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The Production of Reproduction: Economics of sexual behavior, reproduction, and child-rearing in Africa

Author

Jones, Kelly M

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The Production of Reproduction:
Economics of sexual behavior, reproduction, and child-rearing in Africa

By

Kelly M Jones

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Committee in charge:

Professor Elisabeth Sadoulet, Chair
Professor Alain de Janvry
Professor Jeremy Magruder
Professor Edward Miguel

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Abstract

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Economic factors affect nearly every human decision, even those we consider most personal. This body of work demonstrates the presence of economic reasoning in the unexpected realms of sexuality and reproduction. Focusing on Sub-Saharan Africa, where financial concerns are often pressing, I show evidence for the influence of economics on child-rearing, sexual behavior, and reproduction, respectively.

On child-rearing, I model the parents' decision regarding investments in their children, in particular, investments in child health. The question of interest is how a child's cohort within the household affects the amount and type of investment she receives. Partitioning investments into private goods and club goods for children reveals that the size and gender composition of one's cohort affect these types of investments in opposite ways. I use data from Senegal to test the prediction that, *ceteris paribus*, children with larger (and more male) cohorts will receive more club investments. Employing a new method to deal with the endogeneity of siblings in this type of analysis, I exploit a unique characteristic of this data. The predominance of multi-family, cooperative households in this setting offers the existence of non-sibling cohorts that can instrument for a child's full cohort within the household. Using a 2SLS within-household estimation, I find that club investments are increasing in cohort size and male composition. This finding is particularly relevant to child health in Africa, where club investments such as water purification, bed nets, and immunizations could prevent 60% of child death.

In addition, this work builds on existing theories regarding sexual behavior responses to low-income shocks. Social scientists have suggested that African women use transactional sex for both income smoothing and insurance. In an environment of epidemic HIV, increases in casual partnerships, or increases in risks taken within partnerships can increase HIV-risk to a woman and her community. This work shows evidence of this dynamic. I employ individual serostatus data and overlay it with historical, village-level weather data across 19 countries in Africa. I find that when droughts cause economic hardship in rural Africa, women are significantly more likely to become

infected with HIV. Concentration of this effect among women of little means, and the presence of a counterpart effect in men of great means, suggest a behavioral pathway. These findings indicate that crop insurance and social safety nets could significantly stem the spread of HIV in Sub-Saharan Africa.

This work also highlights the impact that economic policies can have on reproductive decisions, even taken at a great distance. Several times in the past quarter-century, the US has employed an economic policy to achieve a social objective: reduce the use of abortion abroad. By withholding funding to certain foreign NGOs, the policy rather had the effect of reducing the availability of contraceptives in poor countries. I estimate the impact of the policy on the use of abortion in Ghana, creating a woman-by-month panel over 25 years and exploiting the off-on-off-on history of the policy. Findings suggest that the policy did not reduce the use of abortion. Further, the reduced contraceptive availability resulted in increased pregnancy rates for rural women – the explaining factor for why the policy *increased* use of abortion among these women. In Ghana alone, the policy resulted in nearly 100,000 additional abortions and up to 500,000 additional unplanned births.

Dedication

To my ever-supportive wife, Danielle.

Your patience astounds me.

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Acknowledgments

Thanks are due to everyone who encouraged me along this path. At times, the destination was unclear. But all winding paths have lead here.

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Chapter 1

Introduction

Economic considerations permeate nearly every part of human life. Even in aspects of life deemed highly personal, such as sex and reproduction, financial concerns exist. This body of work demonstrates the considerable role that economic factors play in decisions regarding the most personal aspects of life: sexual behavior, reproduction, and child-rearing. In particular, the works here focus on such "personal economics" in the context of Sub-Saharan Africa, where financial concerns are likely to be most pressing.

In the next chapter, I consider the cost-benefit analysis that is undertaken by every parent, nearly every day. Goods for children such as food, health care, and education are costly. Yet, the provision of such goods improves the quality of the child's life, both currently and in the future. In this way, such expenditures are considered investments in children. For each investment, parents must decide whether the benefit outweighs the cost.

In Sub-Saharan Africa, investments in child health as simple as immunizations, bed nets, or water purification could prevent sixty percent of child deaths. Yet, even outside the issues of poverty and service provision, some parents simply decide not to provide these basics. Chapter 2 examines such decisions at the household level, focusing on the role of a child's cohort within the household. Existing economic theory predicts that children raised in large cohorts would receive less investment (Becker's quantity-quality trade-off), and that son preference would create gender crowding-out. I present a model of the household decision that separates investments in children into private goods and club goods. This model predicts that both cohort size and male composition reduce private goods per child but increase club investments in children.

I test the prediction in the context of rural Senegal, using data on childhood immunizations. In this setting, immunizations are a club good since the primary cost is (non-rival) adult travel- and wait-time at clinics. As in any study of cohort effects, relying on a child's siblings as a measure of cohort would introduce endogeneity. Surely unobserved parental characteristics have determined both fertility and investment decisions. This data setting provides a unique solution in that most households in rural Senegal are composed of multiple nuclear families, so that a child's cohort is composed of both siblings and non-sibling children. I can thus deal with the endogeneity of siblings by instrumenting cohort size and gender composition with the characteristics of the non-sibling portion of the cohort within the household. Estimating within-household, I find that children with larger (or more male) cohorts of vaccine-eligible age are significantly more likely to receive immunization. Such evidence has implications for child health in Africa, and highlights the role of economic concerns in attempts to improve it.

The third chapter demonstrates the importance of financial matters in very personal decisions regarding sexual behavior. The demonstrated connection between the two suggests potential for new programs to combat the HIV/AIDS epidemic. Numerous causes have been suggested for the epidemic's determined foothold in Sub-Saharan Africa. Among them is the proposal that exposure to income shocks induces behavioral responses that increase the risk of HIV transmission. Such responses include increasing the frequency of, or risks taken during, transactional sex, or expanding one's sexual network to acquire informal insurance against future shocks. I present in Chapter 3 evidence of the impacts of this phenomenon on a widespread scale in SSA.

Lacking modern irrigation, substandard rainfall in Africa reduces crop yields, potentially inducing economic hardship, especially in rural areas. We find that each local shock of this kind over the preceding 10 years predicts an increase in HIV infections in rural women of up to 13%, depending on the existing prevalence. Further, the evidence suggests that the effects are concentrated among the most vulnerable women – those with low levels of wealth and education. These findings indicate a behavioral pathway between economic hardship and HIV. This suggests a role for formal insurance and social safety nets in tackling the epidemic.

The final chapter considers the impact of economic policies on the reproductive outcomes of individuals. The Mexico City policy, known derisively as the global gag rule, is a U.S. policy that restricts USAID funding to foreign NGOs that perform or promote abortion abroad. Chapter 4 examines whether this policy achieves its stated aim of reducing the use of abortion as a means of family planning in countries receiving US assistance, in particular, Ghana.

I employ woman-level fixed effects and a regression discontinuity design, focusing on the 30 months on either side of each change in the policy. I do not find any demographic group that exhibits a significant decrease in abortion as a share of pregnancies during policy periods. On the contrary, rural women significantly increase their use of abortion. This affect seems to arise from their increased rate of conception during these times. The policy-induced budget shortfalls reportedly forced NGOs to cut rural outreach services, reducing the availability of contraceptives in rural areas. The lack of contraceptives likely caused the observed 6% increase in rural pregnancies, ultimately resulting in about 100,000 additional abortions and over 250,000 additional unintended births in Ghana alone. In using economic power to achieve social objectives, the U.S. materially affected the reproductive rights and decisions of women in Ghana.

The body of this dissertation comprises the subsequent three chapters. Each presents evidence and commentary on the link between economic considerations and personal decisions regarding child-rearing, sexual behavior, and reproduction, respectively. Taken together, the work advances theoretical considerations of child investment decision-making, and provides broad-based evidence for existing theories of sexual behavior response to income shocks. The work also offers policy implications on issues including childhood immunization, crop insurance to reduce HIV transmission, and foreign policy legislation in Washington.

Chapter 2

Growing Up Together: Cohort composition and Child Investment

2.1 Introduction

In Sub-Saharan Africa, one in eight children die before age 5. Sixty percent of these deaths are preventable by investments in child health as simple as immunizations, bed nets, or water purification. Poverty and poor service provision are clearly obstacles to such investments. Yet, even households for which these goods are attainable must still make the decision to invest. Factors that may influence such a decision are the focus of this work.

It is not a new observation to state that the number and the composition of children within a household will affect the allocation of resources to these children. Indeed this is central to the seminal work by Becker (1960) suggesting that a quantity-quality trade-off exists in child rearing. In addition, scholars have posited that, in the presence of son preference, boys will "crowd out" girls within the household. That is, girls with more brothers will receive less investment themselves. Both of these theories rest upon the assumption of competition for limited resources within the household.

The contribution of this work is to distinguish between these theories of resource competition among children, which are relevant specifically to private goods, and the opposite phenomena with regard to non-rival investments in children. Non-rival investments in children can be thought of as a "club good" of sorts, such that, for those within the club (in this case, children) the consumption of the good by one member does not diminish the availability of the good to other members. Prominent examples of club goods in child rearing include non-rival adult attention to children (e.g. reading, play interactions), transportation to beneficial services or activities, or non-rival educational games and toys. Important in poor countries are non-rival investments to reduce the contagion of childhood diseases (e.g. a home water sanitation system, mosquito netting, cement floors, clean-air cook system). Even preventative and curative health care for children is not fully private – when the bulk of the cost is adult travel and wait time at the clinic, any children needing to go would be taken together. While much allocation to children is in the form of private goods (e.g. food, educational fees), many investments in child health are fully or partially non-rival.

Given resource competition for private goods, we expect that, *ceteris paribus*, children in larger cohorts will receive less private investments, as will children that have a larger share of their cohort composed of boys (if there is any preference for sons). In contrast, larger and more dominantly male cohorts should predict *greater* investment in club goods for children. Therefore, with regard to club goods, children benefit from, rather than compete with, their cohort; and boys may "crowd in" rather than "crowd out" investment for girls.

In section 2.3, I formalize this proposition using a standard household CES utility function, where the decision-maker is allocating resources to adult goods, private goods to boys, private goods to girls, and club goods for children. In order to derive unambiguous predictions from the model, I simulate data for 49,000 households with unique household compositions and preference parameters. After numerically solving for the optimal bundle in each household, I analyze how allocation responds to household composition within a range of parameters. The model predicts that private goods per child are decreasing in the number of children (Becker's thesis) but that club goods are increasing in that number. Further, given any amount of son preference, private goods per child are decreasing in the share of boys within the cohort, but that consumption of club goods is increasing in that share.

In section 2.4, I present an empirical test of the predictions of the model regarding club goods for children. In particular I deal carefully with the endogeneity of sibling cohort composition, given parental preferences that correlate fertility and child investment decisions. To do this, I exploit the uncommonly large households that are the norm in Senegal, generally composed of extended families and containing multiple mothers of young children. A child's non-sibling coresident children provide the same cohort effects with regard to club goods, but are exogenous with respect to the fertility decisions of the child's mother.

Owing to the large adult time cost required to take children to the health clinic in rural Senegal, the receipt of immunizations is the club good examined in the analysis. The results presented in section 2.7 suggest that each additional member of one's cohort increases the probability of immunization by about 3 percentage points (on a mean of 71%), suggesting that immunization does act as a club good in this context. Further, the benefit from cohort members differs significantly depending on the gender of the additional member. At the extreme, for a given cohort size, the probability of immunization increases by an additional 4.5 percentage points if one's cohort is entirely male rather than entirely female.

Having presented both a theoretical foundation and empirical evidence on the matter, in section 2.5, I conclude that, despite negative cohort effects for consumption of private goods, children benefit from larger cohorts with respect to club goods. Further, while son preference can cause crowding out of girls in private goods consumption, a girl's (and boy's) consumption of club goods is increasing in the male-composition of her cohort. The net result of these conflicting cohort effects will, in the end, depend on the relative importance of private and club goods in child welfare and human capital development. However, given that many investments in child health are club goods, these findings have specific relevance to improving child health and reducing child mortality.

2.2 Existing evidence of cohort effects

Cohort Size

The theory of a quantity-quality trade-off in child rearing, first proposed by Becker (1960) and then formalized by Becker and Lewis (1973), suggests that investments in children should be decreasing in the size of their cohort. It can be easily shown in a broad range of settings that family size and child outcomes exhibit a negative correlation. However, few researchers have been able to establish a *causal* effect of cohort size on outcomes. Studies that deal seriously with the endogeneity of fertility choices often do not find the predicted negative effect of cohort size on schooling or health outcomes (Angrist, Lavy, and Schlosser (2006); Black, Devereux, and Salvanes (2005, 2007); Caceres-Delpiano (2006); Qian (2009)). Such studies generally focus on aggregate outcomes for children, so that the decrease in private goods investment is potentially offset by increased club goods, yielding an indistinguishable effect.

Only one of paper (of which I am aware) finds a significant negative causal effect of an additional child on the average investment across all children in the household (Rosenzweig and Zhang, 2006).[1] In this case, the reduced investment was measured in terms of schooling progress and college enrollment, generally private goods. Interestingly, the authors find a contrasting positive effect of cohort size on adult time spent helping with homework and the likelihood of having an internet connection, both club goods.

Cohort Gender Composition

In addition to cohort size, a child's own gender may also affect parental decisions regarding allocation of resources. In some contexts, parents may view investment in sons as having a higher potential return, either due to patrilocal customs or differential labor market opportunities for women. Further, the opportunity cost of child investment may be higher for girls than boys (as girls are needed for domestic work). And finally, parents may exhibit a pure preference for sons as a means to carry the lineage. Though unable to distinguish between differential returns, opportunity costs or pure

[1] Rosenzweig and Zhang (2006) is also different from the others in that they account for potential differences in endowments (birth weights) introduced by the "twins strategy".

preferences, several empirical studies have shown evidence of son preference in child investment in diverse cultural contexts.[2]

Given that boy-children may command a greater share of household resources than girl-children, some scholars have suggested that girls who have more brothers are worse off (holding total cohort size constant). That is, assuming that parents allocate a fixed share of resources to children, the resources available for a given child are decreasing in the share of her cohort that is male.

Evidence for such gender "crowding out" has been mixed. While some studies focused solely on private investments such as education have found negative effects of a male cohort,[3] effects on composite outcomes such as health and mortality are ambiguous. In India, Makepeace and Pal (2008) show evidence of gender crowding out, in that boys with more male siblings have higher mortality rates. But both Pande (2003) and Mishra, Roy, and Retherford (2004) examine anthropometric measures for children in India and find that in some cases girls are actually disadvantaged by sisters, and not brothers.[4] In Ghana, Garg and Morduch (1998) find that anthropometric measures for children are 25-40% worse for a child with an all-boy cohort (versus all girls). However, the authors also find a contrasting positive effect of the indicator for having *any* brothers. Such seemingly contradictory results might be explained by corresponding increases in club goods investment and decreases in private investment in response to male composition of the cohort.[5]

[2] See studies based in Nepal, Japan, Egypt, the U.S., China, and India: Edmonds 2006; Ono 2004; Yount 2003; Lundberg 2005; Gong, van Soest, and Zhang 2005; Asfaw, Lamanna, and Klasen 2010; Duraisamy and Duraisamy 1995; Rose 2000, respectively.

[3] Regarding education, studies in rich countries have found no evidence of gender crowding: see Butcher and Case 1994; Hauser and Kuo 1998; Kaestner 1997 in the US; Amin 2009 in the UK; and Bauer and Gang 2001 in Germany. Evidence of gender crowding of education in poor and middle income countries includes: Parish and Willis 1993 in mid-century Taiwan; Morduch 2000 in Tanzania & South Africa; Bommier and Lambert 2004 in Brazil; Ota and Moffatt 2007 and Kambhampati and Rajan 2008 in India; Rammohan and Dancer 2008 in Egypt; and Dayioglu, Kirdar, and Tansel 2009 in Turkey. In several of these studies, the effects are strongest for (or are restricted to) households that face greater resource constraints, which may explain why such effects are not observed in higher income countries.

[4] The authors' explanation is that selective neglect arises from a desire for gender balance, rather than a pure desire for sons.

[5] The authors instead attribute this unexpected result to psychological phenomena: "reference effects" for boys, that is, a lone boy with all sisters is treated more like a girl; and "spillover effects" for girls, that is, girls adopt and/or are taught more "masculine" traits in the presence of a brother.

2.3 A theoretical representation of household allocation

Employing a standard social utility function for a consensus household, I assume that the household's utility is a function of the utilities of each of its members

$$\mathcal{U} = \tilde{U}(U_1, U_2, ..., U_T)$$

for a household with members $i = (1...T)$. Dividing the household members into relevant groups, I index adults with $a = (1...N_a)$, boys with $b = (1...N_b)$ and girls with $g = (1...N_g)$, so that total household size is $T = N_a + N_b + N_g$. Assuming a standard CES form of utility, household welfare can be represented as

$$\mathcal{U} = \left[\sum_{a=1}^{N_a} w_a U_a^\rho + \sum_{b=1}^{N_b} w_b U_b^\rho + \sum_{g=1}^{N_g} w_g U_g^\rho\right]^{1/\rho}$$

where $\rho = 1/\sigma - 1$, σ is the elasticity of substitution between the welfare of different individuals, and w's are utility weights such that weights for all individuals in the household sum to one; that is,

$$\sum_{a=1}^{N_a} w_a + \sum_{b=1}^{N_b} w_b + \sum_{g=1}^{N_g} w_g = 1.$$

Let us assume that each adult has identical individual utility equal to the amount of private goods consumed by the individual (A, measured in currency units, so that prices are unity). Then, simply, $U_a = A$ and

$$\sum_{a=1}^{N_a} w_a U_a^\rho = N_a w_a A^\rho.$$

For children, utility is determined by the consumption of both private goods (I) and club goods for children (J), also measured in expenditure units. Private and club goods for children are assumed to produce utility in a Cobb-Douglas form

$$U_b = I_b^\alpha J^{1-\alpha}; \qquad U_g = I_g^\alpha J^{1-\alpha}.$$

Given this, each household will maximize total welfare by choosing the optimal amounts of consumption of goods for adults (A), private goods for boys (I_b), private goods for girls (I_g), and club goods for children (J). That is, households solve the problem:

$$\max_{A, I_b, I_g, J} \quad U = \left[N_a w_a (A)^\rho + N_b w_b (I_b^\alpha J^{1-\alpha})^\rho + N_g w_g (I_g^\alpha J^{1-\alpha})^\rho\right]^{1/\rho} \tag{2.3.1}$$

$$\text{s.t. } N_a A + N_b I_b + N_g I_g + J \leq Y$$

The optimal allocation will be determined not only by household composition and household budget (Y), but also by the degree to which child investments are valued relative to adult consumption (κ), and the degree to which investing in girls is valued relative to investing in boys (γ). These parameters enter the problem through the utility weights w, such that

$$w_a = \frac{1}{\tau}\ ;\ w_b = \frac{\kappa}{\tau}\ ;\ \text{and}\ \ w_g = \frac{\kappa\gamma}{\tau}$$

$$\text{for } \ \tau = N_a + \kappa N_b + \kappa\gamma N_g$$

The expression τ is a measure of household size in adult-equivalent units, given the discount on children, κ, and the further discount on girls, γ. The discounts κ and γ are assumed to vary across households, within the unit interval; that is $\kappa, \gamma \in (0, 1]$.

Predictions

In order to conduct analysis on the impact of number and gender composition of the children within the household. I first transform the problem in the following way. Let $N_K = N_b + N_g$, the total number of children, and $z = \frac{N_b}{N_K}$, the share of children that are boys. Now the problem can be expressed in terms of z as:

$$\begin{aligned}
\max_{A, I_b, I_g, J} \quad U &= \left[N_a w_a (A)^\rho + z N_K w_b (I_b^\alpha J^{1-\alpha})^\rho + (1-z) N_K w_g (I_g^\alpha J^{1-\alpha})^\rho\right]^{1/\rho} \\
&= \left[N_a \frac{1}{\tau}(A)^\rho + z N_K \frac{\kappa}{\tau}(I_b^\alpha J^{1-\alpha})^\rho + (1-z) N_K \frac{\kappa\gamma}{\tau}(I_g^\alpha J^{1-\alpha})^\rho\right]^{1/\rho}
\end{aligned}$$

$$\text{s.t. } N_a A + z N_K I_b + (1-z) N_K I_g + J \leq Y$$

$$\text{for } \ \tau = N_a + \kappa z N_K + \kappa\gamma(1-z) N_K$$

The number and gender composition of children in the household will affect investment in children according to the partial derivatives

$$\frac{\partial I_b}{\partial N_k}, \frac{\partial I_g}{\partial N_k}, \frac{\partial J}{\partial N_k}, \frac{\partial I_b}{\partial z}, \frac{\partial I_g}{\partial z}, \text{ and } \frac{\partial J}{\partial z}$$

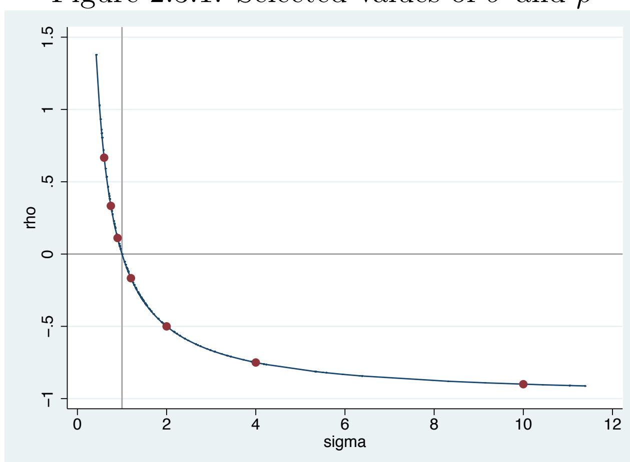
Deriving the comparative statics fails to yield unambiguous signs for these partial derivatives, as the associated cross-partials are difficult to ascertain. Therefore, I simulate household data with known preference parameters $\alpha, \rho, \kappa, \gamma$ that vary across households, and solve numerically for each household's optimized bundle. Based on this data, I ascertain the sign and relative magnitude of the relevant partial derivatives for given parameter values.

