Health Insurance or Food for the Family? An Examination into Unintended Consequences

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HEALTH INSURANCE OR FOOD FOR THE FAMILY? AN EXAMINATION INTO UNINTENDED CONSEQUENCES

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Abstract. In developing countries, where health insurance is not a commonly purchased financial instrument, recent debates have revolved around extending health insurance coverage to a wider range of the population, primarily via compulsory insurance schemes. However, these debates rarely consider the competing demands placed on the family budget, which will unfluence the acceptability of the program by the populace, and can be used to design the optimal policy. In this paper, we examine treatment effects associated with household insurance status providing a detailed examination of expenditure substitution patterns within a highly unequal developing country. In agreement with economic theory, the expansion of health insurance coverage via compulsory schemes creates additional burdens for households, which household accommodate via expenditure substitution. The observed variation in the household's ability to accomodate increased expenditure can and should be used in future to assess policy options and design an optimal social health insurance program.

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1. Introduction

Health care delivery is highly segregated in developing countries, as well as in many developed countries. In these segregated health economies, a large proportion of health seekers are uninsured and purchase their health care from a subsidized public sector,[1] while a smaller proportion purchase their health care from a highly developed private sector, where purchases are often covered by a third-party insurance contract. For the uninsured, efforts to obtain health care creates out-of-pocket expenditures, affecting their ability to purchase other items.[2] In an effort to alleviate out-of-pocket expenditure pressures, governments and health officials have considered both compulsory insurance and subsidized voluntary insurance programs in order to increase insurance coverage. These policies are generally meant to incorporate formal sector employees, although the inclusion of informal sector employees is an important consideration in developing countries.

The primary benefit of compulsory insurance is the inclusion of all consumers within the same pool, resulting in cross-subsidization. However, any policy meant to increase coverage, must also take cognizance of the underlying determinants of low coverage, as well as the subsequent consumer responses to that policy.[3] Although potential efficiency gains can arise from compulsory or social health insurance schemes, the mitigation of adverse selection may lead to the proliferation of moral hazard. For consumers, the decision to purchase insurance through voluntary schemes is discretionary, while compulsory schemes may create additional economic burdens, offsetting the proposed gains. Therefore, it is imperative for policymakers to understand the potential effects of those policies on consumer behaviour.

[1]Although there is often no direct fee for service, especially in poorer developing countries, consumers are subject to opportunity costs related to transport and queueing.

[2]Chou, Liu and Hammit (2003) find that, as a result of a change in Taiwan's insurance regulations, savings decreased from between 5.1% and 13.7%, while consumption increased by up to 5.7%, depending upon the controls included in the regressions. Levy and DeLeire (2003), on the other hand, show that expenditure patterns for the uninsured poor differ from the insured poor in a way that does not arise when comparing the wealthier insured and uninsured.

[3]As shown by both Levy and DeLeire (2003) and Bundorf and Pauly (2006), health insurance coverage is not solely determined by income or poverty; rather, other factors, in addition to income, matter. These factors include, but are not limited to: the prices of other goods, preferences and the expected net benefit of insurance coverage.

Several distinct features of the health system in South Africa make it particularly relevant for analyzing the effect of health insurance on consumer behaviour. The fact that South African health insurance is predominantly geared towards the employed creates problems for individuals who are not employed, and are thus precluded from participating in the insurance market.[4] Bloom and McIntyre (1998), for example, argue that the inequalities in the system require a definitive and targeted response, and that the appropriate response is a social health insurance system. However, as noted by Gilson, Doherty, Lake, McIntyre, Mwikisa and Thomas (2003) a social health insurance program was not favoured by either the Minister of Health, National Treasury or the Congress of South African Trade Unions (COSATU), albeit for different reasons.[5]

From 1994 to 2002, a myriad of mandatory and voluntary health insurance proposals have been suggested. Most recently, and most relevant to this study is the proposal put forth by the Taylor Committee of Inquiry into Comprehensive Social Security, described in more detail by McIntyre and Van den Heever (2007). In 2002, the Taylor committee recommended that health insurance be mandatory for all formal sector employees earning above the income tax threshold, and voluntary for all informal sector workers. Specifically, the committee recommended that everyone would ultimately be required to make income-based contributions. The Ministerial Task Team for Implementing Social Health Insurance made similar recommendations in 2002, also summarized by McIntyre and Van den Heever. The Ministerial Task Team recommended mandatory social health insurance to be financed via a composite social security tax, although allowing for voluntary schemes based on

[4]Söderland and Hansl (2000) suggest that South Africans are uninsured, because medical schemes only cater to the employed.

[5]According to Gilson *et al* (2003), COSATU felt that it was inappropriate to expect their members to pay for care that they currently received for free, while the Minister of Health at the time thought it would be difficult to convince people to buy the service from a costly industry (the medical schemes were meant to be the primary delivery vehicle for the social insurance program) and was ideologically opposed to the industry in the first place. National Treasury, on the other hand, was concerned about the effect such a program might have on their efforts to reduce the budget deficit and the tax burden.

community-rated contributions, which were expected to become mandatory at a later date. However, these policies have yet to be enacted.

Despite the failure, heretofore, to adopt a social health insurance program, the recent obligatory pension program put forth by National Treasury, suggests that requiring all South Africans to participate in a social insurance program may no longer be sacrosanct (National Treasury, 2007); furthermore, the African National Congress (ANC) policy conference in Polokwane in December 2007 committed to the introduction of National Health Insurance (NHI).[6] The ANC recently retained control of the government through an overwhelming win in the April 2009 elections, and, therefore, NHI will remain a policy priority. Since NHI policies have incorporated mandatory contribution, which is based upon employment, as well as an earnings threshold, this study analyzes the effect of mandatory insurance on household expenditure allocations only for households with workers. Due to the fact that income threshholds are also likely to feature as part of NHI, further delineations of the analysis are undertaken across the income distribution for both urban and rural households.

Although gross substitution is an obvious implication of a binding budget constraint, such that households will have to reallocate their expenditures to pay for mandatory health insurance, very little empirical evidence on expenditure reallocations exists. Given that few studies have considered budget reallocations required to accommodate mandatory expenditure requirements, such as health insurance, and that at least two countries (the US and South Africa) are considering the implementation of various national insurance programs, this study is timely.[7] We apply the analysis within a developing country, South Africa, that is considering

[6]We thank a reviewer for pointing out this commitment by the ANC.

[7]Levy and DeLeire (2003) is the only study that considers substitution between household expenditures related to health insurance. However, a number of other studies have considered conditional expenditures. Most recently, Vermeulen (2003) , John(2008), and Kashala and Koch (2008) have examined household expenditure substitution patterns associated with tobacco purchases. The analysis applied in this study, however, differs from those, primarily in the strategy employed to identify treatment effects.

various combinations of voluntary and compulsory health insurance programmes in an effort to alleviate pressure on the public health sector.[8]

The results from this analysis show that a compulsory health insurance policy would impact uninsured household expenditure behaviour in a myriad of ways. Uninsured households would be expected to increase the portion of their budget that includes health insurance expenditures. We find that the poorest rural households would reallocate primarily by reducing their budget related to other food and beverage expenditures. On the other hand, the richest rural households would offset the increased insurance expenditures by reducing the portion of their budgets devoted to transport and communication. The poorest urban households would reallocate their budets primarily by reducing both grain and meat expenditures to raise the necessary funds to pay for mandatory health insurance. The richest urban household would substitute their housing as well as their transport and communication expenditure to cover the increased health insurance burden, while urban households in the middle of the income distribution would reallocate away from both housing expenditures and clothing and miscellaneous expenditures.

The remaining paper is organized in the following fashion. The empirical model used to estimate the expenditure systems and treatment effects is developed in Section 2, while Section 3 provides a discussion of the data and an analysis of the characteristic determinants of insured and uninsured households, in terms of their structure and expenditure patterns. The empirical results and implications are discussed in Section 4. We conclude in Section 5 with a summary and discussion of policy recommendations and limitations of the current study.

