Journal of Business Ethics (2020) 164:243–260
https://doi.org/10.1007/s10551-019-04275-9

ORIGINAL PAPER

Addressing the Ethical Challenge of Market Inclusion in Base-of-the-Pyramid Markets: A Macromarketing Approach

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Received: 20 January 2018 / Accepted: 27 August 2019 / Published online: 11 September 2019

Abstract
Making transformative services such as healthcare accessible to low-income consumers is an ethical challenge of vital importance to marketers. However, most low-income consumers across the world are excluded from the market for such transformative services because of financial constraints arising from poverty. In this paper, instead of focusing on the micro-interplay between firms and consumers, we examine the macro-interplay among firms, consumers, and public policy in addressing the ethical challenge of market inclusion at the base of the pyramid. Specifically, we examine how the Vietnam government used a policy of free and universal health insurance for children under the age of six as a means of lowering affordability barriers and fostering market inclusion in the healthcare market. Overnight in 2005, all children under the age of six living anywhere in Vietnam became eligible for free health insurance. Using this policy intervention as a natural experiment, we compare market inclusion outcomes of children under the age of six with older children who were ineligible before and after the program was implemented. We show that lowering affordability barriers through public policy (1) increases access to target services, (2) increases consumers' overall out-of-pocket spending, and (3) increases access to complementary services. By adopting a macromarketing lens, this study makes a strong case for collaboration among firms, governments, and communities in addressing the ethical challenge of system-wide market inclusion in base-of-the-pyramid markets.

Keywords Market inclusion · Base of the pyramid · Subsistence marketplaces · Poverty

Introduction

Providing low-income consumers access to transformative services such as healthcare is an ethical challenge of vital importance to marketers (Blocker and Barrios 2015; Hill and Martin 2014; Viswanathan and Sridharan 2012). However, most consumers at the base of the pyramid are excluded from the market for such transformative services because of financial constraints due to poverty (Hill 2002; Nakata and Weidner 2012; Rosa and Viswanathan 2007). Prior research indicates that many macromarketing factors in these markets, such as economic deprivation and lack of physical infrastructure, create steep barriers for consumers in using and benefiting from transformative services available in the marketplace (Shultz et al. 2012). Owing to these structural barriers, consumer exclusion from the marketplace is the norm at the base of the pyramid (London and Hart 2004; Prahalad 2009). Therefore, consumers must first be brought into the sphere of the marketplace, if marketing firms are to successfully deliver transformative services to them (Sheth 2011).

Prior micromarketing research has examined how individual firms can address the ethical challenge of market inclusion through innovative business processes that lower the structural barriers to participate in the market (Davies and Torrents 2017; Hart 2005; Mason and Chakrabarti 2017; Weidner et al. 2010). For example, researchers have documented how firms can build dynamic capabilities for base-of-the-pyramid markets to cater to unmet needs in a profitable manner (Tashman and Marano 2009). Extant research has also uncovered profound insights into how firms can

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engage ethically with base-of-the-pyramid consumers in a co-creation process (Hahn 2009; London and Hart 2004; Santos and Laczniak 2009). Although these insights cataloged in the micromarketing literature provide valuable guidance for marketing firms aiming to operate viably in base-of-the-pyramid markets, they offer little guidance on how to address the ethical challenge of system-wide market inclusion. Addressing the ethical issue of system-wide market inclusion requires the adoption of a macromarketing perspective that empirically examines how factors such as public policy can bring about large-scale market inclusion (Hill and Martin 2014; Viswanathan et al. 2012). This approach is necessary because systemic ethical challenges such as market inclusion require collaborative efforts on the part of multiple stakeholders embedded within the system.

In this paper, we focus on how macromarketing actors such as governments can employ public policy as a lever to increase country-wide market inclusion by lowering affordability barriers. We examine this question within the context of Vietnam, where poverty is widespread (Shultz 2012). Since the Doi Moi policy of 1986, Vietnam is transitioning toward a market-oriented economy, characterized by an increasing use of the market mechanism to cater to essential needs in society (Shultz 2012). To make healthcare affordable, in 2005, the Vietnam government introduced a free and universal health insurance program targeted at children under the age of six. This program lowered the affordability barriers for households to access healthcare for their children at government healthcare centers and hospitals. Using this program as a natural experiment, we evaluate the impact of the program on market inclusion in the healthcare market by comparing outcomes of these eligible children with older children who were ineligible for the program across two survey periods (2002 and 2008). We measure market inclusion through three indicators: (1) healthcare utilization, (2) healthcare expenditure, and (3) demand for complementary services.

Our analyses reveal that the utilization of health services increased more for the group covered by the policy than for the ineligible group. We also find that out-of-pocket spending on health more than doubled for children who were covered by the program but not for children who were not covered. Furthermore, we find an increase in utilization of complementary healthcare services for the group covered by the policy compared with the group that was not covered. Broadly, our results indicate that government policy interventions that lower nation-wide affordability barriers within certain markets can be crucial in addressing the ethical challenge of market inclusion.

Our infusion of a macromarketing perspective to base-of-the-pyramid scholarship advances the literature in two important ways. First, our study adopts a political economy–oriented macromarketing lens (Dholakia et al. 1983) to demonstrate that the government sector is an important stakeholder in addressing the ethical challenge of market inclusion for transformative services (Arndt 1983). We show that governments can play a catalytic role in bringing about system-wide market inclusion by lowering affordability barriers for transformative services that affect all consumers and firms in the economy. The scope and scale of the impact of such interventions are likely to be far greater than those of the impact that can be achieved through base-of-the-pyramid initiatives on the part of individual firms. Second, a core strength of our study is the provision of causal evidence to test our hypotheses through a quasi-experimental design, carried out with a nationally representative dataset. Specifically, our empirical work demonstrates that the aggregate marketing system can be shaped through public policy to reduce the deprivation of transformative services, such as healthcare, in society. Our evidence-based approach to systemic ethical challenges (e.g., market inclusion) represents an important contribution. Much of the research in base-of-the-pyramid literature has focused predominantly on theoretical advancements, drawing from case studies of successful firms. Herein, we show that the literature would benefit from carrying out empirical tests that provide causal evidence for the theoretical propositions advanced in the literature (Mahoney and Sanchez 2004).