2.3.1 Simulation of partial effects

In this section I describe the method by which I derive the sign and relative magnitudes of the effects of cohort size and gender composition on child investment as predicted by the theoretical model described above.

The elasticity of substitution is assumed to be in the range $(0, 1) \cup (1, \infty)$ so that goods are considered substitutes ($\sigma > 0$) but not perfect substitutes ($\sigma < \infty$) and are not consumed in constant shares ($\sigma \neq 1$). The relationship between σ and ρ is shown in figure 2.3.1. Values of σ are assigned from the set $\{0.6, 0.75, 0.9, 1.2, 2, 4, 10\}$, implying a set of values for ρ that is $\{-0.9, -0.75, -0.5, -0.1\overline{66}, 0.\overline{11}, 0.\overline{33}, 0.\overline{66}\}$, the elements of which are marked on figure 2.3.1. The elasticity of child utility with respect to private goods, α, has a theoretical range $[0, 1]$. I assume here that child welfare is neither composed entirely of private goods nor entirely of public goods, and as such, I assign α a value from the set $\{.2, .3, .4, .5, .6, .7, .8\}$.

Figure 2.3.1: Selected values of σ and ρ



For each combination of α and ρ values, 1,000 households are simulated. Household composition values are drawn from distributions chosen to closely match distributions observed in the data employed for the empirical test in section 2.4. Distributions of household size, share of adults, number of children, and male share of children are shown in figures 2.3.2 for both the simulated and observed data. For each household, independent values for κ and γ are drawn, where $\kappa \sim N(.7, .1)$ and $\gamma \sim N(.7, .1)$, and a budget constraint is drawn from $Y \sim \ln N(4.5, .45)$. The distribution of realized values of these parameters are shown in figure 2.3.3.

Figure 2.3.2: Distributions of T, ${}^{N_a}\!/_{T}$, N_k, and z

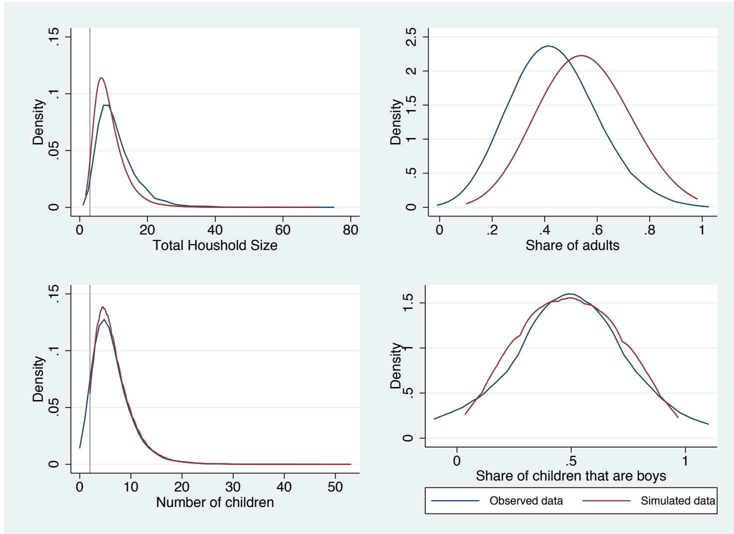
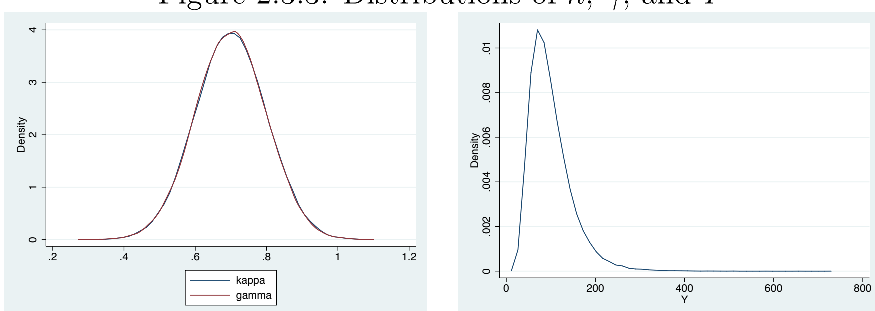


Figure 2.3.3: Distributions of κ, γ, and Y



Having established a household composition and relevant parameters, I use the MATLAB tool “fmincon” to find an approximate solution to expression 2.3.1 for each household. Beginning from an initial bundle, the tool searches for the minimum of the opposite of the household utility function, where the maximum constraint violation is less than 1×10^{-13}. The minimum is assumed to be found when either (i) the magnitude of the search direction is less than 2×10^{-6}, or (ii) the magnitude of the directional derivative in the search direction is less than 2×10^{-13}.

Given a set of 49,000 households with known compositions and optimal consumption bundles, I can estimate the partial derivatives noted above. Using multivariate regression to hold constant all other factors that determine consumption, the partial effects of each parameter on each type of consumption good are given by the standardized beta coefficients shown in the first four columns of table 2.1. We see that all consumption is increasing in the budget, Y. Private consumption per person is decreasing in total household size and the size of any group. However, consumption of club goods for children is increasing in the number of children. Specifically, for each additional child in the cohort, club goods increase by 0.23 standard deviations and private goods decrease by about 0.13 sd.

Table 2.1: Simulation Predictions from Theoretical Model:
Partial derivatives of consumption goods wrt composition and preference parameters.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A	Ib	Ig	J	A	Ib	Ig	J
Y	0.407	0.281	0.308	0.493	0.407	0.281	0.308	0.493
N_a	-0.218	-0.145	-0.157	-0.251	-0.225	-0.150	-0.162	-0.248
N_k	-0.181	**-0.121**	**-0.138**	**0.233**	-0.297	**-0.196**	**-0.222**	**0.273**
${N_k}^2$					0.066	**0.043**	**0.048**	**-0.023**
z	-0.032	**-0.018**	**-0.020**	**0.043**	-0.033	**-0.019**	**-0.021**	**0.044**
z^2					*-0.004*	**-0.005**	**-0.006**	***0.002***
κ	-0.062	0.050	0.051	0.075	-0.062	0.050	0.051	0.076
γ	-0.029	-0.017	0.068	0.035	-0.028	-0.016	0.068	0.035
σ	-0.028	0.028	0.072	-0.008	-0.031	0.027	0.070	-0.007
α	0.020	0.225	0.250	-0.430	0.021	0.226	0.251	-0.431
Cons.	-0.039	-0.096	0.009	-0.062	-0.101	-0.133	-0.033	-0.041
Obs.	320688	320688	320688	320688	320688	320688	320688	320688

Notes: Standardized beta coefficients reported. Nearly all coefficients are significantly different from zero at the 0.1% level. Exceptions: the effect of z^2 on A is significant at the 5% level; the effect of z^2 on J is not distinguishable from zero.

We further see that as the valuation of children increases (κ), adult consumption decreases and all child investments increase. As the relative valuation of girls increases (γ), both adult consumption and private investments in boys decrease, while private investments in girls and club goods for children increase. The consumption of club goods is decreasing in the relative valuation of private goods (α). Though the preference parameters have substantially smaller magnitudes of impact on consumption than the household composition and budget parameters do, all effects are statistically significant at the 0.1% level.

Finally, examining gender composition, we see that the male share of children in the household (z) decreases private investment for both boys and girls, but increases club goods. In particular, a child with an all male cohort would consume 0.04 sd more club goods and 0.02 sd less private goods than a child with an all female cohort. Thus, the net impact of gender composition on child welfare will be determined by the α parameter, which defines the relative importance of private vs. club goods.

In the last four columns of table 2.1, I check for non-linear effects of cohort size and gender composition. The predicted effects of N_k and z on both private and club goods are shown over a range of values for N_K and z in figure 2.3.4. the responses of both private and club goods to N_k is slightly non-linear, with both effects waning and cohort size grows very large. In contrast, the effect of gender composition on private goods increases as the cohort becomes nearly all male. However, the response of club goods to cohort gender seems constant for any male/female ratio (and the coefficient on the squared term is not statistically different from zero).

Figure 2.3.4: Non-linear impacts of N_k and z on child investments

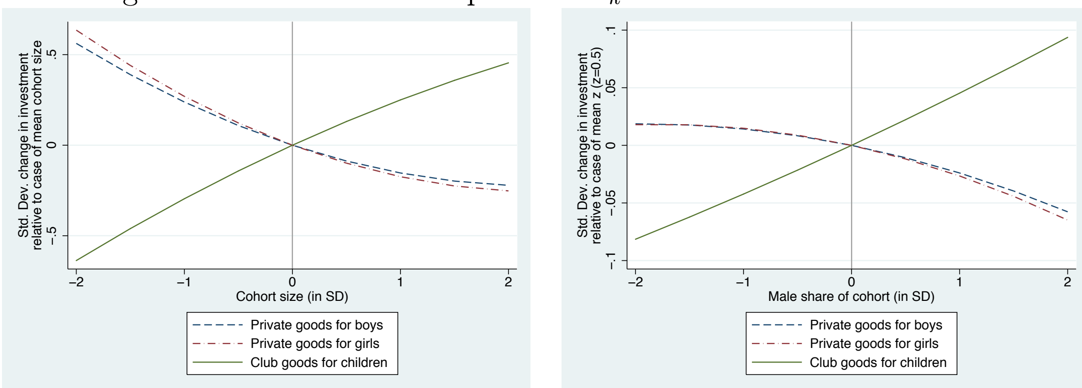
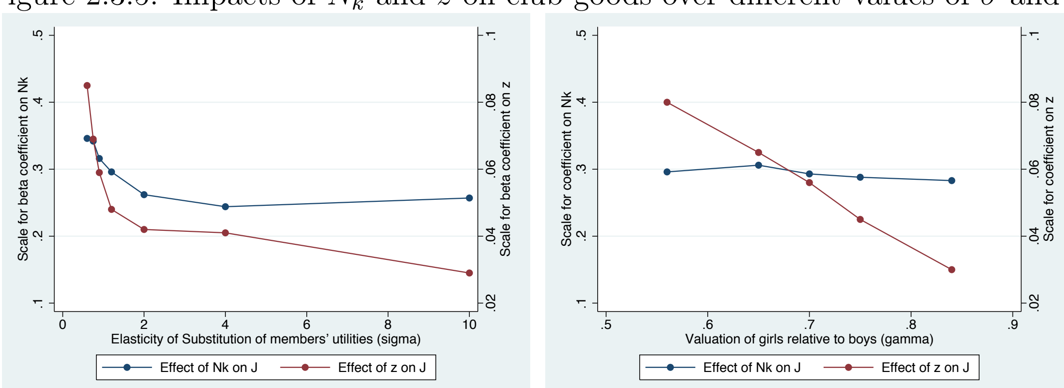


Figure 2.3.5 shows how the cohort size and gender effects on consumption of club goods vary over values of preference parameters.[6]

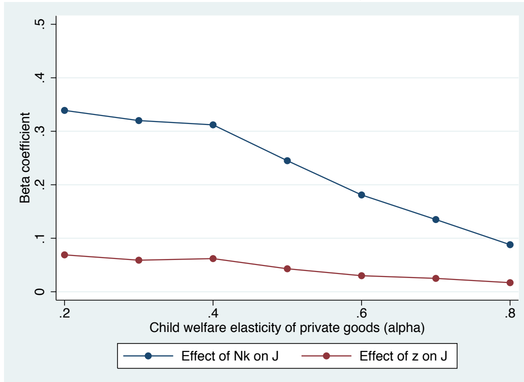
Figure 2.3.5: Impacts of N_k and z on club goods over different values of σ and γ



I find that elasticities closer to zero allow for a larger impacts of both N_K and z on J. Also, the stronger the son preference (the lower the γ), the stronger the response of club goods consumption to the gender composition of the cohort. Further, figure 2.3.6 shows how the effects vary over the range for α.

[6]Note that the effects are drawn on different scales, as the coefficients on N_K are generally an order of magnitude larger than those on z. For both axes, the range is a four-fold increase over the starting value.

Figure 2.3.6: Impacts of N_k and z on club goods over different levels of α



When child welfare is composed mostly of club goods (low values of α), club goods consumption responds much more strongly to cohort size than gender. However, when private goods are more important (high values of α), the effect of cohort size is considerably lessened.

2.4 Empirical Analysis

Theories of quantity-quality trade-offs and gender crowding out suggest that investments in children are decreasing in the size and male composition of their cohort. I have shown here a theoretical prediction that, while private investments may be decreasing in these parameters, investments in club goods for children may increase as cohorts become larger and more predominantly male.

The purpose of the empirical exercise is to test these central predictions of the model, that is, to test whether

$$\frac{\partial J}{\partial N_k} > 0 \text{ and } \frac{\partial J}{\partial z} > 0.$$

While some club goods for children are physical, such as a household water sanitation system, many non-rival investments in children are composed of adult time in some form (e.g. supervision, reading, transportation, etc.). The example club good employed here is the transportation of children from rural areas in Senegal to a health clinic for immunizations. While immunization may be seem to be a private good, the injection itself is often near costless in this context (due to subsidization). However the adult time required to obtain the immunization (via transport and wait time) may be many hours. Pande (2003) notes that, in India, differential immunization rates by gender, despite decades of free provision, imply significant opportunity costs of adult time. Further, it seems likely that parents traveling to the clinic would bring all children in the household in need of an immunization. Therefore, as the explicit cost of the injection gets close to zero, a vaccine becomes (very nearly) a non-rival good for children of a relevant age. In section 2.4.1.1, I provide suggestive evidence that immunizations are a club good in the context of this analysis.

2.4.1 Data

This analysis is set in Senegal, owing to the unique Senegalese households composed of large extended families. This characteristic enables me to resolve an inherent challenge of estimation, as discussed in section 2.4.2.

The data are drawn from two cross-sectional household surveys, the Senegalese Demographic and Health Survey (SDHS) in 1993 and 2005.[7] The DHS interviews all women aged 15-49 from a nationally representative sample of households on topics relevant to fertility, reproductive health, marital relations, and childhood health and nutrition. Specifically, the two surveys employed here recorded whether each child under age five did or did not receive each vaccine recommended by the Senegalese National Immunization Schedule as shown in table 2.2.

Table 2.2: Senegalese National Immunization Schedule & Coverage

		Of children due, % that have received		Sample Size		
Vaccine Dose	Due at age	1993	2005	1993	2005	Total
Tuberculosis	Birth	74%	87%	3,687	6,649	10,336
Oral Polio Vaccine 0	Birth	..	39%	0	6,642	6,642
DPT 1	6 weeks	66%	87%	3,577	6,486	10,063
Oral Polio Vaccine 1	6 weeks	67%	88%	3,577	6,489	10,066
DPT 2	10 weeks	59%	82%	3,494	6,327	9,821
Oral Polio Vaccine 2	10 weeks	59%	79%	3,494	6,331	9,825
DPT 3	14 weeks	49%	71%	3,412	6,172	9,584
Oral Polio Vaccine 3	14 weeks	49%	65%	3,412	6,176	9,588
Measles	9 months	58%	76%	3,071	5,304	8,375
Yellow Fever	9 months	57%	76%	3,063	5,280	8,343
Total		60%	75%	30,787	61,856	92,643

Notes: Rural sample only. Vaccines for which a child is not yet due are excluded. Information regarding Oral Polio Vaccine-0 was not collected in 1993. DPT indicates a combined vaccine for Diphtheria, Pertussis, and Tuberculosis.

Table 2.2 also presents a summary of the coverage of immunizations in rural households. Overall coverage of immunizations is mediocre, though increasing from 60% to 75% over the 12 year period. Vaccines for which a child is not yet due are excluded

[7]SDHS rounds were also collected in other years but were not used for the following reasons: 2008, 2006 and 1997 did not collect immunization information; 1986 collected immunization information only for the small subsample that had a healthcard available for verification.

from the mean, giving rise to the slight decline in sample size for vaccines due at older ages. Further details regarding sample size are show in table 2.3. A decline in sample size results also from non-response to some items regarding immunization. However, children with data missing for any of the ten vaccines make up less than 10% of the sample. The last two rows of table 2.3 show the sizes for the two samples used in most specifications.

Table 2.3: Sample Size

Rural Sample	1993	2005	Total
Households	1,444	3,046	4,490
Mothers	2,325	4,674	6,999
Children U5	3,746	7,364	11,110
Children U5 x vaccines	33,696	73,640	107,336
Children U5 x vaccines due	31,357	68,344	103,408
Children U5 x vaccines reported	30,787	61,856	92,643
CU5 with NSAM x vaccines reported	17,545	33,057	50,602

Notes: "CU5" is children under age 5; "NSAM" is non-sibling age-mates.

2.4.1.1 Immunizations as a club good

In this analysis, immunizations are considered a club good for children in a relevant age group. Although the vaccines considered here are all due by age 9 months, in practice, the children in this data continue receiving vaccines up to 4 years of age. However, over 95% of vaccines administered are received by age 2 years, and this will be the default age group used in this analysis.[8] Therefore, for a given child, i, the relevant age-cohort is composed of all other children in the household potentially needing vaccines at any time during which child i is due for a vaccine; that is, all children within 24 months of his age.

As evidence that immunizations operate as a club good, I examine the proportion of children receiving vaccines on the same day as a cohort member. For approximately half of the full sample, the enumerator was able to view the healthcard that lists the immunization history of a child. For this healthcard sub-sample, the data contain the date on which each immunization was received. Of rural children with completed vaccines, 11% received one or more vaccines at the same time as another child in the household. In contrast, urban children are 40% less likely to do so (difference is statistically significant at the 1% level). This difference likely reflects the increased opportunity cost involved for rural residents to reach a health clinic.[9] This motivates

[8]Results are robust to age groups defined over the range of 9 to 48 months.

[9]The opportunity cost for rural residents is perhaps further increased if rural health clinics exhibit longer wait times.

the restriction of this analysis to the rural areas and suggests that, in rural areas, travel to the clinic is a non-rival good for children in need of immunizations.

2.4.2 Methodological Challenges

2.4.2.1 Endogenous Fertility

In the theoretical model presented in section 2.3, parents choose allocation across members of the household, *for a given household composition.* That is, at the point of decision-making, composition is considered exogenous. However, in any empirical estimation of the response of allocation to composition, one must account for the fact that the number of children in the household is not exogenous. Specifically, unobserved factors that determined parents' past fertility choices will also impact current allocation decisions.

It is uncontroversial to state that parents choose, to some degree, the number of children they bear. Perhaps less obvious, but also true, is the statement that parents choose the gender composition of their children as well, within some bounds. If parents have greater desire for sons (for any reason), it is likely that childbearing will continue until some target number of sons is achieved. Yamaguchi (1989) presents a formal model of how son preference can affect the gender ratio even when manifest only through such differential stopping behavior (DSB). DSB predicts that girls will end up with more siblings than boys, and, ironically, that families with greater son preference, in their effort to acquire sons, will end up with more children and a more daughter-skewed composition than other families. Filmer, Friedman, and Schady (2008) present empirical evidence that DSB is practiced on a significant scale, based on DHS data from 65 low-income countries. While DSB seems most prevalent in South and Central Asia, evidence suggests that DSB is practiced in rural Senegal. Girls in this sample have more sibling age-mates on average than boys, and a negative correlation between parity and male-composition is observed.[10] When DSB is practiced to any degree, it creates correlation between parental preferences and observed gender composition of children.

Such correlations between preferences and both number and gender of children can seriously confound efforts to estimate how child-composition affects intra-household allocations or child outcomes. Ejrnaes and Portner (2004) simulate the relationship between birth order and schooling investment in children based on a model of household allocation in which fertility is endogenous. Their results show that accounting for endogenous fertility reverses the direction of correlation between birth order and schooling; that is, when fertility is assumed exogenous, empirical estimates are seriously biased.

[10]Girls have an average of 0.49 sibling age-mates vs. 0.46 for boys; equality rejected with p-value .0539. Among women with at least one son, an additional child predicts a significant decline in the male share of children by 4% (p-value = 0). Results not shown; available upon request.

Solution

In order to avoid confounding by unobserved household characteristics, I employ a within-estimator using household fixed effects to compare across children within the same household. In combination with an indicator for a child's birth order, this holds constant the total number of children in the household when the child is due for immunizations, as well as time-invariant household characteristics.

However, since a large group of households in the sample include multiple mothers of young children, one may still be concerned that unobservable differences between mothers within a household could confound the estimation.[11] In the language of the model presented in section 2.3, we don't observe a mother's concern for investing in children (κ) or her valuation of girls relative to boys (γ), which are likely to be correlated with both fertility and immunization decisions.[12]

Mother-fixed effects could remedy this problem but are unfortunately not feasible in this sample. Variation in age-cohort gender composition across a mother's children would require her to have at least 3 children in the sample. Since vaccination information is only available for children born within 5 years of the survey, fewer than 10% of mothers have 3 or more children in the sample.

Mother-fixed effects being unavailable, I contend with this potential endogeneity by exploiting a unique characteristic of households in Senegal. As shown in table 2.4, 56% of mothers of children under age five coreside with one or more other mothers of young children. While some of these households result from polygamous unions, over half of them do not. Compared to other rural women, women in households with multiple mothers (that are not co-wives) are much more likely to be the daughter-in-law or a foster daughter of the household head. This suggests that many coresident mothers (CRMs) are often sisters-in-law, foster-sisters, or other relatives. Such relations are discussed in detail in Appendix A.