2. Estimating Treatment Effects within a Demand System

2.1. **Treatment Effects.** Defining $w_{ij} = x_{ij}/x_i$ as the share of the household budget for household i apportioned to good j (where x_{ij} is household expenditure on good j and x_i is total expenditure for household i, such that summing w_{ij} over

[8]These pressures have been observed, for example, by Grobler and Stuart (2007), who find that private facility utilization across all income levels is extensive, despite the relatively high cost associated with those facilities.

all goods j is unity for each household i), there are two potential treatment effects of interest. The first is the average treatment effect on the insured; the second is the average treatment effect on the uninsured. The analysis of treatment effects are becoming increasingly common in the literature.[9] In general, since insured households are likely to have different preferences than uninsured households, these treatment effects will not be the same. In the following analysis, though, the results, where comparable, are similar. Using the terminology from Imbens and Wooldridge (2009), the analysis will focus upon these treatment effects conditional on being in the sample, and are, thus, referred to as conditional average treatment effects; however, for notational simplicity, they will be referred to as average treatment effects.

Denote $h = \{I, U\}$ as the treatment of interest, I for the insured and U for the uninsured and $d_i^h \in \{0, 1\}$ as treatment indicators.[10] Notationally, $w_{ij}^h(d_i^h = 1)$ represents household i's expenditure share on good j assuming the household has received treatment protocol h, while $w_{ij}^h(d_i^h = 0)$ represents household i's expenditure share on good j assuming the household has not received treatment protocol h. We further denote additional observed household covariates as Z_i. Given our notation, the following assumptions are integral to the analysis.

Assumption 1A - Unconfoundedness: $d_i^h \perp\!\!\!\perp \left[w_{ij}^h(d_i^h = 1), w_{ij}(d_i^h = 0) | Z_i\right]$.

Assumption 2 - Overlap: $0 < e_i^h(Z_i) \equiv \text{prob}(d_i^h = 1 | Z_i = z) < 1 \ \forall z, h$.

These assumptions allow for the application of estimation via propensity score matching, as outlined in Rosenbaum and Rubin (1983), and unconfoundedness can be rewritten, as Assumption 1B.

Assumption 1B: $d_i^h \perp\!\!\!\perp \left[w_{ij}^h(d_i^h = 1), w_{ij}^h(d_i^h = 0) | e_i^h(Z_i)\right]$.

[9]Heckman and Vytlacil (2005) present a unifying theory of the disparate treatment definitions and their interpretations. More recently, Imbens and Wooldridge (2009) provide an excellent summary of the literature to date.

[10]In many analyses considering binary treatment effects, only one side of the treatment is considered; however, this analysis considers both sides of the binary outcome in order to verify that the estimators are robust to the direction in which treatment is considered.

From these assumptions, the average effect of treatment on the treated for good j consumed by household i assuming treatment protocol h (where E is the expectations operator) is described by the following equation.

$$\tau_{ij}^h = E\left[w_{ij}^h(d_i^h = 1)|d_i^h = 1, Z_i\right] - E\left[w_{ij}^h(d_i^h = 0)|d_i = 1, Z_i\right]. \tag{1}$$

In equation (1), the final term in the analysis is not observed, since households can only be observed according to their actual treatment status. Generally, in this framework, dropping the i subscripts for notational convenience:

$$E\left[w_j^h(d^h = 1)|d^h = 1, Z\right] = \tau_j^h + E\left[w_j^h(d^h = 1)|d^h = 1, Z\right] - E\left[w_j^h(d^h = 0)|d^h = 0, Z\right]. \tag{2}$$

The final two terms in (2) represent selection bias, which is potentially identified under a number of different assumptions. Selection can be identified via instrumental variables, which is predominately employed in Heckman-type selection models (see, for example, Heckman, 1979) requiring instrumental variables. Additionally, selection can be identified if treatment assignment is random. Furthermore, selection can be identified if it is dependent only upon observable variables (Rosenbaum and Rubin, 1983). Assuming either 1A or 1B is equivalent to assuming that the final two terms are independent of the observed outcome, and can, therefore, be excluded from the analysis, which is the route considered in the following analysis.

Assumptions 1A and 1B are predicated on the fact that the data employed in this analysis, from a cross-sectional survey of household expenditures, is devoid of instrumental variables.[11] It is also not the case that the data was generated through a random experiment; rather, it is survey data. Therefore, the only recourse is to

[11]In an earlier version of the paper, a regime-switching Roy (1951) model was considered and analyzed. However, the performance of the instrument in the first stage regression led to wildly erratic, and, in our view, inconceivable results. For example, the average effect of treatment for the treated on household food shares was nearly 50% or half of the budget. The difficulty that we had with the analysis was that the instruments employed in the analysis were provincial-level instruments, which kept us from including other provincial-level dummies; including provincial-level dummies in addition to the provincial-level instruments led to multicollinearity in the first-stage regression. For that reason, regime-switching was not considered here. Interested readers can request those results from the authors.

assume that selection is driven by observed variables, which allows for estimation via matching.

Estimation of the treatment effects in equation (1) is operationalized via one-to-one caliper matching on the estimated propensity for the household to purchase their own insurance. Specifically, define the set of matches as

$$M_h(e_i^h) = \left[k : |e_i^h - e_k^h| = \min_{\ell \in \{d^h = 0\}} \left(|e_i^h - e_\ell^h| \right) \right] < 0.0025 \tag{3}$$

Intuitively, (3) describes an algorithm whereby an untreated ($d^h = 0$) household is matched to the treated household with the closest propensity score, as long as that propensity score distance is less than one-quarter of one percent. [12] The algorithm allows for replacement, such that some treated households are matched to more than one untreated household; furthermore, some treated households are not matched to anyone, and are not part of the treatment effect estimation sample.[13] Once the set of matched data, M_h, is created, treatment effects can be estimated.

2.2. **The Empirical Specification.** The model developed for this analysis focuses on two issues at the level of the household; the first is general consumption expenditure, in terms of shares, and the second is health insurance status. Although households are a collection of individuals, and there are likely to be interesting internal decision processes within the household, the data collection process treats the household as the unit of analysis. Since the data does not allow for the empirical identification of the intra-household economy, a unitary household model is presumed here.[14] The key assumption underlying the model is that household preferences differ by insurance status, although those differences can be accounted for through observed household characteristics, such that propensity score caliper

[12]Various caliper lengths were considered, and, although the results were affected by that length, the qualitative outcomes did not change, and, therefore, 0.0025 was retained.

[13]Treated households matched to more than one untreated household are repeated in the estimation sample, and repeated as often as they are additionally matched, to account for the multiple matches within the estimation. For example, a treated household that is matched to two untreated households occurs twice in the estimation subsample.

[14]In other analysis with this data, Koch (2007) has shown that single person households do not significantly differ from multiple person households, such that a unitary model is a reasonable approximation in South Africa.

matching is appropriate. However, the underlying model of behaviour is predicated on the Quadratic Almost Ideal Demand System (QUAIDS) developed by Banks *et al* (1997); therefore, treatment effects must be accommodated within an expenditure share system.[15]

Theoretically, health insurance can be modelled as a derived demand within a household health production function or within a model where households are averse to risk. In those settings, the demand for health insurance will depend upon the relative price of insurance, the household's aversion to risk and the household's income. Unfortunately, the data only directly provides information on household income. However, the data does include household-level expenditure on health insurance, which is a composite indicator of both the price of insurance and the household's aversion to risk. Therefore, it is reasonable to assume that the marginal benefit of insurance exceeds the relative cost of insurance, at least for those households purchasing insurance. Those not purchasing insurance, according to this view, do not do so, because the marginal cost exceeds the marginal benefit, although one might also worry that some non-purchasing households would like to purchase insurance, but cannot afford to at current prices. In this analysis, no attempt is made to separate amongst these two different points of view regarding non-participation; economically, they are isomorphic. Households that do not purchase insurance are uninsured, while those that do purchase are insured.[16] Given that insurance purchases under this definition are household choices, they are most likely to be representative of selection into treatment, and are, therefore, most relevant to this study.