In the following section, we furnish a brief literature review on the macromarketing approach to the base of the pyramid from a political economy standpoint. Then, we turn to deriving the core hypotheses we test in the study. Next, we offer a detailed account of our data source and explain our empirical methodology. Finally, we present the results of our analyses and discuss key implications of our research for the ethical challenge of fostering market inclusion at the base of the pyramid.

Macromarketing Approach to Base of the Pyramid: A Political Economy View

The bulk of extant research in base-of-the-pyramid literature implicitly adopts a microeconomic paradigm to marketing that focuses on the dyadic interaction between firms and consumers in bringing about market inclusion (Arndt 1983; Prahalad 2009). Increasingly, however, the literature has adopted a political economy lens that considers the government a vital actor in the process (Jones et al. 2016; Sun and Im 2015). This lens holds that governments can condition the interaction between firms and consumers in the market by employing public policy as a controlling lever (Arndt 1983). The push toward considering the role of governments in the market inclusion process stems from the realization that welfare goals of societies and commercial interests of marketing firms are not always in alignment (Andreasen 1978; Hill 2008; Schwittay 2011). Micromarketing research on base-of-the-pyramid markets is predicated on the assumption that consumers have the financial wherewithal to participate in

markets for goods and services (Prahalad 2009). However, most market offerings, including microfinance, have failed to reach the poorest sections of society, owing to high affordability barriers (Navajas et al. 2000). This problem of low-affordability levels is a structural feature of the macromarketing context in base-of-the-pyramid markets, and firms have limited ability to affect it directly. Governments can alter affordability levels across markets by offering various types of subsidies. For example, in the past, the government of Nicaragua provided cash incentives to poor households in exchange for regular school attendance of children (Gitter and Barham 2008). In Brazil, the government successfully incentivized parents to invest in the health and education of their children through a conditional cash transfer program called the Bolsa Familia (Shei et al. 2014). Quasi-experimental and experimental studies on the take-up of Medicaid in the United States, which is government supported, also show that it successfully increased access to health services for eligible families (Baicker et al. 2013; Sommers et al. 2012). These examples illustrate that governments have the capacity to channel public funds to lower affordability barriers in certain markets, thereby making products and services in those markets more widely accessible to lower-income populations. In other words, governments can play an important role in fostering market inclusion by bringing into alignment social welfare goals and commercial goals of firms operating in base-of-the-pyramid markets (Klein et al. 2010).

The political economy lens acknowledges that public and private interests are fundamentally intertwined in societies and that policy instruments can have a major impact on market inclusion and consumer welfare (Jones et al. 2016; Shultz et al. 2012). Consequently, the marketing firm is not the sole protagonist in the process of engendering market inclusion; rather, bringing about market inclusion necessitates a relational approach involving multiple stakeholders such as governments, firms, communities, and nonprofit organizations (Ozanne et al. 2017). The collective goal is to employ marketing as a form of constructive engagement in society that fosters inclusive markets and caters to society's essential needs (Santos et al. 2015; Shultz 2007). For example, Barrington et al. (2016) provides grounded insights into how firms, governments, and local communities can work together to provide impoverished communities in Melanesian settlements access to sanitation solutions through marketplace exchange.

While the goal of public policy is always to expand market inclusion and maximize social welfare, it can also have reverse effects. Public policy formulated without regard to ground-level conditions of those living in poverty can create barriers for instead of aiding market inclusion (Viswanathan et al. 2012). Furthermore, scholars have noted that market inclusion need not necessarily bring about transformative outcomes for poor populations (Schwittay 2011). Some scholars have even gone so far as to state that markets could be a site for exploitation of the poor (Araujo 2013; Santos and Laczniak 2009). These caveats notwithstanding, in this paper we operate under the assumption that markets have the potential to deliver well-being and to address some important social problems at the base of the pyramid (Sridharan et al. 2017). Therefore, we view market inclusion of hitherto excluded low-income consumers as an act of social innovation in and of itself (Mair et al. 2012).

Hypotheses Development

In affluent markets, consumers are predominantly faced with the choice of what types of brands or products to buy to meet their essential needs (Sheth 2011). However, in base-of-the-pyramid markets, consumers chiefly grapple with the choice of buying or forgoing consumption altogether (Viswanathan et al. 2009). Households must often make difficult choices such as continuing a child's education or sending a child to work to supplement household income (Venugopal and Viswanathan 2015). Such difficult choices of excluding oneself from the market for essential goods and services are common in domains such as healthcare because of the steep prices involved (Xu et al. 2003). Poor consumers often resort to alternative remedies to alleviate their ailments because they cannot afford to visit public or private clinics or hospitals where they will be required to pay a hefty price for the services (Xu et al. 2003). Not only do the poor suffer from chronic financial constraints, but they also experience periodic financial constraints because of seasonality inherent in their livelihoods (e.g., agriculture) (Venugopal et al. 2015). The superimposition of chronic and periodic constraints further increases the barriers to market inclusion. Low-income households are constantly balancing their meager resources between survival and growth needs (Viswanathan et al. 2014). Oftentimes, this implies that even if households possess some liquid cash, they might choose to forgo consumption of essential services such as healthcare to secure meager financial resources for survival needs. Prior research also finds that these household behavior patterns change significantly when financial constraints are eased, subsequently increasing the demand for capacity-building goods and services such as health (Grossman 1972). Building on this line of reasoning, we hypothesize the following:

H1: The Vietnam government's policy to provide universal health insurance for children under the age of six will lead to a greater increase in utilization of healthcare services for the group covered by the program in comparison to the group not covered by the program.