[11]In this analysis (as in the data), a family is defined as a mother and her children. A household is defined as members who coreside – generally extended families.

[12]By making some assumptions on the direction of correlations between κ, γ, N_k, and J, one can attempt to ascertain the direction of any potential bias. For example, we might assume that $Corr(J, N_k) > 0$ and $Corr(N_k, \kappa) < 0$, and thus the exclusion of κ will create a downward bias on the estimated relationship between N_k and J. However, as noted by Greene (2003) (p149), in the presence of multiple regressors, the bias terms on coefficients are composed of partial, not simple correlations. Therefore, determining the sign of the resulting bias requires knowledge of the correlations between regressors and omitted variables, *net of the effects of other regressors.* Given the inclusion of covariates and fixed effects in the regressions presented here, such partial correlations become very difficult to predict. As such, we can say that the coefficients estimates for N_k and J will be inconsistent when κ and γ are excluded, though we cannot predict the direction of the bias.

Table 2.4: Summary Statistics for Rural Sample

	Proportion or Mean	Std. Dev.	Min.	Max.
Households $N = 4,490$				
Children U5	2.47	1.65	1	18
Mothers of children U5	1.47	0.91	0	10
Has children U5 with age-mates	0.53	0.5	0	1
Has variation in number of age-mates across CU5 in household	0.66	0.47	0	1
Mothers of Children U5 $N = 6,999$				
Age	28.99	7.52	15	49
Completed primary school	0.13	0.33	0	1
Number of children	4.3	2.72	1	15
Number of children U5	1.59	0.64	1	5
Has coresident mother(s) of CU5	0.56	0.5	0	1
Has coresident mother that is co-wife	0.14	0.34	0	1
Has coresident mother(s); not co-wives	0.31	0.46	0	1
Children U5 $N = 11,110$				
Percent of due vaccines received	0.63	0.38	0	1
Has any age-mates in household	0.68	0.47	0	1
if so, number of age-mates	2.31	1.65	1	15
if so, non-sibling age-mates	1.82	1.69	0	14
Share of age-mates that are boys	0.51	0.4	0	1

Notes: CU5 is "children under age 5". Age-mates are other children in the household within 24 months of one's age. For 20% of women with coresident mothers (11% of all rural women), determination of co-wife status is not possible, see Appendix A. Standard errors are in parentheses, clustered at the household level.

Foster (2004) provides evidence that coresident families operate altruistically as a household unit, and argues that health statuses of members are complimentary. One would therefore expect that a trip to the health clinic would be non-rival for all children of a eligible age within the household. Thus, all age-mates in the household (sibling or not) would be the relevant cohort for evaluating the gender composition effects of club good consumption. Because endogeneity arises from fertility choices within a nuclear family, it is only the sibling age-mates that confound the analysis. I therefore use the non-sibling coresident age-mates as an exogenous indicator of age-cohort size and gender composition within the household. The primary identification assumption is thus: controlling for anything common across mothers in a household by the use of household-fixed effects, the immunization decisions of one mother are independent of the fertility decisions of her coresident mothers.[13]

2.4.2.2 Measuring gender composition

Gender composition of an age-cohort is defined in this analysis as the proportion of the age-mates that are male. For children that have no age-mates, this is a difficult measure to define. In one sense, the impact of the gender composition of one's cohort is only relevant for children than indeed have a cohort at all. In this view, estimates of the impact of cohort gender should exclude children with no cohort. However, such sample selection could bias the estimates of cohort size effects, by excluding all children with a cohort size zero. In this view, for estimates of cohort size effects, perhaps children with no cohort should also have the proportion of age-mates that are male set to zero; or perhaps these should exclude measures of cohort gender entirely. In the following section, I show results from each of these three options.

2.4.3 Estimation

Absent the endogeneity issues discussed above, the model I would like to estimate is

$$R_{ijv} = \alpha + \beta_1 Z_{ij} + \beta_2 N_{ij} + \beta_3 X_{ij} + \eta_j + \eta_v + \varepsilon_{ijv} \tag{2.4.1}$$

where R_{ijv} is a binary indicator of whether child i in household j received vaccine v. The outcome is predicted by the male-share of child i's age-cohort within the household, Z_{ij}, and the size of that cohort, N_{ij}, as well as child-specific characteristics contained in X_{ij} (such as gender, birth order in the nuclear family, and household birth order), household and vaccine fixed effects, η_j and η_v, and a random, mean-zero error, ε_{ijv}. However, given the potential endogeneity of Z_{ij} and N_{ij}, the model I estimate instead is

$$R_{ijv} = \tilde{\alpha} + \tilde{\beta}_1 \tilde{Z}_{ij} + \tilde{\beta}_2 \tilde{N}_{ij} + \tilde{\beta}_3 X_{ij} + \eta_j + \eta_v + \tilde{\varepsilon}_{ijv} \tag{2.4.2}$$

where $\tilde{Z}_{ij}$ and $\tilde{N}_{ij}$ are the analogs to measures Z_{ij} and N_{ij} for the exogenous portions of the age-cohort (non-sibling coresident children). For comparison purposes, I also estimate the same equation employing only the endogenous portion of the age-cohort (siblings). That is,

$$R_{ijv} = \dot{\alpha} + \dot{\beta}_1 \dot{Z}_{ij} + \dot{\beta}_2 \dot{N}_{ij} + \dot{\beta}_3 X_{ij} + \eta_j + \eta_v + \dot{\varepsilon}_{ijv} \tag{2.4.3}$$

where $\dot{Z}_{ij}$ and $\dot{N}_{ij}$ are the relevant gender composition and size measures for the sibling age-cohort. In this equation, $\tilde{\beta}_1$ ($\dot{\beta}_1$) represents the difference in the probability of receiving a vaccine for a child who has non-sibling (sibling) age-mates that are all boys

[13] In a case where there are three or more CRMs in a household and at least two but not all of them share a husband, this restriction could be violated. However, as shown in section 2.4.3.4, the results are robust to the exclusion of households potentially fitting this unique profile.

versus all girls. The coefficient $\tilde{\beta}_2$ ($\dot{\beta}_2$) represents the predicted change in that same probability for each additional non-sibling (sibling) member in a child's age-cohort. In theory, it should be the case that $\tilde{\beta}_1 = \dot{\beta}_1$ and $\tilde{\beta}_2 = \dot{\beta}_2$, however, we expect that the coefficient estimates based on sibling age-mates will be confounded by unobserved maternal characteristics, so that $\tilde{\beta}_1 = \widehat{\tilde{\beta}}_1 \neq \widehat{\dot{\beta}}_1$ and $\tilde{\beta}_2 = \widehat{\tilde{\beta}}_2 \neq \widehat{\dot{\beta}}_2$. That is, we expect that estimates of $\dot{\beta}$ will be biased, but that the estimates of $\tilde{\beta}$ will be unbiased.

Under the assumption that immunizations are a club good for children of the relevant age within a household, we would expect that $\widehat{\beta_2}$ would be strictly positive, so that consumption of the good is increasing in the size of the cohort. Regarding the impact of gender composition on immunizations, the theory predicts that $\widehat{\beta_1}$ will also be strictly positive, indicating that a child's likelihood for receiving a vaccination is increasing in the male share of her age-cohort.

2.4.3.1 OLS Results

Table 2.5 shows results from the ordinary least squares estimations. As discussed in section 2.4.2.2, I present two versions of equation (2.4.2) in the first two columns. Column 1 shows that each additional non-sibling age-mate predicts an increase in probability of immunization of about 2.5 percentage points. This estimate is robust to whether or not I control for the gender composition, and whether or not children without any age-mates are excluded. In order to estimate the effect of gender composition, column 2 includes only the sub-sample of children that have exogenous age-mates (i.e. non-sibling age-mates; henceforth referenced as the NSAM sample). The point estimate suggests that, controlling for cohort size, having an all-male cohort predicts a 3.6 pp greater probability of immunization than having all female age-mates (statistically significant at the 5% level).

Columns 3 and 4 show estimates of equation (2.4.3) that are analogous to columns 1 and 2, except considering only endogenous age-mates (i.e. siblings). In the full rural sample, the endogeneity of fertility biases the coefficient estimates downward. In fact, using only (endogenous) siblings as the cohort, I estimate a negative impact of cohort size on immunization. The impact of sibling gender composition is indistinguishable from zero (column 4).

Table 2.5: Effect of Age-cohort on Immunization: OLS Results
Dependent variable: Y/N vaccine received

	All	With NSAM	All	With NSAM
	(1)	(2)	(3)	(4)
Size of non-sib age-cohort	.025***	.028***		
	(.005)	(.006)		
Male share of non-sib age-cohort		.036**		
		(.015)		
Size of sib age-cohort			-.020**	-.030
			(.010)	(.021)
Male share of sib age-cohort			.005	.004
			(.013)	(.021)
Male	.006	.021**	.006	-.007
	(.007)	(.011)	(.008)	(.021)
Birth Order	-.002	-.001	-.002	-.003
	(.002)	(.002)	(.002)	(.006)
HH birth order	-.033***	-.025***	-.036***	-.013**
	(.003)	(.003)	(.003)	(.006)
Vaccine FE	9	9	9	9
Household FE	4412	1639	4412	1505
Obs.	92643	50602	92643	26539
R^2	.443	.396	.442	.494

Notes: Columns 1 and 2 employ the age-cohort composed of non-sibling coresident children; columns 3 and 4 employ the age-cohort composed of siblings, see section (2.4.2.1) for details. "With NSAM" indicates the sub-sample having any exogenous age-mates (i.e. non-sibling). Standard errors are in parentheses, clustered at the household level.

2.4.3.2 2SLS Estimation

The OLS estimations are useful for recovering estimates of $\tilde{\beta}_1$ and $\tilde{\beta}_2$ (and $\dot{\beta}_1$ and $\dot{\beta}_2$, though severely biased). However, we are rather more interested in estimates of estimates of β_1 and β_2, that is, the effect of the full age-cohort. Thus, I subsequently employ a two-stage least squares estimation to recover the β coefficients. I use the exogenous portion of the age-cohort to predict the measures Z_{ij} and N_{ij} for the full age-cohort. I estimate

$$\hat{Z}_{ij} = \delta_0 + \delta_1 \tilde{Z}_{ij} + \delta_2 \tilde{N}_{ij} + \delta_3 X_{ij} + \eta_j + \eta_v + u_{ijv} \quad (2.4.4)$$

$$\hat{N}_{ij} = \theta_0 + \theta_1 \tilde{Z}_{ij} + \theta_2 \tilde{N}_{ij} + \theta_3 X_{ij} + \eta_j + \eta_v + v_{ijv} \quad (2.4.5)$$

$$R_{ijv} = \alpha + \beta_1 \hat{Z}_{ij} + \beta_2 \hat{N}_{ij} + \beta_3 X_{ij} + \eta_j + \eta_v + \varepsilon_{ijv} \quad (2.4.6)$$

so that $\widehat{\beta}_1$ and $\widehat{\beta}_2$ have similar identification as $\widehat{\tilde{\beta}}_1$ and $\widehat{\tilde{\beta}}_2$, but are now appropriately scaled to represent effects of the full age-cohort.

Estimations of first-stage equations (2.4.4) and (2.4.5) are shown in table 2.6.

Table 2.6: First Stages for 2SLS
Dependent variables for total age-cohort, shown as column headers.
Independent variables for exogenous age-cohort.

	All Rural		With NSAM	
	Size	Male Share	Size	Male Share
	(1)	(2)	(3)	(4)
Size of age-cohort	.862***	-.008***	.852***	-.004
	(.011)	(.003)	(.013)	(.002)
Male share of age-cohort	-.006	.774***	-.032	.804***
	(.023)	(.011)	(.027)	(.010)
Male	-.011	-.110***	-.016	-.053***
	(.012)	(.007)	(.017)	(.006)
Birth Order	.009**	-.001	.011***	-.001*
	(.004)	(.001)	(.004)	(.001)
HH birth order	-.073***	-.005***	-.067***	-.001
	(.005)	(.002)	(.006)	(.001)
Vaccine FE	9	9	9	9
Household FE	4412	4412	1639	1639
Obs.	92643	92643	50602	50602
R^2	.964	.861	.949	.931

Notes: Columns 1 and 2 employ the full rural sample. Columns 3 and 4 employ the "With NSAM" sub-sample: those having any exogenous (i.e. non-sibling) age-mates. Standard errors are in parentheses, clustered at the household level.

The measures of size and composition for the exogenous age-cohort are clearly strong predictors of the same measures for the full cohort. This is equally true for the full rural sample (columns 1 and 2) and for the sub-sample who have exogenous age-mates (NSAM sample; see columns 3 and 4).

Second-stage estimations of equation (2.4.6) are shown in table 2.7.

Table 2.7: Effect of Age-cohort on Immunization: 2SLS Results
Dependent variable: Y/N vaccine received

	All rural		With NSAM	
	(1)	(2)	(3)	(4) Beta coeffs
Size of age-cohort	.029***	.028***	.033***	.127***
	(.006)	(.006)	(.007)	(.025)
Male share of age-cohort		.031*	.046**	.037**
		(.016)	(.018)	(.015)
Male	.006	.014	.024**	.026**
	(.007)	(.009)	(.011)	(.012)
Birth Order	-.002	-.002	-.001	-.006
	(.002)	(.002)	(.002)	(.011)
HH birth order	-.031***	-.031***	-.023***	-.358***
	(.003)	(.003)	(.003)	(.050)
Vaccine FE	9	9	9	9
Household FE	4412	4412	1639	1639
Obs.	92639	92639	50602	50602
R^2	.127	.128	.119	.119

Notes: Estimates are equation (2.4.6), the second stage of 2SLS. Columns 1 and 2 employ the full rural sample. Columns 3 and 4 employ the "With NSAM" sub-sample: those having any exogenous (i.e. non-sibling) age-mates. Column 4 shows standardized beta coefficients. Standard errors are in parentheses, clustered at the household level.

Columns 1 and 3 are the 2SLS analogs to columns 1 and 2 in table 2.5. The coefficients of interest have increased in magnitude relative to the OLS estimation, though the 95% confidence intervals do overlap. These results suggest that any additional age-mate will increase the probability of immunization by about 3pp. Further, for children having any age-mates, an all-boy cohort predicts a 4.6pp increase in the likelihood of immunization relative to an all-girl cohort. Column 2 shows that the coefficient for cohort size is fully robust to the inclusion of gender composition in the full sample.[14]

In order to compare the magnitude of these results with the results from the theoretical simulation, column 4 provides the standardized beta coefficients for the same estimation as shown in column 3. An increase in male composition of cohort by one standard deviation predicts an increase in the provision of vaccines of .04 sd. This is nearly identical to the linear prediction of 0.43 sd shown in table 2.1. Holding all other parameters constant, this would suggest an elasticity of substitution (σ) between util-

[14]In this case, those with no age-mates have male composition set to zero. This explains the attenuation of the coefficient on gender in this specification.

ities of household members of about 4. Alternatively, holding σ (and others) constant, this would suggest a value for the son preference parameter, γ, of about 0.77. That is, parents value investments in girls at 77% of the value of investments in boys.

The coefficient on cohort size is 0.13, just under half the magnitude predicted by the linear simulation. However, as shown in figure 2.3.6, the magnitude varies greatly depending on the relative value of public and private goods in producing child utility (α). Under the assumption that $\alpha = 0.7$, the predicted effect of N_k on J is 0.135 sd. The empirical result suggests that α is approximately 0.7 in this context, rather than the mean value of α in the simulation exercise, which is 0.5.

2.4.3.3 Specification checks

Table 2.8 presents several alternative specifications of equation (2.4.6). In column 1, the quadratic specification supports the theoretical predictions: the effects of cohort size are slightly non-linear, however the effects of cohort gender appear linear. The coefficient on the square of male share is not significantly different from zero, though the linear and quadratic terms are jointly significant at the 5% level.

In column 2, the specification includes interactions of the variables of interest with the gender of child i. The signs of the coefficients on the interaction terms suggest that cohort effects may be slightly stronger for girls than boys. However, these are not at all estimated precisely and thus we cannot reject that the effects are the same for boys and girls.

Column 3 investigates whether there is any interaction in the effects of cohort size and cohort gender composition. The positive coefficient on the interaction suggests (i) that the effect of an additional age-mate is larger if that age-mate is a boy, and (ii) the effect of gender composition is stronger for larger cohorts. Yet again, we cannot reject that there is no interaction whatsoever, given the rather large standard error on the interaction term. Note that the lack of significance of the coefficient on male share is inconsequential, as that estimate represents the effect of cohort gender when the cohort is of size zero. That is rather meaningless; it is more meaningful to note that the coefficients on male share and the interaction are jointly significant at the 5% level.

Table 2.8: Alternative Specifications; 2SLS
Dependent variable: Y/N vaccine received

	(1)	(2)	(3)
Size of age-cohort	.049***	.036***	.025***
	(.013)	(.007)	(.009)
$(\text{Size})^2$	-.002*		
	(.001)		
Size x Male		-.005	
		(.005)	
Size x Male share			.017
			(.015)
Male share of age-cohort	.091	.058**	.020
	(.064)	(.026)	(.028)
$(\text{Male share})^2$	-.045		
	(.062)		
Male share x Male		-.017	
		(.037)	
Male	.024**	.047*	.026**
	(.011)	(.027)	(.012)
Birth Order	-.001	-.001	-.001
	(.002)	(.002)	(.002)
HH birth order	-.023***	-.023***	-.023***
	(.003)	(.003)	(.003)
Vaccine FE	9	9	9
Household FE	1639	1639	1639
Obs.	50602	50602	50602
R^2	.12	.12	.12
χ^2(Male share, $(\text{Male share})^2$)	7.16		
χ^2(Male share, Size x Male share)			7.21

Notes: Estimations shown are variations on equation (2.4.6); the second stage of 2SLS. All columns show rural sample of children with any exogenous age-mates (NSAM). "Male share" is the proportion of one's cohort that is male. "Male" indicates that child i is male. Standard errors are in parentheses, clustered at the household level.

2.4.3.4 Robustness Checks

Sample selection on survival

The DHS data employed here collect immunization for all *living* children under age five. Therefore, children born within the past five years that have since died are present in the data but have no immunization information and are thus excluded from the estimations. If we assume that the excluded children received immunizations with the same likelihood as surviving children, this sample selection should not be a source of bias.

However, immunizations are intended to prevent potentially fatal illnesses. Therefore, a negative correlation between vaccine status and child death is possible. Yet in order to bias the results presented here, death must also be correlated with size and/or gender of one's cohort. If the excluded children had significantly smaller (or more female) cohorts, then these results are downward bias and serve as a lower bound effect. If excluded children had larger (or more male) cohorts, than these effects are overestimates.

Table 2.9 shows the predictive effect of cohort size and gender composition for a child's survival survival status. Within a household, age-cohort size and composition do not differ significantly by survival status.

Table 2.9: Uncorrelated selection on survival
Dependent variable: Y/N child is alive

	All Rural	With NSAM
Size of exogenous age-cohort	-.003	.000
	(.005)	(.005)
Male share of exogenous age-cohort		.008
		(.013)
Household FE	4489	1649
Obs.	11110	6053

Note: Estimated at the child level.

Polygamy

The end of section 2.4.2.1 notes that the central assumption for identification is that, controlling for all fixed characteristics of the household, the fertility decisions of one mother are independent of the immunization decisions of another. One possible violation of this assumption occurs in households with polygamous marriages, where mothers of young children share a husband. Note however that if all the mothers of young children in the household share the same husband, then the commonalities in preferences are captured by the household fixed effect. The potential violation occurs when there are (at least) two mothers that share a husband and (at least) one other mother with a different husband. There are 161 households in the sample that meet these criteria. The first column of table 2.10 shows the estimation of equation (2.4.6) based on children with exogenous age-mates, excluding the households that potentially violate the identification assumption. The coefficient estimates of interest are not significantly changed by this exclusion.

Table 2.10: Robustness Checks
Dependent variable: Y/N vaccine received

	Excluding		Age-mate is within...				
	Potential Violators	Campaign Participators	36mo.	30mo.	18mo.	12mo.	12mo.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Size of age-cohort	.027***	.029***	.032***	.038***	.023***	.014	.014**
	(.007)	(.010)	(.007)	(.007)	(.007)	(.010)	(.007)
Male share of age-cohort	.044**	.063**	.057*	.062**	.035**	.039***	
	(.019)	(.032)	(.034)	(.025)	(.016)	(.015)	
Male	.028**	.021	.026*	.027**	.019*	.016	.005
	(.012)	(.019)	(.013)	(.012)	(.011)	(.012)	(.007)
Birth Order	-.003	.000	.000	-.001	-.001	-.001	-.002
	(.002)	(.003)	(.002)	(.002)	(.002)	(.002)	(.002)
HH birth order	-.024***	-.027***	-.017***	-.019***	-.027***	-.025***	-.036***
	(.004)	(.006)	(.004)	(.004)	(.003)	(.003)	(.003)
Vaccine FE	9	9	9	9	9	9	9
Household FE	1520	1087	1773	1712	1549	1367	4412
Obs.	44953	25237	54781	53001	47010	39937	92639
R^2	.121	.073	.12	.118	.122	.124	.128

Notes: Estimations shown are variations on equation (2.4.6); the second stage of 2SLS. All columns except the last use the rural sample with exogenous age-mates, with exclusions as noted. See footnote 15 regarding the final column. Standard errors are in parentheses, clustered at the household level.

Vaccination Days

One concern specific to the examination of immunizations in Senegal are the "vaccination days." These are sponsored by a national campaign designed to encourage demand for the service in rural areas. Vaccines are provided from a service site set up at or near a major market, which are generally more accessible than the nearest health clinic. Such a campaign would reduce the adult time cost of immunization, and as such may reduce the degree to which immunizations are considered club goods.

