Estimating Causal Effects from Family Planning Health Communication Campaigns with Panel Data: An Analysis of the "Your Health, Your Wealth" Communication Campaign in Menya Villages, Egypt

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Using data from a panel survey of reproductive age women in Egypt, we estimate the effects of the multimedia health communication campaign "Your Health, Your Wealth" ("*Sehatek Serwetek*") on family planning knowledge, attitudes and behaviors. Difference-in-differences (DID) and fixed effects estimators that exploit the panel nature of the data are employed to control for both observed and unobserved heterogeneity in the sample of women who self-report recall of the messages, thereby potentially improving upon methods that rely solely on cross-sectional data. We examine the performance of these estimators relative to methods that assume – perhaps naively - exogeneity of communication exposure or that control for potentially endogenous exposure using post-only cross-sectional data, though ultimately we find little evidence of endogenous exposure. All of the estimators find positive effects of the "Your Health, Your Wealth" campaign on reproductive health outcomes, though the magnitudes of those effects diverge, often considerably.

1. Introduction

Health communication interventions have long been integral components of national family planning programs. Mass media, counseling and other forms of interpersonal communication have been widely used to inform and create awareness about family planning methods and their availability, to entertain populations and establish influential role models, and to promote specific behaviors, such as the use of condoms or permanent sterilization methods (Bertrand and Kincaid, 1996; Piotrow et al 1997, Rogers 1995, Montgomery and Casterline 1996). However, evaluations of these programs have frequently been plagued by a number of difficulties, which we seek to address by using data from a panel of reproductive age women to estimate the effects of exposure to the "Your Health, Your Wealth" national multimedia campaign in Egypt on a set of family planning outcomes.

At the heart of the problem for the evaluation of many large-scale health communication interventions is the inability or impracticality of using experimental designs in which individuals are randomized into exposed treatment groups and unexposed control groups. The use of randomization and experimental research designs is the predominant mechanism for inferring causal relationships by allowing a set of control individuals – equivalent in all respects except exposure to an intervention – to represent the counterfactual outcome for treatment individuals had they not received the intervention. Causal impacts are therefore measured as the difference in mean outcomes between treatment and control individuals (Holland 1986). But randomization is rarely employed in the evaluation of communication interventions because those interventions often cover entire countries, potentially exposing all targeted individuals, or because localized interventions risk contamination across geographic areas, or because ethical concerns proscribe limiting dissemination of health messages to a subset of potential beneficiaries (Noar 2009, Bertrand, Babalola and Skinner, 2012, Guilkey, Hutchinson and Lance 2006).

In the absence of randomized control designs, evaluations of health communication programs have frequently adopted alternative methods to demonstrate causal relationships, generally using non-equivalent comparison groups and statistical methods that seek to achieve equivalence based on observed characteristics of exposed and unexposed individuals. In many cases, comparison groups can be generated because health communication programs – even those that attempt to target all members of a population - are likely to leave some sub-population unexposed to the intervention, as some individuals may be less regular consumers of media than others or may not recall having been exposed. Population surveys, such as the Demographic Health Surveys or more focused communication surveys, can be used to identify individuals who recall being exposed to campaign messages and individuals who do not, while also collecting information on their health behaviors and outcomes. A common choice for a measure of a communication intervention's effect involves a comparison of average outcomes for those who recall being exposed to intervention messages relative to those who do not. This is the approach taken by a number of evaluations of health communication programs, which use a single equation multivariate regression model to measure the effect of exposure to a health communication program on an outcome of interest, controlling for a limited set of observable characteristics of those individuals (Van Rossem and Meekers 2000; Agha 2002; Agha and Van Rossem 2002; Bessinger, Katende and Gupta 2003; Gupta, Katende, and Bessinger 2003; Kincaid et al. 1993; Kincaid et al. 1996).

Such a measure, however, may contain very serious limitations, as the sample of unexposed individuals may be very different than the sample of exposed in ways that may also affect outcomes under study. As noted in other studies (Hutchinson and Wheeler 2006, Guilkey, Hutchinson, and Lance 2006), exposed individuals likely differ from unexposed individuals in very measurable (exogenous) ways, such as levels of education, income, age, or geographic location. But they may also differ in other less easily measured ways – they may be more media savvy, be more efficient producers of health from available health inputs, or possess some other characteristics that are potentially correlated with both exposure and health behaviors.

Other researchers have attempted to overcome the limitations of regression models (Kincaid and Do 2006) using matching methods that attempt to develop a synthetic cohort of non-intervention individuals drawn from some population – either the same population or a similar enough comparison group – who can be "matched" with intervention individuals of similar characteristics. Average outcomes for untreated matched groups serve as the relevant counterfactual to the missing outcomes of exposed program participants. Program effects, as in an experiment, are measured as the difference in average outcomes for these two groups. As with single-equation control function estimators, however, matching methods assume that selection into the treatment group is determined by observable characteristics only (Rosenbaum and Rubin 1983)..

The fundamental difficulty, in short, is that evaluations of health communication programs generally rely upon measures of program exposure that are at least in part determined by the actions, choices and characteristics of the potential beneficiaries, which may therefore confound estimates of the health communication intervention's effectiveness. In such cases, naïve estimators that assume exogenous exposure – or exogeneity conditional on a limited set of control variables - may be severely biased.

To address the potential endogeneity of program exposure, researchers often turn to instrumental variables approaches or estimators based on the use of panel data (Imbens and Wooldridge 2009). With instrumental variables estimators, for example, evaluators can control for endogenous exposure by identifying some variable (or set of variables) that affects exposure to the intervention but not the outcome itself, thereby purging estimates of the program effect from the confounding effects of the determinants of exposure. In theory, instrumental variables estimators can provide consistent estimates of program effects, assuming that valid exclusion restrictions for the instrumental variables can be identified. In reality, however, identifying suitable instruments is not without some degree of difficulty (Angrist 2001, Cameron & Trivedi 2005; Guilkey, Hutchinson & Lance 2006).

Causal effects can also be estimated data via full-information structural models, with equations specified for both the outcome and the endogenous treatment and with a parametric or semi-parametric specification of the joint distribution of the endogenous treatment variable conditional on exogenous variables (Cameron and Trivedi 2005). This approach has been used for several evaluations of health communication programs (Hutchinson, Guilkey, Lance, Shahjahan and Haque 2006; Kincaid and Do 2006). These evaluations have provided some evidence that exposure may in fact be endogenous in health behavior equations. Such estimation strategies, however, may not perform well with poorly specified models (Angrist 2001, Cameron and Trivedi 2005).