When price barriers in a product/service market are significantly high, consumers might choose to exclude

themselves completely from the market and refrain from allocating any fraction of their expenditure budget to those products/services (Dupas 2011; Viswanathan et al. 2009). However, when the price barriers are lowered to an affordable level, consumers will consider the product/service in their trade-off calculations and devote a higher fraction of their budget to that product/service. Therefore, counterintuitively, provision of government subsidy in a product/service market will bring about market inclusion and increase the levels of consumer out-of-pocket expenditure on that product/service. This is in line with extant research that has consistently argued that the poor are not unwilling to pay for transformative services, as long as prices are affordable (Prahalad and Hammond 2002). Market inclusion means that the poor can exercise agency in the marketplace by spending their meager cash reserves on well-being-enhancing products and services (Viswanathan et al. 2009). Extending this line of reasoning, we hypothesize the following:

H2: Out-of-pocket spending on healthcare will increase more for the group covered by the government insurance program than for the group not covered by the program.

The benefits of market inclusion often spill over into markets for complementary goods and services (Walters 1991). This is because the marginal value of a service increases with an increase in the amount of complementary service consumed. Therefore, when consumers choose to exclude themselves from the market for a certain product or service, they also, by the same token, choose to exclude themselves from the market for complementary goods and services (Walters 1991). By extension, when new segments of consumers are included in the market for a certain product or service, their demand for complementary goods and services also increases. Drawing on this logic, we hypothesize the following:

H3: There will be a greater increase in the demand for complementary healthcare services for the group covered by the free government insurance program than for the group not covered by the program.

The lowering of affordability barriers in a specific market through public policy is the principal economic mechanism underlying our market inclusion hypotheses, which we measure through three indicators captured in H1, H2, and H3.

Methodology

Quasi-experimental Research Design Strategy

The effectiveness of health insurance as a financial tool to increase access to health services has been widely debated in the literature (Escobar et al. 2011; Hsiao et al. 2006; Hsu 2013). The challenge for impact evaluations using secondary data is to tease out selection effects that bias estimates, as people who opt for health insurance often have poorer health and thus are very different from those who do not enroll. While randomized control trials can solve this methodological problem, to our knowledge, past research has not reported such experiments in developing countries in Southeast Asia. The quasi-experimental methodology thus becomes a useful alternative in this context because it allows researchers to address the problem of selection and to estimate the causal impact of a tool such as health insurance (Angrist and Pischke 2008). In this regard, the Vietnam case study presented herein provides a unique opportunity.

At the turn of the century, the government of Vietnam decided to introduce new health insurance programs and consolidate older health insurance programs to bring a larger section of its population into the health insurance safety net. The motivation for this was to align the country's development outcomes with the Millennium Development Goals for improved child health and universal health coverage (Lieberman and Wagstaff 2009). Prior to 2003, children under the age of six were covered by their parents through privately purchased health insurance. By 2003, children from poor households became eligible for free health insurance through the Health Care Fund for the Poor (HCFP) program. However, after 2005, all children under the age of six, across the country and across social and economic classes, received free Child Health Insurance (CHI). The benefits package of the CHI included coverage of expenses for outpatient visits, inpatient visits, and pre-approved diagnostic services and drugs purchased at government-run health institutions. Health services purchased at private institutions and certain diagnostic services and drugs that were not pre-approved were not covered by the CHI (Lieberman and Wagstaff 2009). Between the 2002 and 2008 surveys, the number of households reporting that their children under the age of six were covered by any health insurance thus increased from 20 to 90%.

In our model, we use variation from the exogenously determined age criteria for eligibility and the survey data timing that coincides with the implementation of the program to develop a difference-in-differences (DID) estimate of the effect of health insurance on market inclusion indicators (Duflo 2001). The quasi-experimental nature of the methodology comes from the assumption that the artificial imposition of the age criteria for eligibility created two (thus, exogenously determined) similar cohorts of children in Vietnam: one comprising children who became eligible simply by virtue of being under the age of six (treated group) and one consisting of children who were older and thus ineligible for coverage (control group). Another source of variation in our analysis comes from the survey timing, which provides

information on our sample before program year 2005 (pre-program period) and data after the year 2005 (post-program period). Because data from the Vietnam Household Living Standard Surveys (VHLSS) are collected as a repeated cross section, and the program itself was intended to be universal in coverage, the estimate calculated through the DID captures the changes in the treated group compared with the control group between the pre- and post-program periods as an intent-to-treat estimate (Angrist and Pischke 2008).

Threats to the Identification Strategy

There are two major caveats to estimating a causal channel while using this type of (quasi-) experiment setting. First, the universal nature of the program and the high take-up prevent us from identifying an experimentally similar group of the same age for comparison. Thus, the next-best solution would be to use information from an older cohort (control) who missed the program because of the age criteria but had similar demand for health to that of the treated group before the implementation. Duflo (2001) uses regional variation in school construction along with age cohort information on compulsory schooling to estimate the impact of an additional year of schooling on labor-market outcomes of people living in Indonesia. Almond and Currie (2011) find that children who were in utero during the influenza pandemic had lower health, education, and labor-market outcomes than older birth cohorts. In our model, to make the treated and control groups as similar to each other as possible, we restrict our sample to children between the ages of three and 12. The treated group of children are between the ages of three and five[1] and the control group of children are between the ages of six and 12.