Just over half of the children in the sample received at least one immunization as part of such a campaign. Column 2 of table 2.10 presents results that exclude these children. The estimated effect cohort size is unchanged and the effect of cohort gender is slightly increased, though not significantly different from previous estimates.

Age-cohort definition

Another assumption of this empirical test has been that the age-cohort that is relevant for immunizations as a club good is defined as all children in the household within 24 months of one's age. The remaining columns of table 2.10 present estimations under varying definitions of age-cohort. Whether one defines age-mates as children within 36, 30, 18, or 12 months of one's age, the results do not differ significantly from those based on the 24 month definition (see columns 3 - 6). Coefficients on cohort gender range from .035 to .062; none are significantly different from the originally estimated .046 (or from each other), and all are different from zero at standard levels of significance. Similarly, effects of cohort size range from .014 to .038, not differing significantly from the originally estimated .028. Though the estimations based on the NSAM sample are shown (in order to show the gender effects), all the coefficients on cohort size are different from zero at the 5% level in the full rural sample.[15]

2.5 Conclusions

In this study I have considered how investment in children is affected by the number and gender composition of children within a household. Previous literature has offered (at least) two theories on this. One, that children receive less investment as their cohort increases in size; that is, Becker's theory of quantity-quality trade-offs. And two, given any preference for sons, children with more boys in their cohort will receive relatively less; that is, gender crowding-out.

Evidence for these theories based on rigorous empirical work has been mixed, at best, tending to reject more often than support them. I've proposed that, in focusing exclusively on competition for private goods, these theories are missing a key element

[15]Estimates of cohort size effects based on the full sample do not differ from the estimates shown in columns 3-6, with the exception of the precision under the 12 month definition. The final column of table 2.10 shows that the effect of cohort size is different from zero in the full sample; the lack of precision in the 12mo-NSAM sample is likely due to the reduced sample size.

of child investment. That is, investment in children comes in the form of both private and club goods for children. Further, the effects of one's cohort on goods provision can work in opposite directions for these two types of goods.

I've shown here a simple theoretical representation of household allocation, in which households maximize a CES production function over the members' individual utilities. Households trade-off between goods for adults, private goods for boys and girls (separately) and club goods for children. A simulation exercise confirms the previous theories: yes, private investments in children are decreasing in cohort size and male share of the cohort. However, club goods for children are affected in exactly the opposite ways.

In order to test this theoretical contribution, I've used a novel approach to deal with the endogeneity of fertility. In ascertaining the impact of cohort composition on child investment, unobserved parental preferences can cause serious bias. I employ data on households in rural Senegal, generally composed of multiple nuclear families. I instrument size and composition of a child's cohort with those measures for the "exogenous cohort" – that is, the non-sibling children within the household. The empirical test uses childhood immunizations as the representative club good. In this poor, rural setting, the adult time cost of travel and waiting are the lion's share of the cost for vaccination. Within cooperative households, an adult's trip to the clinic is non-rival for all children in the household in need of immunization.

I estimate a within-household two-stage least squares estimation, with fixed effects for each of the ten required vaccines. I find that the probability of receiving any one vaccine is increasing in both size and male composition of one's cohort. Results suggest that each additional age-mate increases the probability of immunization by 3 percentage points. Relative to the mean probability in this sample of 71%, a child with two age-mates would face an increased probability of 77%. The magnitude of this effect matches the results from the simulation under the condition that α, the elasticity of child welfare with respect to private goods is about 0.7. This is a reasonable level, as we expect that private goods, such as nutrition, have more weight in the production of child welfare.

The results regarding gender composition of one's cohort suggest that children are benefited by the presence of male age-mates. The probability of immunization is 4.6 percentage points greater for a child with an all-boy cohort versus an all-girl cohort. This offers further support for the theoretical model, in that, the standardized coefficient on male share of cohort is nearly identical to that produced by the linear simulation.

I have offered here an additional factor to be considered in the economic analysis of investment in children. Child investment comprises both private and club goods. While children within a household compete with each other for private goods, they also benefit from each other in terms of club goods. I have shown evidence of this empirically, dealing rigorously with the endogeneity of fertility decisions. The results match the simulated predictions of the model. Ultimately, the effect of a child's cohort on her welfare, will depend on the relative values of private and club goods; an estimation of which I leave open for future work.

Chapter 3

Income Shocks and HIV

3.1 Introduction

Although Sub-Saharan Africa makes up only one-tenth of world population, it contains two-thirds of all the HIV infections worldwide. Various explanations have been proposed to explain the stark differences in the HIV/AIDS epidemic between Sub-Saharan Africa and the rest of the world. Differences in government policies[1], circumcision rates[2], marriage formation[3], sexually transmitted infections[4], and culture have been proposed as drivers of the epidemic. Recently, a growing literature has posited links between economic outcomes and HIV rates as well.

In this paper we explore the relationship between community-level economic shocks and HIV prevalence. We model the influence of such shocks on sexual behavior choices and hypothesize that behavioral responses are the link between shocks and increased infections. In Sub-Saharan Africa (SSA), a shock that reduces current income may induce women to engage in (or increase participation in) transactional sex.[5] In this context, women who participate in this market may be married or have other forms of employment, and may not identify as sex workers.[6] Increases in partnerships or risks taken in partnerships in order to supplement current income put women at significantly increased risk of infection.[7]

Further, in contrast to prostitution, these women commonly view male partners as boyfriends or lovers, and the relationships may be long-term. Transfers of money or in-kind gifts may occur throughout the duration of the relationship, rather than in exchange for specific sexual acts. Women may keep multiple concurrent partners long-term as a form of informal insurance. Such networks can seriously exacerbate existing

[1] Epstein (2007)
[2] Halperin and Epstein (2008); Auvert et al. (2005); Gray et al. (2007); Bailey et al. (2007)
[3] Magruder (Forthcoming)
[4] Oster (2005)
[5] Dupas and Robinson (2009); Robinson and Yeh (2011b)
[6] Wojcicki (2002); Hunter (2002); MacPhail and Campbell (2001)
[7] Stoneburner and Low-Beer (2004)

epidemics as disease spreads more quickly via simultaneously partnerships.[8]

Why would this type of behavior be specific to sub-Saharan Africa? When monetary savings are nonexistent and insurance is incomplete, women may engage in transactional sex to smooth current consumption and/or insure against future shocks. This behavior has been documented in Kenya[9], Malawi[10], and Zambia[11]. However, while previous research has shown a behavioral response to income shocks, the link with actual HIV infections is still speculative. To the best of our knowledge, our work is the first to show that income shocks lead to an increase in actual HIV infections.

We employ the latest rounds of Demographic & Health Surveys (DHS) that contain data on actual HIV status for individuals and GIS coordinates for their locations. A major limitation of the DHS is that they contain very little economic information, so that income and expenditures are not observed. To address this limitation, we link the DHS data with weather data from the University of Delaware using GIS coordinates. Since a vast majority of agriculture in SSA is rain fed, rainfall shocks act as a proxy for economic shocks, especially for rural households.[12] We define a shock as annual rainfall that falls below the local historical mean by 1.5 SD or more.

Our main finding is that shocks have a strong effect on HIV infection of females in rural areas where there is a large, generalized HIV/AIDS epidemic (5-15% prevalence). Each shock in the past ten years leads to a 1.3 percentage point increase in the likelihood of infection for this sub-group. The magnitude of the effect is large; given that prevalence for this group is about 7.7%, each rainfall shock amounts to a 17% increase in HIV risk.

In section 3.3 we present a conceptual framework, which predicts the effects to be greatest in rural areas, among women with the least ability to otherwise cope with shocks, and among the most economically stable men. The empirical findings in section 3.5 support these predictions. In sections 3.5.3 and 3.5.4 we verify that these results are robust to alternative assumptions and rule out other explanations. Section 3.6 concludes and discusses policy implications.

3.2 Related Literature

This work contributes to a number of distinct, yet related streams of literature. First and foremost, we build on recent work regarding sexual behavior responses to economic shocks. Robinson and Yeh (2011a; 2011b) employ innovative methods of collecting sexual behavior data, having unmarried women in Busia, Kenya maintain daily journals of their sexual activity. They find that these women engage in transactional sex to supplement current income and as a means of obtaining informal insurance against future

[8]Morris and Kretzschmar (1997)

[9]Dupas and Robinson, 2009; Robinson and Yeh, 2011a,b

[10]Swidler and Watkins, 2007

[11]Byron, Gillespie, and Hamazakaza, 2006

[12]Miguel, Satyanath, and Sergenti, 2004; Burke et al., 2009

shocks. Using the same sample, Dupas and Robinson (2009) find that these women also respond to aggregate shocks; specifically, after the disruptions of the 2007 elections in Kenya, women in Busia increased their likelihood of engaging in unprotected sex, which carriers higher premiums.

In contrast, Dinkelman, Lam, and Leibbrandt (2008) find that self-reported household-level income shocks reduce the number of sexual partners for females and increase that number for males. This finding is based on a sample of 14-22 year old youths in the Cape Area of South Africa. Perhaps the need to compensate for negative income shocks falls less on those still living with parents than upon older, economically independent adults.

Our contribution to this line of literature is twofold. First, in contrast to the studies noted above, we focus not on a specific micro-population, but rather seek evidence of behavioral response more broadly across SSA. Secondly,we focus on actual HIV outcomes, rather than self-reports of risky behavior. This offers a number of advantages. The first is that biological markers of risky sex are not subject to the social desirability bias of self-reports on risky sexual behavior (Padian et al., 2008). And yet, HIV infections are strongly indicative of risky sexual behavior, as this is the primary mode of transmission in this context.[13] The second is that, for policy makers, actual HIV infections are one of the relevant outcomes when studying the effects of income shocks on sexual behavior.

A second stream of literature to which we contribute is that concerning the as-yet ambiguous relationship between wealth and HIV in Sub-Saharan Africa. While there are a number of papers demonstrating a positive correlation between wealth and HIV (Shelton, Cassell, and Adetunji, 2005; De Walque, 2006; Johnson and Way, 2006), there may be considerable differences between countries. For example, Fortson (2008) finds that the relationship between wealth and HIV is positive in Burkina Faso, negative in Ghana, and concave in Tanzania. In this work we focus on the interaction of wealth and negative income shocks. Women with lower levels of assets are less able to cope with income shocks and will subsequently have a larger sexual behavior response compared to women with higher levels of assets.

Thirdly, we contribute to the literature that applies economic reasoning to issues surrounding the HIV/AIDS epidemic in sub-Saharan Africa. Work by Oster (Forthcoming) finds a relationship between export levels and increases in HIV incidence. Such findings are most likely explained by an increase in the movement of high risk individuals, namely truckers who are key players in an export driven economy. Fortson (2009) and Kalemli-Ozcan and Turan (2011) consider how sexual behavior responds to existing HIV prevalence, both finding that it does not. Oster (2007) suggests that such

[13]Other means of HIV transmission are using needles infected with HIV (e.g. intravenous drug use or vaccines) and transfusion from contaminated blood supplies. While we are unable to rule out these channels, it appears unlikely that economic shocks would lead to increases in intravenous drug use or contaminated blood transfusions. Further, in most of our study areas, both intravenous drug use and blood transfusions are extremely rare.

lacking response may be a result of significant competing mortality risks. In contrast, our work considers behavior as a driver of the epidemic rather than a response to it.

Finally, this paper contributes to the literature on economic shocks and health outcomes. Most of the previous literature in this vein has shown a negative relationship between economic shocks and children's health (Alderman, Hoddinott, and Kinsey, 2006; Maccini and Yang, 2009). The proposed channel is that economic shocks lower the availability of nutrients during a key phase in a child's development. However, if women are able to mitigate economic shocks through transactional sex, a mother may trade-off her long-term health (via risk of HIV infection) in order to provide for her children and thereby increase her children's long-term health and educational outcomes.

3.3 Conceptual Framework

3.3.1 HIV and Rainfall Shocks

What we ultimately estimate in this paper is the relationship between rainfall shocks and HIV infection. The purpose of this section is to provide a theoretical framework for why such a relationship should hold. Formally, the relationship we examine is the following:

$$\frac{\partial HIV}{\partial D} = \frac{\partial HIV}{\partial p}\frac{\partial p}{\partial z}\frac{\partial z}{\partial S} \tag{3.3.1}$$

where an individual woman's probability of HIV infection (HIV) is related to rainfall shocks (S) through the following pathway:

- $\frac{\partial HIV}{\partial p}$ is the relationship between HIV and risky sexual behavior (p). In this case, we let p be the number of sexual partners an individual woman has. There is substantial evidence suggesting this relationship is positive , that is, one's risk of HIV infection increases in the number of partners (Halperin and Epstein, 2008; Potts et al., 2008; Stoneburner and Low-Beer, 2004; Epstein, 2007). This relationship will also depend on the prevalence of HIV in an area (λ). Regions with higher HIV prevalence will have a stronger relationship between risky behaviors and new infections than regions with low prevalence $\left(\frac{\partial HIV}{\partial p \partial \lambda}\right) > 0$.

- $\frac{\partial p}{\partial z}$ is the relationship between the number of partners and income shocks (z). We discuss this relationship in more detail below.

- $\frac{\partial z}{\partial S}$ is the relationship between income shocks and rainfall shocks. In rural areas (r), where most income is generated from rain fed agriculture, we expect $\frac{\partial z}{\partial S} > 0$. Droughts lead to lower crop yields which create lower-than-normal income. In urban areas, where agriculture plays a smaller role in the local economy, we expect rainfall to have a more muted effect on income $\left(\frac{\partial z}{\partial S \partial r}\right) > 0$.

The question central to this paper is how does the number of partners change in response to changes in income $\left(\frac{\partial p}{\partial z}\right)$? In the next two sections, we present two simple models that predict the sign of $\frac{\partial p}{\partial z}$. The intuition behind both models is that the experience of income shortfalls due to rainfall shocks pushes women to increase their number of sexual partners for two reasons: 1) to generate current income to smooth present consumption, and 2) to secure informal insurance in the event of future income shocks. Though the models illustrate separate behaviors, in both cases the prediction is that women increase risky sexual behavior in response to shocks, thereby increasing risk of HIV infection.

It is important to note that we are modeling a **woman's** sexual response to decreases in income; there is evidence that the relationship between number of partners and income for men goes the opposite direction $\left(\frac{\partial p}{\partial y} \geq 0\right)$(Kohler and Thornton, 2010).

3.3.2 Current Income

Under this framework, a woman experiences an income shock due to low rainfall. In order to make up the lost income, she engages in a sexual relationship in exchange for a transfer of money or gifts. The trade-off the woman makes is the increased risk of HIV-infection in the future. This behavior has been well documented in Western Kenya, in work by Robinson and Yeh (2011b) and Dupas and Robinson (2009). In both works, individual females respond to both idiosyncratic and aggregate shocks by increasing partners and the level of risky sexual behavior to earn the higher premiums associated with risky sex (i.e. unprotected sex). Here we present a simple model to show the trade-off between current income and future health risk.

Adapting a model from Philipson and Posner (1993), an individual's utility consists of present income and future health risks:

$$U(p) = u\left(y(p) - z + zw\right) - \beta p \lambda c$$

where $u(.)$ is the utility from income, $p \in [0,1]$ is a measure of risky sexual behavior (i.e. number of partners), where $p = 0$ denoting abstinence, and $p = 1$ representing the maximum number of partners an individual can have. A women can generate income $y(p)$ by increasing her risky sexual behavior $\left(\frac{\partial y}{\partial p} > 0\right)$ but it has decreasing returns $\left(\frac{\partial^2 y}{\partial p^2} < 0\right)$. If a rainfall shock occurs, there is a decrease in income represented by z; these shocks are transitory and are normally distributed $(N \sim (0, \sigma^2))$. A woman is also able to mitigate some of the shock if she has assets w where $w \in [0,1]$. The cost of engaging in risky sex is the risk of HIV infection in the future, where β is the discount rate, λ is HIV-prevalence, and c is the utility lost if HIV-positive. We assume a log utility $u = \ln(.)$ and individual's have a minimum level of utility $\bar{U} = 0$. The intuition is that if a rainfall shock occurs, a woman will choose to engage in a level of transactional sex to make up for this income shortfall; however the woman must also take into account the increase risk of HIV infection from transactional sex.

The first order condition is:

$$\left(\frac{1}{y-z+zw}\right)\frac{\partial y}{\partial p}-\beta\lambda c=0$$

where the individual chooses p to equate the marginal benefit of increase income to the cost of future HIV infection.

The following comparative static shows how partners change as shocks increase:

$$\frac{\partial p}{\partial z}=-\left(\frac{-(y-z+zw)^{-1}(w-1)}{-(y-z+zw)^{-1}\left(\frac{\partial y}{\partial p}\right)^{2}+\left(\frac{\partial^{2}y}{\partial p^{2}}\right)}\right) \tag{3.3.2}$$

Given that $\frac{\partial^2 y}{\partial p^2}<0$, the denominator is negative; and given that $(w-1)\leq 0$, the numerator is positive. Therefore, $\frac{\partial p}{\partial z}\geq 0$, or, as shocks increase more partners are added. The model also predicts that as assets increases, the change in partnerships as a result of a shock is mitigated $\frac{\partial p}{\partial z\partial w}\leq 0$ (see Appendix C); intuitively individuals with more assets may be able to draw down these assets during transitory shocks and avoid transactional sex.

Further, let us assume that $y(p)=\bar{y}+\tau\left(p-p^2\right)$, so that income is composed of a baseline income $(\bar{y})$, plus the transfer received from each partner (τ), less a discount which is increasing in the number of partners. Then, we can show that $\frac{\partial y}{\partial p\partial\tau}>0$ and thus $\frac{\partial p}{\partial z\partial\tau}>0$. That is, the larger the potential transfer from partners, the higher the likelihood that a woman will engage.

The predictions that stem from this model are:

1. Number of partners is increasing in shocks $\left(\frac{\partial p}{\partial z}\geq 0\right)$. Given the previous predictions that $\frac{\partial HIV}{\partial p}>0$ and $\frac{\partial z}{\partial S}>0$, this implies that $\frac{\partial HIV}{\partial D}=\frac{\partial HIV}{\partial p}\frac{\partial p}{\partial z}\frac{\partial z}{\partial S}\geq 0$.

2. The effects of shocks on behavior should be highest for women with the fewest assets $\left(\frac{\partial p}{\partial z\partial w}\leq 0\right)$. Since $\frac{\partial HIV}{\partial p}$ and $\frac{\partial z}{\partial S}$ are unaffected by w , this implies that $\frac{\partial HIV}{\partial S\partial w}\leq 0$.

3. Given that a woman's response to shocks is increasing in the potential transfer $\left(\frac{\partial p}{\partial z\partial\tau}\right)$, we would expect to see men's HIV prevalence respond to shocks mainly among the wealthiest men.

3.3.3 Insurance Model

For simplicity, suppose that each woman lives for two periods. In the first period she may choose to build a relationship with a man in addition to her regular partner, in the hopes that he will provide a transfer (τ) to her in the event of a future negative income shock (z). She may also choose to not engage in this insurance networking, in which case, future shocks will force her to rely solely on her regular partner, her savings or other outside option (w). If she chooses to network, there is a chance she will contract HIV, which is an increasing function of the prevailing prevalence at the time (λ). If she does become infected, her ability to earn her baseline income (y) in the second period will be diminished by a factor $r \in (0, 1)$.

If she chooses to network ($p = 1$), her expected utility over the two periods is

$$\mathbb{E}(U|p = 1) = u(y) + \beta u \left[y - h(\lambda)ry - \Pr(z > 0) \left[\mathbb{E}(z|z > 0) - \tau\right]\right]$$

where β is a discount on the future period. Thus, in the second period, there is some chance of having HIV ($h(\lambda)$), and if she does, this will reduce her consumption by ry. Further, if there is a shock ($z > 0$), then consumption is reduced by the size of the shock (z) but increased by the amount of the transfer (τ).

If she chooses not to network ($p = 0$), her expected utility over the two periods is

$$\mathbb{E}(U|p = 0) = u(y) + \beta u \left[y - \Pr(z > 0) \left[\mathbb{E}(z|z > 0) - w\right]\right]$$

so that there is no chance of HIV in the second period. In the event of a shock, her consumption is reduced by z but increased by drawing down her savings, borrowing from better-off family, taking out a loan, or whatever is her outside option (w).

She will choose the network behavior in the first period if and only if $\mathbb{E}(U|p = 0) < \mathbb{E}(U|p = 1)$. Let us assume for simplicity that utility over consumption takes the form $u(c) = \ln(c)$. Then her participation condition can be written as

$$\begin{aligned} -\Pr(z > 0) \left[\mathbb{E}(z|z > 0) - w\right] &< -h(\lambda)ry - \Pr(z > 0) \left[\mathbb{E}(z|z > 0) - \tau\right] \\ h(\lambda)ry &< \Pr(z > 0) \left[\tau - w\right] \end{aligned}$$

Based on this condition we derive the following predictions:

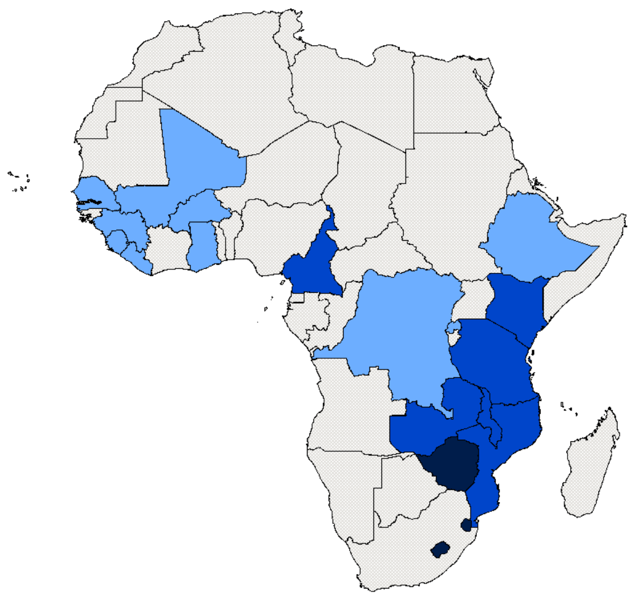
1. Her likelihood of choosing $p = 1$ will be higher if:
   (a) $\Pr(z > 0)$ is high (or perceived to be high)
   (b) The potential transfer (τ) is large, perhaps because the potential partner is of significant means
   (c) She lacks a sufficient outside option (w)
2. Her likelihood of choosing $p = 1$ will be lower if:
   (a) λ is very large, or she personally has a high potential for transmission (e.g. has an STI)
   (b) Contracting HIV is very costly (high r due to lack of treatment and resulting incapacitation)

3.4 Data

3.4.1 Demographic Health Surveys

The data on individuals are taken from 21 Demographic and Health Surveys (DHS) conducted in 19 different Sub-Saharan countries (Figure 3.4.1). Of the existing DHS surveys available in early 2011, we employ all those that (i) include results from individual-level HIV-tests, and (ii) include longitude and latitude information, allowing us to map households to data on shocks.[14] For two countries (Kenya and Tanzania), two survey rounds matched these criteria; however, these are separate cross-sections and creation of panel data at the individual or cluster level is not possible. Nonetheless, for each country both rounds are included in the analysis as entirely separate surveys.