The availability of data collected at multiple time points – pre- and post-intervention – allows for additional estimation strategies. When interventions are spatially or geographically localized, the analysis of pooled cross sectional data allows for the use of difference-in-differences models comparing changes across time for individuals in program areas relative to individuals in suitable – perhaps even matched – comparison areas. Assuming that decisions about which areas receive an intervention are independent of outcomes under study, difference-in-differences models identify the effect of the program as the parameter of geographic program area variable interacted with a post-intervention time dummy variable. Evaluations of these programs may still be complicated, however, if programs are targeted in ways that are unmeasurable or unobservable to the evaluators and in which the targeting is somehow related to program outcomes, as might be the case if programs are targeted to areas with higher fertility or higher fertility norms (Angeles, Guilkey and Mroz 1999; Pitt, Rosenzweig and Gibbons 1993; Gertler and Molyneaux 1994).

Pooled cross sectional data are limited, however, when interventions are individualized (rather than fixed across time by geographically distinct program boundaries) and baseline pre-intervention data cannot make a link between individuals and their subsequent participation and outcomes post-intervention. Conversely, post-intervention observation of a sample of exposed and unexposed individuals permits no knowledge of the pre-intervention measures of outcomes, and therefore the identification of changes across time for exposed versus unexposed individuals.

Fixed effects models with a panel random sample of exposed and unexposed individuals allow researchers to overcome many of the limitations of other analytical methods by allowing researchers to collect information on potential program beneficiaries at a baseline, again at a post-intervention time point, and then to compare changes in outcomes for those individuals who were exposed to the intervention relative to those who were not exposed. Under the assumption that unobserved heterogeneity affecting program exposure is time invariant, evaluators can better attribute changes in outcomes to the program rather than to confounding observed or unobserved characteristics of respondents (Imbens and Wooldridge 2009).

In this paper, we examine measures of the effects of a health communication program in Egypt making use of the panel nature of the data collection. Several estimation methods are used to attempt to develop and to compare estimates of the causal effect of exposure to the "Your Health, Your Wealth" national multimedia health communication campaign in Egypt: (1) a single-equation control function estimator (using endline data), (2) matching on the propensity score, (3) a difference-in-differences estimator, (4) instrumental variables estimation, (5) simultaneous equations models assuming a joint normal distribution for the correlation in unobservables across outcome and exposure equations, and (6) fixed effects estimation.

We examine the effects of the "Your Health, Your Wealth" (*Sahatek, Sarwetek*) health communication campaign on several family planning outcomes, including current contraceptive use, discussions with a spouse about family planning and the use of family planning for birth spacing, and agreement with statements about the benefits of family planning for birth spacing.

The "Your Health, Your Wealth" campaign is a component of the Communication for Healthy Living (CHL) project in Egypt. CHL in turn is one part of the Health Communication Partnership (HCP), a global health communication initiative funded by the United States Agency for International Development (USAID). The CHL program supports activities at both the national and local level in the areas of family planning and reproductive health, maternal and child health, infectious diseases control, healthy lifestyle, household preventive health, and health maintenance practices. The "Your Health, Your Wealth" campaign involves national multimedia and community-based interventions aimed at encouraging families to engage in healthy behaviors at different points in the life stage. Specific family planning messages include the benefits of birth spacing and the need for post-partum resumption or initiation of family planning to avoid early pregnancy (El-Zanaty et al, 2004 and 2005).

We use data from two waves of the Menya Village Health Surveys conducted in seven villages of Menya Governorate in Egypt in 2004 and 2005. Five of these villages received intensive community-based interventions from CHL, while two villages were used as comparison villages.

2. Methodology

In the discussion that follows, each of the outcome and exposure variables are binary, i.e., $Y_i=1$ if the individual engages in the behavior (e.g. is currently using modern family planning) and $Y_i=0$ otherwise. We make a similar assumption about exposure to the health communication program: a woman either recalls hearing or seeing the "Your Health, Your Wealth" messages ($D_i=1$) or not ($D_i=0$).

Estimates of the effects of the "Your Health, Your Wealth" campaign are tied to the fundamental evaluation problem – that the relevant counterfactual information for program participants is inherently unobservable (Holland 1986). Two conventional estimates of program effects are a simple treatment-control comparison – measured as the mean difference in the post-treatment outcome for the treatment group relative to the control group – and a before-after comparison of an outcome for the treatment group relative to itself. A key limitation of the former method is that measured differences may reflect the non-random nature of program exposure or other confounding factors associated with program exposure rather than the effects of the program itself. In the latter estimator, measures of effects may be confounded by other intervening events during the period of the study. These estimators are unlikely to be adequate in most applications.

As a starting point for this analysis, we focus on estimation methods that assume that assignment to the treatment group is exogenous. We employ the following estimators, which we describe using the notation of Cameron and Trivedi (2005), to determine the effects of exposure to the "Your Health, Your Wealth" campaign in Egypt:

1. *Post-treatment, single equation control function estimator*: The single equation model involves a regression of a post-treatment binary response outcome on an intercept and an exposure variable using a combined treatment-control group sample and a set of suitable exogenous control variables. The key limitation of this approach is that it assumes that program participation is exogenous once a set of socio-demographic control variables are included in the model.

$$(1) \qquad Y_i = x_i'\beta + D_i\varphi + u_i$$

In this specification, Y_i is the outcome of interest (e.g. current use of modern family planning), the $\mathbf{x}_i$ represent a vector of exogenous control variables (e.g., wealth, education, exposure to other programs), D_i represents self-reported exposure to the "Your Health, Your Wealth" program, and u_i is a measure of unobservables associated with the outcome Y_i but also potentially correlated with the program exposure variable D_i. The parameter φ represents a measure of the effect of program exposure on the outcome Y_i, controlling for the exogenous control variables $\mathbf{x}_i$. As outcome Y_i is binary, we model the response probability $\Pr[Y_i=1|x_i, D_i]$ as a logit model, in which the marginal effect of exposure to the "Your Health, Your Wealth" campaign is given by:

$$(2) \qquad \frac{d \Pr[Y_i = 1 \mid x_i, D_i]}{dD_i} = \frac{\exp(x_i'\beta + D_i\varphi)}{1 + \exp(x_i'\beta + D_i\varphi)}\varphi$$

2. *Differences-in-differences estimator*: The difference-in-differences estimator uses the full panel sample of baseline and endline observations on treatment and control individuals to estimate the effect of program participation across time for intervention "exposed" individuals relative to comparison "unexposed" individuals. The measure of the causal effect is represented by the coefficient of the interaction term λ in a regression of the family planning outcome on a year dummy variable T_t, a participation dummy variable D_{it}, their interaction $D_{it} \cdot T_t$, and a set of controls. Estimation requires having a panel with observations on individuals at multiple points in time or pooled cross sectional data with different sets of individuals identified as treatment and controls either geographically or by some other mechanism. In this case, T=2.