[15]Although a demand system is considered, treatment effects are also estimated via simple differences in mean expenditure shares. As noted below, the differences in estimation do not yield substantially different estimates of the mean treatment effects.

[16]The survey also asks for employer based expenditures on health insurance, which could also be included in the analysis. However, the level of employer based expenditure is extremely low; job classification and other characteristics of employment are also scant in the data. Alaba and Koch (2009) provide a more detailed analysis of which South African households are insured and which are not using the same data. His results are used to model the propensity score that underpins the matching algorithm.

The household's propensity to be insured is determined by a standard binary choice model, where

$$e_i^h(Z_i) = \text{prob}(d_i^h = 1 | Z_i = z). \tag{4}$$

Variables included in Z_i are indicators for the population group to which the household head belongs,[17] the number of children in the household aged less than five years, aged five to ten years, the number of children aged 10 to 15 years, the elderly dependency ratio (the number of pension eligible adults in the household relative to the number of workers),[18] the number of workers in the household, an indicator for the quality of the household's water source (whether or not water is directly piped into the house), and sanitary facilities (whether or not household members have access to a flush toilet), an indicator of whether or not the household purchases tobacco products,[19] whether or not the household has access to either other insurance products or incurs either a gambling loss or gain,[20] as well as household income and its square.[21]

Each expenditure share, on the other hand, is assumed to be generated from the following stochastic relationship, modified from Banks *et al* (1997), equation (10).

$$w_{ij}^h(M_i^h) = X_i \Gamma_j^h + \tau_j^h d_i^h + \epsilon_{ij}^h \qquad \forall j \in \{1, \ldots, J\}, h \in \{I, U\} \tag{5}$$

In equation (5), w_{ij}^h indicates household i's expenditure share associated with good j, d_i^h indicates treatment status, and X_i represents matrix of household specific covariates, while Γ_j^h is a vector of parameters to be estimated.[22] The treatment

[17]Black African households are the base category. Very few Asian households exist in the data, and, therefore, they were not included in the analysis.
[18]In South Africa, females are eligible at the age of 60, while males are eligible at the age of 65.
[19]Household expenditure on both alcohol and tobacco are not included within any of the shares, nor are they included in total household expenditure.
[20]These variables are meant to capture attitudes towards risk.
[21]Deaton (1997) argues that expenditure is a better measure of household resources than income, since it is more likely to be correctly recorded. Labour Force Surveys in South Africa, on the other hand, which explicitly try to collect income information are fraught with non-responses for income (Daniels, 2008), further validating our choice of expenditure over income.
[22]Banks *et al* (1997) stress that the quadratic expenditure component cannot be independent of prices in the QUAIDS model. However, our data does not contain price information, such that prices were not included in the model. However, in estimates not reported here, minimum

effect, the primary interest of the analysis, is estimated through τ_j^h, see (1). Shares are only estimated for matched data, based on the matching algorithm M_i^h.[23] Two versions of the analysis are presented. In one version, Γ_j^h is set to zero, such that the differences across treatment are assumed to be independent of additional covariates. In the other version, Γ_j^h are estimated, assuming that additional covariates do affect the shares and the treatment effects. Therefore, additional covariates control for omitted variables bias.

The independent variables used to estimate equation (5) are classified into two categories, namely: basic individual and household variables and capability controls. The basic individual and household variables include: the number of children aged between 0 and 5, 5 and 10 and 10 and 15, the age and squared age of the household head, the ratio of males to the total number of household members, an indicator for whether or not the household head is male, and indicators of the population group of the household head. The capability factors control for the economic status of the household, including employment and financial empowerment, which are associated with financial security. Included in these factors are: the proportion of working adults relative to the number of working-age adults, the child dependency ratio (the number of young children relative to the number of working household members), the elderly dependency ratio (the number of pension eligible adults to the number of working household members), and household income and its square, although total expenditure is used to proxy for income. Descriptive statistics for the variables included in the analysis for the matched and unmatched data, by household insurance status, are contained in Tables A2 and A3.

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expenditure within survey clusters was used to control for price differences within each cluster. The results from that analysis pointed to generally insignificant *real* expenditure effects, but only minor differences in the underlying treatment effects; therefore, those results are not reported here.

[23]For completeness, unmatched regression results are also included. Comparisons of the matched and unmatched sample results shows the impact of matching on the estimation of the treatment effects.

3. The South African Income and Expenditure Survey, 2000

The data for the analysis is sourced from the Income and Expenditure Survey (IES) of South Africa, conducted in October 2000 by Statistics South Africa. The IES is a quinquennial cross-sectional survey based on the master sample of South African census enumeration areas; in this case, the sample is based upon the 1996 census. Weights are included in the survey to make the estimation results nationally representative; however, the weights are a source of contention in the survey, as highlighted by Van Walbeek (2005) and Koch (2007), amongst others. Due to the contention surrounding the weights, the weights are not used in the analysis; thus, the results can only be interpreted relative to the sample.

In the IES data, an insured household is identified by reported expenditure on any kind of health insurance, while an uninsured household does not report any expenditure on health insurance. In terms of expenditure categories, an aggregation decision had to be made, due to the fact that the IES contains information on expenditures for hundreds of detailed categories, as well as the fact that a large number of expenditures are zero.[24] For this analysis, expenditures were aggregated into 7 monthly expenditure categories. These are: (1) Housing, (2) Grain, (3) Meat, (4) Other food and non-alcoholic beverages, (5) Clothing and miscellaneous items, (6) Transport and Communication and (7) Recreation, Education, Personal Care and Health Expenditures (including health insurance expenditures).[25]

The data set provides detailed expenditure and demographic information on 26249 households. Given the nature of the data, it has been used extensively for consumption and income studies by Burger, Van der Berg and Nieftagodien (2004), Simkins (2004), and Koch (2007), as well as for the analysis of poverty

[24]A large number of zeroes requires the analysis to consider the behavioural reasons for missing observations; however, aggregation that reduces the number of zeroes allows the analysis to focus on behavioural aspects associated with positive expenditures, which limits the complexity of the analysis. Shonkweiler and Yen (1999) develop a procedure for estimating a Tobit demand system, that could be applied here; however, since the focus is on the overall average treatment effect, empirically modelling observed zeroes is unecessary, since actual zeroes are an appropriate indicator of observed zeroes.

[25]Since health insurance expenses will differ across treatment status, by definition, estimated treatment effects will take these differences into account.

in South Africa; see, for example, Özler (2007). The data has also been used to consider the consumption patterns of specific products. For example, tobacco and alcohol consumption have been examined by Van Walbeek (2005), Ground and Koch (2008), Koch, Ground and Van Wyk (2008), as well as Kashala and Koch (2008).

The main shortcoming of the data set, relevant to this analysis, is the lack of information on health status or other direct indicators of cost of insurance. These shortcomings are to be expected, because the IES was designed for calculating the consumption basket used to underpin the South African Consumer Price Indexes, CPI and CPI-X. Despite the targeted purpose of the IES, some health indicator proxies and attitudes towards risk are available in the data set, and are used in the analysis. Therefore, the IES is a suitable data source for the analysis of policies that might affect consumption behaviour, as discussed above.

In order to conduct the analysis, the data set was restricted to those households most likely affected by the proposed mandatory health insurance programs. Since the most recent programs proposed by both the Taylor Committee and the Ministerial Task Team in 2002 explicitly mentioned employment, while the Taylor Committee also mentioned income thresholds, the data is limited to households with at least one working adult in the household. Although the IES 2000 does not focus on labour force participation, all household members are asked whether or not they have worked in the past week. Despite the fact that this measure does not properly capture attachment to the formal sector, we believe that employees in the formal sector are more likely to answer in the affirmative, when asked this question, than are informal employees.[26] Since income threshholds are also important to at least one of threshholds, households with at least one working adult were further segregated by income group. For rural households the break was at the 85^{th} percentile, due to the fact that so few rural residents purchase their own insurance, possibly due to the distance that would be required to reach private health facilities,

[26]The employment rate calculated from this question is similar to the overall employment rate of 49.5% in South Africa in 2000.

which are all located in urban areas. Urban households, on the other hand, were segregated by income tercile. Noticeably, very few urban households in the bottom third of the urban income distribution purchase their own insurance.