Figure 3.4.1: DHS Countries Included



Each of these surveys randomly samples clusters of households from stratified regions and then randomly samples households within each cluster. In each sampled household, every woman aged 15-49 is asked questions regarding health, fertility, and sexual behavior.[15] A men's sample is composed of all men within a specified age range within households selected for the men's sample.[16] Depending on the survey, this is

[14]The one exception is the Mali 2001 survey. We must exclude this survey as it is not possible to link the HIV results to individuals in the GIS-marked clusters.

[15]Mozambique 2009 samples women up to age 64.

[16]The age range for men is 15 to either 49, 54, 59 or 64, depending on the survey. Sampling details are shown in Appendix Table B.1.

either all sampled households, or a random half (or third) of households within each cluster. In all households selected for the men's sample, all surveyed men and women are asked to provide a finger-prick blood smear for sero-testing.[17] By employing cluster-specific weights, the HIV prevalence rates estimated with this data are representative at the national level.[18]

Table 3.1 gives the list of included surveys along with basic survey information. The compiled data contains over 8,000 clusters. On average, there are 25 surveyed individuals per cluster, and 90% of clusters contain between 10 and 50 surveyed individuals. In total, there are over 200,000 individuals in the pooled data.

Table 3.1 also shows HIV prevalence rates for each survey. Overall, women's prevalence is 9.2% and men's is 6.2%. However, these numbers mask a range that varies widely from over 30% prevalence for women in Swaziland to less than 1% prevalence in Senegal. Given that the sexual behavior response to economic shocks may have different implications depending on HIV risk, we classify countries into two HIV prevalence groups: low prevalence countries with less than 5%; and high prevalence countries with over 5% prevalence.[19]

We present historical trends in HIV prevalence for the countries in our study (Figure 3.4.2). For each country, we take the ten years preceding the survey year and plot yearly estimates of HIV prevalence from UNAIDS (2010).[20] For a majority of countries, HIV prevalence has been declining over the ten years prior to the DHS survey. With the exception of Cameroon, the high and low prevalence classifications for each country remains stable for the ten years preceding the survey year.

The DHS data also provide information on individual characteristics, which we employ as controls in our analysis. Level of education is categorized as none, some primary, completed primary or beyond primary. For nearly all individuals over age 25, this will have been determined prior to the time period included in our analysis. DHS also provide a country-specific indicator of wealth quintile for each household, estimated from a principle components analysis of household assets, housing quality, access to improved water, etc (Filmer and Pritchett, 2001). We interpret this as rough indicator of socio-economic status that is relatively constant over time.

[17]Testing success rates for each survey are shown by sex in Appendix Table B.2.

[18]These are inverse-probability of sampling weights provided by DHS.

[19]Kenya and Tanzania are consistently categorized as high prevalence by estimates from both survey rounds.

[20]Ethiopia and Democratic Republic of Congo are not included in the figures as UNAIDS does not have historical estimates of HIV-prevalence for either country. We assume that both countries remained in the low prevalence category over the past ten years.

Table 3.1: DHS Survey Information

	Country	Year	Clusters	Individuals	Prevalence Female	Male	Overall
HIGH prevalence							
1	Swaziland	2007	271	8,186	31.1%	19.7%	25.9%
2	Lesotho	2004	381	5,254	26.4%	18.9%	23.2%
3	Zambia	2007	398	26,098	21.1%	14.8%	18.1%
4	Zimbabwe	2006	319	10,874	16.1%	12.3%	14.2%
5	Malawi	2004	521	5,268	13.3%	10.2%	11.8%
6	Mozambique	2009	270	10,305	12.7%	9.0%	11.1%
7	Tanzania	2008	345	10,743	7.7%	6.3%	7.0%
8	Kenya	2003	399	6,188	8.7%	4.6%	6.7%
9	Kenya	2009	397	6,906	8.0%	4.6%	6.4%
10	Tanzania	2004	466	15,044	6.6%	4.6%	5.7%
11	Cameroon	2004	466	10,195	6.6%	3.9%	5.3%
LOW prevalence							
12	Rwanda	2005	460	10,391	3.6%	2.2%	3.0%
13	Ghana	2003	412	9,554	2.7%	1.6%	2.2%
14	Burkina Faso	2003	399	7,530	1.8%	1.9%	1.9%
15	Liberia	2007	291	11,688	1.9%	1.2%	1.6%
16	Guinea	2005	291	6,767	1.9%	1.1%	1.5%
17	Sierra Leone	2008	350	6,475	1.7%	1.2%	1.5%
18	Ethiopia	2005	529	11,049	1.9%	0.9%	1.4%
19	Mali	2006	405	8,629	1.5%	1.1%	1.3%
20	Congo DR	2007	293	8,936	1.6%	0.9%	1.3%
21	Senegal	2005	368	7,716	0.9%	0.4%	0.7%
	Total		8031	203,796	9.2%	6.2%	7.8%

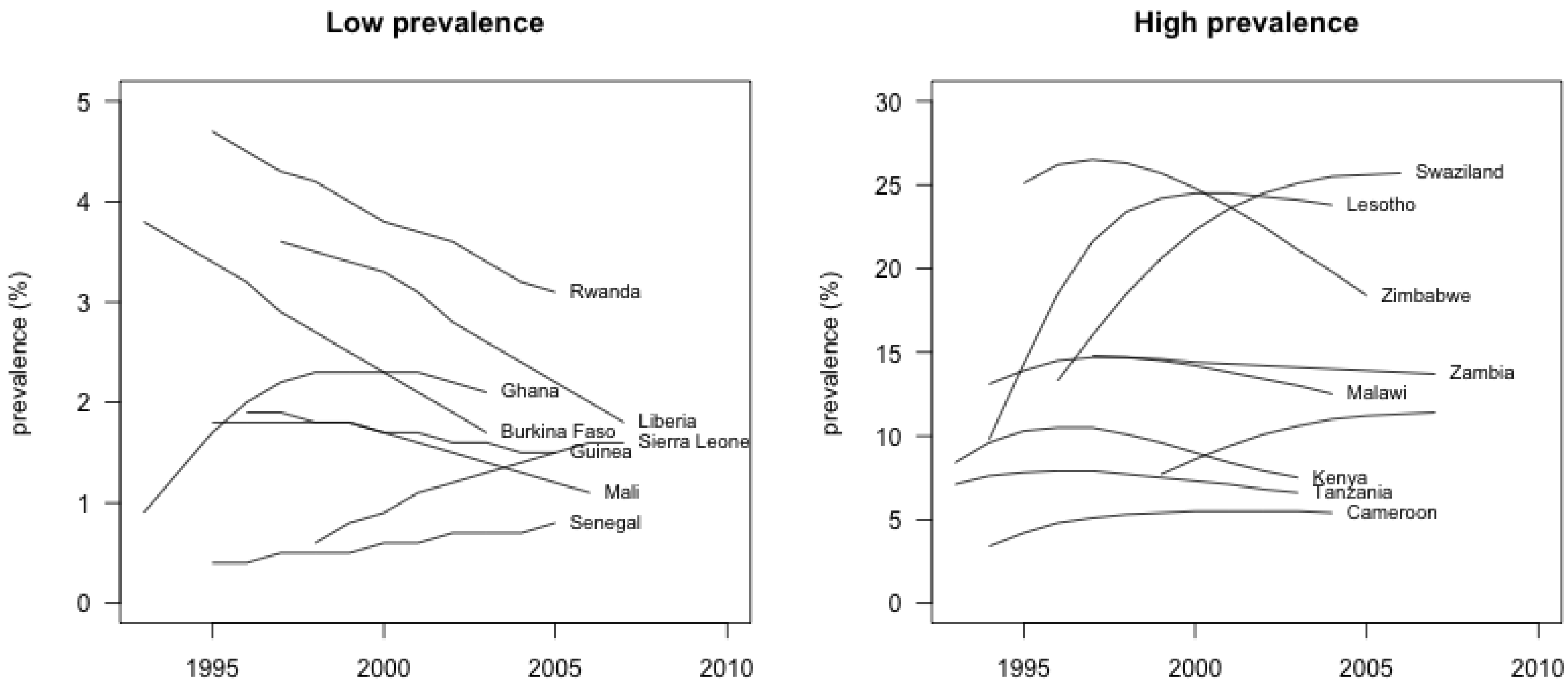
Prevalence estimates are weighted to be representative at the national level.

3.4.2 Weather Data

Weather data are from the "UDel" (University of Delaware) data set, a 0.5 x 0.5 degree gridded monthly temperature and precipitation data set (Matsuura and Willmott, 2009).[21] These gridded data are based on interpolated weather station data and have global coverage over land areas from 1900-2008. Using the latitude/longitude data in the DHS, we match each DHS cluster to the nearest cell in the gridded weather data.

[21] 0.5 degrees is roughly 50 kilometers at the equator.

Figure 3.4.2: Pre-survey 10-year HIV trends, Low and High Prevalence Countries



Because GIS data are at the cluster level, all individuals within a given cluster are assigned the same weather.

To capture the seasonality of agriculture, we construct cluster-level estimates of "crop year" rainfall, where the crop year is defined as the twelve months following planting for the main growing season in a region. Estimates of planting dates are derived from (Sacks et al., 2010); planting of staple cereal crops for the primary growing season typically occurs in the boreal (northern hemisphere) spring across most of West and Central Africa, and in the boreal autumn across most of Southern Africa. Annual crop year estimates are generated by summing monthly rainfall across the twelve months at a given location.

In our main specifications, we define a "shock" as a year in which crop year precipitation is more than 1.5 standard deviations below the cluster-specific mean, where the cluster mean is defined over 1970-2008.[22] While we cannot directly show the importance of these shocks for household income (as noted above, the DHS do not include income or consumption measures), aggregate data suggest that these shocks are economically important. Table 3.2 shows the impact of 1 and 1.5 standard deviation shocks on country-level maize yields across Sub-Saharan African countries. Maize is the most widely grown crop in Africa, and annual maize yields are strongly affected by precipitation: for instance, a 1 sd precipitation shock lowers yields by about 13%, and a 1.5 sd shock lowers yields by about 20%. With 60-80% of rural African incomes derived directly from agriculture, these productivity impacts likely represent significant shocks to household incomes.[23]

Table 3.2: Impact of Precipitation Shocks on Yields

	(1)	(2)
drought 1sd	-0.134***	
	(0.019)	
drought 1.5sd		-0.204***
		(0.048)
Observations	1916	1916
R^2	0.319	0.319
Pct. drought	0.144	0.052

Dependent variable is country-level maize yield. Regressions cover years 1961-2008 and include country fixed effects, year fixed effects, and a constant, and are weighted by country average maize area. Yield data are from FAO (2010). Weather data are from UDel.

[22]The choice of 1970-2008 is somewhat arbitrary, but was chosen to be a long enough period to be relatively insensitive to the recent shocks of interest, but short enough to capture relatively recent averages if long run means are changing (e.g. with climate change).

[23]Schlenker and Lobell (2010) demonstrate that these strong negative impacts of weather shocks generalize to other African staples, not just maize.

Table 3.3: Frequency of Rain Shocks over 10 years

	Survey		Clusters with X droughts 0	1	2	3	4
1	Swaziland	2007	16	181	74	0	0
2	Lesotho	2004	79	253	49	0	0
3	Zambia	2007	214	159	25	0	0
4	Zimbabwe	2006	260	58	1	0	0
5	Malawi	2004	517	4	0	0	0
6	Mozambique	2009	195	75	0	0	0
7	Tanzania	2008	264	79	1	1	0
8	Kenya	2003	201	172	26	0	0
9	Kenya	2009	200	168	29	0	0
10	Tanzania	2004	143	225	94	4	0
11	Cameroon	2004	120	329	17	0	0
12	Rwanda	2005	31	64	307	58	0
13	Ghana	2003	367	45	0	0	0
14	Burkina Faso	2003	243	118	38	0	0
15	Liberia	2007	179	1	89	22	0
16	Guinea	2005	78	114	99	0	0
17	Sierra Leone	2008	0	0	0	350	0
18	Ethiopia	2005	292	146	32	59	0
19	Mali	2006	295	105	5	0	0
20	Congo DR	2007	134	63	91	4	1
21	Senegal	2005	312	53	3	0	0
	Total		4140	2412	980	498	1
	Percent of clusters		52%	30%	12%	6%	0%

3.5 Empirical Test

3.5.1 Estimation

Using weather shocks as an independent variable is attractive because weather variation over time at a given location is generally considered as good as randomly assigned. Our definition of shocks helps us avoid many of the typical omitted variables problems that generally plague cross-sectional studies. In particular, because shocks are defined relative to local means, and these shocks are presumably accumulated randomly, then shocks should be orthogonal to other unobserved factors that might also affect HIV prevalence.

In order to ensure that our measure of shocks is a random variable, rather than a proxy for other unobserved differences across clusters, we regress the accumulated shocks on the local mean and standard deviation of rainfall. Table 3.4 shows these results. When all clusters in the sample are pooled, we in fact find that recent shocks are positively correlated with a history of generally volatile rainfall. However, if we estimate across clusters *within a given survey* we find that recent shocks are orthogonal to overall rainfall variance. For this reason, in our primary specification, we include survey fixed-effects to ensure that the accumulation of recent shocks is effectively random.[24] We also find that, even when including survey fixed effect, there still exists a small positive correlation between recent shocks and mean rainfall. For this reason, we consistently control for local mean rainfall in all specifications.

Table 3.4: Rainfall Shocks and Overall Variability

	(1)	(2)	(3)	(4)
SD of Annual Rainfall (mm)	0.272	-0.002		
	(25.32)	(-0.20)		
Mean Annual Rainfall (mm)			0.367	0.123
			(35.40)	(8.76)
Survey FE	No	Yes	No	Yes
Observations	8031	8031	8031	8031
R^2	0.074	0.527	0.135	0.531

Dependent variable is number of 1.5SD shocks in the past 10 years. Estimates shown are beta coefficients. t-statistics are shown in parentheses.

We estimate

$$HIV_{ijk} = \alpha + \beta_1 S_j^t + C_j'\zeta + X_i'\delta + \omega_k + \varepsilon_{ijk} \tag{3.5.1}$$

where HIV_{ijk} is an indicator that individual i in cluster j tested HIV-positive in survey k. The vector C_j contains characteristics of the cluster j such as location type (rural or urban) and historical average rainfall. The vector X_i contains characteristics of individual i, including gender, age, marital status, and indicators for education level and wealth level. The survey fixed effect is ω_k and ε_{ijk} is a mean-zero error term. Rather than assuming that ε_{ijk} is independent across individuals, we allow for correlation of error terms across individuals in the same village by clustering standard errors at the village level.

[24]There are a host of other reasons for including survey fixed-effects as well. Innumerable differences across countries exist that we cannot observe, including: social norms on sexual behavior, male circumcision rates, access to health services, and the national response to the AIDS epidemic. Such unobservable differences may also apply to different time periods within the same country, thus motivating a within-survey estimation.

S_j^t is the number of rainfall shocks that cluster j has experienced in the t years before the survey. The default indicator for Z is the number of crop-years with rainfall at least 1.5 SD below the historical average for the cluster. The default for t is the 10 years preceding the survey, since the median survival time at infection with HIV in sub-Saharan Africa, if untreated, is 9.8 years (Morgan et al., 2002). Both Z and t are varied over a range to test the robustness of results.

3.5.2 Results

Table 3.5 shows estimations of equation (3.5.1) for the full sample and six sub-samples: women, rural women, urban women, men, rural men and urban men. The overall effect of the full sample (column 1) is positive (.002) and statistically significant at the 90% confidence level. We expect the effect of rainfall shocks to be focused in rural areas (where agricultural income is a more important component of total income), and this appears to be the case (columns 3 & 6). For rural women, we estimate that each shock over the past 10 years increased HIV prevalence by 0.6 percentage points (p-value = .001). For rural men, the effect is somewhat smaller, with an estimated effect of 0.2 percentage points per shock (p-value = .084). The magnitude of both effects is surprisingly large. For rural women, where the mean HIV prevalence is 8%, this amounts to a 7.5% increase in HIV risk per shock; for rural men, who have a mean HIV prevalence of 2.8%, each shock increases HIV risk by 3.7%.

The model predicts that the effect should depend not only on the occurrence of a shock, but also the prevailing prevalence at the time of the shock. We therefore examine the estimated effect on rural women by low and high prevalence groups (Table 3.6). As predicted, countries with low prevalence have a near-zero effect (columns 1 & 3). In countries with high prevalence, there is a large effect for rural females (column 2) and smaller effect for rural males (column 4). For rural women in high prevalence countries, we estimate that each shock increases the likelihood of HIV by 1.4 percentage points, which is a 10.7% increase in HIV risk, given HIV prevalence of 13% for rural women in high prevalence countries. For rural males, the 0.7 ppt increase per shock is a 8% increase in HIV risk (mean HIV = 8.8%).

Our theoretical model predicts that the sexual behavior response to economic shocks should have the strongest effect in women with fewer assets and savings. During economic shocks, women with more assets maybe able to draw down on these assets to smooth consumption; women with fewer assets may need to trade off longer term health risks (HIV infection) to meet current consumption needs. This ultimately should be reflected in higher HIV infections for those with fewer assets. Our model also suggest that there should be an asymmetry in HIV rates between men and women as a result of economic shocks. If a large number of vulnerable women are partnering with a smaller number of economically secure men, then we expect large effects of shocks on HIV rates in women with fewer assets, and a smaller effect of shocks on men with more assets.

Table 3.7 examines this by dividing our sample by asset groups. We find evidence supporting our model predictions: the effects of shocks on women in the three lowest assets categories is large and statistically significant, yet we fail to reject that the effect for highest wealth group is zero (Table 3.7; top panel). With men, the effect of shocks on HIV rates is strongest in the highest wealth quintiles (Table 3.7; bottom panel).[25]

Since assets were measured at the time of the survey, they may not reflect the assets at the time of the shock, or - worse - they might be endogenous to realized shocks. A potentially more durable measure of socio-economic status is educational attainment. We limit the sample to those who would have completed their schooling at the time of the shock (age 25 and older at time of survey). Table 3.8 examines the effects of shocks by educational attainment . We find strong effects of shocks on rural females for those with little education (columns 1-2), and no statistically significant effect for those who have completed primary school and beyond (columns 3-4).

Table 3.5: Effect of Shocks on HIV

		Female			Male		
	All	All	Rural	Urban	All	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1.5 SD Shocks per 10 yrs	.002*	.003**	.006***	.000	.001	.002*	.000
	(.001)	(.002)	(.002)	(.003)	(.001)	(.001)	(.002)
Male	-.022***						
	(.001)						
Age	.002***	.002***	.001***	.003***	.001***	.001***	.002***
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Married	-.010***	-.022***	-.024***	-.020***	.009***	.007***	.015***
	(.002)	(.002)	(.003)	(.005)	(.002)	(.003)	(.005)
Urban	.026***	.031***			.019***		
	(.003)	(.004)			(.003)		
Obs.	176102	96810	64128	32682	79292	52917	26375
R^2	.049	.054	.044	.062	.041	.031	.054

All specifications include controls for mean rainfall, location type, gender, age, marital status, education and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

[25] Note that the two highest quintiles are combined as "BetterOff", as there are too few individuals in the highest quintile in rural areas to compose a sub-group.

Table 3.6: Effect of Shocks on HIV in Rural Areas: By Country Prevalence

Prevalence	Female Low	Female High	Male Low	Male High
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	.000	.014***	-.001	.007**
	(.002)	(.004)	(.001)	(.003)
Obs.	31074	33054	26035	26882
R^2	.005	.033	.004	.027

All specifications employ the rural sample and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Table 3.7: Effect of Shocks By Wealth

WOMEN	Poorest	Poor	NotPoor	BetterOff
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	.014**	.021***	.010*	.009
	(.006)	(.007)	(.006)	(.007)
Obs.	7821	8084	7870	9279
R^2	.032	.029	.03	.059

MEN	Poorest	Poor	NotPoor	BetterOff
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	.007	.003	-.002	.016***
	(.005)	(.005)	(.005)	(.006)
Obs.	5988	6688	6767	7439
R^2	.021	.022	.029	.044

All specifications employ the rural sample from high-prevalence countries and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. Note that there are too few rural individuals in the highest wealth quintile and thus it is combined with the 4th quintile as "BetterOff". All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Table 3.8: Effect of Shocks By Education

WOMEN	NoEduc	SomePrim	CompletePrim	BeyondPrim
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	.016***	.040***	-.002	.020
	(.006)	(.010)	(.008)	(.014)
Obs.	5509	5796	5363	2884
R^2	.05	.07	.049	.112

MEN	NoEduc	SomePrim	CompletePrim	BeyondPrim
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	-.003	.034***	.004	.022*
	(.010)	(.011)	(.006)	(.012)
Obs.	2460	4925	4850	3461
R^2	.031	.023	.019	.049

All specifications employ the rural sample from high-prevalence countries and include controls for mean rainfall, age, marital status and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Overall, our main results are consistent with the following: 1) rainfall shocks only affect the income of those living in rural areas, 2) rural females respond to income shocks by increasing their risky sexual behavior as a means to smooth consumption during transitory shocks, 3) this sexual behavioral response leads to higher HIV infection rates for rural females. In addition, we find evidence that those less able to cope with shocks are increasing their sexual behavior more in response to shocks leading to higher HIV rates in the lower wealth quartiles for women.