Second, the main identifying assumption driving the quasi-experiment is that changes in the outcomes of the treated group relative to the control group are driven by changes in insurance status. Any changes in the control group happen independent of changes in the insurance status of the treated group. In our quasi-experiment, the re-organization of the HCFP program in 2003 poses a challenge to this assumption. For example, Wagstaff (2010) shows that by 2006, HCFP households had reduced expenditures on health compared with a group of similar non-HCFP households, even though service use had not changed. To account for this program, we exclude children (treated and control) who may have been eligible for HCFP from our sample. We do this by using information from the literature on the eligibility criteria for the program. Using program information, we are able to estimate a predicted probability of being an HCFP beneficiary. We then exclude children who have a 40% or greater predicted probability of being covered. We choose 40% because it allows us to exclude approximately 20% of the households. The head-count ratio of those living under the $1.25 poverty line was around 20.7% in 2010 (https://data.worldbank.org/country/vietnam, accessed October 17). Table 6 in the Appendix provides a detailed explanation of the methodology. We also run various alternative scenarios on HCFP eligibility selection and show that the results are robust to the criteria we choose.

Data and Estimation Model

For this analysis, we use the VHLSS of 2002 and 2008. Each survey consists of socioeconomic and demographic information and information on health and education outcomes of individuals and their households residing in Vietnam. Each survey year is cross-sectional in nature and representative at the province and national levels. In the health domain, the VHLSS contain information on outpatient and inpatient[2] visits, types of doctors visited (private or public), and out-of-pocket spending by individuals who report being sick before the survey. In our analysis, we use only information on the most recent outpatient or inpatient health visit made by the individual before the survey. This reduces the problem of nonrandom measurement error that may arise from differences in the ability of individuals to recall older visits in ways that are correlated with their socioeconomic status, age, and so forth (Strauss and Thomas 1998). The most recent visit is known to have the least noise in terms of recall bias (Bhandari and Wagner 2006). We calculate out-of-pocket spending per month to increase comparability between outpatient and inpatient estimates. We convert expenditures to real values with a base year of 2008 using the implicit gross domestic product deflator available online at the World Bank Databank (accessed December 2015). In the 2008 survey, details on the different types of insurance an individual has are available, regardless of whether he or she was recorded as ill or not before the survey but not in the 2002 survey.

For the estimating strategy, we use a DID reduced-form model. The estimating equation for the survey year 2008 as the post-program dataset is as follows:

[1] Prior research recognizes that interventions for children between the ages of zero and three have the most impact on long-term adult health outcomes (Almond and Currie 2011).

[2] Outpatient visits are when people visit doctors without admission to hospitals. This includes checkups, taking a test, or buying drugs. Inpatient visits are those in which the patient is put under observation and admitted into the hospital for at least 24 h. Visits are categorized into outpatient and inpatient experiences, and the recall period, on outpatient visits and expenditures, is all visits up to 4 weeks before the survey. For inpatient visits and expenditures, the recall period is 1 year.

$$Y_{it} = \beta_0 + \beta_1 \times T_{it} + \beta_2 \times \text{Post05}_t + \beta_3 \times T_{it} \times \text{Post05}_t + \Sigma\beta^k \times X_{it}^k + \varepsilon_{it}, \quad (1)$$

where *i* represents the individual (child) and t represents the survey year. The variable T_{it} takes the value of 1 if the child is between the ages between three and five, and 0 otherwise. This indicator helps create the experimentally similar group members, some of whom became eligible for the treatment (health insurance), and captures their trajectory in the absence of the intervention. The variable Post05_t captures the changes in the trends between the pre- and post-program periods. This dummy variable takes the value of 1 if the individual is captured in the 2008 dataset and 0 otherwise. The coefficient of interest is β_3, which captures the changes in outcomes of the treatment group between the pre- and post-program period over the change in the control group in the same period. In the absence of the program, the treated and control groups would have continued along the same development trajectory they were on. However, the implementation of the program affects only the trajectory of the treated group (not the control group). The DID estimate thus captures the changes in the trajectory of the treated over the control group due to this intervention. As we assume that age eligibility is the main criteria for insurance take-up and we do not exploit the use of actual insurance usage, our DID estimates capture the intent-to-treat rather than a local average treatment effect of the program (Angrist and Pischke 2008).

Outcome Variables

To measure the program's impact on healthcare utilization (H1), we construct a dummy variable for whether a child had any outpatient or inpatient visits from the data. We run probit models on the incidence of visits and report the log-odds ratio in the regression tables. To test H2, we use ordinary least squares methods to estimate the impact of the program on out-of-pocket spending per month on the visits (in '000 DNG). For the program's impact on access to complementary services (H3), we use data on whether the child visited a government health center or a government hospital during his or her last visit. We use probit regressions to estimate the log-odds ratios of the program's impact on the outcome measures.

Control Variables

In our model, the control variables account for observables that may affect access to health services and, thus, market inclusion. These observables may vary across the program period or between the treatment and control groups. At the individual level, because the child's age and gender may determine their human capital needs, we control for the same in the regressions (Currie 2000; Grossman 1972). Many household-level factors can also affect access. For example, more educated households may know more about child care and thus have greater demand for and be willing to spend more on care for their children. Moreover, being a part of an ethnic minority, having a female head of household, and living in a rural location may affect whether households can access healthcare for their children. To control for these observables, we include controls such as education of the highest educated men and women, gender of the head of the household, the ethnicity of the household, and whether the household resides in an urban area (Currie and Almond 2011; Bairagi 1980; Currie 2000; Currie and Madrian 1999; Deaton 2007; Nguyen and Knowles 2010; Thomas 1994; Thomas et al. 1991). At a broader level, some provincial governments may have greater political autonomy to invest in the quality of government health institutions in their respective states depending on their financial capacity and power (Currie 2000; Viner et al. 2012). To control for such time-invariant institutional factors, we include province-level fixed effects in the regressions. Our regressions are clustered at the level of the province to account for any heterogeneity within provinces, and thus the standard errors presented in the tables are robust.