4. The Results

Economic theory asserts that households select their insurance status by virtue of their own optimising decisions. Insurance selection in this study is estimated via a binary response model, based upon a logit specification. Although the logit results are not reported, due to space limitations, descriptive statistics for both the matched and unmatched samples are reported in Tables 1 and 2. These tables provide evidence regarding the success of the matching algorithm. For the unmatched samples, most of the variables are significantly different across insurance status, and are significant in the logit specification. Once the data has been matched within calipers of the propensity score, the empirical significance disappears.[27]

As previously noted, the underlying assumption for identification of treatment effects is that selection is based upon observed data, and can be controlled through the application of a matching estimator. However, from the preceding discussion regarding the data, it is clear that the data does not include all of the necessary information, and, therefore treatment status depends upon some unobserved covariates. Therefore, treatment effects bias will arise if the missing data cannot be adequately measured through existing variables in the data. The most obvious concerns relate to: (a) the price of insurance, (b) household aversion to risk, and (c) the health status of household members, although (a) and (c) are likely to be co-determined. In this analysis, we account for household composition - specifically, the number of very young and the number of elderly household members - and household access to sanitation to control for the health status of household members. We also control for household income and the number of working adults in the household to control for the ability to pay for insurance. Although it is

[27]For a more detailed description of these results, the interested reader is directed to Alaba and Koch (2009), upon which the propensity score model is based.

not a perfect measure of the health insurance premium that the household would expect to pay, it does proxy for the relative ability of the household to pay that premium. Finally, in an effort to control for risk aversion, we include a dummy variable accounting for whether or not households purchase tobacco products or gamble (purchase lottery tickets) or have access to other insurance products. Given the success of the matching algorithm, we feel that the remaining bias, although not zero, has, at least, been mitigated. The treatment effects estimates resulting from the matching estimators are discussed, below.

4.1. **Rural Households - No Controls.** The results for rural households, assuming no additional expenditure share controls, is incorporated in Table 3. Within the table results are reported for the average treatment effect on the treated for all households, poorer households and richer households; the average treatment effect on the uninsured are only reported for the richer households. Within each subsample treatment effects based on the entire subsample, reported as *unmatched* are compared to propensity score matched treatment effects, reported as *matched*.

For the unmatched data, the effects of insurance are generally statistically significant and economically quite large. For all rural households, the average insured household share is between 10.8% lower and 13.1% higher than for uninsured households. For the poorer households, ranging from -4.9% to 6.9%, and for the richer households, ranging from -3.6% to 9.3%, are much smaller, due to the fact that subsampling reduces some of the household differentiation; importantly, that differentiation is further mitigated by matching and reweighting the subsamples. For many goods, matching and reweighting results in a statistically insignificant difference; the remaining significant differences are closer to zero.

For the insured, as expected, a larger share of the budget must be devoted to care and recreation, which includes health, education and personal care expenditures; estimates range between 7.2% and 9.6%, and these effects are both statistically and economically significant. The increased focus on care and recreation is, however, offset differently, depending upon household wealth. Treatment results in poorer

rural households spending a smaller proportion of their budget on both housing (-2.8%) and other foods and beverages (-2.3%). For the richer insured households, budget shares are lower for grain expenditures (-1.2%), other food and beverage expenditures (-1.1%), and transport and communication expenditures (-4.8%).[28]. Uninsured households, on the other hand, naturally devote a smaller current share of their budget to care and recreation (-9.8%). The expenditure that is currently not devoted to insurance is, instead, allocated towards increased grain (1.1%), meat (1.6%), clothing and miscellaneous items (3.2%), as well as transport and communication (2.5%). Therefore, requiring the richer households that do not currently have insurance to purchase insurance would force them to reallocate their expenditures away from necessicites, which could cause undue sacrifice for these households, and other goods.

4.2. **Urban Households - No Controls.** Urban households were analyzed separately from rural households; the treatment effects for both insured and uninsured urban households, assuming no additional empirical controls, is presented in Table 4. Qualitatively, the urban results are similar to the rural results. For all urban households in the urban subsample, due to heterogeneity, the underlying difference between insured and uninsured households is larger than it is for any of the urban household subsamples, while the treatment effects for all urban households are a matched average of the treatment effects within the urban subsamples.

For the poorest third of urban households, the share differences range from -4.0% to 4.7%. However, once the households within this subsample are matched, the differences range from -4.3% to 4.3%. Although the subsample of matched observations is quite small, only 124, the effect of treatment on the insured is a significantly larger share of the budget devoted to care expenditures (4.3%) that is offset by significantly reduced expenditure shares on grain (-2.8%) and meat (-4.3%). Although the middle third of households, by total expenditure, also devote

[28]For all rural households, the reported treatment effects on the insured most closely resemble those of the richer households, due to the large number of insured household in the richer subsample than in the poorer subsample

a significantly larger share of their budget to care and recreation (8.6%), that increase is offset by reductions in housing (-3.7%), other foods and beverages (-1.3%) and clothing and miscellaneous items (-3.6%). The effect of insurance on the richest households, on the other hand, is across the board. As expected, the care and expenditure share is larger for the richest households, by 5.8%; however, in addition to that increase, they spend an additional 1.8% on clothing and miscellaneous goods. These two increases, due to being insured, are offset by reductions in housing (-3.3%), grain (-0.4%), meat (-1.2%), other foods and beverages (-0.8%), and transportation and communication (-2.0%).

Given the fact that a larger number of urban households are insured, it is also possible to reverse the match, and consider the effect of treatment on the uninsured. The final two sets of results in Table 4 provide the average effect of treatment on the uninsured, although only for the richest two-thirds of the sample as there were not enough observations in the poorest third subsample. For both subsamples, as expected, a smaller share of household budgets was devoted to care and recreation, -7.8% and -7.1% for the middle and top third, respectively. Furthermore, the treatment effects for the uninsured differ both qualitatively and quantitatively from the treatment effects for the insured. Both the top and middle third of urban households reallocate their budgets towards housing, grain and other foods and beverages. However, while the middle households further reduce their transport and communication budget, the top third increases their transport and communication budget share. In addition, although the top third devotes more of its budget to meat, the middle third of households expenditure on meat is not significantly altered, as a result of insurance status. Regardless of the specifics, the average effect of treatment for both insured and uninsured urban households is an extensive reallocation of their expenditure budget.

4.3. **Rural and Urban Households - Additional Controls.** Additional budget share analysis was conducted with additional household level controls. Inclusion of these controls was based on the application of consumer demand theory. For

rural households, the results are presented in Table 5, while Table 6 contains the results for urban households. The primary treatment effects on the insured and the uninsured, inclusive of additional covariates, remain similar to those already reported without additional control variables. Comparisons of the final columns of Tables 3 and 5, as well as the final columns of Tables 4 and 6 show that the change in the budget associated with care expenditures is not greatly affected by the addition of other covariates in the explanation of expenditure shares. However, the significance of the reallocation estimates is affected. The inclusion of additional controls to explain expenditure shares for the poorer rural households results in only one significant offset to the increase in care expenditures, rather than two - other foods and beverages fall by 2.5%. For the richer households, one additional offset becomes significant - the share of the meat budget falls by 0.8% - while the other previously noted offsets become slightly smaller. Similarly, for the richest uninsured households, one budget share, in addition to those previously noted, is significantly affected by treatment - the meat share is 0.8% larger. Although the inclusion of additional controls does not drastically alter the estimated average treatment effects on the insured and uninsured, that same inclusion does significantly alter the difference between the matched and unmatched estimates, such that the underlying bias in the naïve treatment effect estimates are lower. Comparing Tables 4 and 6 for the urban households yields conclusions similar to those reported for the rural households: only minimal changes in the underlying matched treatment effect estimates are uncovered, while smaller difference between the matched and unmatched estimates obtain. Although smaller differences between the matched and unmatched samples obtain, once additional controls are included in the regressions, significant differences between the matched and unmatched average treatment effect estimates remain.[29] The reported results are consistent with the general application of doubly-robust estimators (Imbens and Wooldridge, 2009). Although propensity

[29]For example, average uninsured treatment effects for the middle and top third of urban households are notably different with respect to transportation and communication. For both subsamples, the unmatched difference is not significantly different from zero, but is negatively significant from zero for the matched difference.

score matching provides relatively consistent estimates across the specification, the inclusion of additional regression controls provides additional robustness with respect to the estimation of average treatment effects.

