – jobs; homes; resources; freedom from hunger, disease, ignorance; time for development of innate capacities – in short, enriching the quality of life for an increasing proportion of the world's people."

– Leona Baumgartner, USAID, 1965
(at the launch of USAID's population assistance program[1])

1. Introduction

Family planning programs have been a part of development agendas for nearly half a century.[2] However, considerable disagreement about what they actually accomplish emerged in the last decade. Without access to modern contraceptives, the costliness of fertility control was historically thought to cause vicious cycles of high fertility, poverty, and slowed economic growth. A refined version of this view was embraced by the landmark International Conference on Population and Development (ICPD) in 1994. Not coincidentally, renewed objections to this perspective were raised the same year (Pritchett, 1994). The alternative put forward is that development simply causes women to want fewer children – and that the true effect of contraceptive supply on fertility is negligible (even if lower fertility is achieved with modern contraceptives).

The debate about family planning has very real consequences. Not only is there disagreement about the relative importance of supply- versus demand-side interventions in reducing fertility, but family planning's broader potential to improve economic and social conditions remains uncertain as well. Its omission from the Millennium Development Goals provides a visible example of this. It is therefore striking is how strongly beliefs on both sides of

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[1] USAID has been a principal supporter of PROFAMILIA.
[2] Family planning programs are a bundle of services that generally aim to reduce the costs of fertility control. They typically include the provision of contraceptive services and devices as well as reproductive education.

the debate are held in the absence of solid empirical evidence.[3] Because the placement and use

of family planning programs are generally related to the demand for children, rigorous

evaluations of them have proven to be difficult (Schultz, 1994).

To help advance this debate, I provide new evidence on the role of contraceptive supply

drawn from one of the world's oldest and largest family planning organizations – PROFAMILIA

of Colombia (Asociacion Pro-Bienestar de la Familia Colombiana). PROFAMILIA was founded

in 1965 under an informal political compromise and became the country's dominant family

planning provider for several decades. Several key features of its programs allow me to address

the problems of previous research: their massive country-wide coverage, their duration over

several decades, their financial sustainability, and their surprisingly haphazard geographic

spread. This last feature provides variation in contraceptive supply not related to demand.

Figure 1 provides suggestive evidence of PROFAMILIA's success. Fertility declined more

abruptly in Colombia than in any other South American country over any period of time

precisely as it was scaling-up its operations during the late 1960s and early 1970s.

PROFAMILIA was founded during a period of unprecedented social and economic

change, so an empirical strategy capable of isolating its impact is critical. Because the

introduction of its programs varied across municipalities and over time, variation in

contraceptive access among women of the same age in different municipalities and among

women of slightly different ages in the same municipality can be combined to estimate family

planning effects. Several other studies have used similar approaches, but they have generally

found that programs were established in areas and at times of relative need or relative prosperity

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[3] Views range from "… although development and social change create conditions that encourage smaller family size, contraceptives are the best contraceptive" (Robey, Rutstein, and Morris, 1993) to "The decision to have another child is simply too important and too costly for contraceptive costs to play a major role" (Pritchett, 1994).

2

(Gertler and Molyneaux, 1994; Pitt, Rosenzweig, and Gibbons, 1993; Rosenzweig and Wolpin, 1986). Remarkably, this does not appear to be the case with PROFAMILIA.

I first investigate the effects of family planning on fertility. My estimates suggest that between 1965 and 1993, the availability of modern contraceptives in Colombia allowed women to postpone their first birth and reduced completed lifetime fertility by 6% to 13% (two-fifths to three-quarters of a child) among women ever having children. These results imply that fertility control is indeed costly in the absence of modern contraceptives. However, they only explain between 9% and 12% of the fertility decline in program areas, suggesting that other factors are more important.

Next, I estimate program effects on women's socio-economic status. Women with access to family planning at all fertile ages received nearly 0.15 more years of schooling, were more likely to work in the formal sector, and were less likely to cohabitate. The age pattern of results suggests that delayed first births are more responsible than reduced lifetime fertility. This finding is important given an empirical literature that often emphasizes completed lifetime fertility and child quantity over the lifecycle timing of births. More tentative evidence then suggests that children benefited from family planning as well (boys more than girls) and that socio-economic inequality fell with contraceptive access. Finally, I provide informal comparisons with other well-regarded development interventions. Despite the modest role of family planning in explaining Colombia's fertility decline, these comparisons suggest that it may be among the most effective interventions to alleviate poverty.

A natural concern with my empirical strategy is that the spread of family planning in Colombia was related to secular changes in fertility, socio-economic conditions, or the demand for children generally. I present a variety of evidence to address concerns about non-random

program placement and timing. During interviews, PROFAMILIA's early leaders volunteered that the expansion of programs across municipalities was haphazard and unrelated to the demand for children. Corroborating graphical evidence shows identical completed lifetime fertility among women too old to benefit from family planning (both in level and trend) across municipalities with differentially-timed programs – and staggered fertility declines among younger women that coincide precisely with the introduction of modern contraceptives. Implicit program targeting would also be evident as program effects among women just beyond fertile ages when family planning became available. Statistical analyses demonstrate the absence of any such fertility or socio-economic effects. Because family planning did not influence the decision to become a mother, secular socio-economic changes specific to cohort by municipality cells should also be evident among women who never had a child but were the peers of affected women. Analyses confirm the absence of program effects on socio-economic indicators in this control group of women. Finally, all estimates are robust to controlling for fixed and age-varying differences across Colombia's municipalities.

This paper is organized in nine sections. The second section provides background on family planning in Latin America and Colombia. The third discusses relevant conceptual issues and briefly reviews gaps in previous research. The fourth describes my data and presents graphical analyses; the fifth details my empirical strategy. The sixth section reports results for fertility choices and women's socio-economic status, and the seventh presents a variety of validity tests. The eighth extends the analyses to children's socio-economic outcomes and inequality and then compares family planning with other development interventions. The ninth section concludes.

4

2. PROFAMILIA and Family Planning in Latin America

2.1 The Evolution of Family Planning in Latin America

The prevailing government philosophy in Latin America during the 1950s and early 1960s was strongly pronatalist, as exemplified by the motto "gobernar es poblar," or "to govern is to populate" (Mundigo, 1996). Nation-building required a large body of citizens to create international standing as well as deep consumer markets and a large workforce. Rather than concern about a Malthusian 'population problem,' it was the alarming incidence of unsafe abortions that first led Latin American governments to take interest in family planning in the mid-1960s. Abortions performed in unsanitary conditions by unqualified personnel were certainly troubling in their own right (they were thought to have been the leading cause of maternal mortality at the time). But they also resulted in large government expenses as women with abortion-related complications sought care en masse from public hospitals (Mundigo, 1996).

As Latin American governments began warming up to family planning, they faced daunting opposition to providing it directly from the Catholic Church and other conservative elements. Consequently, early family planning efforts in Latin America were small, private initiatives funded by international donors and NGOs.[4] Over time, growing awareness of family planning as a health input allowed governments to incorporate it into their public health systems. The prolonged dominance of private family planning services in Colombia was uncommon.

2.2 PROFAMILIA and the Introduction of Family Planning in Colombia

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[4] The major organizations involved were the International Planned Parenthood Federation (IPPF), the United Nations Population Fund (UNFPA), and the United States Agency for International Development (USAID).

In Colombia, the National Association of Medical Schools was the early leader on population issues (Ott, 1977). It conducted the first fertility surveys and introduced reproductive health training into the medical curriculum, but intense political pressure prevented it from providing any services directly. The government faced similar political constraints, but it chose not to actively oppose private family planning.

The government's position opened the door for the establishment of the private, not-for-profit PROFAMILIA. Its founder (Dr. Fernando Tamayo) was a young physician who worked in one of Bogota's public hospitals and operated a small private practice on the side. When he began providing contraceptive devices (primarily IUDs at first) through his own practice, he was overwhelmed by the enormous latent demand for them. Poor women waited in long lines, upsetting more affluent patients and prompting his practice partners to complain. Rather than turn these women away, he sought a new location for his practice, and PROFAMILIA was born in Bogota in 1965.[5]

Because of the political quagmire surrounding family planning, PROFAMILIA was the dominant family planning provider in Colombia for many years after its inception.[6] Its operations spread to all of Colombia's significant municipalities in subsequent years, growing into a network of 40 municipal program areas over the next decade (Table 1). Oral contraceptives and IUDs were the most popular devices in the early years. Over time, however, female sterilization has become the most common form of birth control. Table 2 shows

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[5] The new location first chosen had to be abandoned when the landlord discovered how the space was being used, reportedly exclaiming: "I don't mind if it is used as a whorehouse, but I will not allow it to be used for family planning!" (Singh, 1994)
[6] As late as 1990, PROFAMILIA still provided about 70% of all family planning services in Colombia.

estimates of contraceptive prevalence by type among married women in Colombia from 1969 to 1990.[7]

PROFAMILIA pioneered new ways of reaching women with modern contraceptives. Distinguishing features of its philosophy include its focus on the poor and its recruitment of lay-people to provide outreach in their own communities. In municipalities where it operated, PROFAMILIA also supplied vast quantities of contraceptive devices not requiring medical supervision to local drugstores at cost, and it advertised its services by radio. In 1971, PROFAMILIA became the first family planning organization in Latin America to serve rural areas as well.

Figure 1 shows that the demographic transition in Colombia does not resemble the transition in other South American countries. The fertility decline in Colombia during the late 1960s and early 1970s (when PROFAMILIA was expanding) was the most rapid decline on record in South America, and Colombia was not developing more rapidly than its neighbors over this decade. A rough comparison with Venezuela is informative. Venezuela is geographically adjacent to Colombia, shares much of its colonial history, experienced a similar shift from natural resource exports to industrialization in the post-war era, and had comparably high fertility rates during the late 1950s and early 1960s. However, its privately-funded family planning programs established during the 1960s collapsed, and its public sector family planning activities languished for several decades (Parrado, 2000). From 1960-65 to 1985-90, fertility fell by about 3.5 children per woman in Colombia and about 2.75 children per woman in Venezuela – a difference of 0.75 children. This admittedly crude difference-in-difference closely matches the family planning effects that I estimate more formally using within-country variation in PROFAMILIA's location and timing.