3.5.3 Robustness Checks

We conduct a variety of robustness checks on our main results, including varying the time window that rain shocks occur, varying the set of individual and cluster level controls, and estimating our results without population weights. We first show that our main results are not sensitive to changes in the time period. Table 3.9 shows the following specifications: 1) limiting the age group to be consistent across all surveys (column 1), without individual and cluster level controls (column 2), and without sampling weights (column 3), neither of which vary significantly from our previous estimates. Finally, we replace the survey fixed effects with country and year fixed effects (column 4) and with sub-national-region and year fixed effects (column 5). Overall, our main results are robust to each of these alternative specifications.

Table 3.9: Robustness Checks

	(1)	(2)	(3)	(4)	(5)
1.5 SD Shocks 10 Years	.014***	.015***	.009**	.014***	.011***
	(.004)	(.004)	(.004)	(.004)	(.004)
Controls	Yes	No	Yes	Yes	Yes
Weights	Yes	Yes	No	Yes	Yes
Fixed Effects	Svy	Svy	Svy	Co&Yr	Reg&Yr
Obs.	32652	33055	43147	33054	33054
R^2	.022	.022	.058	.033	.06

Specifications employ the rural female sample from medium-prevalence countries and include controls and fixed effects as shown. Column (1) restricts the age range to 15-49 (excluding 50-64 yr olds in one survey). Specifications are weighted to be nationally representative, as shown. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Table 3.10: Robustness to Length of Shock Window & Placebo Test

	(1)	(2)	(3)	(4)	(5)
1.5 SD Shocks 5 Years	.012***				
	(.004)				
1.5 SD Shocks 7 Years		.011**			
		(.004)			
1.5 SD Shocks 13 Years			.016***		
			(.003)		
1.5 SD Shocks 3 Years Ahead				-.007	
				(.006)	
1.5 SD Shocks 4 Years Ahead					-.005
					(.006)
Obs.	33054	33054	33054	17242	14022
R^2	.032	.032	.033	.039	.032

All specifications employ the rural female sample from high-prevalence countries and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Next, we also present results based on inclusion of shocks from the preceding 5, 7 or 13 years, rather than 10 (Table 3.10; columns 1-3). In each case, the point estimate is between 1 and 2 percentage points, and always distinguishable from zero, but never from our baseline 1.5 SD shock point estimate of 1.4 percentage points. Additionally, we run specifications where rainfall shocks are re-defined as deviations that are more than 1, 1.25, 1.75, or 2 SD from the local mean (Table 3.11). As expected, point estimates of the impact of shocks are generally increasing with the intensity of the shock, and the estimates are suggestively non-linear: a 2 SD negative shock has more than twice the effect of a 1.5 SD shock.

Table 3.11: Effects of Shocks of Varying Size

	(1)	(2)	(3)	(4)	(5)
1.0 SD Shocks 10 Years	.013***				
	(.003)				
1.25 SD Shocks 10 Years		.012***			
		(.003)			
1.5 SD Shocks 10 Years			.014***		
			(.004)		
1.75 SD Shocks 10 Years				.026***	
				(.005)	
2.0 SD Shocks 10 Years					.031***
					(.007)
Obs.	33054	33054	33054	33054	33054
R^2	.034	.033	.033	.034	.034

All specifications employ the rural female sample from high-prevalence countries and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

One important factor to note, is that if rainfall shocks can be accurately predicted, there could exist selection issues whereby certain types who can anticipate rainfall shocks move from a village. If these types are less likely to be infected with HIV, the effect that we observed of shocks on individual HIV infection could be a result of attrition. Another concern is that shocks could somehow be proxying for other time-invariant cluster characteristics that are also associated with HIV risk, causing us to conflate the effect of shocks with some other unobservable[26]

We test for both of these potential confounders using rainfall shocks that occur *after* the survey year of each sample. Given that the DHS surveys were conducted between 2003-2009 and our weather data ends in 2008, we are not able to use similar time windows (i.e. 10 years) that are used for our main analysis. We create two time windows: 1) all shocks four years after the survey year and 2) all shocks three years after the survey year. We find no evidence that future shocks predict HIV rates

[26]Note that by construction, this is presumably not the case: the number of shocks a given location experienced over the last 10 years should be random.

(Table 3.10; columns 4-5). This placebo test suggests both that shocks are relatively unanticipated, and that our pre-survey shocks measure is unlikely to be proxying for other factors that also affect HIV risk.

3.5.4 Confounders

We assert that the main channel by which rain shocks affect HIV rates is a sexual behavior response to loss in income. Another possible channel is that income shocks cause rural women to leave school prematurely which may lead them to be sexually active at an earlier age (Baird et al., 2009). If this is occurring, we would expect the effect of shocks on HIV to be concentrated in the women who were of schooling age when the shocks occurred.

In Table 3.12, we divide the sample into four categories based on age at the time of survey and re-estimate the main equation for each. Women aged 15-21 at the survey ranged in age from 5 to 20 over the preceding ten years – prime schooling age. In contrast, women aged 32-41 at the survey were aged 22 or older when any of the shocks occurred, an age past which these women are unlikely to be in school. We find no statistically significant differences in the effects between these two groups (or the one in between). If anything, the effects are slightly larger for the older age groups. This seems to rule out the notion that leaving school is the primary driver of our results.

A second potential confounder is the possibility of selective out-migration from rural areas in the event of droughts. If certain types respond to shocks by permanently migrating and if these types are more likely to be HIV negative, then the types that remain might be more likely to be HIV positive. In this case, we observe a spurious correlation of shocks and HIV infections that reflect migration, rather than behavioral response. In order to test whether selective migration can account for the results we find, we simulate the replacement of the assumed migrants into the sample.

In adding such "ghost" individuals to our data, two questions arise: (1) how many people left per shock? and (2) what was the HIV prevalence of those that left? In order to answer question (1), we could make a variety of assumptions regarding the share of a rural village that migrates during a shock. The column headers in table 3.13 show several possible assumptions ranging from 1% to 20% *per shock*. A bit of algebra reveals that if, for example, 5% of the population leaves during each shock, a village with three shocks over the past ten years has lost 14.3% of its population in that time. The calculation of lost population by number of shocks and assumption maintained are shown in the body of table 3.13. By applying these calculations to the rural clusters in our data according to each cluster's number of shocks, we calculate the total population lost in our rural sample over the ten years before the applicable survey. The bottom row of table 3.13 shows these estimates.

Table 3.12: Are school age females driving results?

	age15to20	age21to31	age32to41	age42to49
	(1)	(2)	(3)	(4)
1.5 SD Shocks 10 Years	.010***	.014**	.018**	.015*
	(.003)	(.006)	(.007)	(.008)
Obs.	6877	10499	6451	3752
R^2	.022	.035	.069	.059

All specifications employ the rural female sample from high-prevalence countries and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

Table 3.13: Potential Reductions in Rural Populations due to Shock-induced Migration

	Share of Population Emmigrating Per Shock				
Shocks / 10 yrs	1%	5%	10%	15%	20%
0	0%	0%	0%	0%	0%
1	1%	5%	10%	15%	20%
2	1.99%	9.8%	19.0%	27.8%	36.0%
3	2.97%	14.3%	27.1%	38.6%	48.8%
Total	0.7%	3.6%	7.1%	10.5%	13.7%

Based on the World Bank Development Indicators, the share of Sub-Saharan Africa's population that lives in rural areas dropped from 68% to 63% from 1999 to 2009. This suggests that out-migration drains 7.4% of rural populations over a 10-year period. Based on the assumption that 10% of a village leaves during each shock, we estimate that our rural sample has lost 7.1% of its population in the past ten years. This suggests that an assumption of 10 to 15% population loss per shock approaches reality.

The second question is to what degree the folks that left were less likely to be HIV-positive than those that stayed. In order to be as conservative as possible, we assume that every migrant was HIV-negative. We then create enough "ghost" women to increase the female population in each cluster according to the schedule shown in table 3.13 for the 10% assumption.

Table 3.14 first reproduces our primary result: in high-prevalence countries, rural women's probability of infection increases 1.4 percentage points per shock. The second column shows the same estimation based on data which includes the additional "ghost" migrants under the 10% assumption. We see that, while the point estimate is mechanically reduced, the phenomenon cannot fully explain the positive and statistically significant results we estimate. In the third column, we repeat the entire exercise under the 15% assumption and find that, even accounting for massive out-migration (nearly 40% in some clusters), we an still reject that the effect is zero.

Table 3.14: Main Results, Accounting for Potential Migration

	Observed	TenPct	FifteenPct
	(1)	(2)	(3)
1.5SD shocks / 10yrs	0.014***	0.011***	0.008**
	(0.004)	(0.004)	(0.004)
R^2	0.010	0.010	0.010
Observations	27677	29491	29973

All specifications employ the rural female sample from high-prevalence countries and include controls for mean rainfall, age, marital status, education and wealth, as well as survey fixed effects. Columns (2) and (3) include additional observations to account for out-migration (see text). All specifications are weighted to be representative at the national level. Standard errors are shown in parentheses, adjusted for clustered sample design. Stars indicate significantly different from zero at 99(***), 95(**), and 90(*) percent confidence.

3.6 Conclusion

The intention of this work is to seek evidence on a broad scale for the proposition that vulnerability to economic shocks exacerbates the AIDS epidemic. We postulate that the pathway by which shocks increase HIV infections is an increase in risky sexual behavior taken by vulnerable women. Our work is preceded by anecdotal reports that in the face of economic hardship, women in Sub-Saharan Africa are pushed into "survival sex." In an attempt to smooth income, or perhaps insure themselves against future shocks, women may increase partnerships or increase the risks taken within existing partnerships. These actions are reportedly common in many SSA countries, and are not considered prostitution by societal norms. Nonetheless they contribute significantly to increasing the risk of HIV transmission.

We investigate whether such behavioral responses to income shocks yield significant increases in HIV infections across SSA. In 19 countries in West, Central, East, and Southern Africa, we match serostatus test results to the GIS location of the individual's home. Lacking any information on income or shocks at the individual level, we proxy village-level economic shocks by the number of droughts experienced over the preceding ten years. In rural areas of Africa, the majority of income is agriculture-based and nearly all farming is rain fed. As a result, shortfalls in annual precipitation can devastate crop yields and create significant economic hardship.

In countries with severe epidemics (upwards of 5% prevalence), the results suggest that each shock in a rural village increases the risk of HIV by 11% for women and 8% for men. In order to probe the potential pathway for this relationship, we test several hypotheses suggested by our theoretical models. We find that the effects are concentrated among women with lower levels of wealth and education and among men with the highest levels of wealth, supporting the theory that women are engaging in "survival sex" with more economically stable men. Other potential pathways suggested include early termination of schooling as a result of shocks, which leads to earlier marriage and sexual activity and thus higher levels of risk; or, selective out-migration from rural villages following shocks, which would bias the observed sample. Further empirical evidence and a simple simulation reject both alternative pathways.

The collection of evidence presented here strongly suggests that changes in sexual behavior in response to economic shocks are a contributing factor in the AIDS epidemic in Africa. Further, it seems that such behavior is specifically motivated by the vulnerability of certain groups of rural women. In countries where HIV prevalence is already high, the benefits of reducing such vulnerability could be far-reaching. Each additional infection increases risk for everyone in the network. Efforts to protect these target groups from income volatility could reduce negative externalities for society, such as the increases in prevalence that we have estimated here.

It's clear that government implementation of comprehensive social safety nets may be unrealistic in these impoverished nations. However, specific efforts such as group-based crop insurance, if properly targeted, could stem the spread of HIV by mitigating the sexual response to agricultural shocks. One could make the case that the financing of such programs by external donors is justified. In countries where prevalence has been consistently above 5% for a decade, reducing rural vulnerabilities could reap health benefits for the entire nation.

Chapter 4

The Mexico City Policy Effect

4.1 Introduction

The issue of abortion has had a long and colorful history in U.S. politics. Often considered a political flash-point, debates and lawsuits surrounding the issue span the years from 1820 up to the present day. In 1973, the issue became entwined in American foreign policy when the Helms Amendment decreed that U.S. foreign assistance funds could not be used to perform or promote abortion abroad.

In 1984, a Republican president issued an executive order that further restricted foreign aid where abortion is concerned. In the 25 years that followed, this order has been successively repealed and reinstated by Democratic and Republican administrations, respectively. It has been the concern of several major court battles, one of which ended in the Supreme Court; and at least twenty congressional debates or votes have been taken on the matter (see Appendix Table D.2). It has been officially in effect during the periods 1984-1992 and 2001-2009. It's potential reinstatement in 2011 was one of the "policy riders" that created a roadblock in the Congressional budget negotiations, nearly shutting down the federal government. The policy is clearly a divisive issue of some import in US politics.

This executive order is known as the Mexico City Policy, based on its introduction at the International Conference on Population held in Mexico City in 1984. It states the following:

> "U.S. support for family planning programs is based on respect for human life, enhancement of human dignity, and strengthening of the family. Attempts to use abortion... in family planning must be shunned... [T]he United States does not consider abortion an acceptable element of family planning programs and will no longer contribute to those of which it is a part. ... Moreover, the United States will no longer contribute to separate nongovernmental organizations which perform or actively promote abortion as a method of family planning in other nations." [The White House Office of Policy Development, 1984]

As a result of this policy, foreign NGOs were required to sign official affidavits stating that they would not perform, or lobby for, safe abortion. If they refused, they would forfeit any and all population assistance provided by the United States Agency for International Development (USAID).[1]

At the time of the policy's creation, and still today, abortion on-request is not legal in many countries that receive US population assistance. Further, the Helms Amendment already forbade the use of US monies for that purpose. Therefore, it was the forbidding of organizations to use their own funds to education women about safe abortion options or lobby the government for legalization that earned the policy the derisive nickname "the global gag rule."

Upon imposition of the policy, especially the reinstatement in 2001, advocacy organizations reported that the policy exerted numerous negative side effects. Country reports gave details of funding lost by specific organizations and the breakdown of sector-specific government-NGO partnerships (Turnbull and Bogecho, 2003). In many cases, organizations were reportedly forced to reduce rural outreach services, claiming that many poor, rural women were left without access to contraceptives. In some countries, several reproductive health clinics were closed. An investigative report further suggested a "chilling" effect, whereby signatory organizations also cutback on certain reproductive health activities out of fear of also losing funding (Blane and Friedman, 1990).

Given the extensive time and energy devoted to debating this rule in the U.S., and its potential for adverse side effects, whether or not its imposition achieves its stated aims should be of some interest to policy makers. It seems from the issuing statement that the purpose of its imposition was two-fold: (1) to reduce the use of elective abortion for family planning purposes in foreign nations, and (2) to impede countries' potential movements toward increased legalization of abortion. To my knowledge, there is no existing research on the degree to which the policy makes progress toward either of these objectives.

It is the aim of this paper to ascertain whether or not objective (1) is achieved by the Mexico City policy (MCP). In a companion paper, I also offer suggestive evidence regarding the policy's effectiveness on the legislative objective. Here, I investigate whether the use of abortion is, in fact, less prevalent in recipient countries during the years of the policy. Such an investigation presents two key challenges. The first is that very little data exists on the use of abortion in poor countries, either at the individual level or in aggregate. I am benefited in this regard by one survey conducted by MEASURE DHS in Ghana in 2007, which explicitly asks women about the abortion of past pregnancies.

Secondly, lacking any logical comparison group, it is difficult to say whether differences during the years of the policy are actually attributable to the policy, or other events of the time. To address this issue, I use the complete pregnancy histories col-

[1] Population assistance is defined as funding to support the provision of contraception and family planning in foreign nations.

lected in this special DHS to create a woman-by-month panel from 1975 to 2007. Given the age range of women at the time of the survey, I observe women moving both into and out of MCP periods during their reproductive years. The creation of this panel allows a within-woman estimation, which controls for unobservables at the individual level. Further, to focus on effects specifically resulting from the policy, I use a regression discontinuity design, restricting the window of analysis to the few years surrounding each policy change.

I find that, on average, a woman is no less likely to abort a pregnancy when the policy is in effect than at any other time. Examining demographic subgroups by rural location, wealth level, and education, I find no significant reduction for any group. However, for certain subgroups, evidence suggests that women increase the use of abortion during MCP periods. Similar estimations show that such women also experience increased likelihood of conception during these periods, which is consistent with the advocacy groups' claims that access to contraception was restricted. If women were more likely to experience unwanted pregnancies during these times, this would explain the increase in abortion rates.

In the following section I provide further history of the Mexico City policy, and discuss the Ghana case in detail. In section 4.3, I describe the data employed and the creation of the woman-by-month rolling panel. Section 4.4 presents empirical specifications and estimation results. I discuss the findings and the implications for policy in the concluding section 4.6.

4.2 Background

In August 1984, the United Nations held the International Conference on Population in Mexico City. The official statement of the United States at this conference unveiled a new policy regarding the use of American population assistance funds. The administration of President Reagan issued an executive order stating that any non-governmental organization receiving such funding must attest that they do not perform or actively promote abortion as a means of family planning.

Certainly, many NGOs were willing to make such attestations. However, some organizations were unwilling, in particular, those for which reproductive health and family planning were the foremost objective. These organizations saw the provision of safe abortion (as an alternative to pervasive unsafe abortions) and the fight for legalization of safe abortion as central to their mandate. Both large, international organizations such as International Planned Parenthood Federation (IPPF) and Marie Stopes International (MSI), and small local NGOs such as Family Guidance Association of Ethiopia and Family Planning Association of Kenya were affected. NGOs that refused to sign the policy lost all funding from USAID, amounting to 10-60% of organizational budgets.

Funding shortfalls resulting from lost USAID funding took effect in early 1985. The policy remained in effect, virtually unchanged, until it was repealed by President

Clinton in January, 1993. A modified version of the policy was implemented in 1999 and the full policy was reinstated by President Bush in January, 2001. The policy was extended to apply to State Department funds as well in August, 2003. Despite many Congressional votes on the matter, the policy remained in effect until it was rescinded by President Obama in January, 2009. It is significant that for Presidents Clinton, Bush, and Obama, their change to the policy's effectiveness was issued on the first or second day following inauguration.

In the interim period 1993-2000, when the Mexico City Policy was not in effect, the U.S. provided nearly 40% of population assistance worldwide (UNFPA, 2004). On average, about half of that funding flowed to non-governmental organizations (PAI, 1999). According to USAID documents in late 1999, funding allocated to reproductive health NGOs in Ghana for FY2001 was \$1.4m, higher than in nearly all other countries (USAID, 1999).[2] While organizations in many countries lost funding as a result of the policy re-imposition, Ghana stood to lose much more than most.

Repercussions of the policy in Ghana

Information regarding NGO funding prior to the 1984 implementation of the Mexico City policy is not readily available. However, the situation surrounding the re-imposition of the policy in 2001 provides some insight regarding the policy's effect. Planned Parenthood Association of Ghana (PPAG) was (and is) the leading NGO-provider of reproductive and sexual health services in Ghana.. As of late 1999, PPAG was slated to receive \$565,000 from USAID in 2001 (USAID, 1999). Upon the executive order in January 2001, these funds would only be disbursed if the organization agreed to the Mexico City policy.[3]

Under normal circumstances, nearly all the funding for PPAG comes from the International Planned Parenthood Federation (IPPF). However, at this time, USAID was funding a large Community-Based Services (CBS) project through PPAG. As such, USAID was slated to provide 1/4 of PPAG's budget for FY2001. The CBS project was scheduled to run through 2003 and in order to preserve this project, PPAG agreed to the MCP to keep its USAID funding (Turnbull and Bogecho, 2003; IPPF, 2002).

However, from 2001 to 2003 PPAG did experience significant budget losses, as its funding from IPPF was reduced by 54% (reducing the total budget by 40%) (IPPF, 2002). As IPPF had refused to sign the policy, it had experienced budget cuts. Out of necessity, these were passed on to its member organizations.[4] In 2003, at the conclusion of the CBS project, PPAG rejected the policy and lost USAID funding (and in-kind

[2] Ghana is second only to Nepal, with \$1.9m allocated to RH NGOs. However, detailed abortion data is not available for Nepal.

[3] Each of these organizations discussed below also existed prior to the 1984 enactment of the policy and likely reacted similarly at that time.

[4] Prior to the 2001 re-imposition of the Mexico City policy, USAID was providing 7.3% of income for IPPF (IPPF, 2002). It's not clear why cuts to PPAG were so large relative to IPPF losses. Perhaps this reflects IPPF's displeasure with PPAG for agreeing to the policy from 2001 to 2003.

donations of contraceptives) in addition to previous budget cuts from IPPF. Funding from IPPF did not recover until after the repeal in 2009 (see Table 4.1).