(3) $$Y_{it} = x_{it}^{'}\beta + D_{it}\varphi + T_t\gamma + D_{it} \cdot T_t\lambda + u_{it}$$

As with the control function estimator, we again model the response probability $\Pr[Y_{it}=1|x_{it}, D_{it}, T_t]$ as a logit.

3. Propensity score matching: Matching methods reduce bias from non-random treatment assignment by balancing on observed covariates (Rosenbaum and Rubin 1983; Rosenbaum and Rubin 1985; Becker and Ichino 2002; Ho, Imai et al. 2007). A central assumption of matching methods is that treatment assignment is strongly ignorable, i.e., that assignment and outcomes are independent conditional upon measured characteristics of survey respondents (Imbens 2004). The propensity score, or the conditional probability of exposure, is defined as $p(\mathbf{x})=\Pr[D=1|\mathbf{X}=\mathbf{x}]$ for given data (D_i, $\mathbf{x}_i$). The measure of the average treatment effect on the treated (ATT) is given by:

   (4) $$ATT = E[Y_1 \mid x, D = 1] - E[Y_0 \mid x, D = 1].$$

   In this model, Y_1 and Y_0 represent the outcomes for exposed and unexposed individuals respectively. The propensity score is estimated as a function of a set of pre-determined characteristics of respondents hypothesized to be independent of our ultimate outcomes: age, education, wealth, presence of a recognizable village leader, and being in a program village. In this analysis, the propensity score is constructed and tests of covariate balance are performed using the STATA 12.0 command *pscore* (Becker and Ichino 2002). We estimate the average treatment on the treated (ATT) effect using kernel matching with the STATA 12.0 command *psmatch2* (Leuven and Sianesi 2003). The kernel matching procedure uses a weighted average of all controls, where the weights are inversely proportional to the distance between the propensity score of treated and controls (Becker and Ichino 2002). We restrict our matching to the area of common support between exposed and unexposed respondents. Overall, all 378 exposed respondents were matched.

In addition to the above methods assuming exogenous program exposure, we also use the following estimators that assume and test for endogenous exposure:

1. Instrumental variable estimation: With the instrumental variable approach, in addition to estimating equation (1), we also estimate an equation for program exposure:

$$D_i = \gamma_0 + \gamma_1 x_i + \gamma_2 z_i + v_i \tag{5}$$

where the $\mathbf{x}_i$ represent an overlapping vector of exogenous control variables that also affect the outcome Y_i, the $\mathbf{z}_i$ are a non-overlapping vector of variables that are correlated with D_i but not Y_i, and v_i is a measure of unobservables associated with D_i but also potentially correlated with u_i in equation (1). Because of this correlation, estimation of the parameter α in equation (1) – as well as the other parameters of the model - may be biased by some measure of the degree of correlation in the unobservables affecting both D_i and Y_i. Estimation involves use of the post-treatment sample of treatment and control individuals (Imbens and Angrist 1994, Angrist, Imbens and Rubin 1996). IV estimation is undertaken in Stata 12.0 using the conditional maximum likelihood *ivprobit* estimator.

2. Bivariate Probit: Alternatively, we estimate both equations (1) and (5) simultaneously using a bivariate probit model for two binary outcomes. The model is motivated using a continuous underlying latent variable specification for

both exposure and the outcome, whose discrete realizations are given as above by D_i and Y_i respectively. In equations (1) and (5), the disturbance terms u_i and v_i are joint normal with means of 0 and variances of 1. The likelihood function is constructed as the product of the four mutually exclusive outcomes – (Y_i=1, D_i=1), (Y_i=1, D_i=0), (Y_i=0, D_i=1), and (Y_i=0, D_i=0). Importantly, this specification allows for correlation ρ between the unobservables in the two equations, thereby controlling for unobserved heterogeneity in the samples of self-reported exposed and unexposed individuals. Standard statistical software provide an estimate of ρ as part of standard regression output. As with the IV estimation, only the 2005 wave of post-treatment data are used. We estimate the bivariate probit model using the Stata 12.0 command *cmp* developed by David Roodman for conditional recursive mixed-process estimators. We use the Likelihood Ratio test proposed by Maartin Buis (2011) to test for the exogeneity of exposure.

3. Fixed effects logit. Making use of the panel nature of the data, we also estimate a fixed effects logit model using conditional maximum likelihood. In the fixed effects model, the error term in equation (1) can be expanded to include both a time-invariant individual-specific effect α_i and time-varying component u_{it}. A time period specific effect T_t is also included as in the DID model.

$$(6) \qquad Y_{it} = x_{it}^{'}\beta + D_{it}\varphi + T_t\gamma + \alpha_i + u_{it}$$

In the fixed effects model, the correlation between the exposure variable D_{it} and the error term is assumed to be with the time invariant component α_i. In the linear outcome case, this underlying heterogeneity can be removed by differencing means within groups (i.e. within individuals observed across time), but this is not possible in the binary outcome case. Instead, parameters are estimated using the conditional likelihood function constructed from observations in which Y_{it} varies from time period 1 to 2. For these observations, the individual effects α_i can be shown to drop out of the probability density in the likelihood function, and the conditional maximum likelihood estimator of φ and β can be shown to be consistent. A consequence of this estimation method, however, is that the effects of variables that do not vary across time cannot be determined (Arellano and Honore 2001, Chamberlain 1984).

As noted, each of the above estimation methods is conducted using the Stata 12.0 statistical software package. With the exception of propensity score matching estimates, marginal effects are calculated using the *margins* command.

3. Data

Data Source

We use data from two waves of the Menya Village Health Surveys conducted in seven villages of Menya Governorate in Egypt in 2004 and 2005. The surveys were funded by the United States Agency for International Development (USAID) as part of the external evaluation of the impact of the Health Communication Partnership (HCP). This evaluation – part of a multi-country study – was conducted by Tulane University's Department for International Health and Development (Tulane/IHD), School of Public Health and Tropical Medicine. Collection of data was undertaken by El-Zanaty and Associates.