Results

In Table 1, we present the summary statistics that compare the treated children with the control children in the pre-program period. Here, we show that the treated group is 4 years

Table 1 Pre-program period summary statistics for controls

	(1) All	(2) 3 to 5 years	(3) 6 to 12 years
Female	0.48 (0.50)	0.45 (0.50)	0.48 (0.50)
Age (years)	8.01 (2.87)	4.10 (0.81)	9.25 (1.98)
Female head of household	0.19 (0.39)	0.20 (0.40)	0.18** (0.38)
Father's educ (years)	8.21 (3.31)	7.97 (3.26)	8.06 (3.34)
Mother's educ (years)	7.82 (3.17)	7.76 (2.99)	7.70 (3.16)
Household size	4.90 (1.41)	4.83 (1.54)	5.00** (1.43)
Urban	0.27 (0.44)	0.24 (0.43)	0.24 (0.43)
Ethnic majority labels ("Observations")	0.50 (0.50) 37,029	0.95 (0.22) 4076	0.95 (0.22) 14,437

Columns 2 and 3 contain pre-program period summary statistics

*0.10, **0.05, ***0.01

Table 2 Summary statistics for outcomes

	(1) All	(2) Pre-Treat	(3) Post-Treat	(4) Pre-Control	(5) Post-Control	(6) DID
Prob of visit	0.26 (0.44)	0.19 (0.40)	0.48 (0.50)	0.14 (0.34)	0.32 (0.47)	0.11
Total visits	0.31 (0.59)	0.25 (0.57)	0.61 (0.73)	0.16 (0.45)	0.38 (0.60)	0.18
Govt. HC	0.07 (0.26)	0.05 (0.23)	0.16 (0.36)	0.03 (0.18)	0.09 (0.29)	0.05
Govt. hosp	0.09 (0.28)	0.06 (0.24)	0.16 (0.37)	0.05 (0.22)	0.11 (0.31)	0.06
Tot health	68.57 (346.28)	56.83 (249.06)	139.13 (491.71)	40.13 (299.19)	76.66 (350.03)	45.77
N	37,029	4076	4939	14,437	13,577	

Numbers in parentheses represent standard deviation of the group

Pre pre-program, *Post* post-program, *Treat* treated group, *Control* control group, *DID* difference in difference

of age and the control group is approximately 9 years on average. This difference is to be expected, as the groups are defined by their age eligibility. The treated child was more likely to come from a household headed by a woman and lived in a smaller household in 2002. The groups are balanced on other control variables in the data.

Table 2 provides summary statistics of the outcome variables. In the pre-program period, younger children (Pre_treat) are more likely to be taken to doctors, and parents tend to spend more on their health than those in the control group. This is also to be expected, as younger children tend to be more ill than older children. However, in the post-program period, although spending on older children is increasing (Post_Control), it increases more for the children who were eligible for the CHI program (Post_Treat). The simple DID measure, which shows how the variables changed between the treated and control groups over both survey periods, reveals this to be true for all the outcome variables.

Figure 1 depicts group mean values across various conditions. In Table 3, we provide results for H1. Columns 1 and 3 provide results from the simple DID estimates without any controls. Columns 2 and 4 provide the results after the addition of controls and includes province fixed effects. Columns 1 and 2 capture the log-odds ratio of a child visiting the doctor. The coefficient Treated × Post05 captures our preferred coefficient, β_3. In support of the first hypothesis (H1), this robust coefficient shows that in the post-program period, the predicted probability of a child covered by health insurance is higher than the control group. Column 4 shows that the total visits of a treated child increased by more than 90% over the control group.

Table 4 shows that with greater access to health services, parents also spent more out of pocket on their children who were covered by the program, corroborating H2. Column 2 reveals an increase in out-of-pocket spending by approximately 2 USD per treated child per month, which is double that for a child who was not covered during the same period. This increase of 2 USD made up less than 1% of the mean out-of-pocket spending made on a younger child (whose parents reported spending out of pocket on his or her health) in 2002. Preliminary analysis finds that out-of-pocket spending was mostly for outpatient services.[3] Typically, these types of services require the purchase of drugs or diagnostics and other forms of complementary care that may not have been covered in the benefit package. Thus, parents who entered the market for healthcare would have had to pay for these services out of pocket.

In Table 5, we provide evidence for H3. Columns 2, 4, and 6 include controls and are the preferred specifications. The coefficient on the type of visit (Treated × Post05) reveals that children were more likely to be taken to government health centers that were covered by the health insurance. Parents also took children to more complementary health service providers at government hospitals, which required referrals from the health center. Thus, this is in line with the hypothesis H3.

Robustness Checks

A major condition for the DID estimate to be causal is that in the pre-program period, the treated and control groups would need to show no differences in the trends in their outcomes (Angrist and Pischke 2008). As we cannot test parallel trends with only one data point in the pre-program period, we run two types of checks. First, we show that

[3] We also found that average spending on health for the treated child decreased from 1% to approximately 0.06% of household income between 2002 and 2008.