5. Conclusions and Recommendations

In this paper, we have presented estimates of the treatment effects associated with health insurance on household expenditure behaviour. In order to estimate those treatment effects, we have employed propensity score matching to re-weight the data, and that re-weighted data has been estimated within a demand system to further examine the robustness of the treatment effect estimates. These estimated treatment effects are calculated based on counterfactuals, i.e., we consider expenditure behaviour for the insured compared to their expenditure under the counterfactual of not having access to insurance. The treatment effects analysis applied in this study has shown that household budget allocations will be strongly affected by the imposition of a mandatory health insurance policy, although households at different levels of the income distribution and in different locations will not reallocate their expenditures in exactly the same fashion.

The results of the analysis point to rather large increases in health insurance expenditures, as would be expected if uninsured households were required to purchase health insurance. These increases result in an estimated increase in household expenditures on personal care, education, and health of between 33% and 100%. Given the fact that household budget constraints are binding, these increases are an economic burden that must be accommodated through budgetary reallocations. In the analysis, the estimated reallocations range from statistically insignificant decreases in certain expenditures to both statistically and economically significant decreases in expenditures on other items. The analysis suggests that rural households would accommodate the increased insurance burden by either decreasing other food expenditures by about 20% or by decreasing their transportation and communication budgets by about 15%. The analysis also suggests that the urban poor,

who may not, in the end, be subjected to a mandatory health insurance program, bear an extensive food burden, as both grain and meat expenditures would decrease by one-quarter to one-third. Urban households in the top two-thirds of the income distribution, those most likely to be included in the mandatory health insurance programs, would also be affected. The effects for these richer urban households range from an expected decrease in their housing budgets - between 15% and 20% - to decreases in either clothing and miscellaneous expenditures or transport and communication expenditures - of about 15%.

The results reported in this paper are another reminder that even good-intentioned social policies can have negative consequences, and that those consequences must be weighed-up against the expected benefits of the social policy. The research presented here cannot speak to the net benefits of a national health insurance programme, since it only focuses upon household expenditure behaviour. However, the results show that not all effects are necessarily positive. However, the results here do indicate that an optimal NHI will need to carefully balance the expected costs of the policy - household welfare reductions for those required to participate - with the expected benefits of the policy - increased access to private health facilities. The results also indicate that inclusion of the poorest households in a mandatory health insurance program would have dire consequences on food and nutrition intake for those households, and, therefore, those households would need their contributions to be subsidized, which would increase the burden on upper income households. To our knowledge this is the first research to quantify the dire nature of these consequences, and, therefore, this research is the first that provides quantitative support for a policy that subsidizes health insurance for the poorest households. However, further research to design the policy that simultaneously balances household welfare related to accessing better healthcare against reduced spending opportunities and the proper taxation and subsidization program to achieve that policy is necessary. The results presented here provide a window into that policy design; however, more research is needed. At this point, the analysis suggests that compulsory insurance

should be implemented along with other subsidies, possibly towards reducing the price of food, transportation, housing or communication. Implementation of any of these subsidies will have consequences for the nation's fiscus.

There are other limitations in the analysis, as well. Importantly, we assume that the proposed voluntary or compulsory schemes are based upon insurance contracts that are already in force. In other words, the analysis cannot directly consider the generosity of the proposed insurance plan, and, therefore, the results should be taken as an upper bound to the required subsidies or the estimated effects of the policies. As already noted, there are two very important missing variables in the analysis. The first of which is prices, the second of which is household health status. Although we have used a number of proxies to control for these missing variables, there is no substitute for better information. Future research must strive to uncover additional data to provide a more complete picture of proposed national health insurance plans in both developing and developed countries. Finally, the results in this paper cannot speak to the moral hazard that might arise as a result of universal coverage, and potential moral hazard should also be included within the design of an optimal NHI.

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TABLE 1. Rural Household Balance Comparison - Before and After Propensity Score Match

VARIABLES	coloured	white	nchild1	nchild2	nchild3	ageddr	workers1	pipein	fltoilet	postob	altins	gamble	lnx	lnx2
All Rural Households - ATT														
unmatched	-0.00	0.24**	-0.11**	-0.16**	-0.10*	0.01	0.15**	0.25**	0.17**	-0.04	0.40**	0.14**	1.55**	30.82**
	(0.02)	(0.01)	(0.04)	(0.04)	(0.04)	(0.01)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.82)
Observations	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084
matched	0.02	0.01	0.03	0.05	-0.03	0.01	0.00	0.03	0.01	0.06	0.01	0.02	0.02	0.46
	(0.02)	(0.03)	(0.05)	(0.05)	(0.06)	(0.02)	(0.06)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.06)	(1.33)
Observations	782	782	782	782	782	782	782	782	782	782	782	782	782	782
Poorest 85% of Rural Households - ATT														
unmatched	-0.00	0.01	-0.12	-0.07	0.08	-0.02	0.06	0.02	0.15**	-0.04	0.18**	0.03	0.62**	11.31**
	(0.03)	(0.01)	(0.08)	(0.09)	(0.09)	(0.02)	(0.08)	(0.03)	(0.04)	(0.05)	(0.05)	(0.03)	(0.07)	(1.24)
Observations	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172
matched	-0.02	-0.01	0.01	0.11	0.15	-0.00	0.01	-0.04	0.02	-0.01	0.03	0.03	0.01	0.04
	(0.05)	(0.02)	(0.10)	(0.11)	(0.12)	(0.02)	(0.14)	(0.05)	(0.04)	(0.07)	(0.07)	(0.05)	(0.08)	(1.39)
Observations	178	178	178	178	178	178	178	178	178	178	178	178	178	178
Richest 15% of Rural Households - ATT														
unmatched	-0.01	0.19**	-0.15**	-0.21**	-0.22**	0.00	-0.12	0.14**	0.02*	-0.01	0.16**	-0.00	0.40**	8.88**
	(0.02)	(0.03)	(0.05)	(0.05)	(0.06)	(0.02)	(0.07)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)	(0.93)
Observations	912	912	912	912	912	912	912	912	912	912	912	912	912	912
matched	0.03	-0.06	0.12*	-0.03	0.07	0.00	0.05	-0.04	0.00	-0.02	0.01	0.05	0.04	0.98
	(0.02)	(0.04)	(0.05)	(0.05)	(0.06)	(0.02)	(0.07)	(0.04)	(0.01)	(0.04)	(0.03)	(0.04)	(0.05)	(1.21)
Observations	604	604	604	604	604	604	604	604	604	604	604	604	604	604
Richest 15% of Rural Households - ATUT														
unmatched	0.01	-0.19**	0.15**	0.21**	0.22**	-0.00	0.12	-0.14**	-0.02*	0.01	-0.16**	0.00	-0.40**	-8.88**
	(0.02)	(0.03)	(0.05)	(0.05)	(0.06)	(0.02)	(0.07)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)	(0.93)
Observations	912	912	912	912	912	912	912	912	912	912	912	912	912	912
matched	0.01	-0.05*	-0.02	-0.04	-0.03	0.00	0.06	-0.03	0.01	0.08**	-0.01	0.03	-0.01	-0.17
	(0.02)	(0.02)	(0.04)	(0.05)	(0.05)	(0.01)	(0.05)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.64)
Observations	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220