Table 4.1: IPPF Funding to Planned Parenthood Association of Ghana

Allocation Year	Funding from IPPF	As percent of funding in 2000
2000	$1,694,592	
2001	$926,706	55%
2002	$780,000	46%
2003	$902,851	53%
2004	$1,199,589	71%
2005	$1,114,402	66%
2006	$1,125,598	66%
2007	$1,148,371	68%
2008	$1,270,742	75%

Source: IPPF financial statements 2001-2009

Table 4.2: Family Planning Commodity Availability (as percent of clinics)

	1993 (MCP1)	1996 (NoMCP)		2002 (MCP2)	
Combined Pill	92%	92%		82%	**
Progesterone Pill	62%	86%	**	75%	**
Condom	85%	93%	**	87%	**
Injectable	94%	90%		93%	
Spermicide	85%	91%	*	74%	**
IUD	89%	89%		76%	**

Source: Hong et al. (2005)
* Indicates that the measure is significantly different from the measure in the previous survey at the 5% level (** 1%).

Data from a nationally representative Demographic and Health Survey (DHS) in 1998 suggest that of Ghanaian women using contraceptives at that time, 44% were acquiring them from private providers such as PPAG, and 48% from government providers.[5] Surveys of both government and NGO providers of family planning services in Ghana were undertaken in 1993, 1996 and 2002.[6] A comprehensive report based

[5] The remaining 8% reported acquiring them from shops, churches, friends, or other.

[6] In 1993 and 1996 by the Population Council's Africa Operations Research and Technical Assistance Project. In 2002 by Macro International as part of the MEASURE DHS+ project.

on these surveys suggests that contraceptive availability was lower during the years the policy was in effect (Hong et al., 2005). The availability of contraceptive methods (weakly) increased from 1993 to 1996 for five out of six methods, and decreased from 1996 to 2002 for five out of six methods (see Table 4.2).

4.3 Data

Macro Internationale's MEASURE project routinely conducts nationally representative Demographic and Health Surveys (DHS) in developing countries, focusing on women aged 15-49. In 2007, DHS conducted a non-standard survey in Ghana composed of special modules on maternal mortality and abortion. Unlike most DHS, which collect a woman's complete birth history, this survey queried each woman's complete *pregnancy* history, including pregnancies that ended in miscarriages, stillbirths and abortions. While a handful of other DHS also collect pregnancy (rather than birth) histories, the Ghana 2007 survey is the only one that explicitly records the use of induced abortion.[7]

The survey contains information for 10,370 women. For each pregnancy in a woman's lifetime, the following information is recorded: the duration of the pregnancy, the month and year it ended (from which one can deduce the month it began), how it ended (live birth, stillbirth, miscarriage, or abortion), and further information about the child if it was a live birth. Using this, I create a woman-by-month panel. In each month, a woman has one of the following seven statuses: conceived, is pregnant, birthed a live child, had a stillbirth, miscarried, aborted a pregnancy, or was not pregnant. Moving consecutively through the months, summing the live births, I calculate her existing parity (number of children previously born) in each month. The survey also collects information regarding the woman's date of birth and month and year of first marriage (or cohabiting union). Using these, each observation is assigned the woman's age at the time, and whether or not she has ever been in union. Months in which the woman is at least 15 years of age compose the complete data set.

Other information collected about the woman does not vary over time, but is useful for dividing women in to demographic subgroup. A wealth index for her household is created based on a principle components analysis of information about housing quality, drinking water source, toilet facilities and durable assets (Filmer and Pritchett, 2001). From this, women are classified by national wealth quartile. While wealth may vary throughout a woman's life, it seems that wealth *quartile* is likely somewhat stable. Nonetheless, one might prefer an alternative indicator of a woman's socio-economic status; and for this I use educational attainment. This too is measured at the time of the survey only, but we can be reasonably sure that it has not changed since age 18 for most women. Based on the 1998 Ghana DHS, 82% of 18 year old women are no longer in

[7]Further, other surveys conducted after 2001 that include pregnancy histories are in countries unlikely to be as affected by the Mexico City policy: Armenia 2005, Azerbaijan 2006, Moldova 2005, Philippines 2008, and Ukraine 2007.

school. In the 2007 data, just over a quarter of women have never attended school. The remainder are classified as having attended primary (21%), middle (40%), or secondary or higher (13%). In some specifications, I classify women as "low education" (primary or none) and "high education" (middle school or higher).

For each woman, her panel begins when she turns 15 and ends when she is interviewed (max age is 49). There are 1.85 million observations from November, 1972 to December, 2007. Each woman has between 23 and 444 observations (mean is 185). Figure 4.4.1.A shows the conception rate by age; that is, the share of fecund woman-months in which a conception occurred.[8] The conception rates are highest (over 2.5%) for women aged 22 - 27. A gradual decline begins around age 28, becoming steeper at age 37. For women younger than age 17, or aged 40+, the chance of conception is less than 1%.

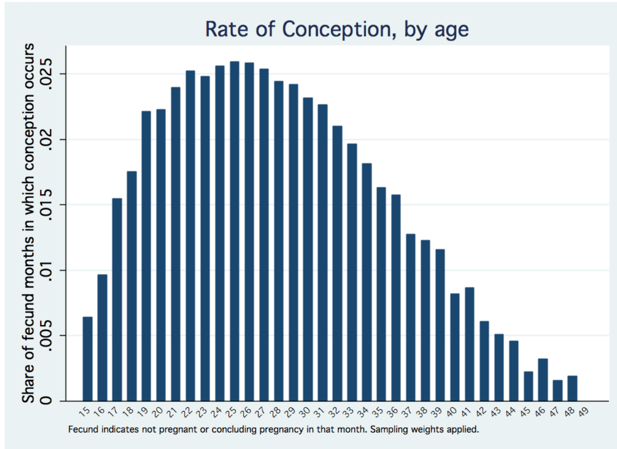
Figure 4.4.1.B shows the abortion rate by age; that is, the share of pregnancy conclusions that are abortions. The likelihood of aborting a pregnancy is greatest for the youngest women; over 15% for 15 year olds. However, considering their low number of pregnancies, this represents a small share of total procedures. The likelihood of aborting a pregnancy declines with age, generally remaining below 5% for women over age 25. Figure 4.4.1.C shows the probability of having an abortion, by age. The combination of high conception rates and high abortion rates yield the greatest chance of having an abortion for women aged 18 - 20: about 2% per year (.0018*12). Women outside the 17 - 25 age range have a considerably lower probability: less than 1% per year.

4.4 Estimation

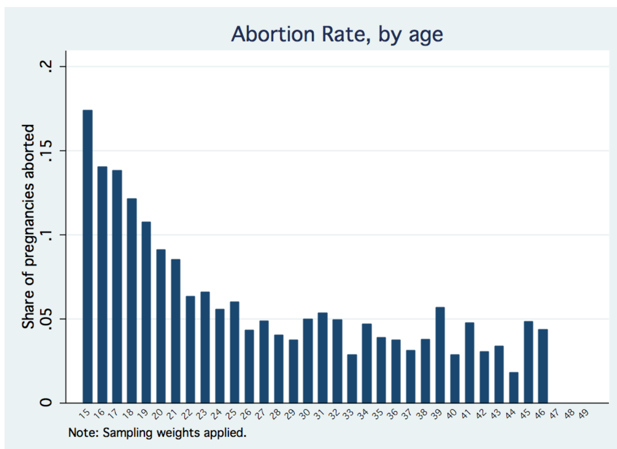
It is the intention of this estimation to determine whether the imposition (or removal) of the Mexico City policy had any discernible effect on the degree to which abortion is used as a method of birth control in developing countries. Ideally, this estimation would encompass all recipient countries of USAID population assistance. However, given the existence of detailed pregnancy history and abortion data for only one of these countries (Ghana), the estimation is thus restricted. Nonetheless, Ghana seems to be a reasonable test case for this question, given that it seems to be a primary destination of population funding to NGOs (which is the focus of the policy). Therefore, if the policy were to accomplish the objective of reducing abortion anywhere, Ghana would be a most likely setting.

[8] Women are considered fecund (capable of conceiving) if they are not already pregnant or concluding a pregnancy. Information about an individual's natural fecundity or menopausal status is not available.

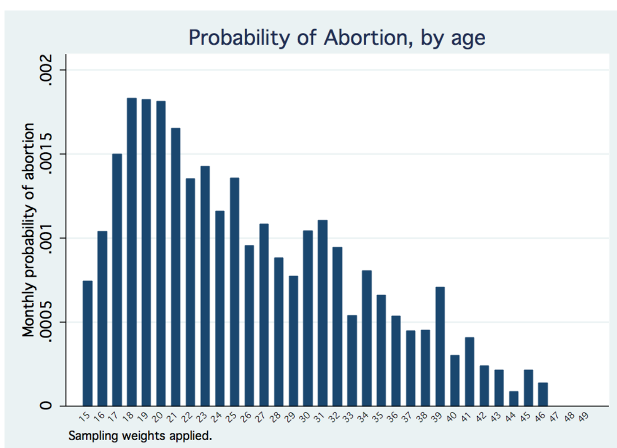
Figure 4.4.1: Conception & Abortion Statistics



4.4.1.A



4.4.1.B



4.4.1.C

Creating a panel data set which encompasses the years 1972 to 2007 allows for examination of four different periods in relation to the policy. Period 0 ("PRE") from 1972 to 1984, period 1 ("ON 1") from 1985 to 1992, period 2 ("OFF") from 1993 to 2000, and period 3 ("ON 2") from 2001 onward. However, a simple comparison of abortion statistics across these periods would be misleading, as the sample characteristics differ across the periods as well. Table 4.3 shows that the mean age for the full sample in significantly increasing over the periods. In order to keep the sample more consistent across periods, one can restrict the age range, effectively creating a rolling panel. The last two columns of Table 4.3 show that when restricting observations to those for women aged 17 - 25 (the primary group for abortion procedures), the mean age is much more similar across periods. Further restricting to the group in which most abortions occur (18 - 20 year olds) produces mean ages nearly identical across periods.

Table 4.3: Mean Age of Sample, by period

Period	Years	Full Sample	17 - 25 yos	18 - 20 yos
PRE	1972-1984	18.3	19.7	18.9
ON 1	1985-1992	21.9	20.7	19.0
OFF	1993-2000	25.1	20.8	19.0
ON 2	2000-2007	28.2	20.9	19.0

Another concern is the degree to which conception and abortion are affected by environmental and situational concerns beyond the policy of focus. For example, birth rates fluctuate in tandem with business cycles, as couples are more reluctant to have children during recessions (Kirk and Thomas, 1960). For this reason, it is important to control for other, unobservable factors changing over time. Because the imposition (or removal) of the policy always coincided with the change in calendar year, year fixed effects would be perfectly collinear with an indicator for the policy. I therefore employ a regression discontinuity design, whereby I compare the use of abortion just before versus just after a change in the policy, focusing on a reasonably narrow window of time around each change.

Finally, in order to control for the host of unobservable characteristics about each woman that certainly affect such decisions, I employ woman-fixed effect to compare each woman only with herself. Further, because a woman's preference for having a child changes throughout her life, I include controls for time-varying characteristics that often predict conception and childbirth: age (in a quadratic form) and parity (previous number of births).

The primary estimation is

$$A_{imc}^{t} = \alpha + \beta ON_m + X_{im}'\phi + \gamma m + \nu_i + \nu_c + \varepsilon_{imc} \tag{4.4.1}$$

where A_{imc}^{t} indicates that the pregnancy of woman i that ended in month m was aborted, where m is within t months of the policy change c.[9] The index c takes the value 1, 2, or 3, representing which policy change is within t months of m.[10] X_{im} is a vector containing quadratic functions of age and parity specific to woman and month, plus an indicator for whether she has ever been in a cohabiting union. Fixed effects for both the individual and the policy change for which the observation is in the relevant window are included as ν_i and ν_c, and γm represents a linear time trend.

The independent regressor of interest is ON_m, which indicates that the policy was in effect in month m. If β is significantly less than zero, this would indicate that, conditional on age, existing parity and ever-unioned status, a given woman is less likely to abort a pregnancy when the policy is in effect than at other times. Such a finding would provide evidence that the policy achieves this primary objective. If I fail to reject that β is zero, it will be difficult to say whether the policy has any effect, though perhaps bounds on the effect size (and direction) could be obtained. In this case, interactions of demographic indicators with ON_m can be used to check for significant effects for separate sub-groups.

There are several considerations in the appropriate selection of t, the window size for the regression discontinuity design (RD). For policy changes that are cleanly applied and immediately effective, one prefers a narrow window. While the budget cuts resulting from this policy were rather immediate, it likely took some time for effects to be felt at the clinic-level, which suggests that a window of at least one year on either side of the change would be required. Further, by including woman-fixed effects, one desires to have a large enough window that a woman might reasonably have at least two pregnancies in that time. In this regard, a larger window is better, as more observations per woman will increase precision. This suggests a window of two to four years would be best. However, given the possibility that the policy has the greatest effect immediately after its imposition, especially considering the chance of compensatory funding from other donors as time passes, one prefers to keep the window from becoming too wide. I therefore select $t = 30$ as the default, selecting 30 months of time on either side of each change for inclusion in my primary specifications. In robustness checks I will let t vary from 24 to 48.

4.5 Results

Table 4.4 presents summary statistics regarding conception and abortion rates for various sub-samples discussed below. Overall, for women aged 17 - 25, the probability of conception in a given month (when not already pregnant) is .022. Over the course of a year, the summation of probabilities over each month, conditional on non-conception in the previous months, yields an annual probability of conception of .29. This differs

[9]Here, m is a continuous measure of months from January 1972, not the calendar month.

[10]For example, $c = 1$ represents the change in 1985 from PRE to ON1, $c = 2$ represent the change in 1993 from ON1 to OFF, etc.

significantly between rural and urban populations (.286 vs. .293), but differs little between the rural sub-groups shown. In contrast, the share of pregnancies aborted differs significantly between rural and urban sectors and between rural sub-groups. In total, 4.8% of rural pregnancies are aborted. However, the poorest of the poor have a rate of 1.4% vs. 5.9% for the less poor; and those not completing primary school have a rate of 2.7% vs. 9.1% for those that have. Based on this use data, it would be surprising to find any affect on abortion use for the poorest (and least educated) of the rural populations, since they are either unable or unwilling to access this service in general.

Table 4.5 shows results from estimations of equation (4.4.1) for the full sample and several subgroups. For the full sample, the coefficient is positive, the order of magnitude suggests a 2.5% increase in abortion (as a share of pregnancy conclusions) during policy periods. The standard error is quite large, however, and the 95% confidence interval ranges from an increase of 21% to a decrease of 16%.

The lack of precision in the full sample results reflect the significant differences between the policy's effect in urban versus rural areas. The point estimate for the urban population is negative, yet also very imprecise. We cannot reject that the effect in urban areas is zero. However, in rural areas, the estimation suggests that the policy increased the use of abortion by 1.2 percentage points; we can reject with 90% confidence that this effect is zero. Given that only 4.8% of pregnancies are aborted in rural areas, this change reflects a 25% increase in the use of abortion – a surprisingly large effect, the potential cause of which is discussed shortly.

In order to check thoroughly for any sub-population that could potentially exhibit the intended effect of the policy (a *reduction* in abortion use), the last four columns of Table 4.5 and all columns of Table 4.6 examine various sub-groups. In urban areas, I find that the poorest quartile also show an increase in abortions, though it is not statistically significant. In rural areas, I find that it is the less poor that exhibit significantly increased use, while the poorest of the poor have a near zero effect. The main rural population (excluding the poorest of the poor) increases abortion use by 1.68 percentage points as a result of the policy; a 28% increase from the average rate of 5.9%.[11]

Because wealth quartiles are based on the wealth indicators of the household at the time of the survey, these groups may not reflect the wealth of the woman at the time of each pregnancy. In particular, one might be concerned that the decision to abort an early (or an additional) pregnancy may increase a woman's potential for future wealth. Therefore I employ education as an alternative indicator of socio-economic status, focusing on whether or not the woman completed primary school. The primary school completion rate is 70% in the urban population and 40% in the rural population. This is a characteristic of a woman that is unchanging over time, after about age 12, and certainly by age 16.

[11]It is notable that this group excludes only the poorest quarter of rural populations. Given rural poverty levels, this group cannot truthfully be referred to as non-poor. Even in this group 70% are poor by international standards, that is, living on less than $2/day.

Table 4.4: Summary of Contraceptive Use, Conception and Abortion

	N	Ever Used Contraceptives	Ages 17 - 25: Rate of Conception	Ages 17 - 25: Aborted share of Pregnancies
All	10,370	52.5%	2.2%	8.6%
Urban	5,410	59.0%	1.8%	15.3%
Rural	4,960	46.8%	2.5%	4.8%
Rural Sub-groups				
Poorest	1,335	**26.6%**	2.6%	**1.4%**
Less Poor	4,075	53.0%	2.5%	5.9%
Less than Primary School	3,270	**37.6%**	2.7%	**2.7%**
At least Primary School	2,140	60.7%	2.3%	9.1%

Notes: Rates of conception and abortion are for months when women are aged 17 - 25. Rate of conception is the probability of conception in a month when not already pregnant. "Less Poor" is the top 3 wealth quartiles in the rural population; note that 70% of this group is poor by the international standard of $2/day. "Less than Primary School" includes those with no education and those attending some primary school but not completing grade 6.

Table 4.5: Policy's Effect on Share of Pregnancies ending by Abortion

	All	Urban	Rural	Urban: Poor	Urban: NonPoor	Rural: Poorest	Rural: Less Poor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	0.0022	-0.0136	0.0123*	0.0123	-0.0193	-0.0010	0.0168*
	(0.008)	(0.018)	(0.007)	(0.023)	(0.026)	(0.005)	(0.009)
Obs.	8085	3220	4865	948	2201	1275	3526
R^2	0.705	0.735	0.660	0.680	0.750	0.662	0.663
Mean	0.086	0.153	0.048	0.090	0.182	0.014	0.059

Samples include all pregnancy conclusions within 30 months of a policy change, and women aged 17-25. All specifications include woman-fixed effects, woman level controls as described in the text, a time trend, and indicators for which policy change is relevant. "Poor" indicates the lowest wealth quartile (specific to rural/urban sector). In the urban sample, the top three quartiles are all non-poor. In the rural sample, 70% of the top three quartiles are still poor by international standards ($2/day). Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

Table 4.6 shows estimates of the policy's affect for each of the education sub-groups. For neither education group in the urban area can the effect of the policy be statistically distinguished from zero. In rural areas, the effect is non-zero only for women that have completed primary school. This likely reflects the very low use of abortion in general for the rural population with less than primary school education. For women completing basic education, the policy increases the share of pregnancies aborted by 3.87 percentage points, an increase of 42%.

Table 4.6: Policy's Effect on Abortion by Education Level

	Urban		Rural	
	< Primary	Primary +	< Primary	Primary +
	(1)	(2)	(3)	(4)
Policy	-0.0132	-0.0082	0.0063	0.0387**
	(0.018)	(0.030)	(0.007)	(0.019)
Obs.	1318	1902	3398	1467
R^2	0.626	0.754	0.608	0.714
Mean of Dep. Var.	0.067	0.210	0.027	0.091

Samples include all pregnancy conclusions within 30 months of a policy change if the woman was aged 17-25. All specifications include woman-fixed effects, woman level controls as described in the text, a time trend, and indicators for which policy change is relevant. Categories delineate whether or not the woman *completed* primary school. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

4.5.1 Policy effects on conception rates

Given that urban populations in this sample have a fairly high abortion rate, the lack of policy effect in this sector is surprising. Further, considering that the policy was intended to decrease the use of abortion as a means family planning, the increase in usage for some groups is surprising.

Advocacy groups have claimed that the funding losses resulting from this policy primarily impacted the availability of contraceptives to poor, rural populations, rather than the provision of abortion services (Cincotta and Crane, 2001; Crane and Dusenberry, 2004). In particular, a report states that in Ghana, "the major cutbacks in PPAG staff and the loss of its community-based distributors have limited its outreach capabilities, particularly in the most remote areas of Ghana" (Turnbull and Bogecho, 2003). If such claims are true, we would expect that the reduction in access to contraception would increase rates of conception.

Table 4.7 provides estimates of the policy's effect on the probability of conception in a month when a woman is not already pregnant, for women aged 17-25. Results for the urban population are not significantly different from zero, reflecting the fact that contraceptives are more broadly available in urban areas. However, according to reports, contraceptive access in rural areas depends on the outreach services provided by groups such as PPAG. The estimates show that when this NGO lost funding as a result of the policy, the probability of conception per month in rural areas increased by 0.0016. This represents a 6.4% increase in pregnancies and is statistically different from zero with 89% confidence. As shown in Table 4.5, an additional 1.2% of pregnancies in rural areas were aborted during the periods of the policy, suggesting that the remaining 5.2% increase in unplanned pregnancies resulted in unplanned or unwanted births.

Columns 4 -7 of Table 4.7 show similar estimations for rural sub-groups. I find that the policy had little-to-no effect on conception rates of the poorest of the poor. This is surprising, given the focus of advocacy reports on services to the rural poor. Table 4.4 shows historical use of contraception by these sub-group. It reveals that the poorest of the poor are far less likely than others to have ever used contraception. Therefore, reductions in contraceptive availability are less likely to affect their conception rate. However, for the top three quartiles of the rural population, the probability of conception increases by 0.0023 during policy periods. This reflects a 9.2% increase in pregnancies and is statistically significant at the 5% level. While this group is not the poorest, most are indeed poor by international standards: 70% of them live on less than $2/day. Results from Table 4.5 suggest that this group aborted an additional 1.68% of pregnancies as a result of the policy – a large increase relative to the baseline abortion rate, but one that is more than explained by a 9% increase in pregnancies.

Focusing on sub-groups by basic education status, rather than current wealth, the results look very similar. Those lacking basic education are significantly less likely than others to have ever used contraception, and thus have conception rates unaffected by the policy. However, the remainder of rural women, who have contraceptive ever-use rates similar to those of urban women, experienced significant increases in conception during policy periods. For this group, a 12% increase in pregnancies precipitated an additional 4% of pregnancies aborted.