Our analysis focuses on ever-married women aged 15-49 years. For the 2005 sample, women who were interviewed in 2004 and completed 50 years by the date of the interview in 2005 were excluded. Only usual household residents were eligible for interview.

Two types of questionnaires were used in the data collection: (1) a household questionnaire which identified eligible respondents and collected information on household socioeconomic characteristics and living conditions and (2) eligible respondent questionnaires which focused on health knowledge, attitudes and behaviors, as well as detailed questions about exposure to different health communication messages and campaigns.

A multi-stage cluster sample design was used to identify respondents. At the first stage, five intervention villages (Zohra, Saft El khamar El sharkia, Nazlet Hussein Ali, Monshaat El Maghalka, and Koloba) and two control villages (Toukh El khail and Ebshedat) were selected. At the second stage, each village was divided into segments of approximately 1000 households. Each village had 10 segments, except for Koloba (which had 11 segments) and Ebshedat (which had 13). One segment was then selected at random, and a household listing was conducted by El-Zanaty and Associates. At the third stage, approximately 35 households were systematically sampled at random from the household listing. The sampling interval was determined by dividing the total number of households in each segment by 35 (El Zanaty et al. 2004).

Prior to the implementation of the surveys, the Tulane University Biomedical Institutional Review Board (IRB) reviewed and approved the study designs, research protocols, and questionnaires to ensure that they met the qualifications and restrictions of the Tulane University Human Research Subject Protection Program.

Fieldwork for the 2004 MVHS was conducted over three weeks beginning in late July and ending in mid-August 2004. The 2005 MVHS was conducted over a two-week period in August and September 2005. For quality control, 5 percent of the sample was selected for re-interview using shorter versions of the original questionnaires. The re-interviews occurred following the main fieldwork and involved special teams that did not involve the original interviewers. During the re-interviews, teams also attempted to visit households or individuals whose interviews were not completed during the initial village visits.

Attrition across the two waves was negligible. In 2004, 2,316 households were selected for interview, and 2,298 households were interviewed. In 2005, 205 of the original households were no longer eligible, while 2,093 of the 2004 households were re-interviewed and 126 new households were added to the sample. A total of 2,240 ever-married women were interviewed in 2004 (response rate of 99.7 percent). By 2005, 2,073 of these women were still eligible, while an additional 86 youth were married and became eligible for interview and 201 women had a new husband and were also eligible. Of these women, a total of 2,284 were successfully interviewed (response rate of 96.8 percent).

Measures

In this analysis, we focus on several outcomes related to family planning and use of family planning in Egypt. Specifically, we examine whether or not a woman is currently using modern family planning, whether or not she had a discussion with her spouse about family planning in the past 12 months, whether or not she has discussed the use of family planning for birth spacing, and whether or not she agrees with statements about the benefits of family planning for birth spacing. These outcomes are chosen because they are believed to be directly influenced by the program.

As our measure of exposure to the "Your Health, Your Wealth" campaign, we use the variable measured in 2005 for whether or not the respondent reported having seen the "Your Health, Your Wealth" messages in the last 12 months and specifically mentioned that those messages pertained to either "birth spacing" or "family planning use in the 40 days following birth." In our sample of 2,088 women in 2005, 378 (18.1%) recalled having seen either messages; 321 (15.4%) recalled the messages related to postpartum family planning use, and 151 (7.2%) recalled the messages related to birth spacing.

Key explanatory variables in the multivariate models include a categorical variable for a woman's level of education (none, primary or secondary/university), a categorical variable for a woman's age (in 5- or 10-year increments), a categorical variable of household wealth constructed from a principal components analysis of household ownership of a set of consumer durables, and a continuous variable for the number of children ever born to a woman. In addition, to achieve model identification in the bivariate probit and instrumental variables models, we included variables hypothesized to be statistically associated with exposure to "Your Health, Your Wealth" but not with the outcomes under study. These included variables for whether or not a woman lived in a designated program village, whether or not a woman was willing to participate in community-organized activities to improve family health, and whether or not a woman had ever heard of community gatherings to discuss health and family planning. These variables were chosen as proxies for informal communication about family planning. Model identification was tested using the Stata 12.0 command *ivreg2* to construct the Kleibergen-Paap rk Wald F statistic. These values were then compared with the Stock-Yogo critical values (Stock and Yogo 2005).

4. Results

Descriptive

Characteristics of the samples of exposed and unexposed women for 2005 are shown in Table 1. On average, women who reported having seen "Your Health, Your Wealth" were approximately 3 years younger (29.76 years versus 32.80 years, $p<0.001$), had fewer children (3.40 versus 4.09, $p<0.001$), were more likely to report that there was a leader in their community (24.3% versus 16.5%, $p<0.001$), and were less likely to live in a treatment village (62.7% versus 70.9%, $p=0.002$). Exposed women were more educated; 29.9% had a secondary or higher level of education versus 19.0% of unexposed women. They were also wealthier on average; 25.7% of exposed women were in the highest wealth quintile versus 20.3% of unexposed women.

Table 2 presents outcomes for 2004 and 2005 for those who recalled exposure to the "Your Health, Your Wealth" messages (in 2005) as well as for those who did not. At the baseline, few statistically significant differences in family planning outcomes were observed. For example, contraceptive use was nearly identical - 39.2 percent of exposed ever married women versus 40.3 percent of unexposed women. The only statistically significant difference was for the variable "Discussed birth spacing in the last 6 months" – 36.8 percent for exposed women versus 31.1 percent for unexposed women ($p=.031$).

At the endline, statistically significant differences were observed for three of the four outcomes. The lone exception was for modern contraceptive use – 47.6 percent for the exposed relative to 44.2 percent for the unexposed (p=.228). Nonetheless, the changes across time for the exposed relative to the unexposed – equivalent to the difference-in-differences model without controls - were statistically significant for each of the outcomes. For example, the difference-in-differences estimate of the program effect on modern contraceptive use was 4.6 percentage points (p=0.088), while the estimates for "Discussed birth spacing in the last 6 months" and "Agrees that spacing improves child health" were 9.3 (p=0.006) and 8.3 (p=0.023) percentage points respectively. The largest effect was for "Discussed FP with partner in the last 6 months" which showed a 15 percentage point difference between treatment and comparison, due entirely to a significant decrease in the prevalence of discussion for the comparison group.