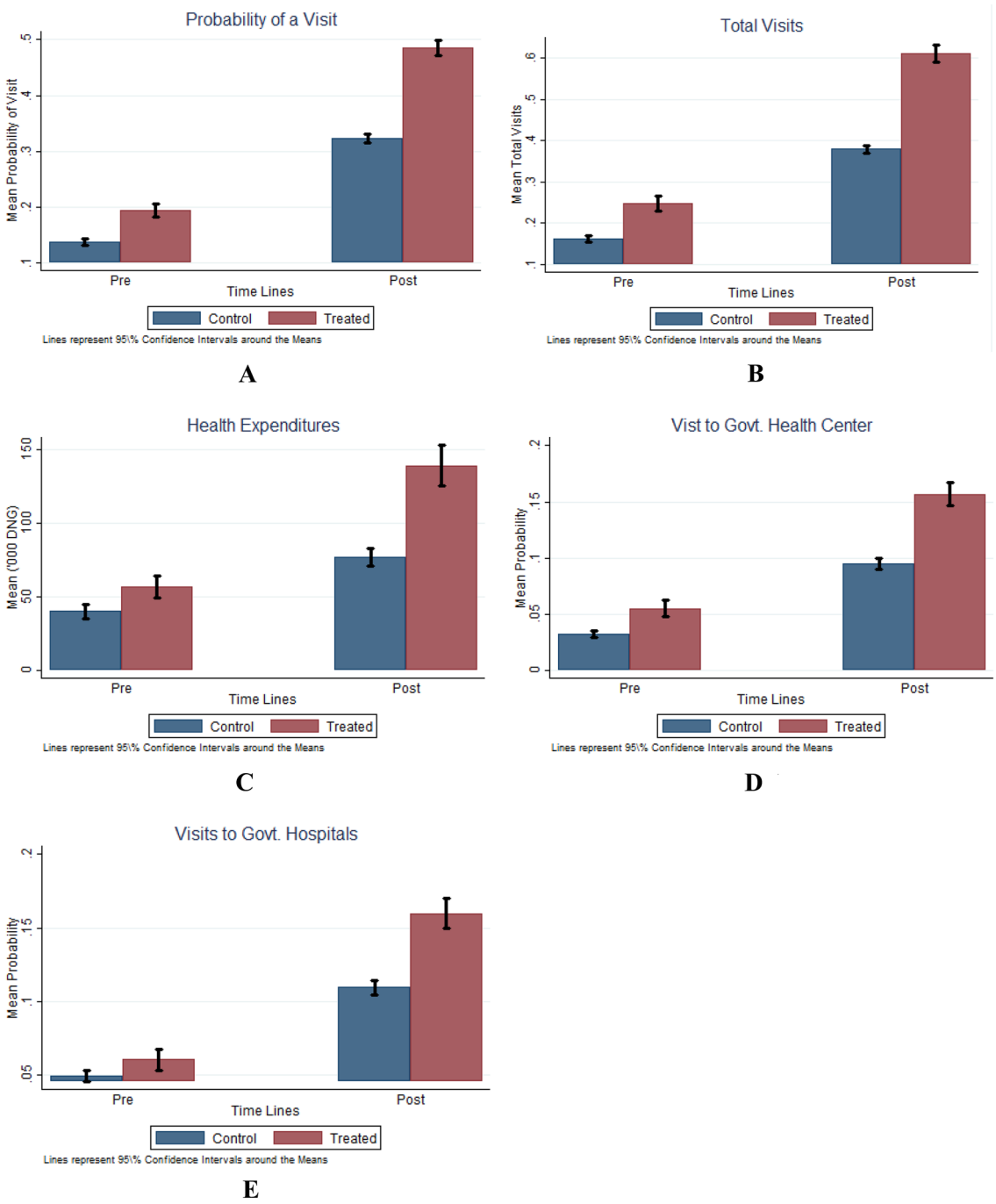

Fig. 1 Pictorial depiction of group mean values across conditions.**a** Probability of any visit. **b** total visits. **c** Out-of-pocket spending ('000 DNG) **d** Visit to government health center. **e** Visit to government hospital

before the implementation of the program, the outcomes of the treated and control groups were statistically similar in most cases. Table 7 in the Appendix shows that the main differences between the groups in the pre-program period are that the treated group has slightly more visits to government health centers and parents spend more on their children's health (see "Only 2002 data"). However, this result can be explained by the fact that younger children are generally more likely to be ill and thus need to visit the doctor more. In the absence of health insurance, it is to be expected that parents would need to spend more out of pocket during these visits to the clinics. This difference does not have a direct bearing on our hypotheses though. Our primary concern is with the comparability of the increase in dependent variables between 2002 and 2008 for the treatment and control groups. Second, a somewhat restrictive test would be to test

Table 3 Total visits

	(1) Any Visit	(2) Any Visit	(3) Tot Visits	(4) Tot Visits
Treated	0.23***	−0.05	0.09***	−0.02
	(0.03)	(0.04)	(0.01)	(0.01)
Post2005	0.64***	0.64***	0.22***	0.20***
	(0.04)	(0.05)	(0.02)	(0.02)
Treated × Post05	0.19***	0.22***	0.15***	0.15***
	(0.03)	(0.04)	(0.01)	(0.01)
Female		−0.04***		−0.02***
		(0.01)		(0.00)
Age (years)		−0.05***		−0.02***
		(0.00)		(0.00)
Female HoH		−0.00		−0.00
		(0.02)		(0.01)
Father's educ (years)		0.01**		0.00
		(0.00)		(0.00)
Mother's educ (years)		0.01**		0.00**
		(0.00)		(0.00)
Household size		−0.09***		−0.03***
		(0.01)		(0.00)
Urban		0.01		0.01
		(0.04)		(0.01)
Ethnic majority		0.02		0.01
		(0.04)		(0.02)
R^2	0.07[P]	0.12[P]	0.07	0.12
Obs.	37,099	37,099	37,099	37,099
Sample mean	0.14	0.14	0.16	0.16
Province FE		Y		Y
Cluster	Y	Y	Y	Y

Columns1 and 2: probit estimates; Columns 3 and 4: ordinary least squares. *P* pseudo-R^2, *HoH* head of household

*0.10, **0.05, ***0.01

Table 4 Out-of-pocket spending

	(1) OOPs ('000DNG)	(2) OOPs ('000DNG)
Treated	16.76***	−3.05
	(5.50)	(5.57)
Post2005	37.01***	17.40***
	(4.99)	(4.82)
Treated × Post05	46.58***	45.17***
	(10.23)	(10.16)
Female		−10.60***
		(3.10)
Age (years)		−3.28***
		(1.10)
Female head of household		2.47
		(6.39)
Father's educ (years)		2.70***
		(0.89)
Mother's educ (years)		3.65***
		(0.95)
Household size		−11.51***
		(2.15)
Urban		14.96**
		(5.67)
Ethnic majority		−12.49***
		(4.19)
R^2	0.008	0.021
Obs.	37,099	37,099
Sample mean	39.79	39.79
Province FE		Y
Cluster	Y	Y

Ordinary least squares estimates, *OOPs* out-of-pocket spending; *15,000DNG* 1USD

*0.10, **0.05, ***0.01

the assumption of parallel trends in the control group during the same period. Here, we randomly assign treatment to children aged from 9- to 12-year-olds and compare outcomes with an older group aged from 13- to 16-year-olds.[4] If the program indeed only affected the trajectory of the treated group, there should be no changes in the outcomes for the falsely treated groups during this period. We find in the falsification test that this is the case for all the coefficients except the coefficient on total visits. The coefficient here seems to suggest that although the probability of visits does not change between the groups, overall health visits increase. This result suggests that the actual coefficient of interest in Table 3 is a lower-bound estimate of the changes in total visits occurring during the same period for the treated group and the control group.