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[7] Modern contraceptives were not readily available in Colombia before 1965.

2.3 The Spread of PROFAMILIA's Programs

Critical to the empirical strategy of this paper is the way that PROFAMILIA's programs spread across Colombia's municipalities. Because the precise timing of PROFAMILIA's programs across municipalities is assumed to be largely exogenous, it is important to investigate (or infer) the allocation rule that governed the timing and placement of its programs. One of PROFAMILIA's leaders during the 1960s and 1970s (Dr. Gonzalo Echeverry) explained to me that he views the timing of municipal programs to have essentially been arbitrary. While traveling or vacationing in some part of the country, he would by chance meet a fellow physician at a café or a bus stop. The subject of PROFAMILIA would occasionally, the other physician would sometimes express interest, and with that physician's help, PROFAMILIA would establish a program in that municipality some time later.

Table 1 shows that after being founded in Colombia's largest cities, PROFAMILIA spread to many small departmental capitals like Sogamoso, Armero, and Puerto Berrio before reaching other major cities like Cali, Cartagena, and Santa Marta. Program expansion also did not follow a clear geographic pattern. For example, a program began in the coastal city of Barranquilla in 1967, but none was established in neighboring Cartagena or Santa Marta until 1970 and 1972 (respectively). Although travel itineraries, interest from local physicians, and logistical time required to establish programs were surely not completely random, the key question is whether or not their geographic pattern and timing were correlated with secular changes (not just levels) in the demand for children. This anecdotal evidence provides no immediate cause for concern; a variety of more careful formal evidence is presented in Section 7.

8

3. Conceptual Issues on Why Family Planning Might Matter and Existing Evidence

Whether or not family planning programs reduce fertility or improve socio-economic outcomes is ultimately an empirical question. Before turning to existing empirical evidence, this section first summarizes theoretical insights into how and why access to modern contraceptives might have these effects.

3.1 Theory on Family Planning and Fertility

Despite their many incarnations, two basic types of economic models have been used to conceptualize fertility choices: household demand models (Becker, 1960; Becker and Lewis, 1973; Schultz, 1974; Willis, 1973) and "synthesis" models[8] (Easterlin, 1978; Easterlin, Pollak, and Wachter, 1980). In household demand models, child quantity and child 'quality' are explicit arguments of household utility. Modern contraceptives decrease the costs of fertility control, raising the relative price of child quantity and inducing substitution from quantity to quality. Synthesis models differ by explicitly incorporating the supply of births ("synthesizing" economic and demographic theories of fertility). Household utility does not depend on child quality but includes additional arguments – the frequency of sexual activity and disutility due to contraception and infant mortality. Modern contraceptives can be considered technological innovations that lower the costs of fertility control by reducing the disutility of contraception and allowing more frequent sexual contact for a given fertility rate. This is perhaps a more straightforward way to understand the theoretical role of family planning. Despite their differences, both models yield the same basic insight into how family planning might reduce fertility: limiting births is costly, and family planning plausibly reduces these costs.

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[8] Both generally assume unitary household decision-making, an important shortcoming that I set aside for the sake of brevity. There are many theories outside of economics, too – prominent ones include Caldwell's theory of generational wealth flows (Caldwell, 1982) and the diffusion of innovations theory (Rogers, 1962).

More elaborate versions of these models incorporate the consequences of market incompleteness (Cain, 1981; Schultz, 1997). Lack of formal public or private pensions may increase the demand for children to provide old-age security. Large families may also provide a form of informal insurance against idiosyncratic income shocks due, for example, to illness or crop failure. While these and other refinements may provide important insights into the demand for children, they do not substantially alter the theoretical way that family planning might reduce fertility.

The family planning debate is primarily about the size of fertility control costs relative to the costs of having children. Although not well understood, there is reason to think that the costs of fertility control may be quite large in the absence of modern contraceptives. Experience with HIV/AIDS suggests that individuals may not deviate much from their desired sexual behavior even when confronted with potentially grave consequences (Gertler, Shah, and Bertozzi, 2003; United Nations Program on HIV/AIDS, 2004). Women in developing countries are also thought to have relatively little bargaining power within households (Rangel, 2004), so negotiating sexual activity and fertility choices with male partners may be very costly for them.

3.2 Theory on Family Planning and Socio-Economic Status

How family planning might improve socio-economic outcomes is arguably more complicated (and less-developed theoretically). Access to modern contraceptives can allow previously unattainable combinations of fertility, birth timing, and other consumption and investment as a household's feasible production set expands (Miller, 2003). In non-unitary household models, changes in socio-economic status depend not only on individual preferences, but also on intra-household bargaining.

Women might invest more in their education or increase their labor supply simply because fewer household resources are devoted to children as their number falls. This would not necessarily be true, however, if more is invested in each child or if women's time becomes a larger share of investments in children. The ability to postpone first births and more optimal birth timing generally could also allow women to obtain more schooling and invest more in their careers. Less obvious pathways from family planning to women's socio-economic status are also possible. Having fewer children might increase the expected amount of time that women work, raising the lifetime return to education and inducing women to obtain more of it. In high maternal mortality rate environments, fewer births might also raise the lifetime return to education by increasing life expectancy.

Children might also fare better under family planning for a variety of reasons. A reduction in the number of children mechanically raises available resources per child (Becker and Lewis, 1973), although allocation within the household presumably changes with family size and composition. If more optimally-timed births allow parents to invest more in their own education and careers (Hotz, Klerman, and Willis, 1997), lifetime household income will grow, likely resulting in greater child investments (Deaton and Paxson, 2003; Elo and Preston, 1996). Although this is an incomplete list of potential mechanisms, it suggests some of the principal ways that family planning might improve socio-economic status.

3.3 Evidence from Observational Studies and the Matlab Experiment

The more careful observational studies of contraceptive access and fertility in developing countries have generally exploited variation in their timing and location. However, the non-random placement of family planning programs complicates the interpretation of their results

(Gertler and Molyneaux, 1994; Pitt, Rosenzweig, and Gibbons, 1993; Rosenzweig and Wolpin, 1986). Evidence from the United States suggests that oral contraceptives reduced pregnancies among young women (Goldin and Katz, 2002), but it is unclear how these findings generalize to developing countries.

Related research on fertility and socio-economic outcomes is also difficult to interpret in the context of the family planning debate. Higher fertility due to twinning reduced children's educational attainment in 44 Indian families (Rosenzweig and Wolpin, 1980), but this exogenous increase in family size could have later been offset by subsequent fertility choices. The availability of abortions resulted in better socio-economic outcomes among women and children in the United States and Romania (Angrist and Evans, 1999; Donohue and Levitt, 2001; Gruber, Levine, and Staiger 1999; Pop-Eleches, 2002), but these child benefits may be marginal child effects rather than birth timing or completed lifetime fertility effects. Oral contraceptives in the United States increased women's career investments and short-run labor supply (Bailey, 2004; Goldin and Katz, 2002), but it is again unclear how these results generalize to developing countries.

Arguably the most convincing empirical evidence comes from the famous family planning experiment in Matlab, Bangladesh that began in 1978. Within a homogenous region of about 70 square miles, family planning was randomly assigned to 70 of 142 villages. In the treatment villages, health workers visited each married woman of fertile age every two weeks, providing reproductive education and offering modern contraceptives at no charge. By 1980, fertility was 24% lower in the treatment villages relative to the control villages (Koenig et. al., 1992; Phillips et. al., 1988). However, the practical relevance of this result has been questioned

because of the experimental program's unrealistic intensity and prohibitive costs[9] (Pritchett 1994). Research on the Matlab experiment's socio-economic effects has also produced mixed results. One study reports that family planning raised age-specific educational attainment, while another finds no evidence of educational benefits after a longer follow-up period (Foster and Roy, 1997; Sinha, 2003). There is also concern about the confounding influence of health services that were integrated into the family planning treatment four years after it began. More generally, field experiments may not capture the general equilibrium effects of country-wide programs.

With the experimental evidence from Matlab undermined by its unrealistic program intensity and the rest of the literature hampered by causal inference problems, the ability of family planning to reduce fertility – and by implication improve socio-economic well-being – has been called into question altogether (Pritchett, 1994). This is an empirical issue that remains unresolved.

4. Data and Graphical Analysis

4.1 Data

Longitudinal household data from Colombia is not readily available. However, cohorts with varying age-specific access to family planning can be recovered from cross-sectional data that contains individuals' age, place of residence, and migration history. There are several candidate household surveys including Demographic and Health Surveys first conducted during the 1980s and more recent quality of life surveys ("Encuestas de Calidad de Vida"). The

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[9] Experimental program expenditures were 10% of per capita GDP per fertile woman, 120% of per capita GDP per averted birth - about 35 times more than average public family planning spending in other Asian countries at the time.

difficulty with these surveys is their sample size: most contain 5,000 to 7,000 nationally-representative households. The staggered introduction of family planning programs across Colombia's municipalities means that program access varies at the age by municipality level, and these cells become very small in the survey data. Precision to detect the absence of program effects is particularly important given the controversial claims that they are not statistically different from zero.

To get around this difficulty, I use population censuses. The Colombian national statistical agency (DANE[10]) conducted censuses that asked questions about fertility (children ever born and children surviving) in 1973, 1985, and 1993. The censuses also contain demographic characteristics, measures of socio-economic status, and self-reported infant/child mortality. Table 3 shows descriptive statistics from the 1973 and 1993 censuses. The virtue of the censuses – their enormous size – ironically introduces computational difficulties under some circumstances. For this reason, random draws of approximately one million individuals are used throughout this paper.[11] I rely primarily on the 1993 census because it allows me to investigate long-run outcomes and exploit considerable variation in access to family planning among women with complete fertility histories. I also use the 1973 census to confirm the robustness of the results and to investigate the timing of first births.