Estimations

Table 3 summarizes the marginal effects for each of the estimation methods. Full results are shown in subsequent tables.

As an initial rough test of the possible endogeneity of exposure to the "Your Health, Your Wealth" campaign, we ran a single-equation logit estimation of each family planning outcome on the exposure variable and a set of control variables using the pre-intervention 2004 sample only. For none of the outcomes was the program exposure variable statistically significant, providing a general indication that – conditional on the controls – there was little baseline evidence of unobserved heterogeneity.

Two further tests for endogenous exposure were also conducted. First, in the bivariate probit estimations, an exogeneity test of $\rho = 0$, representing the correlation in the unobservables across the outcome and exposure equations, was conducted. For two of the outcomes – discuss birth spacing with a spouse and agree that spacing is healthy – we rejected the null of exogeneity (LR chi2(1) = 8.23, p=.004; LR chi2(1)=61.17, p<0.001), indicating that the simpler methods may fail to address the bias introduced by unobserved heterogeneity. These tests were mirrored in the instrumental variables estimations.

While all methods – with the exception of the bivariate probit and IV estimates - were largely consistent in the direction and levels of statistical significance of program exposure, there was considerable variation in the magnitude of the effects. In general, the largest effects – and the ones that are least able to control for non-random exposure based on either observable nor unobservable characteristics of respondents – were the difference-in-differences estimates absent pre- or post-treatment control variables. These estimates were shown in Table 2 and are presented again for comparison in Table 3.

The measures of program effects derived by both the 2005 cross sectional control function estimates and propensity score matching (PSM) were roughly similar. For example, the marginal effect of exposure to the "Your Health, Your Wealth" on modern contraceptive use was 3.9 percentage points by the 2005 logit control function estimator, as compared with 3.7 percentage points for PSM, though in neither case were the results statistically significant. For the outcome "agree that spacing is healthy," PSM and the control function estimates were also nearly identical – 6.8 and 6.6 percentage points respectively.

The difference-in-differences models with control variables showed the effects of changes in family planning outcomes from 2004 to 2005 for the exposed relative to the unexposed case. For all outcomes, the estimates were less than in the difference-in-differences models without controls. For two of the four outcomes, the estimates were smaller than those estimated by PSM and the 2005 control function estimator.

The methods that test and control for endogenous exposure – which are inefficient when exposure is in fact exogenous – provide very divergent results from the other methods. For example, the marginal effect of exposure to “Your Health, Your Wealth” on current contraceptive use was 9.8 percentage points for the bivariate probit model and 17.7 percentage points for the instrumental variables probit model, both several times larger than for any of the methods assuming exogenous exposure. Nonetheless, given that we failed to reject the null of exogeneity in either case, the bivariate and instrumental variables estimates for the contraceptive use model can be discounted. For the two outcomes in which there was evidence of endogeneity, however, the marginal effects appear implausibly large – 34.0 percentage points for “discuss birth spacing” and 53.5 percentage points for “agree that spacing is healthy,” perhaps indicating that the assumption of normality in the joint distribution of the error terms is not valid. Further, while it appears that we have models that meet the technical criteria for the Stock-Yogo weak identification test, our models fail to meet the Sargen-Hansen test for over-identification. In other words, for three out of four outcomes we rejected the null that our instruments are uncorrelated with the error term and that the excluded instruments were correctly excluded from the estimated equation. Attempts to improve model identification through different combinations of variables that both technically and theoretically met the criteria for model identification proved unsuccessful, and hence we express little confidence in our IV and bivariate probit models.

The final estimation method – that incorporating individual-level fixed effects and using only time-varying characteristics of individuals - provides estimates of exposure that do not widely diverge from the simpler models. Again, exposure to the "Your Health, Your Wealth" campaign was shown to yield a 13.9 percentage point increase in the probability of discussing family planning with a spouse, similar to the 13.0 percentage point difference for PSM and 14.6 percentage point difference for the DID with controls. For the outcome "agree that spacing is healthy for the child," the marginal effect from the fixed effects model was 13.5 percentage points, considerably larger than the estimates of 6.6 percentage points from the PSM model and the estimate of 7.6 percentage points from the DID model.

5. Conclusion

This paper assesses the effects of exposure to a family planning health communication program – the "Your Health, Your Wealth" national multimedia campaign in Egypt – on a set of family planning outcomes, including current use of modern contraception, measures of interpersonal communication regarding family planning, and attitudes towards birth spacing. The aim of the paper is in large part methodological – to control appropriately for non-random (self-reported) exposure to the program in order to obtain more accurate measures of the program's effects.

We make use of an atypically robust set of data – panel data with data collection occurring pre- and post-campaign and involving very low levels of attrition from the sample. The advantage of this data is that it allows for the use of estimation strategies that are not generally permitted by pooled cross-sectional data, a key limitation of many previous analyses of family planning communication efforts. Because the interventions are individualized – only through self-reported recall can researchers identify who is exposed and then trace that back to their pre-intervention outcomes and characteristics – cross-sectional models cannot identify changes across time in the treatment "exposed" group relative to the comparison "unexposed" group. Cross-sectional methods assuming exogenous exposure therefore assume that treatment and comparison individuals are statistically equivalent at baseline conditional on a limited set of control variables. Absent panel data, this assumption cannot be tested for time varying variables. Further, cross-sectional methods that attempt to control for endogenous exposure – instrumental variables methods or simultaneous equations models assuming a specific parametric distribution for the relationship between outcomes and exposure – must confront difficult issues of model identification or assume that identification is attained through assumptions about the parametric distribution. In this paper, these issues appeared to be problematic.

By using panel data, in contrast, we can examine changes across time among a set of individuals who recall having been exposed to the campaign by the endline relative to individuals who do not recall such exposure. This allows for both difference-in-differences estimation and fixed effects estimation that can address changes across time or difference out unobserved heterogeneity affecting exposure and family planning outcomes. It also allows for the use of matching methods in which matching is determined by baseline characteristics of respondents rather than concurrently measured characteristics which may be more susceptible to underlying, unobserved heterogeneity.

The results, in this case, provide mixed evidence that estimates of exposure to the "Your Health, Your Wealth" campaign on family planning outcomes are confounded by unobserved heterogeneity associated with program exposure. The cases in which we detected evidence of endogeneity failed to meet technical criteria for model identification and provided implausibly large estimates of program effects, rendering suspect the underlying assumptions of those models.