Standard errors in parentheses
** p<0.01, * p<0.05

TABLE 2. Urban Household Balance Comparison - Before and After Propensity Score Match

VARIABLES	coloured	white	nchild1	nchild2	nchild3	ageddr	workers1	pipein	fltoilet	postob	altins	gamble	lnx	lnx2
All Urban Households - ATT														
unmatched	0.06**	0.32**	-0.03*	0.01	0.02	0.01	0.25**	0.43**	0.03**	-0.01	0.34**	0.08**	1.39**	28.56**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.38)
Observations	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754
matched	0.01	-0.01	0.04**	0.03	0.02	-0.00	0.03	-0.01	0.00	0.00	0.01	-0.02	-0.01	-0.23
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.48)
Observations	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974
Poorest Third of Urban Households - ATT														
unmatched	0.12**	0.01	-0.15*	-0.15*	-0.10	0.03	-0.03	0.06	0.03	0.01	0.06	-0.04	0.28**	4.77**
	(0.04)	(0.01)	(0.07)	(0.07)	(0.08)	(0.02)	(0.06)	(0.06)	(0.03)	(0.06)	(0.05)	(0.04)	(0.07)	(1.18)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.08	0.02	0.00	0.08	0.02	0.01	-0.02	-0.03	-0.03	0.10	-0.05	0.03	-0.02	-0.31
	(0.07)	(0.02)	(0.07)	(0.06)	(0.09)	(0.04)	(0.07)	(0.09)	(0.02)	(0.09)	(0.08)	(0.05)	(0.06)	(1.14)
Observations	124	124	124	124	124	124	124	124	124	124	124	124	124	124
Middle Third of Urban Households - ATT														
unmatched	0.05*	0.06**	-0.11**	-0.07	-0.05	-0.02	-0.16**	0.16**	0.01	-0.03	0.08**	0.01	0.12**	2.41**
	(0.02)	(0.01)	(0.04)	(0.04)	(0.04)	(0.01)	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.02)	(0.01)	(0.26)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	-0.02	0.00	-0.02	0.03	0.03	-0.00	0.00	0.00	0.00	-0.00	0.03	0.02	-0.00	-0.03
	(0.03)	(0.02)	(0.04)	(0.04)	(0.05)	(0.01)	(0.03)	(0.04)	(0.01)	(0.04)	(0.04)	(0.03)	(0.02)	(0.33)
Observations	674	674	674	674	674	674	674	674	674	674	674	674	674	674
Richest Third of Urban Households - ATT														
unmatched	-0.00	0.22**	-0.04*	-0.01	-0.06**	-0.00	-0.01	0.14**	0.00	0.00	0.16**	0.01	0.39**	8.76**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.00)	(0.02)	(0.01)	(0.01)	(0.02)	(0.40)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	-0.00	0.03	0.02	-0.00	-0.01	-0.01	0.00	-0.01	-0.00	0.01	-0.00	-0.03*	-0.02	-0.44
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.42)
Observations	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176
Middle Third of Urban Households - ATUT														
unmatched	-0.05*	-0.06**	0.11**	0.07	0.05	0.02	0.16**	-0.16**	-0.01	0.03	-0.08**	-0.01	-0.12**	-2.41**
	(0.02)	(0.01)	(0.04)	(0.04)	(0.04)	(0.01)	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.02)	(0.01)	(0.26)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	-0.02*	0.00	0.08**	0.12**	0.00	-0.03**	-0.01	-0.01	-0.01**	0.01	-0.04**	-0.05**	-0.00	-0.02
	(0.01)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.11)
Observations	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162
Richest Third of Urban Households - ATUT														
unmatched	0.00	-0.22**	0.04*	0.01	0.06**	0.00	0.01	-0.14**	-0.00	-0.00	-0.16**	-0.01	-0.39**	-8.76**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.00)	(0.02)	(0.01)	(0.01)	(0.02)	(0.40)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.00	0.02	-0.01	0.02	-0.02	-0.01	0.01	0.00	-0.00	-0.00	-0.02	-0.02	-0.01	-0.23
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.00)	(0.02)	(0.02)	(0.02)	(0.02)	(0.39)
Observations	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660

Standard errors in parentheses
** p<0.01, * p<0.05

TABLE 3. Rural Household Treatment Effects Before and After Match - No Covariates

VARIABLES	whs	wgr	wmt	wof	wcm	wtc	wcr
All Rural Households - ATT							
unweihted	-1.67**	-10.80**	-6.18**	-8.32**	13.09**	4.43**	9.44**
	(0.54)	(0.58)	(0.47)	(0.46)	(0.91)	(0.45)	(0.38)
Observations	6083	6083	6083	6083	6083	6083	6083
matched	-1.08	-0.89	-0.87	-1.52**	-0.90	-2.29**	7.57**
	(0.85)	(0.58)	(0.53)	(0.48)	(1.40)	(0.78)	(0.72)
Observations	782	782	782	782	782	782	782
Poorest 85% of Rural Households - ATT							
unmatched	-3.65**	-4.83**	-1.34	-4.87**	5.60**	2.18**	6.91**
	(1.04)	(1.20)	(0.98)	(0.94)	(1.71)	(0.77)	(0.71)
Observations	5171	5171	5171	5171	5171	5171	5171
matched	-2.81*	-0.25	0.23	-2.34*	-2.45	0.40	7.22**
	(1.32)	(1.61)	(1.31)	(1.12)	(2.92)	(1.16)	(1.24)
Observations	178	178	178	178	178	178	178
Richest 15% of Rural Households - ATT							
unmatched	0.12	-2.94**	-3.60**	-2.79**	1.53	-1.66	9.34**
	(0.91)	(0.43)	(0.53)	(0.45)	(1.41)	(0.86)	(0.69)
Observations	912	912	912	912	912	912	912
matched	-1.94	-1.17*	-0.97	-1.09*	0.37	-4.84**	9.64**
	(1.07)	(0.46)	(0.51)	(0.48)	(1.60)	(0.98)	(0.81)
Observations	604	604	604	604	604	604	604
Richest 15% of Rural Households - ATUT							
unmatched	-0.12	2.94**	3.60**	2.79**	-1.53	1.66	-9.34**
	(0.91)	(0.43)	(0.53)	(0.45)	(1.41)	(0.86)	(0.69)
Observations	912	912	912	912	912	912	912
matched	1.25	1.12**	1.59**	0.20	3.20**	2.48**	-9.84**
	(0.73)	(0.32)	(0.42)	(0.40)	(1.06)	(0.65)	(0.58)
Observations	1220	1220	1220	1220	1220	1220	1220

Standard errors of the estimated treatment effect in parentheses
** p<0.01, * p<0.05

TABLE 4. Urban Household Treatment Effects Before and After Match - No Covariates