4.5.2 Specification Tests

The regression discontinuity design is based on the assumption that the best estimate of a policy's effect is the comparison of events just before and after it, within a narrowly defined window. Under this assumption, one should find the strongest effects when using the narrowest window of estimation, with effects diminishing as observations farther from the policy change are included.

Table 4.8 shows the estimation of policy impacts on abortion use for rural women, excluding those without basic education, using four different windows of estimation. The smallest feasible window that allows a enough women to have at least two preg-

Table 4.7: Policy's Effect on Probability of Conception per Month

			Rural				
	All	Urban	All	Poorest	Less Poor	< Primary	Primary +
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	0.0006	-0.0005	0.0016^{+}	0.0005	0.0023**	0.0015	0.0028**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Obs.	370516	179456	191060	48707	139703	126918	64142
R^2	0.043	0.042	0.043	0.046	0.043	0.045	0.042
Mean of Dep. Var.	0.022	0.018	0.025	0.026	0.025	0.027	0.023
Effective Increase in Pregnancy	..	..	6.4%	..	9.2%	..	12.2%

Samples include all observations within 30 months of a policy change in which a woman was aged 17-25 and not already pregnant or concluding a pregnancy. All specifications include woman-fixed effects, woman level controls as described in the text, a time trend, and indicators for which policy change is relevant. For descriptions of sub-groups by schooling and wealth, see notes to Tables 4.5 and 4.6. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%, + 11%.

nancies, and thereby allows the use of woman-fixed effects, is 24 months. Column 1 shows that this window provides a larger estimate of policy impact, suggesting that the 12% increase in pregnancies precipitated the abortion of an additional 5.8% of births. As the window is expanded from the default of 30 months, the effect remains positive but becomes gradually smaller and statistically indistinguishable from zero.

Table 4.8: Variation in Window Around Discontinuity

	24 mos.	30mos.	36 mos.	48 mos.
	(1)	(2)	(3)	(4)
Policy	0.0583**	0.0387**	0.0198	0.0090
	(0.030)	(0.019)	(0.016)	(0.014)
Obs.	1184	1467	1732	2240
R^2	0.769	0.714	0.695	0.633

Samples include pregnancy conclusions within the specified number of months of a policy change for rural women with at least primary school education. All specifications include woman-fixed effects, woman level controls as described in the text, a time trend, and indicators for which policy change is relevant. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

In order to further ensure that the effects I am estimating are due to the policy, I perform a placebo test. In this, I select three months, each approximately two years before a true policy change, and falsely assume the the policy changed at these times. The estimations of equation (4.4.1) are performed under this assumption and are shown in Table 4.9. Neither for the full sample, nor for any of the ten sub-groups examined, does the false policy change exhibit any effect on abortion use. Coefficients alternate between positive and negative, but none is significantly different from zero, even at the 15% level. This suggests that the estimates of policy effect shown in the preceding sections are, in fact, due to the policy and not to other factors occurring around the same time.

Table 4.9: Placebo Test

	All	Urban				
		All	Poor	NonPoor	< Primary	Primary +
	(1)	(2)	(3)	(4)	(5)	(6)
Faux Policy Change	0.0107	0.0088	-0.0136	0.0274	-0.0141	0.0394
	(0.008)	(0.017)	(0.031)	(0.022)	(0.015)	(0.030)
Obs.	7523	3028	875	2081	1287	1741
R^2	0.735	0.755	0.703	0.764	0.699	0.771
		Rural				
		All	Poor	Less Poor	< Primary	Primary +
		(7)	(8)	(9)	(10)	(11)
Faux Policy Change		0.0021	0.0028	-0.0012	0.0060	-0.0043
		(0.009)	(0.007)	(0.011)	(0.008)	(0.022)
Obs.		4495	1165	3270	3183	1312
R^2		0.704	0.694	0.706	0.629	0.769

Samples include pregnancy conclusions within the 30 months of one of three false policy changes for rural women with at least primary school education. All specifications include woman-fixed effects, woman level controls as described in the text, a time trend, and indicators for which false policy change is relevant. Sampling weights are employed. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%, + 15%.

4.5.3 Robustness Checks

In order to check for the sensitivity of the results to the assumptions made herein, I present the results for rural women with basic education under slightly different assumptions in Table 4.10. In the opening of section 4.4, I discuss the need to restrict the age range of women in included observations. The default age range is 17 - 25, based on the natural breaks in abortion use on either side of this range. Columns 1 and 3 present results under a larger and smaller age range, respectively. Neither of these differ significantly from the primary estimation.

In the remaining columns I variously estimate without the time trend (column 4), without the full set of controls (column 5), and without the sampling weights (column 6). None of these changes affects the result substantially.

Table 4.10: Robustness Checks

				Ages 17-25, without...		
	Ages 16-26	Ages 17-25	Ages 18-24	Time Trend	Full Controls	Weights
	(1)	(2)	(3)	(4)	(5)	(6)
Policy	0.0306*	0.0387**	0.0466*	0.0428**	0.0380**	0.0309*
	(0.017)	(0.019)	(0.024)	(0.019)	(0.019)	(0.018)
Obs.	1666	1467	1184	1467	1467	1467
R^2	0.684	0.714	0.775	0.712	0.711	0.714

Samples include pregnancy conclusions within 30 months of a policy change for rural women with at least primary school education. All specifications include woman-fixed effects and indicators for which policy change is relevant, with woman-month level controls and a time trend as noted. Sampling weights are employed, except where noted. Standard errors are shown in parentheses, clustered at the cluster level. ** indicates statistical significance at the 5% level; * 10%.

4.6 Discussion

This exercise has endeavored to show whether or not the Mexico City policy accomplished the aim of reducing the use of abortion in foreign nations that receive USAID funding. Lack of data on abortion use in most recipient countries prevents the answering of this question comprehensively. Nonetheless, available data for Ghana allows an analysis of the policy's effect in one country, which provides suggestive evidence of the policy's effect more broadly.

The richness of the Ghana DHS data enables the creation of a woman-by-month panel data set of conception, pregnancy, and various types of pregnancy conclusions, including abortion. Because the policy was implemented in 1984, rescinded in 1993, and re-imposed in 2001, there are three clear breaks in the policy that can be exploited for analysis. Using a regression-discontinuity design, I compare whether a given woman is more or less likely to abort a pregnancy that occurs just after the policy is enacted (or re-enacted) or just before it is rescinded vis-a-vis her other pregnancies that occurred just before enactment or after removal of the policy.

Despite the fact that most abortions in Ghana occur among the urban population, the policy did not have a discernible effect in urban areas. This likely reflects the fact that women in urban areas have many options for pregnancy prevention and conclusion, including both public provision as well as numerous private providers. Budget cuts to PPAG would be unlikely to significantly alter service provision in urban areas. Given the standard errors in the estimations, I cannot reject the possibility that the policy slightly increased or decreased abortion use in urban areas. Nonetheless, I find no statistically significant evidence that the U.S. policy reduced the use of abortion among urban women in Ghana.

The situation among rural women in Ghana appears to be quite different. According to advocacy groups, it was this sector that was reportedly most affected by the policy – primarily by reduced access to contraception. I find evidence that this did occur; the conception rate among rural women increases by 6.4% when the policy is in effect. Surprisingly, it is not the poorest of the poor (or the least educated) that were most affected by this. Because these groups are significantly less likely to have ever used contraception, the reduction in access was less salient for them. It seems that the women most likely to choose contraception – those with at least basic education – were the most affected. For these women, pregnancies increased by 12% as a result of the policy.

With pregnancy increasing at a time when contraceptive access is restricted, one assumes that the additional pregnancies are unwanted, or at least unplanned. This is borne out in the results for abortion use. Nearly 20% of the additional pregnancies of rural women ended in abortion. For rural women with basic education, one-third of additional pregnancies were aborted. Considering the increase in conception, this suggests that the total number of children born in rural areas during these periods was increased by more than 5% – all of which were likely unwanted or unplanned.

If the intent of the proponents of the Mexico City policy is to reduce the use of abortion as a means of family planning, it appears that this policy misses its mark. For no demographic group was evidence found of a significant reduction in abortion during the periods in which the policy was effective. On the contrary, because organizations affected by the policy are also those that provide contraceptives in rural areas, the policy increased the occurrence of unwanted pregnancy for rural women. As unwanted pregnancy increased, the use of abortion increased, particularly for women with the means to do so. For some groups, the rate of abortion, as a percent of total pregnancies, increased by as much as 43%.

While the "pro-life" contingent in the U.S. would deem the increase in abortion to be the greatest downfall of this policy, a further harm is done by it as well. The increase in pregnancy resulting from reduced access to contraception was only partially offset by the use of abortion. The majority of these unplanned pregnancies were brought into the world, on average into poor, rural homes without the ability to care for them comfortably or provide for them basic education. Further, women who would otherwise have chosen to have no more children experienced the unnecessary risk of additional childbirth. And finally, young women who would otherwise choose to continue their education or further their career were forced into early motherhood.

I cannot conclude based on Ghana alone that the policy is wholly unsuccessful in its aims worldwide, or that the unintended consequences are widespread. In many recipient countries the conditions for a legal abortion are much more restrictive than in Ghana. In such countries, it is possible that we would not observe the offsetting of increased pregnancy with increased abortion. In these cases, the policy may not increase the use of abortion but would increase the occurrence of unwanted births even more. It is important to note that under normal circumstances USAID funding to PPAG is quite large relative to reproductive health NGOs in other countries. Therefore, Ghana stood to lose more than other countries from the policy. So while it may not have increased abortion in some of the other recipient countries, it seems unlikely that it could decrease it, if it did not do so in Ghana.

Much of the American public holds strong opinions on the issue of abortion, on both ends of the spectrum. As such, it is common for both political parties to use this issue to engage their constituents. Each party enacts or repeals this policy as a means of garnering popular support. The evidence provided here suggests that such efforts are merely theatrics, as the policy does not seem to accomplish its most basic objective. On the contrary, its imposition has the potential to exhibit considerable unintended consequences, which both parties would agree are undesirable. Following the presentation of this evidence, any further efforts to reinstate this policy could only be considered a wrong-headed political stunt.

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Appendix A

Relations of co-resident mothers

Explicit relations between household members are not given in the data. Only each respondent's relation to the household head is given. From this, for households that have more than one mother of young children, I can determine which are resulting from polygamy in the following way:

- For those responding that they are not in a polygamous union, clearly co-resident mothers (CRMs) are not co-wives.
- For those that are in a polygamous union:
  - if the respondent is listed as wife of the household head, co-wife status of CRMs is determined by whether any CRM is also listed as wife of head;
  - for female heads of household, CRMs listed as "co-spouse" are considered co-wives, as would be the head;
  - for those not listed as wife, head, or co-spouse, whether CRMs are co-wives cannot be determined (11% of sample). However, it is of note that for women in polygamous unions listed as wife of head, 50% of the time her CRMs are not co-wives (i.e. co-wives live elsewhere); therefore for the 11% of women for whom co-wife status of CRMs is indeterminate, it is likely that half will have CRMs that are not co-wives.

Table A.1: Relations of Co-resident Mothers

Woman's relation to household head	Women in households with: Only one mother	CRMs not co-wives	CRM with co-wife status unknown	Total
Self	6%	1%	7%	4%
Wife	55%	24%	..	42%
Daughter	9%	10%	10%	8%
Daughter-in-law	**13%**	**23%**	**28%**	17%
Foster/Adopted daughter	**5%**	**20%**	**19%**	11%
Other relative	9%	15%	31%	15%
Not related	2%	5%	5%	3%

Notes: Distribution not shown for CRMs that are co-wives, as the ability to identify them is contingent on relation to household head. CRMs that are co-wives are included in the Total.

Appendix B

Serostatus Testing

Table B.1: DHS Sampling for Serostatus Testing

Country	Year	Men Aged	Women Aged
Testing in all sampled households			
Mozambique	2009	12-64	12-64
Swaziland*	2007	15-49	15-49
Tanzania	2004, 2008	15-49	15-49
Liberia	2007	15-49	15-49
Zimbabwe	2006	15-54	15-49
Zambia	2007	15-59	15-49
Ghana	2003	15-59	15-49
Testing in random 50% of sampled households			
Sierra Leone**	2008	6-59	6-59
Kenya	2003, 2009	15-49	15-49
Lesotho	2004	15-59	15-49
Cameroon	2004	15-59	15-49
Congo DR	2007	15-59	15-49
Ethiopia	2005	15-59	15-49
Guinea	2005	15-59	15-49
Rwanda	2005	15-59	15-49
Testing in random 33% of sampled households			
Malawi	2004	15-54	15-49
Burkina Faso	2003	15-59	15-49
Mali	2006	15-59	15-49
Senegal	2005	15-59	15-49

* Swaziland: additional HIV testing for those aged 12-14 and 50+ in a random 50% of sampled households. ** Sierra Leone: Individual questionnaires were administered only to those aged 15-49 (59 for men)

Table B.2: Non-response for Serostatus Testing

		Men		Women	
Country	Year	Tested	Refused	Tested	Refused
Lesotho	2004	68%	16.6%	81%	12.0%
Swaziland	2007	78%	16.6%	87%	9.5%
Zimbabwe	2006	63%	17.4%	76%	13.2%
Malawi	2004	63%	21.9%	70%	22.5%
Mozambique	2009	92%	6.1%	92%	6.1%
Zambia	2007	72%	17.6%	77%	18.4%
Cameroon	2004	90%	5.6%	92%	5.4%
Kenya	2003	70%	13.0%	76%	14.4%
Kenya	2009	79%	7.8%	86%	8.2%
Tanzania	2008	80%	8.0%	90%	6.3%
Tanzania	2004	77%	13.9%	84%	12.3%
Burkina Faso	2003	86%	6.6%	92%	4.4%
Congo DR	2007	86%	5.7%	90%	4.4%
Ethiopia	2005	75%	12.6%	83%	11.2%
Ghana	2003	80%	10.7%	89%	5.7%
Guinea	2005	88%	8.5%	93%	5.0%
Liberia	2007	80%	11.3%	87%	7.3%
Mali	2006	84%	4.8%	92%	3.2%
Rwanda	2005	96%	1.9%	97%	1.1%
Sierra Leone	2008	85%	5.5%	88%	4.7%
Senegal	2005	76%	16.0%	85%	9.9%
Average		79%	11%	86%	9%

Note: Rates are for the full HIV testing sample, with the exception of Mozambique. Rates for MZ are for the 15-49 sample

Appendix C

Comparative Statics for Wealth Effect

We simplify equation (3.3.2) by canceling terms and incorporating the external negative to yield

$$\frac{\partial p}{\partial z} = \frac{\overbrace{(y-z+zw)^{-1}(w-1)}^{u}}{\underbrace{-(y-z+zw)^{-1}\left(\frac{\partial y}{\partial p}\right)^2 + \frac{\partial^2 y}{\partial p^2}}_{v}}$$

We can deduce that both u and v are negative. We then calculate

$$\frac{\partial p}{\partial z \partial w} = \frac{vu' - uv'}{v^2}$$

and know that the sign of the denominator is always positive, so we are interested only in the sign of the numerator. We calculate

$$\begin{aligned} \frac{\partial u}{\partial w} &= -z(y-z+zw)^{-2}(w-1) \\ \frac{\partial v}{\partial w} &= (y-z+zw)^{-2}\left(\frac{\partial y}{\partial p}\right)^2 \end{aligned}$$

implying that both u' and v' are positive. The numerator will be negative if $vu' - uv' < 0$. That is, iff

$$\begin{aligned} vu' &< uv' \\ (y-z+zw)^{-3}(w-1)\left(\frac{\partial y}{\partial p}\right)^2 - \frac{\partial^2 y}{\partial p^2}(y-z+zw)^{-2}(w-1) &< (y-z+zw)^{-3}(w-1)\left(\frac{\partial y}{\partial p}\right)^2 \\ -\frac{\partial^2 y}{\partial p^2}(y-z+zw)^{-2}(w-1) &< 0 \end{aligned}$$

Given that ${}^{\partial^2 y}/_{\partial p^2} < 0$ and $(w-1) \leq 0$, we find that this condition is true. Therefore

$$\frac{\partial p}{\partial z \partial w} \leq 0.$$

Appendix D

Background for Chapter 4

Table D.1: History of Family Planning in Ghana

Year	Event
1961	Christian Council of Churches begins providing family planning information.
1966	Small-scale family planning program emerges in clinics.
1967	Planned Parenthood Association of Ghana (PPAG) is established.
1968	USAID supports Family Planning and Demographic Data Development Project in FY1968-1970.
1970	Ghana National Family Planning Program is established, with a Secretariat to coordinate all ministries. Between 1970 and 1976, 306 new family planning clinics are registered with the Ministry of Health (MOH).
1971	USAID Phase I assistance to GOG 1971-1975 trains providers, and provides contraceptives and informational materials.
1979	USAID support from Phase II (1976-1982) increases access to family planning.
1981	More than 5,000 providers have been trained in family planning.
1985	Ghana Social Marketing Program is established. Contraceptive Supplies Project (1985-1990) ($7 million) increases access to modern methods through improved logistics, clinical training, and IEC in public and private sectors.
1990	MOH and nongovernmental organizations (NGOs) are trained in family planning, especially Ghana Registered Midwives Association and PPAG.
1991	Ghana Family Planning and Health Program (FPHP), a USAID-funded project, begins (and continues until 1996), including funds for contraceptives.
1992	National Population Council reporting directly to the president is established.
1994	Navrongo Community Health and Family Planning Project (CHFP) is launched. USAID funds 10-year, $6 million project on Improving Access and Quality of Clinical Family Planning Services in the Public and Private Sectors in Ghana.
1995	Ghana Population and AIDS Project (GHANAPA), a $45 million project, begins. It operates from 1995 to 2000 and is extended to 2002.
1999	National Reproductive Health Service Protocols are established.
2001	Life Choices behavior change campaign for family planning is launched.
2004	Vasectomy promotion campaign begins.

Source: Solo et al. (2005)

Table D.2: Presidential, Congressional & Litigative Actions Regarding the Mexico City Policy

Date		Action	Details
1984	August	Enact	US delegation to International Conference on Population in Mexico City announces policy as executive order.
1985	January	Lawsuit	DKT Memorial Fund brings legal challenge to US Court of Appeals in DC. Case fails in 1989.
1987		Lawsuit	PPFA sues USAID. Case fails in Supreme Court in 1990.
1990		Lawsuit	USAID is sued by coalition of organizations* in US District Court in DC
1991		Debate	House debates reversal of policy in Foreign Aid Authorization Bill
1992	October	Vote	Congress approves language in FAA Bill that reverses policy; Language dropped under threat of veto by President G. W. Bush
1993	January	Repeal	President Clinton repeals the policy
1996	February	Vote	Population funding is capped and release of funds blocked; requiring special congressional votes to release funding.
1999	Fall	Bargain	In bargaining over other matters, U.S. House leadership elicits agreement from President Clinton to re-impositionof a modified version of the policy
2000	Fall	Vote	FOA Act delays USAID 2001 funding decisions until February 2001
2001	January	Enact	President G. H. W. Bush reinstates the policy
2001	February	Debate	Bills sponsored in House and Senate to repeal the policy
2001	March	Enact	President Bush issues memorandum preventing Congress from challenging the policy
2001	May	Vote	Lee Bill attempts to amend FRA Act to repeal the policy; fails in the House. Similar attempt fails in the Senate.
2001	June	Lawsuit	Center for Reproductive Law and Policy sues President Bush. Fails in US Court of Appeals 2nd Circuit.
2001	October	Vote	Senate approves language overturning policy. Fails in the House.

See continuation on following page.

Table D.3: Continuation of Table D.2

Date		Action	Details
2003	February	Debate	White House proposes expansion of policy
2003	March	Debate	In face of opposition, White House abandons expansion of policy
2003	July	Vote	Senate votes to overturn policy. Under threat of presidential veto, bill fails in the House.
2003	August	Enact	President Bush extends policy to apply to State Departement funding
2003	October	Vote	Senate passes Foreign Operations Bill overturning policy; bill fails in the House.
2005	April	Vote	Senate approves ammendment to overturn policy; bill fails in the House.
2005	November	Vote	Senate approves ammendment to FOA bill to exempt contraceptives from policy; bill fails in the House
2006	June	Vote	Senate repeals policy; bill fails in the House
2007	June	Vote	House votes to exempt contraceptives from the policy
2007	September	Vote	Senate votes to exempt contraceptives and repeal policy entirely
2007	December	Veto	Despite votes to exempt contraceptives in both chambers, the bill is dropped due to threat of veto by President Bush
2008	July	Vote	Senate Appropriations committee adopts a full repeal of the policy; House is silent on the matter
2009	January	Repeal	President Obama rescinds the policy
2009	January	Vote	Senate defeats amendment proposed to nullify the Presidential repeal
2010	July	Vote	Senate Appropriations committee adopts bill to make future enactments of the policy impossible
2011	April	Vote	House adds language reinstating the policy to federal budget bill. Senate refuses to pass the budget due to policy riders. Federal government comes within hours of a shut-down when Congress cannot pass a budget.

Source: Population Action International's Global Gag Rule Timeline.
Online at http://www.populationaction.org/Publications/Reports/The_Global_Gag_Rule/Index.shtml
*The coalition of organizations bringing suit in 1990 was The Pathfinder Fund, The Population Council, and the Association for Voluntary Surgical Contraception.

Acronyms:
USAID: United States Agency for International Development
IPPF: International Planned Parenthood Federation
PPFA: Planned Parenthood Federation of America
FAA: Foreign Aid Appropriations
FOA: Foreign Operations Appropriations
FRA: Foreign Relations Authorization