The models that made use of the panel nature of the data set – difference-in-differences, propensity score matching and fixed effects logit - provided similar results in terms of direction and levels of statistical significance but the magnitudes of effects often diverged widely. This is an important finding. If, for example, we applied the marginal effects from exposure to the "Your Health, Your Wealth" to a hypothetical population of 100,000 women exposed to the program, a doubling of the effect of the program – as was observed for the fixed effects model (ME=0.135) relative to the difference-in-differences model (ME=0.076) – would increase the number of women who agreed with the statement that "birth spacing is healthy" by approximately (13,500-7,600=) 5,900 women, an important programmatic result. On the other hand, simpler methods such as the single equation cross-sectional estimator, which attribute to the program an increase in contraceptive use of 3.9 percentage points relative to the fixed effects estimate of only 1.6 percentage points, might overstate program effects.

In short, while the results from the different estimation methods are similar in direction and levels of statistical significance, the overall effects when applied at the population level can substantially alter conclusions about program success. Analysts and program managers who increasingly rely on estimates of program effects – particularly in estimates of cost-effectiveness - need to be cognizant of the limitations of their methods, particularly those based on cross sectional data.

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Table 1. Descriptive Statistics

Characteristic	Heard YHYW Pct	Heard YHYW N	Didn't Hear YHYW Pct	Didn't Hear YHYW N	p
Age (years)					
15-19	6.9%	26	4.1%	70	
20-24	24.1%	91	18.4%	315	
25-29	25.1%	95	19.2%	327	
30-34	14.0%	53	16.2%	276	
35-39	15.6%	59	14.9%	254	
40-44	7.7%	29	12.5%	214	
45-49	6.6%	25	14.8%	252	<0.001
Mean Age (years)	29.76		32.80		<0.001
Children Ever Born (mean)	3.40		4.09		<0.001
Wealth Quintile					
Poorest	10.9%	41	21.0%	358	
2nd Poorest	21.0%	79	20.1%	342	
Middle	22.0%	83	19.5%	332	
2nd Wealthiest	20.4%	77	19.1%	326	
Wealthiest	25.7%	97	20.3%	346	<0.001
Education					
None	52.4%	198	60.5%	1,035	
Primary	17.7%	67	20.5%	351	
Secondary or Above	29.9%	113	19.0%	324	<0.001
Community Leader					
No	75.7%	286	83.5%	1,428	
Yes	24.3%	92	16.5%	282	<0.001
Treatment Village					
No	37.3%	141	29.1%	497	
Yes	62.7%	237	70.9%	1,213	0.002
Total		378		1708	

Table 2. Family Planning Outcomes for Women, by exposure to the "Your Health, Your Wealth" messages, 2004 and 2005

	MVHS 2004 Exposed	MVHS 2004 Unexposed	MVHS 2004 p	MVHS 2005 Exposed	MVHS 2005 Unexposed	MVHS 2005 p	2004-2005 Diff	2004-2005 p
Modern Contraceptive Use	0.392	0.403	0.682	0.476	0.442	0.228	0.046	0.088
Discussed FP with partner in last 6 months	0.386	0.377	0.743	0.386	0.226	<0.001	0.151	<0.001
Discussed birth spacing in last 6 months	0.368	0.311	0.031	0.349	0.199	<0.001	0.093	0.006
Agree that spacing improves child health	0.759	0.753	0.805	0.780	0.692	<0.001	0.083	0.023
N	378	1,708		378	1,708			

Table 3. Marginal effects of exposure to "Your Health, Your Wealth," by estimation method and family planning outcome

Method		Modern Contraceptive Use	Discuss FP with spouse	Discuss birth spacing	Agree that spacing is healthy
Methods assuming exogenous exposure					
Control function - 2004 cross section	dy/dx	**-0.001**	**-0.021**	**0.032**	**0.015**
	SE	0.027	0.026	0.026	0.024
	P	0.973	0.434	0.213	0.640
Control function - 2005 cross section	dy/dx	**0.039**	**0.107**	**0.108**	**0.068**
	SE	0.027	0.210	0.021	0.027
	P	0.147	<0.001	<0.001	0.012
Difference in differences – no controls	dy/dx	**0.045**	**0.154**	**0.100**	**0.083**
	SE	0.040	0.035	0.033	0.037
	P	0.256	<0.001	0.003	0.024
Difference in differences with controls	dy/dx	**0.027**	**0.146**	**0.092**	**0.076**
	SE	0.393	0.035	0.034	0.044
	P	0.85	<0.001	0.006	0.081
PSM	ATT	**0.037**	**0.130**	**0.142**	**0.066**
	SE	0.29	0.027	0.027	0.025
	T-stat	1.27	4.75	5.31	2.67
Methods with controls for endogenous exposure					
Bivariate probit (cmp)	dy/dx	**0.098**	**0.108**	**0.340**	**0.535**
	SE	0.132	0.207	0.054	0.017
	P	0.456	0.602	<0.001	<0.001
Test for exogeneity: LR chi2(1)		0.19	0.01	8.23	61.17
Prob>chi2		0.667	0.935	0.004	<0.001
IV probit	dy/dx	**0.177**	**0.461**	**0.727**	**0.624**
	SE	0.174	0.533	0.060	0.024
	P	0.308	0.387	<0.001	<0.001
Wald Test for exogeneity: Chi2(1)	chi2(1)	0.59	0.34	6.19	28.87
Prob>chi2		0.444	0.561	0.0123	<0.001
Fixed Effects	dy/dx	**0.016**	**0.139**	**0.037**	**0.135**
	SE	0.014	0.058	0.044	0.045
	P	0.238	0.017	0.396	0.003