To estimate family planning effects, it is necessary to assign age-specific program access to individuals in the censuses. PROFAMILIA's research department provided data on when its operations began in each Colombian municipality (Table 1). Because of uncertainty about when many rural programs began, I focus on municipal programs in this paper. With these program dates, I constructed cohorts of women with differential family planning access by age and

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[10] Departamento Administrativo Nacional de Estadistica
[11] The results are not sensitive to the particular draw used.

14

municipality of residence. Following the demography literature, I define women's reproductive

ages to be 15-44[12] and divide all ages into five-year intervals (10-14, 15-19, 20-24, etc.).

Dummy variables code each woman's age interval when a family planning program began in her

municipality of residence.[13]

4.2 Graphical Analysis

Before turning to formal statistical analyses, I present graphical evidence that illustrates

PROFAMILIA's effects and supports the assumption that program placement and timing was not

related to levels or changes in the demand for children. Figure 2 plots average completed

fertility among women ever having children by year of birth in the 1993 population census.[14]

Because cohort-specific fertility is difficult to illustrate separately for each municipality shown in

Table 1, I divide these municipalities into two groups: those with family planning programs

before 1969 ("early" program municipalities), and those with programs in 1969 or later ("late"

program municipalities).[15]

The figure first shows that among women too old to benefit from family planning (those

born before 1920, who were 45+ when PROFAMILIA was established in 1965), there were no

differences in either fertility levels or trends across areas with differentially-timed programs.

Then, among women late in their fertile years when PROFAMILIA was founded (those born

between 1920 and 1930, who were 35-45 in 1965), fertility in the early program municipalities

pulls slightly below fertility in the late ones. This divergence between early and late areas

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[12] The results are not sensitive to this choice.

[13] I choose dummy variables because not all individuals appear to know their precise birth year. Nearly identical results are obtained using the number of years of program access within each age interval. It would also be preferable to assign family planning access according to women's age and place of birth rather than place of residence. However, many women did not report their municipality of birth in the censuses. Concerns about the potentially confounding role of migration are addressed in Section 7.

[14] The same patterns are found using all women.

[15] 1969 is the population-weighted median year that PROFAMILIA's programs began in these municipalities.

15

becomes much more pronounced among women born in 1930 or later (who were 35 or younger in 1965). Among women just becoming fertile when PROFAMILIA was established (those born in 1950 or just before 1950), the amount of time it took fertility in late program municipalities to catch up to fertility in the early ones (the horizontal distance between the early and late program series) is three to four years. Strikingly, this is exactly the difference in time between the average early group program and the average late group program. The two fertility series then re-converge among younger women having access to family planning at all fertile ages regardless of where they live (women born around 1965 or later).[16] This convergence should not be complete because a few municipalities in the late group did not have programs until the 1980s. The young birth cohorts also reflect intergenerational program effects because they include the daughters of women who had varying access to modern contraceptives.

To see the age and timing of program effects more clearly, Figure 3 shows average completed fertility by age at first access to family planning. After removing cohort effects, age when programs were introduced is negatively related to completed lifetime fertility. More precisely, this association is present at fertile ages from the mid-teens through the mid-30s and absent at infertile ages. Figure 3 shows that access to family planning at all fertile ages is associated with a completed lifetime fertility reduction of about 0.75 children controlling only for cohort effects. This reduction very closely matches statistical estimates that I present later.

Figures 2 and 3 show clear evidence that PROFAMILIA's programs were associated with lower fertility rates. They also support the assumption that program placement and timing was not related to the demand for children. Pre-program levels correlated with program timing would be evident in Figure 2 among women born before 1920, and pre-program trends correlated

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[16] Women born in the early 1950s or later were still fertile at the time of the 1993 census, so their fertility should not be interpreted as completed lifetime fertility.

with program timing would be evident in Figure 3 among women first exposed to family planning at infertile ages.

5. Empirical Strategy

5.1 Fertility

This paper presents reduced-form estimates of family planning's consequences by exploiting the distinct timing, geographic pattern, and age-specific nature of PROFAMILIA's programs. Unobserved individual characteristics presumably influence the use of contraceptive services and devices, so program effects identified by variation in utilization would be biased. For this reason, the effects of family planning access (intent-to-treat effects) are estimated rather than the effects of contraceptive use (treatment-on-treated effects).

A simple approach would be to compare the fertility of women who are the same age but live in different municipalities and therefore differ in access to family planning. However, municipalities with differentially-timed programs could vary in fertility for reasons unrelated to family planning (although Figures 2 and 3 suggest that this was not the case). An alternative would be to compare the fertility of women of slightly different ages in the same municipality. In this case, program effects could not be disentangled from cohort effects.

To circumvent these difficulties, I combine the two approaches, using only joint variation in the timing and location of PROFAMILIA's programs to identify their effects. By doing so, I assume that the combination of variation in contraceptive access across municipalities and over time is exogenous. This implies that women the same age in municipalities with differentially-timed programs would have experienced the same changes in fertility over time in the absence of

family planning. It also implies that after controlling for age, fertility differences between younger and older women in the same municipality are attributable only to family planning. Section 7 devotes considerable attention to evaluating the validity of the identifying assumption more carefully.

I focus on two distinct lifetime fertility choices that contraceptive access could affected: whether or not to have children and how many births to have conditional on having any.[17] A two-part empirical model allows family planning effects to differ for these two choices. This approach is also one way of handling censoring at zero in the distribution of births. For individual women i, reproductive age ranges a, and ages y, I begin by estimating:

$$\Pr\left(Any\ Birth_i = 1\right) \quad = \quad \alpha + \sum_a \beta_a access_{i,a} + \sum_y \tau_y age_{i,y} + \varepsilon_i \qquad (1)$$

$$\text{and}$$

$$\ln\left(n_i\right)\big|\left(Any\ Birth_i = 1\right) \quad = \quad \delta + \sum_a \gamma_a access_{i,a} + \sum_y \psi_y age_{i,y} + \xi_i \qquad (2)$$

In these simple specifications, the choices of ever having any children (*Any Birth*=1) and the natural logarithm of the number of births (*n*) conditional on having any are assumed to depend on a constant, age interval when modern contraceptives first became available in a woman's municipality (*exposure*, constructed as dummy variables), age dummies (*age*), and an error term. By controlling only for age, these equations estimate program effects as if the location and timing of PROFAMILIA's programs were randomized. The *β*s and *γ*s are estimates of family planning effects in each reproductive age range (15-19, 20-24, ..., 40-44). The interpretation of the *γ*s would be more complicated if I found that family planning affected the decision to have children (if the *β*s≠0), but this is not the case. I next re-estimate these equations including a

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[17] The former provides a means of investigating the timing of first births, too, as discussed in Section 6.1.

number of additional independent variables for individual women i, age ranges a, municipalities m, and ages y:

$$\Pr\left(Any\ Birth_i{=}1\right)\quad and\quad \ln\left(n_i\right)\big|\left(Any\ Birth_i{=}1\right)\quad =$$
$$\alpha + \sum_a \beta_a access_{i,a} + \sum_y \tau_y age_{i,y} + \sum_m \mu_m muni_{i,m} + \delta cohort_i$$
$$+ \sum_m \rho_m \left(muni_{i,m} * cohort_i\right) + \sum_k \pi_k migrate_{i,k} + \varepsilon_i \qquad\qquad (3, 4)$$

Equations (3) and (4) also include municipality dummy variables (*muni*), a continuous variable for age (*cohort*), interactions between age and municipality (*muni*cohort*) to capture municipality-specific linear time trends, and dummy variables for whether or not a woman has ever moved and has moved in the last five years (*migrate*).[18] If joint variation in the location and timing of PROFAMILIA's programs was truly exogenous, the age-specific program effect estimates in (3) and (4) should not be different than those obtained from (1) and (2).

The logarithmic transformation of births in (2) and (4) is reasonable given its right-skewed distribution (conditional on any births).[19] However, count data models also fit the distribution of births well. A negative binomial model provides an attractive alternative and is more appropriate than a poisson model in instances of unequal mean and variance because of its flexible dispersion parameter (Cameron and Trivedi, 1998).[20] The NB2 density function (which allows for overdispersion) is given by:

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[18] Section 7 presents a more careful analysis of migration.
[19] Retransformation back to the raw scale is problematic when the error term is heteroskedastic across treatment groups (Manning, 1998; Buntin and Zaslavsky, 2004). However, regressions of log squared residuals on program exposure suggest that this is not problematic in my case.
[20] Estimates of the dispersion parameter α range from 0.04 to 0.10 in the models shown in Table 4. These estimates are statistically different from zero at conventional levels and suggest the presence of modest overdispersion.

$$f(y|\mu,\alpha) = \frac{\Gamma(y+\alpha^{-1})}{\Gamma(y+1)\Gamma(\alpha^{-1})}\left(\frac{\alpha^{-1}}{(\alpha^{-1}+\mu)}\right)^{\alpha^{-1}}\left(\frac{\mu}{\alpha^{-1}+\mu}\right)^{y} \qquad (5)$$

where the dependent variable y is assumed to have mean $\mu=exp(X\beta)$ and variance $\mu+\alpha\mu^2$.

Estimates of α (the dispersion parameter) and β (the coefficients for the independent variables X) are obtained by maximum likelihood; estimates of β are compared with corresponding estimates from (2) and (4) to assess sensitivity to functional form.

5.2 Socio-Economic Status

To estimate the consequences of contraceptive access for women's socio-economic status, I employ a reduced-form approach exactly as shown in (3) and (4). A variety of socio-economic outcomes are used as dependent variables: women's educational attainment, formal sector employment, occupational choice (conditional on working in the formal sector), cohabitation, and self-reported infant/child mortality.[21] I rely exclusively on a reduced-form approach because family planning may affect socio-economic outcomes through pathways other than completed lifetime fertility. For example, I present evidence in Section 6.1 that family planning influenced the timing of births as well as the number of births. The exclusion restriction required to instrument for completed lifetime fertility using family planning access is probably not met.