VARIABLES	whs	wgr	wmt	wof	wcm	wtc	wcr
All Urban Households - ATT							
unmatched	3.44**	-6.41**	-5.56**	-6.15**	4.94**	2.74**	7.01**
	(0.33)	(0.16)	(0.18)	(0.18)	(0.43)	(0.22)	(0.18)
Observations	11752	11752	11752	11752	11752	11752	11752
matched	-3.06**	-0.68**	-0.96**	-0.84**	1.21*	-1.61**	5.93**
	(0.47)	(0.12)	(0.19)	(0.16)	(0.50)	(0.30)	(0.27)
Observations	4974	4974	4974	4974	4974	4974	4974
Poorest Third of Urban Households - ATT							
unmatched	0.04	-3.95**	-2.51*	-2.18*	4.73*	1.31	2.57**
	(1.68)	(1.10)	(1.13)	(1.11)	(2.10)	(1.10)	(0.91)
Observations	3916	3916	3916	3916	3916	3916	3916
matched	-0.44	-2.76*	-4.30*	-1.92	3.30	1.83	4.29**
	(2.17)	(1.17)	(1.67)	(1.49)	(2.60)	(1.56)	(1.26)
Observations	124	124	124	124	124	124	124
Middle Third of Urban Households - ATT							
unmatched	-1.72*	-2.23**	-1.06*	-2.40**	-1.03	0.19	8.25**
	(0.74)	(0.33)	(0.47)	(0.43)	(1.16)	(0.49)	(0.43)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	-3.74**	-0.59	0.31	-1.26*	-3.56*	0.27	8.57**
	(1.00)	(0.36)	(0.64)	(0.54)	(1.53)	(0.60)	(0.73)
Observations	674	674	674	674	674	674	674
Richest Third of Urban Households - ATT							
unmatched	0.17	-1.78**	-2.67**	-2.11**	0.66	-0.56	6.29**
	(0.53)	(0.12)	(0.20)	(0.17)	(0.57)	(0.34)	(0.30)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	-3.27**	-0.36**	-1.18**	-0.77**	1.79**	-2.04**	5.83**
	(0.53)	(0.10)	(0.18)	(0.16)	(0.52)	(0.34)	(0.29)
Observations	4176	4176	4176	4176	4176	4176	4176
Middle Third of Urban Households - ATUT							
unmatched	1.72*	2.23**	1.06*	2.40**	1.03	-0.19	-8.25**
	(0.74)	(0.33)	(0.47)	(0.43)	(1.16)	(0.49)	(0.43)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	2.80**	1.14**	0.21	1.29**	2.77**	-0.42*	-7.79**
	(0.27)	(0.12)	(0.18)	(0.16)	(0.42)	(0.20)	(0.20)
Observations	7162	7162	7162	7162	7162	7162	7162
Richest Third of Urban Households - ATUT							
unmatched	-0.17	1.78**	2.67**	2.11**	-0.66	0.56	-6.29**
	(0.53)	(0.12)	(0.20)	(0.17)	(0.57)	(0.34)	(0.30)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	3.28**	0.50**	1.16**	0.83**	-0.17	1.50**	-7.10**
	(0.54)	(0.13)	(0.21)	(0.19)	(0.59)	(0.35)	(0.33)
Observations	3660	3660	3660	3660	3660	3660	3660

Standard errors in parentheses
** p<0.01, * p<0.05

TABLE 5. Rural Household Treatment Effects Before and After Match - Covariates Included

VARIABLES	whs	wgr	wmt	wof	wcm	wtc	wcr
All Rural Households - ATT							
unmatched	-2.14**	-0.40	-2.46**	-1.98**	-0.92	-1.64**	9.54**
	(0.60)	(0.56)	(0.52)	(0.49)	(0.92)	(0.48)	(0.43)
Observations	6083	6083	6083	6083	6083	6083	6083
matched	-1.12	-0.87	-0.85	-1.54**	-1.22	-1.95**	7.55**
	(0.81)	(0.46)	(0.44)	(0.40)	(1.23)	(0.73)	(0.71)
Observations	782	782	782	782	782	782	782
Poorest 85% of Rural Households - ATT							
unmatched	-2.93**	0.01	-1.22	-2.42**	-0.29	-0.09	6.95**
	(1.03)	(1.04)	(0.95)	(0.91)	(1.59)	(0.75)	(0.70)
Observations	5171	5171	5171	5171	5171	5171	5171
matched	-2.08	-0.83	0.34	-2.49*	-2.97	0.74	7.29**
	(1.22)	(1.27)	(1.25)	(1.05)	(2.60)	(1.14)	(1.27)
Observations	178	178	178	178	178	178	178
Richest 15% of Rural Households - ATT							
unmatched	-1.95*	-0.66	-1.51**	-0.97*	-1.13	-3.35**	9.57**
	(0.93)	(0.41)	(0.49)	(0.43)	(1.38)	(0.90)	(0.73)
Observations	912	912	912	912	912	912	912
matched	-0.90	-1.10*	-0.81*	-0.88*	-2.09	-4.01**	9.79**
	(1.01)	(0.44)	(0.41)	(0.44)	(1.41)	(0.98)	(0.81)
Observations	604	604	604	604	604	604	604
Richest 15% of Rural Households - ATUT							
unmatched	1.95*	0.66	1.51**	0.97*	1.13	3.35**	-9.57**
	(0.93)	(0.41)	(0.49)	(0.43)	(1.38)	(0.90)	(0.73)
Observations	912	912	912	912	912	912	912
matched	1.59*	0.80**	1.41**	-0.15	3.34**	2.33**	-9.32**
	(0.71)	(0.28)	(0.37)	(0.35)	(0.99)	(0.65)	(0.57)
Observations	1220	1220	1220	1220	1220	1220	1220

Standard errors of the estimated treatment effect in parentheses
** p<0.01, * p<0.05

TABLE 6. Urban Household Treatment Effects Before and After Match - Covariates Included

VARIABLES	whs	wgr	wmt	wof	wcm	wtc	wcr
All Urban Households - ATT							
unmatched	-1.69**	-0.79**	-1.47**	-1.45**	-0.62	-1.11**	7.13**
	(0.40)	(0.17)	(0.21)	(0.20)	(0.47)	(0.26)	(0.22)
Observations	11752	11752	11752	11752	11752	11752	11752
matched	-3.23**	-0.58**	-0.95**	-0.82**	1.16*	-1.54**	5.96**
	(0.46)	(0.09)	(0.16)	(0.14)	(0.47)	(0.30)	(0.27)
Observations	4974	4974	4974	4974	4974	4974	4974
Poorest Third of Urban Households - ATT							
unmatched	0.65	-2.19*	-3.28**	-1.73	2.13	0.58	3.84**
	(1.65)	(1.04)	(1.11)	(1.08)	(1.95)	(1.08)	(0.87)
Observations	3916	3916	3916	3916	3916	3916	3916
matched	-0.53	-2.86*	-4.18*	-1.05	3.15	0.29	5.18**
	(2.08)	(1.20)	(1.84)	(1.61)	(2.63)	(1.57)	(1.42)
Observations	124	124	124	124	124	124	124
Middle Third of Urban Households - ATT							
unmatched	-3.10**	-1.31**	-0.72	-1.87**	-0.80	-0.25	8.05**
	(0.73)	(0.29)	(0.43)	(0.41)	(0.96)	(0.49)	(0.42)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	-3.68**	-0.53	0.42	-1.20*	-3.52**	0.10	8.41**
	(0.92)	(0.31)	(0.57)	(0.52)	(1.24)	(0.60)	(0.73)
Observations	674	674	674	674	674	674	674
Richest Third of Urban Households - ATT							
unmatched	-3.17**	-0.49**	-1.15**	-0.93**	1.24*	-1.93**	6.43**
	(0.56)	(0.11)	(0.19)	(0.18)	(0.57)	(0.37)	(0.32)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	-3.51**	-0.29**	-1.24**	-0.83**	2.10**	-2.04**	5.80**
	(0.52)	(0.08)	(0.16)	(0.15)	(0.50)	(0.34)	(0.29)
Observations	4176	4176	4176	4176	4176	4176	4176
Middle Third of Urban Households - ATUT							
unmatched	3.10**	1.31**	0.72	1.87**	0.80	0.25	-8.05**
	(0.73)	(0.29)	(0.43)	(0.41)	(0.96)	(0.49)	(0.42)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	3.00**	1.09**	0.37*	1.43**	1.90**	-0.14	-7.65**
	(0.27)	(0.11)	(0.17)	(0.15)	(0.36)	(0.20)	(0.20)
Observations	7162	7162	7162	7162	7162	7162	7162
Richest Third of Urban Households - ATUT							
unmatched	3.17**	0.49**	1.15**	0.93**	-1.24*	1.93**	-6.43**
	(0.56)	(0.11)	(0.19)	(0.18)	(0.57)	(0.37)	(0.32)
Observations	3918	3918	3918	3918	3918	3918	3918
matched	3.68**	0.34**	1.00**	0.76**	-0.14	1.41**	-7.06**
	(0.52)	(0.11)	(0.19)	(0.18)	(0.55)	(0.35)	(0.32)
Observations	3660	3660	3660	3660	3660	3660	3660