n Estimates, 2005

Modern Contraception			Discuss FP			Discuss Birth Spacing			Spacing is healthy		
Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
0.178	**0.123**	**0.147**	**0.654**	**0.131**	**0.000**	**0.673**	**0.131**	**0.000**	**0.354**	**0.142**	**0.013**
0.305	0.254	0.230	-0.422	0.235	0.073	0.003	0.259	0.991	0.463	0.254	0.068
0.096	0.258	0.711	-0.658	0.243	0.007	-0.006	0.264	0.980	0.221	0.255	0.386
0.706	0.272	0.009	-1.033	0.270	0.000	-0.010	0.286	0.973	0.310	0.273	0.256
0.331	0.284	0.243	-2.011	0.306	0.000	-0.304	0.305	0.319	0.426	0.286	0.136
-0.199	0.300	0.508	-2.760	0.363	0.000	-0.200	0.325	0.539	0.491	0.303	0.105
-1.568	0.327	0.000	-4.656	0.555	0.000	-0.606	0.345	0.079	0.126	0.307	0.680
0.263	0.027	0.000	0.136	0.035	0.000	-0.051	0.031	0.104	-0.019	0.027	0.466
0.056	0.151	0.714	0.172	0.182	0.345	0.699	0.221	0.002	0.046	0.159	0.771
0.095	0.153	0.535	0.242	0.181	0.181	0.827	0.219	0.000	0.160	0.160	0.319
0.220	0.155	0.157	0.299	0.184	0.103	1.466	0.212	0.000	0.237	0.162	0.145
0.121	0.152	0.428	0.267	0.182	0.143	1.454	0.209	0.000	0.791	0.168	0.000
-0.122	0.131	0.349	-0.057	0.162	0.725	0.261	0.149	0.080	0.042	0.138	0.760
0.001	0.252	0.996	0.005	0.263	0.986	-0.167	0.282	0.553	-0.135	0.272	0.619
0.069	0.123	0.575	0.037	0.142	0.793	0.107	0.141	0.446	-0.225	0.133	0.089
0.071	0.105	0.500	0.659	0.128	0.000	0.398	0.130	0.002	-1.337	0.131	0.000
-1.461	0.265	0.000	-1.137	0.265	0.000	-2.374	0.306	0.000	1.385	0.269	0.000
2079.000			2079.000			2079.000			2079.000		
242.100			324.990			173.430			163.530		
0.085			0.138			0.078			0.065		

Table 5. Difference-in-Differences Estimations

	Modern Contraceptive Use			Discussed FP with Spouse			Discussed Birth Spacing with Spouse			Agree that Birth Spacing is Healthy		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
Heard YHYW	0.0176	0.1380	0.898	-0.1206	0.1067	0.259	0.1606	0.1228	0.191	0.0055	0.1610	0.973
Year=2005	0.1924	0.0594	0.001	-0.7601	0.1180	0.000	-0.5451	0.1243	0.000	-0.2990	0.1868	0.109
YHYW x 2005	**0.1238**	**0.1449**	**0.393**	**0.7708**	**0.1872**	**0.000**	**0.4961**	**0.1844**	**0.007**	**0.3943**	**0.2240**	**0.078**
Age (base=15-19)												
20-24 years	0.6678	0.1578	0.000	0.0776	0.1555	0.618	-0.0950	0.1520	0.532	0.2705	0.1745	0.121
25-29 years	0.5486	0.1716	0.001	-0.1569	0.1644	0.340	-0.0968	0.1644	0.556	0.1884	0.1687	0.264
30-34 years	0.8983	0.1685	0.000	-0.6136	0.1895	0.001	-0.1190	0.1796	0.508	0.2066	0.1808	0.253
35-39 years	0.5795	0.1732	0.001	-1.3495	0.1870	0.000	-0.3875	0.1789	0.030	0.2165	0.1783	0.225
40-44 years	0.0922	0.1883	0.624	-2.1678	0.2489	0.000	-0.4970	0.2090	0.017	0.1431	0.2182	0.512
45-49 years	-1.3327	0.2454	0.000	-3.0716	0.3001	0.000	-0.5271	0.2236	0.018	-0.1228	0.2322	0.597
Children ever born	0.2613	0.0210	0.000	0.1474	0.0221	0.000	-0.0486	0.0171	0.005	0.0047	0.0194	0.807
Wealth (base=poorest)												
2nd poorest	0.0942	0.1137	0.407	0.1552	0.1145	0.175	0.4172	0.1243	0.001	-0.0631	0.1257	0.615
Middle	0.1486	0.1242	0.231	0.2358	0.1045	0.024	0.6356	0.1331	0.000	-0.0696	0.1275	0.585
2nd wealthiest	0.3588	0.1333	0.007	0.1635	0.1194	0.171	0.8751	0.1499	0.000	0.0098	0.1324	0.941
Wealthiest	0.4327	0.1293	0.001	0.3655	0.1165	0.002	0.9346	0.1358	0.000	0.3769	0.1555	0.015
Education (base=none)												
Primary	-0.0116	0.1038	0.911	-0.1471	0.1103	0.182	0.0566	0.1011	0.576	0.0575	0.1027	0.575
Secondary or above	-0.0774	0.2146	0.718	-0.2591	0.1855	0.163	-0.3913	0.2306	0.090	0.1083	0.1881	0.565
Has a community leader	0.1274	0.0857	0.137	-0.1010	0.0997	0.311	0.3680	0.0973	0.000	0.1856	0.1076	0.085
Lives in program village	0.0411	0.1245	0.741	0.3715	0.1042	0.000	0.3871	0.1057	0.000	-0.0793	0.1845	0.667
Intercept	-2.0565	0.2033	0.000	-0.6494	0.1556	0.000	-1.3700	0.2031	0.000	0.9053	0.2709	0.001
N	4163						4163			4163		
Wald chi2(18)	418.56			557.17			384.78			45.21		
Prob>chi2	0.00			0.00			0.00			0.00		