To investigate whether or not there were relative increases or decreases in socio-economic conditions or the demand for children just before programs began, I also include a dummy variable for whether or not a woman was just beyond fertile age (45-49) when modern contraceptives became available. Relative improvements among these women might suggest

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[21] Cohabitation is defined as either marriage or an extra-marital "free-union." Infant/child mortality is defined as the difference between women's reported number of births and reported number of surviving children.

that family planning programs were allocated to areas with differentially declining demand for children (or differentially rising demand for contraception) or that estimated program effects mistakenly capture pre-existing socio-economic trends. Alternatively, relatively worse socio-economic outcomes among these women might suggest that program effect estimates confuse mean reversion for true effects. I find no evidence of either.

6. Results

6.1 Fertility Results

Tables 4 and 5 present estimates of how age-specific access to family planning affected women's fertility choices. Dependent variables are shown at the top of the columns, and cells within columns show the association between first having access to family planning within a given age-range (15-19, 20-24, etc.) and the corresponding fertility outcome. The first column for each dependent variable generally provides program effect estimates conditional only on cohort dummy variables ("Cohort Only"). The second column for each dependent variable generally shows estimates conditional on the full set of covariates ("All Covariates"). These specifications include municipality fixed-effects and municipality-specific linear trends exactly as shown in equations (3) and (4). I emphasize the results obtained using the larger set of covariates but highlight that the estimates are robust to controlling for fixed and time-varying differences across municipalities. The statistical equivalence of estimates obtained using different sets of covariates supports the assumption that the spread of family planning was unrelated to the demand for children.

The second column of Table 4 presents log-linear program effect estimates for lifetime fertility among women ever having a child (the second part of the two-part model as shown in equation 4). Because the dependent variable is in logarithmic form, coefficient estimates can roughly be interpreted as percent changes in lifetime births associated with first having access to family planning in each age interval. In general, gaining access to PROFAMILIA's programs was negatively associated with lifetime fertility for women in their late teens through their early thirties. Relative to women without family planning access while fertile, lifetime access was associated with about 13% fewer births (the estimate for "Family Planning Access 15-19"). From a base of six births, this reduction is equivalent to about three-quarters of a child. Lifetime fertility effects are progressively smaller for women who were older when modern contraceptives became available. Women first exposed to family planning at ages 30-34 had about 4% fewer children, and no program effects are evident among women first exposed at age 35 or beyond.

The third and fourth columns of Table 4 show comparable elasticities obtained from the negative binomial model. Although somewhat smaller than the log-linear estimates in the first two columns, these estimates tell the same story.[22] Women ages 15-19 when family planning programs began gave birth to about 7% fewer children in their lifetime (or about 0.4 children). Program effects are again present among women first exposed to family planning at ages up to their early thirties.

The second column of Table 5 shows program effect estimates from the first part of the two-part model (equation 3) for women's decision ever to have children. They show no evidence that access to family planning at any age is associated with the choice to become a mother.

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[22] The negative binomial model produces a slightly larger log-likelihood value than the maximum likelihood equivalent of the log-linear OLS model.

These point estimates that are very near zero are precisely estimated. Combining estimates from both parts of the model, PROFAMILIA's programs explain between 9% and 12% of the fertility decline in program municipalities from 1964 to 1993.

Although the Colombian population censuses do not report the year of women's births, the result that family planning did not influence the decision ever to have children provides a way of investigating program effects on the timing of first births. The 1973 census was conducted shortly after PROFAMILIA established programs in many areas, and young women exposed to programs in these areas still had many fertile years ahead of them. Program effects on whether or not young women had children at the time of the 1973 census can therefore be interpreted as effects on the timing of first births. The third column of Table 5 presents estimates from the 1973 census comparable to those from the 1993 census shown in the second column. The major difference is that women gaining access to family planning in their late teens were about 3% less likely to be mothers in 1973. This result suggests that PROFAMILIA's programs allowed women ages 15-19 to postpone their first birth. Because this estimate only captures first births postponed from before until during or after 1973, the magnitude of delayed first births is presumably much larger.

6.2 Socio-Economic Results for Women

Table 6 shows results for women's socio-economic status among women ever having children (who were directly affected by family planning). Each column shows estimates for a different socio-economic outcome with the dependent variable at the top of the column. The first column presents program effect estimates for years of education. Relative to women without access to family planning while fertile, women ages 15 to 19 when PROFAMILIA established a

program completed 0.14 more years of schooling. From a base of slightly less than 7 years of schooling, this is an increase of more than 2%. There is also weaker evidence that women first exposed to family planning in their early twenties completed more schooling as well. In general, these are large effects that could be due to the postponement of first births, reductions in completed lifetime fertility, or both. The absence of meaningful effects among fertile women just beyond school age provides additional support for the assumption that family planning program placement was unrelated to the demand for children.[23]

The second column of Table 6 shows that family planning access at young ages was associated with greater probabilities of working in the formal sector in 1993. Women first exposed to family planning at ages 15-19 and 20-24 were 3 percentage points more likely to be working. These gains represent increases of 5% in the probability of formal sector employment. However, because the censuses only report employment in the formal sector, it is possible that these estimates also capture occupational choice effects. Shifts from informal to formal sector work would be combined with increases in employment. The third column shows no evidence that conditional on formal sector employment, women were not more likely to have "white collar" or professional occupations because of family planning access.

The fourth column of Table 6 shows that access to family planning under age 25 reduced the probability that women were cohabitating in 1993 (either married or in a "free-union"). Women 15-19 and 20-24 when PROFAMILIA established a program were about 2 percentage points less likely to be cohabitating in 1993, a reduction of about 3%. The welfare consequences of cohabitation are more ambiguous than for other socio-economic outcomes considered in this paper; even the direction of the effect may depend on the circumstances. For example, there may

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[23] Higher education in Colombia requires early specialization and is still uncommon. Five-year undergraduate degrees are often terminal, even among the elite. Some limited masters-level training also occurs.

be gains from specialization in household production (Becker, 1981), but women with little bargaining power within households (and their children) may do better by living independently as women's labor market opportunities improve.

Finally, the last column shows no evidence that family planning affected self-reported infant and child mortality. The point estimates corresponding to first being exposed to family planning in each age interval are very close to zero, and they are precisely estimated. However, measurement error is likely more problematic for self-reported infant and child mortality. These inaccuracies could be correlated with family planning access, or they could simply result classical measurement error; the sign of any potential bias is uncertain. Changes in the composition of births could also be relevant. Because first-born children are generally more likely to die, lower age-specific infant and child mortality rates could be offset as first births grow as a share of total births (Bongaarts, 1987). Nevertheless, the absence of detectable family planning effects stands in stark contrast to the common belief that the number and timing of births are important determinants of infant mortality (Wolpin, 1997).

It is noteworthy that all statistically meaningful socio-economic effects shown in Table 6 occur among women who first had access to modern contraceptives at young ages. Although Table 4 shows that gaining access to family planning was associated with lower completed lifetime fertility among women up to age 35, Table 6 reports no socio-economic program effects past age 25. The close correspondence between ages at which first births were postponed and ages at which socio-economic benefits occurred suggests that the timing of first births is likely to be a stronger determinant of socio-economic outcomes than completed lifetime fertility.

7. Validity Tests

There are a variety of potential concerns about this paper's empirical strategy and the validity of the results presented above. This section examines the identifying assumption of exogenous program expansion in detail and then investigates other possible concerns about the confounding influence of migration, selective attrition due to differential mortality, incomplete fertility histories, and contamination.

Although the graphical evidence in Section 4 suggests that joint variation in program location and timing is exogenous, this assumption can be evaluated statistically, too. One approach is to test formally for program effects on any outcome among women just past fertile ages (45-49) when modern contraceptives became available. Effects at these ages might suggest that family planning programs were implicitly targeted to areas with differentially declining demand for children or that my estimates mistakenly capture pre-existing socio-economic trends or mean reversion. Tables 4 through 6 show the absence of program effects at these ages for any fertility or socio-economic outcome available in the censuses (coefficient estimates for *Family Planning Access 40-44 Lead*). Table 7 shows results obtained by re-estimated equations (3) and (4) for fertility and socio-economic outcomes using the entire census population of women ages 45-49 and 50-54 when family planning programs began. Doing so follows the logic of estimating program effects for women ages 45-49 in Tables 4 and 6 (all of which are statistically insignificant) but has greater power to detect meaningful differences from zero. For educational attainment, I conduct this test for women just past schooling age (25-29) relative to women five years older (30-34). Table 7 shows no evidence of program effects on any outcome at these ages.

Another important way to test the identifying assumption is to test for program effects at all ages among women who never had children. Because family planning did not affect women's decisions to become mothers, women the same age and in the same municipalities but who never gave birth form a reasonable control group for women with children. This test allows me to detect confounding secular socio-economic trends that are specific to cohort by municipality cells of women. Effects among these women would again imply implicit program targeting related to socio-economic status or the presence of other confounding forces that varied systematically across municipalities and over time in the same pattern as PROFAMILIA's programs. Table 8 shows that there is no evidence of any program effects on any socio-economic outcome among women never having children.[24] In short, any confounding force would have had to vary across municipalities and over time in the same way as PROFAMILIA's programs, only affect women at fertile ages, and only affect women who ever had children – which seems unlikely.

In addition to the validity of the identifying assumption, there are also other potential concerns to address. One is the possibility of selection due to unobserved migration. By influencing socio-economic conditions, the establishment of family planning programs might have non-randomly attracted women from surrounding areas. There was in fact unprecedented migration from rural to urban areas during the period studied; about 15% of individuals in the 1993 census had moved during the last five years, and nearly 40% had moved in their lifetime. Because family planning access is assigned by age and municipality of residence at the time of the census, unobserved migration could bias my estimates. However, the estimates presented in Tables 4 through 6 are not sensitive to conditioning on having moved in the last five years or

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[24] About 15% of women in Colombia never had children during their lifetime over these years. The sample used for analyses shown in Tables 4 through 6 is used here as well; larger draws of women never giving birth confirm that the absence of statistical significance in Table 8 is not due to sample size.

ever having moved. Although movers and never-movers are presumably different, Table 3 also suggests that their observable characteristics are very similar (other than age). To verify that migration does not explain my results, I re-estimated program effects using a restricted sample of women who had never moved. Table 9 shows that these estimates are very similar (statistically identical in many cases) to those obtained using movers and never-movers. Migration therefore does not appear to explain this paper's major findings.