Standard errors of the estimated treatment effect in parentheses
** p<0.01, * p<0.05

TABLE A-1. Rural System Regression Covariates - Before and After Match

VARIABLES	male	age	age2	nchild1	nchild2	nchild3	wr	sr	childdr	ageddr	white	coloured	lnx	lnx2
All Rural Households - ATT														
unmatched	0.09	1.51	103.56	-0.11	-0.16	-0.10	0.02	0.01	-0.15	0.01	0.24	-0.00	1.55	30.82
	(0.03)	(0.73)	(72.76)	(0.04)	(0.04)	(0.04)	(0.02)	(0.01)	(0.04)	(0.01)	(0.01)	(0.02)	(0.04)	(0.82)
Observations	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084	6084
matched	-0.04	0.24	21.15	0.03	0.05	-0.03	0.02	0.01	0.07	0.01	0.01	0.02	0.02	0.46
	(0.03)	(0.92)	(93.47)	(0.05)	(0.05)	(0.06)	(0.02)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)	(0.06)	(1.33)
Observations	782	782	782	782	782	782	782	782	782	782	782	782	782	782
Poorest 85% of Rural Households - ATT														
unmatched	0.07	1.24	70.09	-0.12	-0.07	0.08	-0.03	0.02	-0.13	-0.02	0.01	-0.00	0.62	11.31
	(0.05)	(1.51)	(148.95)	(0.08)	(0.09)	(0.09)	(0.03)	(0.03)	(0.08)	(0.02)	(0.01)	(0.03)	(0.07)	(1.24)
Observations	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172	5172
matched	0.09	-1.66	-195.60	0.01	0.11	0.15	-0.00	0.03	0.09	-0.00	-0.01	-0.02	0.01	0.04
	(0.07)	(2.00)	(210.12)	(0.10)	(0.11)	(0.12)	(0.04)	(0.04)	(0.08)	(0.02)	(0.02)	(0.05)	(0.08)	(1.39)
Observations	178	178	178	178	178	178	178	178	178	178	178	178	178	178
Richest 15% of Rural Households - ATT														
unmatched	-0.01	-1.20	-127.71	-0.15	-0.21	-0.22	0.06	-0.01	-0.08	0.00	0.19	-0.01	0.40	8.88
	(0.03)	(0.93)	(96.31)	(0.05)	(0.05)	(0.06)	(0.02)	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.04)	(0.93)
Observations	912	912	912	912	912	912	912	912	912	912	912	912	912	912
matched	-0.13	-0.28	-25.57	0.12	-0.03	0.07	0.03	-0.03	0.10	0.00	-0.06	0.03	0.04	0.98
	(0.03)	(1.05)	(107.76)	(0.05)	(0.05)	(0.06)	(0.02)	(0.02)	(0.05)	(0.02)	(0.04)	(0.02)	(0.05)	(1.21)
Observations	604	604	604	604	604	604	604	604	604	604	604	604	604	604
Richest 15% of Rural Households - ATUT														
unmatched	0.01	1.20	127.71	0.15	0.21	0.22	-0.06	0.01	0.08	-0.00	-0.19	0.01	-0.40	-8.88
	(0.03)	(0.93)	(96.31)	(0.05)	(0.05)	(0.06)	(0.02)	(0.02)	(0.05)	(0.02)	(0.03)	(0.02)	(0.04)	(0.93)
Observations	912	912	912	912	912	912	912	912	912	912	912	912	912	912
matched	0.10	2.06	234.60	-0.02	-0.04	-0.03	-0.03	0.01	-0.24	0.00	-0.05	0.01	-0.01	-0.17
	(0.03)	(0.72)	(74.37)	(0.04)	(0.05)	(0.05)	(0.02)	(0.01)	(0.06)	(0.01)	(0.02)	(0.02)	(0.03)	(0.64)
Observations	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220	1220

Standard errors of the mean difference in parentheses

TABLE A-2. Urban System Regression Covariates - Before and After Match

VARIABLES	male	age	age2	nchild1	nchild2	nchild3	wr	sr	childdr	ageddr	white	coloured	lnx	lnx2
All Urban Households - ATT														
unmatched	0.12	-0.24	-54.15	-0.03	0.01	0.02	0.04	-0.02	0.04	0.01	0.32	0.06	1.39	28.56
	(0.01)	(0.28)	(26.75)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.02)	(0.38)
Observations	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754	11754
matched	0.01	-1.32	-117.40	0.04	0.03	0.02	0.04	-0.01	0.06	-0.00	-0.01	0.01	-0.01	-0.23
	(0.01)	(0.32)	(30.55)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.48)
Observations	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974	4974
Poorest Third of Urban Households - ATT														
unmatched	0.24	-1.42	-100.35	-0.15	-0.15	-0.10	0.07	0.18	-0.19	0.03	0.01	0.12	0.28	4.77
	(0.06)	(1.59)	(145.33)	(0.07)	(0.07)	(0.08)	(0.04)	(0.04)	(0.08)	(0.02)	(0.01)	(0.04)	(0.07)	(1.18)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.27	-4.87	-401.13	0.00	0.08	0.02	-0.01	0.22	-0.01	0.01	0.02	0.08	-0.02	-0.31
	(0.08)	(2.31)	(222.63)	(0.07)	(0.06)	(0.09)	(0.05)	(0.06)	(0.07)	(0.04)	(0.02)	(0.07)	(0.06)	(1.14)
Observations	124	124	124	124	124	124	124	124	124	124	124	124	124	124
Middle Third of Urban Households - ATT														
unmatched	-0.03	-1.77	-188.80	-0.11	-0.07	-0.05	-0.00	-0.04	-0.05	-0.02	0.06	0.05	0.12	2.41
	(0.03)	(0.73)	(71.79)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.26)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.02	-1.47	-143.23	-0.02	0.03	0.03	0.00	-0.03	0.01	-0.00	0.00	-0.02	-0.00	-0.03
	(0.04)	(0.91)	(86.33)	(0.04)	(0.04)	(0.05)	(0.02)	(0.03)	(0.04)	(0.01)	(0.02)	(0.03)	(0.02)	(0.33)
Observations	674	674	674	674	674	674	674	674	674	674	674	674	674	674
RichestThird of Urban Households - ATT														
unmatched	0.04	-1.70	-173.91	-0.04	-0.01	-0.06	0.06	-0.02	0.03	-0.00	0.22	-0.00	0.39	8.76
	(0.01)	(0.37)	(36.49)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.40)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.04	-1.03	-90.75	0.02	-0.00	-0.01	0.03	-0.01	0.02	-0.01	0.03	-0.00	-0.02	-0.44
	(0.01)	(0.35)	(33.27)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.42)
Observations	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176	4176
Middle Third of Urban Households - ATUT														
unmatched	0.03	1.77	188.80	0.11	0.07	0.05	0.00	0.04	0.05	0.02	-0.06	-0.05	-0.12	-2.41
	(0.03)	(0.73)	(71.79)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.26)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	0.03	0.51	29.96	0.08	0.12	0.00	-0.01	0.09	0.04	-0.03	0.00	-0.02	-0.00	-0.02
	(0.01)	(0.31)	(30.96)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.11)
Observations	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162	7162
RichestThird of Urban Households - ATUT														
unmatched	-0.04	1.70	173.91	0.04	0.01	0.06	-0.06	0.02	-0.03	0.00	-0.22	0.00	-0.39	-8.76
	(0.01)	(0.37)	(36.49)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.40)
Observations	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918	3918
matched	-0.00	1.54	141.49	-0.01	0.02	-0.02	-0.06	0.03	-0.06	-0.01	0.02	0.00	-0.01	-0.23
	(0.01)	(0.40)	(39.34)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.39)
Observations	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660	3660

Standard errors of the mean difference in parentheses

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