Table 6. Bivariate Probit Estimates

	Modern Contraceptive Use			Discussed FP with Spouse			Discussed Birth Spacing with Spouse			Agree that Birth Spacing is Healthy		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
Heard YHYW	**0.2685**	**0.3634**	**0.460**	**0.3787**	**0.7366**	**0.607**	**1.3066**	**0.2452**	**0.000**	**1.5968**	**0.0530**	**0.000**
Age (base=15-19)												
20-29 years	0.2114	0.1460	0.147	-0.2453	0.1388	0.077	0.0422	0.1455	0.772	0.2337	0.1291	0.070
30-39 years	0.5021	0.1583	0.002	-0.6685	0.1620	0.000	0.0176	0.1586	0.912	0.3322	0.1396	0.017
40-49 years	-0.2913	0.1783	0.102	-1.6269	0.2191	0.000	-0.0537	0.1749	0.759	0.4435	0.1510	0.003
Children ever born	0.1297	0.0148	0.000	0.0345	0.0173	0.046	-0.0345	0.0154	0.025	-0.0101	0.0136	0.459
Wealth quintile (base=poorest)												
2nd poorest	0.0042	0.0956	0.965	0.1276	0.1235	0.302	0.2916	0.1112	0.009	-0.1721	0.0853	0.044
Middle	0.0294	0.0968	0.761	0.1978	0.1277	0.121	0.3737	0.1131	0.001	-0.1955	0.0850	0.021
2nd wealthiest	0.1116	0.0973	0.252	0.2616	0.1232	0.034	0.7599	0.1154	0.000	-0.1904	0.0844	0.024
Wealthiest	0.0424	0.0989	0.668	0.2232	0.1363	0.101	0.7167	0.1156	0.000	-0.0336	0.0896	0.707
Educ. (base=none)												
Primary	-0.1029	0.0794	0.195	-0.0672	0.0946	0.477	0.1632	0.0853	0.056	0.0523	0.0729	0.473
Secondary or above	0.0466	0.1524	0.760	0.0687	0.1633	0.674	-0.1011	0.1522	0.506	-0.1202	0.1398	0.390
Intercept	-0.8943	0.1657	0.000	-0.4244	0.1980	0.032	-1.3305	0.1644	0.000	0.0176	0.1370	0.898
/atanhrho_12	-0.0905	0.2087	0.664	-0.0235	0.4271	0.956	-0.6151	0.1955	0.002	-2.4667	1.1134	0.027
rho_12	-0.0903	0.2070		-0.0235	0.4269		-0.5477	0.1369		-0.9857	0.0316	
N	2081			2081			2081			2081		
Wald chi2(11)	178.35			195.91			186.04			933.71		
Prob>chi2	0.00			0.00			0.00			0.00		

Table 7. Instrumental Variables Probit Estimates

	Modern Contraceptive Use			Discussed FP with Spouse			Discussed Birth Spacing with Spouse			Agree that Birth Spacing is Healthy		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
Heard YHYW	**0.4483**	**0.4471**	**0.316**	**1.2873**	**1.4394**	**0.371**	**2.2226**	**0.3790**	**0.000**	**2.6229**	**0.0750**	**0.000**
Age (base=15-19)												
20-29 years	0.1704	0.1263	0.177	-0.2237	0.1469	0.128	0.0695	0.1472	0.637	0.1449	0.1540	0.347
30-39 years	0.4558	0.1335	0.001	-0.5934	0.2720	0.029	0.0993	0.1664	0.551	0.2486	0.1546	0.108
40-49 years	-0.3292	0.1455	0.024	-1.4336	0.5712	0.012	0.1341	0.2031	0.509	0.4013	0.1707	0.019
Children ever born	0.1336	0.0128	0.000	0.0387	0.0164	0.018	-0.0197	0.0159	0.217	0.0030	0.0099	0.765
Wealth quintile (base=poorest)												
2nd poorest	-0.0160	0.0956	0.867	0.0392	0.1813	0.829	0.0900	0.1307	0.491	-0.2136	0.0617	0.001
Middle	0.0069	0.0829	0.933	0.0952	0.2038	0.640	0.1352	0.1651	0.413	-0.2382	0.0683	0.000
2nd wealthiest	0.0937	0.0984	0.341	0.1681	0.2111	0.426	0.4246	0.2268	0.061	-0.2173	0.0667	0.001
Wealthiest	0.0181	0.1092	0.868	0.1007	0.2439	0.680	0.3504	0.2428	0.149	-0.2293	0.0749	0.002
Education (base=none)												
Primary	-0.0993	0.0725	0.171	-0.0355	0.1083	0.743	0.1550	0.0652	0.017	0.0744	0.0604	0.218
Secondary or above	0.0372	0.1685	0.825	0.0297	0.1722	0.863	-0.1223	0.1560	0.433	-0.1092	0.1398	0.435
Intercept	-0.8830	0.1349	0.000	-0.5518	0.2171	0.011	-1.1800	0.2091	0.000	-0.4198	0.1577	0.008
/athrho	-0.1308	0.1709	0.444	-0.3950	0.6793	0.561	-0.9911	0.3983	0.013	-2.2281	0.4147	0.000
/lnsigma	-0.9832	0.0280	0.0000	-0.9827	0.0287	0.000	-0.9807	0.0281	0.000	-0.9723	0.0270	0.000
rho	-0.1301	0.1681		-0.3756	0.5834		-0.7578	0.1696		-0.9771	0.0188	
sigma	0.3741	0.0105		0.3743	0.0108		0.3750	0.0105		0.3782	0.0102	
Kleibergen-Paap rk Wald F statistic	19.003			19.003			19.003			19.003		
Stock & Yogo critical value 5% maximal IV relative bias	16.85			16.85			16.85			16.85		

	Modern Contraceptive Use			Discussed FP with Spouse			Discussed Birth Spacing with Spouse			Agree that Birth Spacing is Healthy		
	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z	Coef.	Std. Err.	P>z
Hansen J statistic for Over-identification	2.156			20.951			15.630			25.249		
Chi2(3) p value	0.5408			0.001			0.0014			<0.001		
N	2079			2079			2079			2079		
Wald chi2(11)	240.8200			270.71			416.59			1512.99		
Prob>chi2	0.00			0.00			0.00			0.00		

Table 8. Fixed Effects Logit Results

	Modern Contraception			Discuss FP			Discuss Birth Spacing			Spacing is healthy		
	Coef.	SE	P>z	Coef.	SE	P>z	Coef.	SE	P>z	Coef.	SE	P>z
Hear YHYW	**0.387**	**0.277**	**0.162**	**0.932**	**0.191**	**0.000**	**0.194**	**0.222**	**0.382**	**0.561**	**0.185**	**0.002**
Year=2005	-0.096	0.229	0.674	-1.409	0.177	0.000	1.242	0.161	0.000	-0.726	0.166	0.000
Children ever Born	2.427	0.283	0.000	0.667	0.173	0.000	0.174	0.204	0.393	0.008	0.178	0.964
Age in Years	-0.036	0.039	0.353	-0.016	0.035	0.643	-0.032	0.026	0.218	-0.013	0.028	0.641
Wealth Quintile	-0.001	0.095	0.993	0.011	0.069	0.869	-0.121	0.065	0.065	0.145	0.067	0.031
Community Leader	0.146	0.200	0.465	-0.112	0.149	0.453	-0.052	0.145	0.722	0.198	0.144	0.169
Treatment*Year	-0.235	0.253	0.354	0.458	0.188	0.015	-2.217	0.180	0.000	0.143	0.182	0.430
Obs	928			1512			1710			1512		
Groups	464			756			855			756		
LR chi2(7)	131.08			135.85			215.29			69.87		
LL	-256.079			-456.094			-484.997			-489.084		