Another concern might be the possibility of selective attrition. If family planning altered women's survival rates (by resulting in fewer unsafe abortions, for example), it would also alter the composition of women across municipalities in the censuses, biasing program effect estimates. Colombia's low-quality mortality statistics make it difficult to investigate this possibility directly. However, the direction of any selective attrition bias is most likely downward. Poor women are disproportionately likely to seek unsafe abortions. Because abortions and modern contraceptives are presumably substitutes, poor women would therefore have higher survival rates in municipalities with earlier family planning programs. So poor women would comprise a larger share of all women in municipalities with earlier programs, and poor women have higher fertility rates, earlier first births, and worse socio-economic indicators on average. Any selective attrition would therefore most likely result in downward bias.

Additionally, it might seem that incorporating women with incomplete fertility histories (women younger than 45 in 1993) into the analyses could mistake the postponement of births for fertility reductions. This concern would be valid if all programs began at the same time or if completed fertility histories were not observed for any women with access to family planning at all fertile ages. Otherwise, if the identifying assumption is valid, the counterfactual experience of women the same age but with program access varying from none to access at all fertile ages

28

isolates completed lifetime fertility effects. Restricting the analyses to women with completed

fertility (ages 45+) in 1993 reduces the variation in age-specific access to family planning used

for identification but tells the same basic story.

Finally, although the municipalities examined by this paper are not geographically

contiguous, a potential concern might be that PROFAMILIA's programs benefited areas other

than the municipalities in which they were based. This possibility is unlikely. Colombia is

environmentally diverse, and most of its major population centers are separated by hostile

mountainous stretches of the Andes. Colombians will readily attest that geographic distance and

travel time are nearly orthogonal to each other. It is therefore unlikely that modern

contraceptives reached areas other than those immediately served by PROFAMILIA's programs.

In the unlikely event that they did, the resulting bias would be downward.

8. Extensions

8.1 Socio-Economic Results for Children

If contraceptive supply affects the number and timing of women's births, it may have

very important consequences for investments that parents make in their children, too. Because

the Colombian population censuses do not completely specify intra-household relationships,

however, I am unable to match many children to their mothers and therefore cannot estimate

program effects on children directly. Instead, I construct "statistical mothers" for children and

young adults using the mean characteristics of women who ever had children and were fertile in

each child's municipality of residence and year of birth. I match these characteristics to children

and assign age-specific family planning access according to statistical mother's age and

municipality of residence. The "statistical mother" approach reduces the variation in program exposure that I am able to exploit considerably; the resulting estimates should only be interpreted as suggestive.[25]

The relationship between women's access to family planning and child outcomes is estimated by the general equation:

$$
\begin{aligned}
child\ outcome_i \quad = \quad & \alpha + \sum_a \beta_a mother's\ access_{i,a} + \sum_k \pi_k mother's\ age_{i,k} + \sum_y \tau_y age_{i,y} \\
& + \sum_m \mu_m muni_{i,m} + \delta cohort_i + \sum_m \rho_m \left(muni_{i,m} * cohort_i \right) + \varepsilon_i \qquad (6)
\end{aligned}
$$

Child outcomes include school attendance, educational attainment, formal sector employment, own fertility, and own infant/child mortality. Statistical mother's age-specific access to family planning (*mother's access*) is coded using dummy variables as before. To investigate intra-household gender differences in the distribution of family planning benefits, I estimate equation (6) for boys and girls both separately and together. Analyses are restricted to children ages 10-19 in 1993 because it is not possible both to condition on a child's access to family planning when estimating the effect of statistical mother family planning access (both are a function of children's age and place of residence). Everyone 19 or younger in 1993 had access to family planning at least since age 15.

Table 10 shows estimates for children's socio-economic status obtained from equation (6). Dependent variables are shown at the top of each column; rows correspond to different samples (boys and girls together, boys only, and girls only). Each cell presents results for statistical mother family planning access at all fertile ages (beginning at ages 15-19). Estimates in the first row suggest that the children of women with lifetime access to family planning were

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[25] This approach also requires that family planning access be unrelated to the distribution of maternal characteristics at the age by municipality level. I find no evidence of any relationship.

two percentage points more likely to be attending school, had received 0.08 more years of education, and were less likely to be working in the formal sector. Broken down by gender, the second and third rows together suggest that boys enjoyed these benefits disproportionately more than girls. Girls possibly gained some additional schooling and were less likely to become teenage mothers, but boys enjoyed nearly all of the gains in school attendance and reductions in child labor.

On the whole, Table 10 provides suggestive evidence that family planning played an important role in reducing the intergenerational transmission of poverty. However, there were striking gender disparities in distribution of these benefits. These disparities are consistent with the literature on gender bias in household resource allocation (Duflo, 2003; Rosenzweig and Schultz, 1982; Sen, 1992; Thomas, 1994). They may also reflect changes in the sex composition of children. If modern contraceptives allowed parents to adhere more closely to fertility rules based on male preference, girls' outcomes may be relatively worse because they are in relatively larger families.[26]

8.2 Family Planning and Inequality

In addition to improving average levels of well-being, contraceptive access may have important consequences for socio-economic inequality. Because better educated women have larger opportunity costs of time and stronger incentives to limit their fertility (Chen and Kremer, 2002), it could increase inequality. There may also be diminishing returns to averted births, so fertility reductions at lower levels might produce larger benefits. Alternatively, less educated

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[26] There is evidence of such rules in other settings (Ahn, 1991; Park, 1983) as well as anecdotal evidence from Colombia: "…The total number of children was related to the order in which sons and daughters were born. Couples continued to have children until they had enough sons to balance the financial demands of raising their daughters" (Ortiz, 1999).

women may face larger costs of fertility control in the absence of modern contraceptives (if they are less empowered, for example). In this case, they might benefit more from family planning, and socio-economic inequality would fall.

Plausibly exogenous variation in education or socio-economic status is necessary to test this hypothesis using individual-level data. Instead, I exploit the staggered introduction of PROFAMILIA's programs by regressing the standard deviation (and log standard deviation) of education at the municipal level in 1993 on the number of years that family planning was available, controlling for municipal characteristics in 1973.[27] The results (not shown) provide evidence of statistically meaningful reductions in educational inequality under family planning (standard deviation reductions of 0.01 years of schooling per year of family planning availability, or about 0.3% per year).[28] For 25 years of program exposure, this translates into standard deviations that are smaller by about 0.25 years (or 7.5%). These results suggest that family planning may have reduced socio-economic inequality in Colombia and that poorer women may face relatively larger costs of fertility control. There is independent evidence that income inequality declined substantially in Colombia during the 1970s as PROFAMILIA was scaling-up its programs (Birchenall, 2001).

8.3 Program Costs

In addition to the size of family planning's benefits, a key issue is how much it costs to produce them. A formal cost-benefit analysis of PROFAMILIA's programs is not generally possible because many of its benefits cannot be directly estimated and valued. Women's

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[27] These are mean age, share female, share employed in the formal sector, share cohabitating, share ever moved, and share moved in the last 5 years in 1973. Although not possible, it would be preferable to control for municipal characteristics in 1964.
[28] These results are available upon request.

empowerment is a good example of this. Crude program cost and cost-effectiveness calculations are feasible, however. Arguably the strongest criticism of the Matlab experiment is its unrealistic program expenses and its large cost-effectiveness ratio. The Matlab family planning 'treatment' cost about 10% of per capita GDP per fertile woman and about 120% of per capita GDP per averted birth (Pritchett, 1994). PROFAMILIA's programs were dramatically less expensive relative to national income (Seltzer and Gomez, 1998). Specifically, program costs were about 0.1% of per capita GDP per fertile woman in program municipalities (about 1/100th of the Matlab costs relative to income) and about 0.25% of per capita GDP per birth averted (about 1/500th of the Matlab costs relative to income).[29] These rough calculations suggest that family planning can be successful for considerably less than in Matlab.

8.4 Comparisons with Other Interventions

Given debate over the importance of family planning in reducing poverty and promoting development, informal comparisons with other development interventions are informative. The results in Section 6.2 demonstrate that educational gains are a clear benefit of family planning. Improving education is a cornerstone of efforts to alleviate poverty, increase productivity, and promote economic growth in poor countries (Glewwe, 2002). Rough comparisons with other interventions that specifically target education therefore help to put the importance of family planning into context.

One well-documented program provided school vouchers to subsidize private secondary school tuition in Colombia (where private education is thought to be higher-quality) (Angrist et. al., 2002). In the short-run, vouchers raised educational attainment by 0.1 years of schooling, or about 1.5%. Another initiative was Indonesia's massive school construction program during the

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[29] These calculations are available upon request.

33

1970s (Duflo, 2001). The construction of over 60,000 schools during a five-year period increased schooling by about 0.15 years on average, or roughly 2%. A third intervention was the introduction of compulsory school attendance and child labor laws in the early 20th Century United States (Lleras-Muney, 2002). Taken together, these laws increased educational attainment by 5%, or around half a year. Finally, a study of school resources and education among blacks under South Africa's apartheid-era government suggests that reducing class size by one-quarter would increase schooling by half a year, or about 6% (Case and Deaton, 1999). PROFAMILIA's programs increased women's education by an amount roughly equivalent to some these initiatives that specifically targeted education (about 0.14 years, or more than 2%). In other words, its education benefits alone place it among some of the most highly-regarded development initiatives.

9. Conclusion

In light of the ongoing debate about family planning's ability to reduce fertility in developing countries, this paper suggests that neither side is exactly right. However, it sides more closely with the view that other forces (originating either on the supply- or demand-side) are more potent. PROFAMILIA's programs reduced women's completed lifetime fertility by roughly half a child, but they explain only 9% to 12% of the fertility decline in program municipalities between 1964 and 1993 as Colombia underwent its demographic transition.