[4] Here, we need to exclude the group aged from 6- to 8-year-olds because we know that they received some treatment in the pre-program period, and their trajectories were different in the post-program period.

By 2008, children between the ages of six and eight, who were between three and five in 2005, had been eligible for the program for at least 1 year. As it is possible that the program changed their health trajectory, this may have affected the control group. On the one hand, if the treatment reduces the need to access healthcare for the control group, our estimates of the program's impact may be biased upward. On the other hand, if after accounting for the treatment of this partially treated group we find that the original estimates are biased downward, we can be confident in our original estimates. We conduct two separate robustness checks to test for this. First, we exclude children between these ages from the sample and estimate our results (see Table 8 in the Appendix: Without the group aged from 6- to 8-year-olds). Second, we parse these partially treated children into a separate group and identify the trends in their outcomes relative to those in the control group. 9- to 12-year-old children were ineligible for the program in 2005 but are part of our control group in the latter specification (see Table 8: Dummy for past treated). In both the checks in Table 8, we find that

Table 5 Type of places accessed

	(1) HC	(2) HC	(3) Govt	(4) Govt
Treated	0.25***	0.09**	0.10**	−0.06
	(0.03)	(0.04)	(0.04)	(0.05)
Post2005	0.53***	0.69***	0.43***	0.35***
	(0.05)	(0.06)	(0.03)	(0.05)
Treated × Post05	0.05	0.10**	0.13***	0.13***
	(0.05)	(0.05)	(0.05)	(0.05)
Female		−0.02		−0.08***
		(0.02)		(0.02)
Age (years)		−0.03***		−0.03***
		(0.01)		(0.01)
Female HoH		0.01		−0.01
		(0.03)		(0.03)
Father's educ (years)		−0.00		0.01*
		(0.00)		(0.00)
Mother's educ (years)		−0.02***		0.01***
		(0.00)		(0.00)
Household size		−0.04***		−0.07***
		(0.01)		(0.01)
Urban		−0.52***		0.12***
		(0.05)		(0.03)
Ethnic majority		0.15**		−0.06
		(0.06)		(0.05)
Pseudo-R^2	0.047	0.117	0.032	0.059
Obs.	37,099	37,099	37,099	37,099
Sample mean	0.03	0.03	0.05	0.05
Province FE		Y		Y
Cluster Y	Y	Y	Y	Y

Probit estimates; *HC* visit to government health center, *Govt* visit to government hospital, *HoH* head of household

*0.10, **0.05, ***0.01

coefficients increase slightly for the treated group, indicating that our results in the main specification are at best underestimates of the program's effects.

Next, we consider other possible alternative exogenous criteria for choosing a non-HCFP household as a way to estimate our outcomes (see Table 9 in the Appendix). First, we estimate the regressions using the criteria that the highest educated man in the household in which the child belonged had to have at least the median level of education (8 years). This criterion is a useful method to gauge the income levels of households and, thus, (in)eligibility to the HCFP program. Second, we estimate the regressions only for those whose income quintiles were greater than or equal to the mean (third quintile). Again, these criteria would mean that richer households would have been less likely to be a part of the HCFP program, and the regressions would only include children who were less likely to be covered by the HCFP. In both cases, we find that our estimates are robust.

Finally, we add additional robustness checks to try to eliminate other competing hypotheses. First, if the aforementioned theorized mechanism is indeed the driver of the focal phenomenon, we should only observe market inclusion with regard to healthcare firms that are covered by the public policy. In our empirical context, this means that market inclusion will only be facilitated by government healthcare firms. More concretely, we should not observe market inclusion with regard to private healthcare firms, which provide similar services but do not fall under the purview of the new public policy. Table 10 in the Appendix shows that access to private health centers did not change during this time. Second, we construct a new indicator that captures multiple usages of the health service providers. Using a multinomial logit model, in Table 11, we show that the relative probability of accessing government health centers (32%) and hospitals (51%) increases more than the relative probability of accessing private health centers (24%) for those who do access health service providers. We also show that the relative probability of accessing more than one center, especially the government health centers and private health centers, increases more than for all other groups from the multinomial logit. Third, we find that total health visits and health expenditures are robust to a log specification (Table 12, columns 1 and 2). The coefficient of interest is positive and significant, showing an increase in the mean of the distribution in these variables. This also shows that our estimates are not sensitive to skewness in distribution of outcomes or the presence of outliers in the data. Fourth, among those who reported having any visits between the two program years, we find that health spending per visit does not change. Although overall spending increases over that in the pre-program period, the coefficient suggests that the government intervention was successful in moving the needle on access as defined by greater visits to doctors, though for those who spend on their health, the average spending did not change (Table 12, columns 3 and 4). These results further strengthen our claim that the program was successful in increasing market inclusion.