Despite its modest role in reducing fertility, this paper also demonstrates that the ability of family planning to fight poverty cannot easily be dismissed. Colombian women with access to modern contraceptives at young ages experienced substantial socio-economic gains. A formal cost-benefit analysis of PROFAMILIA's programs would be difficult because many of its

benefits are hard to value. However, the gains in education alone attributable to family planning place it among some of the best-regarded education interventions. Given that 350 million couples worldwide presently lack access to family planning services (United Nations Population Fund, 2004), family planning may deserve more attention in dialogue surrounding the Millennium Development Goals and poverty relief in general.

The age pattern of results suggests that family planning most likely generated socio-economic benefits by allowing young women to postpone their first birth. This finding implies that emphasis on completed lifetime fertility and child quantity overlooks a critical dimension of fertility – the lifecycle timing of births (and first births in particular).[30] It also suggests that Latin America's contemporary increases in teenage motherhood (Florez and Nuñez, 2002) may have more detrimental consequences than is presently understood.

Finally, this paper's major findings imply that fertility control can be quite costly in the absence of modern contraceptives. Significant dimensions of these costs may include forgoing desired sexual activity and women with relatively little bargaining power negotiating sexual behavior and fertility with men. Because the costs of fertility control in developing countries are poorly understood, this is an important topic for further research. Policies that aim to help women achieve their desired level of fertility or to improve economic and social conditions generally may do well to emphasize reducing the costs of fertility control beyond what family planning achieves.

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[30] The empirical literature on fertility and socio-economic status in developed countries has paid more attention to the lifecycle timing of births than has the development literature (Ribar, 1999).

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Figure 1: Total Fertility Rates in Latin America by Country and Quinquennia, 1955-60 to 1985-90
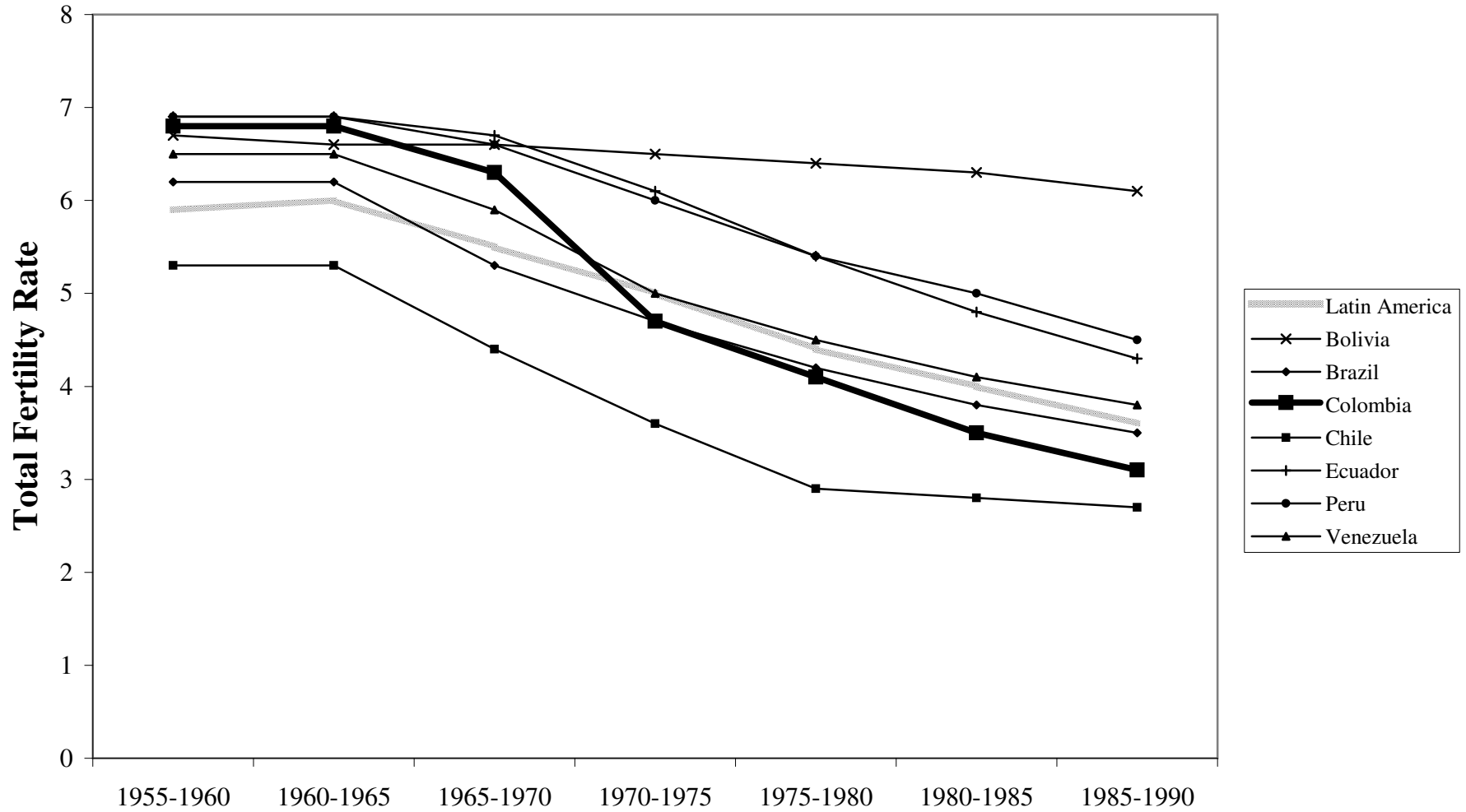
Source: Guzman, Singh, Rodriguez, and Pantelides (1996)

Figure 2: Women's Lifetime Fertility by Birth Cohort and Program Group
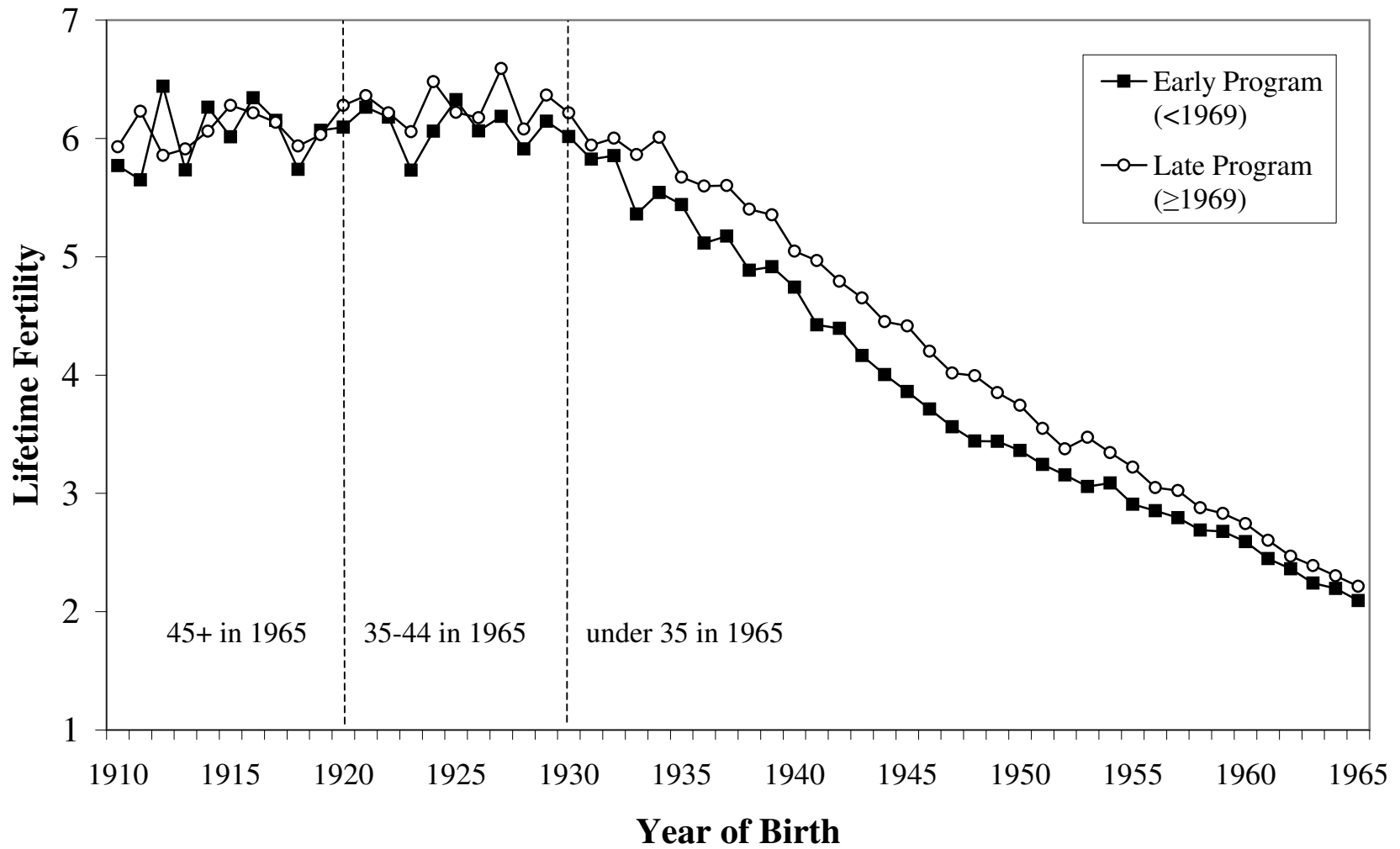
Figure 3: Women's Lifetime Fertility (Net of Cohort Effects) by Age at First Access to Family Planning
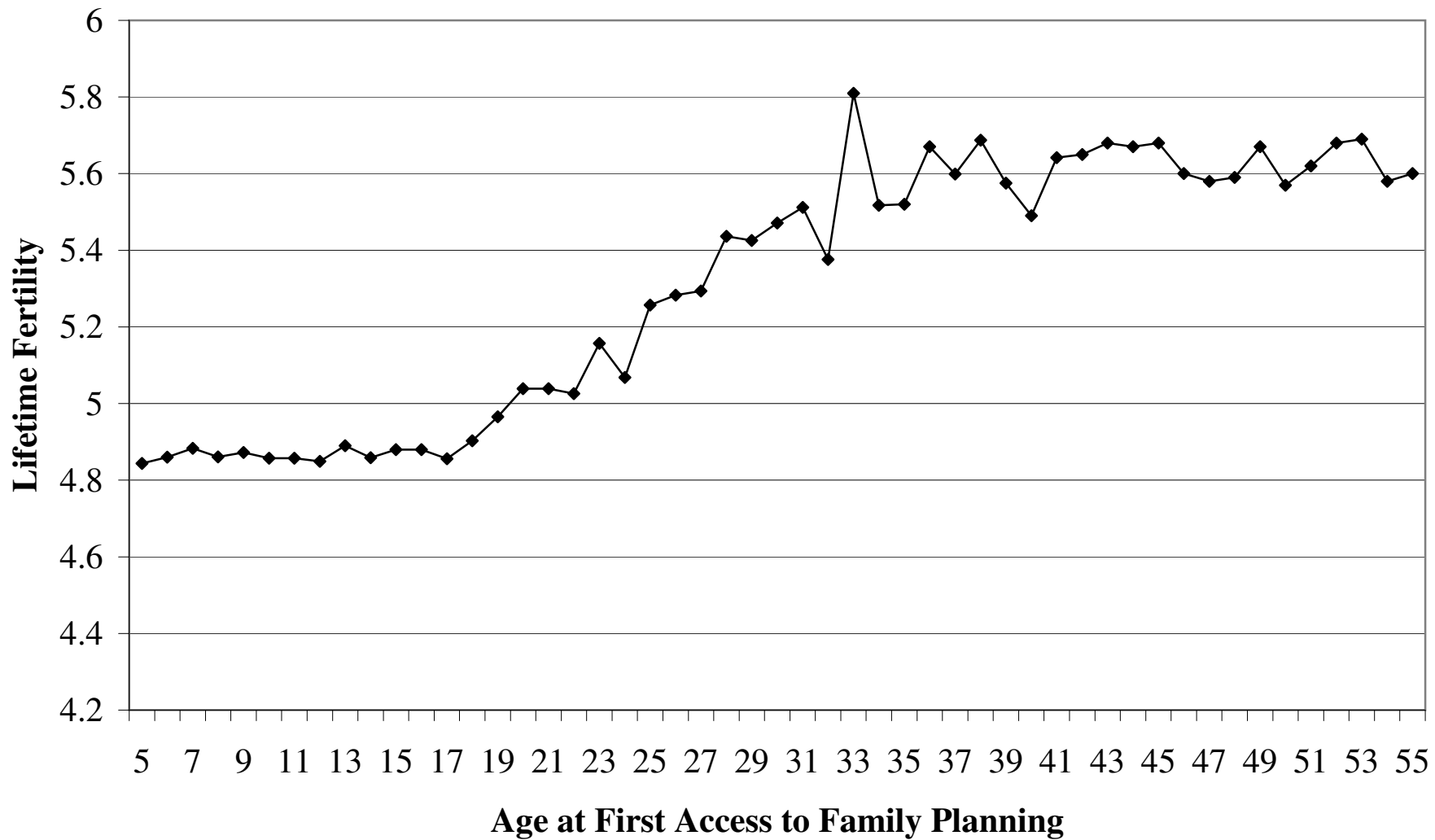
Table 1: The Timing of PROFAMILIA's Programs in Colombian Municipalities

City:	Date:	1964 Population:	City:	Date:	1964 Population:
Bogota	September 1965	1,697,311	Girardot	May 1971	68,525
Medellin	September 1966	772,887	Barrancabermeja	June 1971	71,096
Barranquilla	May 1967	498,301	Tulua	June 1971	80,394
Sogamoso	February 1968	51,639	Valledupar	October 1971	68,479
Cucuta	Mar 1968	162,945	Popayan	November 1971	76,568
Neiva	June 1968	89,790	Tumaco	January 1972	65,736
Ibaque	August 1968	163,661	Santa Marta	March 1972	104,471
Pasto	August 1968	112,876	Villavicencio	April 1972	58,430
Armero	October 1968	26,734	Caucasia	1976	21,480
Bucaramanga	October 1968	229,748	Rionegro	March 1981	30,637
Buenaventura	October 1968	96,708	Tunja	March 1985	55,823
Puerto Berrio	November 1968	21,856	Quibdo	1985	42,926
Manizales	December 1968	221,916	Soledad	1986	38,456
Cali	February 1969	637,929	Ocana	October 1987	38,445
Monteria	April 1969	124,105	Bello	1987	93,207
Pereira	June 1969	188,365	Florencia	1987	30,445
Armenia	September 1969	137,222	Riohacha	1987	31,897
Palmira	April 1970	140,889	San Andres	1987	14,413
Sincelejo	June 1970	55,705	Apartado	1988	7,304
Cartagena	September 1970	242,085	Aguablanca	1989	N/A

Notes: Program dates obtained by personal communication with PROFAMILIA. 1964 population data obtained from the 1964 population census.

Table 2: Contraceptive Prevalence by Type Among Married Colombian Women of Reproductive Age, 1969-1990

Contraceptive Method	1969	1976	1986	1990
Pill	8.5	16.2	16.4	14.1
IUD	3.5	10.4	11	12.4
Injection	N/A	0.5	2.4	2.2
Implant	N/A	N/A	N/A	N/A
Vaginal	2.3	3.4	2.3	1.7
Condom	2.3	2.1	1.7	2.9
Female Sterlization	1.6	4.9	18.3	20.9
Male Sterilization	N/A	0.2	0.4	0.5
All Modern	**18.2**	**34.1**	**52.5**	**54.6**
Rhythm	5.5	7.3	5.7	6.1
Withdrawal	8.8	5.8	5.7	4.8
All Traditional	**14.3**	**13.1**	**11.4**	**10.9**
Folk	**2.9**	**1**	**0.9**	**0.5**
All Methods	**35.4**	**51.8**	**64.8**	**66.1**

Notes: All numbers are percentages of the Colombian population obtained from Seltzer and Gomez (1998).

Table 3: Descriptive Statistics, 1973 and 1993 Colombian Population Censuses

	1973 Census	1993 Census	
	Total	Total	Never-Movers
Age	21.89	25.86	21.55
	(18.16)	(19.03)	(17.62)
Number of Children Born to Women	5.14	3.76	3.65
	(3.63)	(2.90)	(2.86)
Proportion of Children Dead if Ever Have Children	0.11	0.04	0.04
	(0.20)	(0.13)	(0.13)
Years of Education	N/A	5.29	5.32
		(3.43)	(3.49)
Share Cohabitating	0.51	0.28	0.24
Share Employed in the Formal Sector	0.48	0.55	0.55
Share of Females Ever Having Children	0.60	0.68	0.62
Share in Same Municipality for Entire Life	0.63	0.60	---
Share Moved in Past 5 years	0.33	0.15	---

Note: Standard deviations shown in parentheses

Table 4: Estimated Program Effects on Completed Lifetime Fertility Among Women Ever Having Children

Dependent Variable:	ln(Births)	Any Birth	Births	Any Birth (NB)
	Cohort Only	All Covariates	Cohort Only	All Covariates
Family Planning Access 15-19	-0.116***[B]	-0.132***[B]	-0.064***[B]	-0.072***[B]
	(0.012)	(0.027)	(0.004)	(0.012)
Family Planning Access 20-24	-0.071***[B]	-0.089***[B]	-0.036***[B]	-0.042***[B]
	(0.018)	(0.022)	(0.009)	(0.013)
Family Planning Access 25-29	-0.054***[B]	-0.062***	-0.028**	-0.038***[B]
	(0.016)	(0.025)	(0.011)	(0.010)
Family Planning Access 30-34	-0.030***[B]	-0.042***[B]	-0.015*	-0.020**
	(0.010)	(0.010)	(0.009)	(0.007)
Family Planning Access 35-39	-0.021	-0.014	-0.011	-0.001
	(0.017)	(0.016)	(0.023)	(0.017)
Family Planning Access 40-44	-0.030	-0.033	-0.029	-0.002
	(0.034)	(0.048)	(0.045)	(0.036)
Family Planning Access 40-44 Lead	-0.009	0.001	0.007	0.004
	(0.013)	(0.002)	(0.008)	(0.004)
Cohort Fixed-Effects	Yes	Yes	Yes	Yes
Municipality Fixed-Effects	No	Yes	No	Yes
Municipality-Specific Linear Trends	No	Yes	No	Yes
N	678,387	678,387	678,387	678,387
R^2	0.40	0.40	-------	-------

Notes: Standard errors clustered at the municipality level shown in parentheses. "Cohort Only" specifications include birth cohort dummy variables; "All Covariates" include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. *p<0.1, **p<0.05, ***p<0.01. [B]Significant (α=0.05) after a mutiple comparison correction (the Bonferroni multiple comparison correction for six tests of significance requires a significance threshold of α=0.0085 for each test to recover an overall significance level of α=0.05).

Table 5: Estimated Program Effects on Probability of Ever Having a Child

Dependent Variable:	Any Birth		
	Cohort Only, 1993 Census	All Covariates, 1993 Census	All Covariates, 1973 Census
Family Planning Access 15-19	0.007	-0.001	-0.028***[B]
	(0.005)	(0.008)	(0.008)
Family Planning Access 20-24	-0.005	0.009	-0.011*
	(0.003)	(0.006)	(0.006)
Family Planning Access 25-29	-0.002	0.002	-0.004
	(0.005)	(0.002)	(0.004)
Family Planning Access 30-34	-0.004	0.003	0.002
	(0.004)	(0.005)	(0.003)
Family Planning Access 35-39	0.007	-0.009	-0.021
	(0.004)	(0.007)	(0.019)
Family Planning Access 40-44	-0.011	0.017	-0.003
	(0.014)	(0.013)	(0.003)
Family Planning Access 40-44 Lead	-0.012	-0.008	-0.007
	(0.008)	(0.006)	(0.005)
Cohort Fixed-Effects	Yes	Yes	Yes
Municipality Fixed-Effects	No	Yes	Yes
Municipality-Specific Linear Trends	No	Yes	Yes
N	1,064,874	1,064,874	798,392
R^2	0.70	0.80	0.80

Notes: The results shown are marginal probabilities obtained from probit models calculated at the mean of the independent variables. Standard errors clustered at the municipality level shown in parentheses. "Cohort Only" specifications include birth cohort dummy variables. "All Covariates" include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. *p<0.1, **p<0.05, ***p<0.01. [B]Significant (α=0.05) after a mutiple comparison correction (the Bonferroni multiple comparison correction for six tests of significance requires a significance threshold of α=0.0085 for each test to recover an overall significance level of α=0.05).

Table 6: Estimated Program Effects on Socio-Economic Status Among Women Ever Having a Child

Dependent Variable:	Years of Education	Work in Formal Sector	"White-Collar" Job	Cohabitate	Share of Children Dead
Family Planning Access 15-19	0.142***[B]	0.027***[B]	0.019	-0.023**	-0.002
	(0.048)	(0.007)	(0.015)	(0.011)	(0.002)
Family Planning Access 20-24	0.067**	0.033***[B]	0.009	-0.021***[B]	0.001
	(0.031)	(0.009)	(0.013)	(0.004)	(0.003)
Family Planning Access 25-29	0.097	0.006	0.002	-0.008	0.003*
	(0.078)	(0.008)	(0.009)	(0.006)	(0.002)
Family Planning Access 30-34	-0.012	0.009	-0.008	-0.014**	-0.002
	(0.083)	(0.011)	(0.016)	(0.007)	(0.003)
Family Planning Access 35-39	-0.026	-0.005	-0.013	0.010	-0.003
	(0.033)	(0.014)	(0.016)	(0.008)	(0.002)
Family Planning Access 40-44	-0.011	-0.012	0.049	0.004	0.004
	(0.064)	(0.012)	(0.037)	(0.021)	(0.004)
Family Planning Access 40-44 Lead	-0.016	0.011	-0.004	0.004	-0.002
	(0.048)	(0.019)	(0.032)	(0.014)	(0.003)
N	669,446	631,784	169,285	643,854	624,399
R^2	0.23	0.15	0.04	0.08	0.07

Notes: The results shown for *Work in Formal Sector*, *"White-Collar" Job*, and *Cohabitate* are marginal probabilities obtained from probit models calculated at the mean of the independent variables. All specifications include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. Standard errors clustered at the municipality level shown in parentheses. *$p<0.1$, **$p<0.05$, ***$p<0.01$. [B]Significant ($\alpha=0.05$) after a mutiple comparison correction (the Bonferroni multiple comparison correction for six tests of significance requires a significance threshold of $\alpha=0.0085$ for each test to recover an overall significance level of $\alpha=0.05$).

Table 7: Estimated Pre-Program Effects on Various Outcomes

Dependent Variable	Estimate[§]	Std Err	N	R^2
Any Birth (1993 Census)	0.004	(0.004)	121,738	0.26
Number of Children if Any Birth (1993 Census)	0.010	(0.010)	104,413	0.08
Share of Children Dead if Any Birth (1993 Census)	0.124	(0.234)	104,413	0.01
Years of Education (1993 Census)	0.026	(0.027)	104,413	0.18
Cohabitate (1993 Census)	0.002	(0.006)	104,413	0.08
Work in the Formal Sector (1993 Census)	-0.003	(0.004)	104,413	0.07
"White-Collar" Job if Work in Formal Sector (1993 Census)	0.011	(0.034)	36,458	0.20
Any Birth (1973 Census)	-0.001	(0.003)	142,937	0.21
Number of Children if Any Birth (1973 Census)	0.004	(0.005)	128,294	0.09
Share of Children Dead if Any Birth (1973 Census)	0.001	(0.002)	128,294	0.04
Cohabitate (1973 Census)	0.002	(0.004)	128,294	0.09
Work in the Formal Sector (1973 Census)	0.003	(0.003)	128,294	0.08

Notes: The results shown for *Any Birth*, *Cohabitate*, *Work in Formal Sector*, and *"White-Collar" Job* are marginal probabilities obtained from probit models calculated at the mean of the independent variables. All specifications include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. Census year (1973 or 1993) shown in parentheses next to each dependent variable. Standard errors clustered at the municipality level shown in parentheses. [§]Estimates are for those 45-49 when family planning programs began (relative to those 50-54) except for years of education, which is for those 25-29 when programs began (relative to those 30-34). *p<0.1, **p<0.05, ***p<0.01.

Table 8: Estimated Program Effects on Socio-Economic Status Among Women Never Having a Child

Dependent Variable:	Years of Education	Work in Formal Sector	"White-Collar" Job	Cohabitate
Family Planning Access 15-19	0.046	0.010	0.005	-0.002
	(0.167)	(0.014)	(0.029)	(0.010)
Family Planning Access 20-24	-0.071	-0.010	0.037	-0.016
	(0.144)	(0.017)	(0.031)	(0.013)
Family Planning Access 25-29	-0.306	0.007	0.046	0.012
	(0.182)	(0.028)	(0.067)	(0.020)
Family Planning Access 30-34	0.309	0.006	-0.008	0.018
	(0.230)	(0.025)	(0.017)	(0.017)
Family Planning Access 35-39	-0.050	-0.004	-0.024	-0.021
	(0.034)	(0.010)	(0.079)	(0.021)
Family Planning Access 40-44	-0.010	-0.012	0.139	-0.003
	(0.092)	(0.045)	(0.136)	(0.036)
Family Planning Access 40-44 Lead	0.025	-0.026	-0.002	-0.050
	(0.068)	(0.041)	(0.120)	(0.031)
N	191,433	186,829	47,128	188,763
R^2	0.67	0.15	0.08	0.07

Notes: The results shown for *Work in Formal Sector*, *"White-Collar" Job*, and *Cohabitate* are marginal probabilities obtained from probit models calculated at the mean of the independent variables. All specifications include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. Standard errors clustered at the municipality level shown in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table 9: Estimated Program Effects on Fertility and Socioeconomic Outcomes Among Never-Movers

Dependent Variable:	ln(Births) │ Any Birth	Births │ Any Birth (NB)	Any Birth	Years of Education	Work in Formal Sector	"White-Collar" Job	Cohabitate
Family Planning Access 15-19	-0.118***[B]	-0.064***[B]	0.001	0.114***[B]	0.020***[B]	-0.007	-0.032***[B]
	(0.026)	(0.016)	(0.005)	(0.036)	(0.005)	(0.017)	(0.009)
Family Planning Access 20-24	-0.076***[B]	-0.043***[B]	0.012	0.058**	0.037***[B]	-0.005	-0.020**
	(0.019)	(0.013)	(0.008)	(0.024)	(0.010)	(0.021)	(0.009)
Family Planning Access 25-29	-0.041***	-0.031**	0.008	0.038	-0.007	0.000	-0.015
	(0.013)	(0.014)	(0.006)	(0.035)	(0.010)	(0.023)	(0.011)
Family Planning Access 30-34	-0.032	0.002	0.006	0.016	-0.018	0.006	0.001
	(0.018)	(0.018)	(0.006)	(0.024)	(0.012)	(0.028)	(0.009)
Family Planning Access 35-39	0.018	0.015	0.004	-0.043	-0.020	0.026	-0.005
	(0.024)	(0.019)	(0.008)	(0.057)	(0.016)	(0.049)	(0.012)
Family Planning Access 40-44	-0.028	-0.009	0.017	-0.031	-0.028	0.002	0.014
	(0.039)	(0.007)	(0.014)	(0.093)	(0.026)	(0.008)	(0.033)
Family Planning Access 40-44 Lead	0.042	-0.012	0.015	0.026	-0.020	0.009	0.008
	(0.035)	(0.021)	(0.012)	(0.025)	(0.021)	(0.009)	(0.019)
N	413,833	413,833	651,184	409,217	386,474	112,377	394,756
R^2	0.41	-------	0.82	0.26	0.13	0.05	0.08

Notes: The results shown for *Any Birth*, *Cohabitate*, *Work in Formal Sector*, and *"White-Collar" Job* are marginal probabilities obtained from probit models calculated at the mean of the independent variables. All specifications include migration history, municipality dummies, cohort dummies, and municipality-specific linear time trends. Standard errors clustered at the municipality level shown in parentheses. *p<0.1, **p<0.05, ***p<0.01. [B]Significant (α=0.05) after a mutiple comparison correction (the Bonferroni multiple comparison correction for six tests of significance requires a significance threshold of α=0.0085 for each test to recover an overall significance level of α=0.05).

Table 10: Estimated Effects of Mother's Lifetime Family Planning Access on Child Socio-Economic Status

Dependent Variable:	Attending School	Work in Formal Sector	Years of Education	Any Birth	[§]Share of Children Dead
Mother's Access 15-19 (Boys and Girls)	0.019***	-0.016**	0.077***	-------	-------
	(0.007)	(0.008)	(0.021)		
Mother's Access 15-19 (Boys Only)	0.033***	-0.091***	0.090***	-------	-------
	(0.009)	(0.010)	(0.029)		
Mother's Access 15-19 (Girls Only)	0.007	0.047**	0.066**	-0.005**	0.001
	(0.006)	(0.021)	(0.031)	(0.002)	(0.003)

Notes: Each cell is produced by a different regression and shows an estimate for statistical mother family planning access at ages 15-19 (relative to no access at reproductive ages). The results shown for *Attending School*, *Work in Formal Sector*, and *Any Birth* are marginal probabilities obtained from probit models calculated at the mean of the independent variables. All specifications include statistical mother's age, statistical mother's migration history, own migration history, municipality dummies, cohort dummies, and municipality-specific linear trends. Standard errors clustered at the municipality level shown in parentheses. *p<0.1, **p<0.05, ***p<0.01. [§]For the sake of sample size, this regression uses all girls in the census ages 10-19 with children of their own.