Implications

A preponderance of marketing research is carried out in Western-affluent contexts (Dholakia et al. 1980), due to which the influence of the macromarketing context remains largely implicit in theorizing (Dholakia et al. 1980; Venugopal and Viswanathan 2017). Moreover, micromarketing theories evolving from Western-affluent contexts tacitly assume that most consumers in society have the financial wherewithal to participate in markets. These theories possess limited efficacy in capturing lived experiences in base-of-the-pyramid markets, in which exclusion from the marketplace for transformative services represents a pressing ethical challenge. Macromarketing scholars have long

understood this and systematically examined how diverse institutional and political macro contexts affect marketing exchanges (Campana et al. 2017; Godinho et al. 2017; Shultz 1997). For example, Shultz (2012) unpacks how unique macromarketing factors in Vietnam shape various facets of consumption and marketing. This type of holistic perspective allows for a panoramic view of the phenomenon (Layton 2007) and allows scholars to appreciate the complex interdependencies within systems (Dholakia 2012), which a micromarketing perspective cannot afford. Furthermore, this lens expands the focus of scholars from an exclusive emphasis on the firm–consumer dyad to a broader array of stakeholders in the marketplace that can influence the nature of these relationships (Laczniak and Murphy 2012). Thus, use of a macromarketing approach for addressing the ethical challenges of market inclusion makes way for possible solutions that involve relational engagement of new stakeholders, such as policy makers, local communities, and grassroots nongovernmental organizations (Ozanne and Anderson 2010).

Our study adopts such a macromarketing focus on base-of-the-pyramid markets, thereby offering new insights that can address important ethical challenges in these markets. First, our study brings into sharp focus the need for multilevel theorizing. That is, we are able to test the notion that micromarketing interactions between firms and consumers are fundamentally determined by their context and thus shaped by macromarketing factors (Dholakia et al. 1983). We demonstrate that a macromarketing tool such as public policy can play a crucial role in addressing the ethical challenge of market inclusion into the base-of-the-pyramid markets by removing affordability constraints for consumers and allowing them to engage in markets. These actions then enable firms to reach and serve a larger fraction of consumers in society with transformative services that were previously inaccessible. Much of the micromarketing literature is too narrowly focused on marginal changes for currently available consumers and thus may miss the importance of bringing more consumers into markets. Second, in the absence of a large-scale economy-wide experiment that measures the systemic factors determining the level of access to markets, we are able to test our hypotheses using an economy-wide representative dataset through a unique quasi-experiment. Our natural experiment methodology provides a way to tease out the causal impact of a macromarketing factor (public policy) on the ethical challenge of increasing market inclusion. This methodology clearly shows that government policy can enable greater market inclusion for consumers, thus reiterating both the general insights into the role of macromarketing factors in the relationship of stakeholders and the causal evidence of how these factors can influence market inclusion. The general insights that flows from our findings is that scholarship focused on addressing ethical challenges in the base-of-the-pyramid markets stands to benefit immensely from the use of multilevel frameworks when exploring how stakeholders, like governments, can alter the macromarketing context within which firms and consumers operate (Lindeman 2014).

Limitations and Future Research

We investigate how public policy can act as a lever to foster market inclusion for transformative services such as healthcare in base-of-the-pyramid markets. However, we do not document the downstream effects of access to transformative services on health indicators because of the lack of data availability in the VHLSS. Prior research from developed countries indicates that access to health insurance (Medicaid) for those who are poor could improve health outcomes (Baicker et al. 2013; Sommers et al. 2012). Exploring the relationship between insurance access and long-term health outcomes for individuals in a developing country can shed light on the health benefits of market inclusion in addition to the private benefits that have been explored in this paper.

There is also tremendous scope for future research to study and document the downstream impact of market inclusion on important indicators such as subjective well-being, financial security of households and food security of households. Poor households can benefit from purchasing health insurance if they are able to redirect indirect gains from time savings from better health and the associated lower health spending toward greater market engagement in welfare enhancing activities. In addition, healthier children may be more likely to stay in school and complete their education and hence earn higher wages, thus increasing welfare in the longer term as well. Finally, there may be spillover effects that impact other members within households in the short term. Altruistic households may be able to reinvest savings from health gains (from covered children) toward other deserving family members thus increasing overall welfare of all its members. Indeed, market inclusion should be considered an important policy prescription only in cases when it directly leads to improvements in the well-being of individual consumers and society at large both in the short term and the long term.

In this paper, we focused on lowering economic barriers to market inclusion through public policy interventions. It should be noted, however, that other barriers to market inclusion exist. For example, social barriers to market inclusion may prevent socially marginalized consumers from benefiting from transformative services available in the marketplace (Scaraboto and Fischer 2012; Vikas et al. 2015). In the same vein, literacy barriers may prevent consumers with low literacy from accessing services in the marketplaces (Adkins and Ozanne 2005). Future research should thus identify alternative sources of barriers to market inclusion and devise interventions to lower these systematically.

Finally, prior research indicates that the quality of market-based services available in low-income contexts is drastically lower than that in more affluent contexts (Gulyani and Talukdar 2008). In such cases, reducing affordability barriers may bring more people to markets but their overall welfare may not improve. Future research on market inclusion in base-of-the-pyramid contexts should not only focus on access to market-based services but also examine the challenges that impact the delivery of quality services.

Conclusions

The base-of-the-pyramid literature has largely settled into a micromarketing focus, emphasizing the dyadic interaction between the firm and consumers in small regional markets (Jha et al. 2016; Venugopal and Viswanathan 2015). Firms and consumers are often thought to be key stakeholders in enabling market inclusion, especially when systemic forces prevent consumers from accessing transformative services. In this paper, we infuse a macromarketing perspective into the literature in recognition that interactions between firms and consumers are embedded within and conditioned by the macro context (Layton 2007). We show that governments have the power to alter this context with the tool of policy, lowering affordability barriers and increasing market inclusion for those previously constrained by systemic forces characteristic of bottom-of-the-pyramid markets. Thus, we show that governments can become key stakeholders in addressing the ethical challenge of market inclusion.

To empirically tease out the impact of possible government actions on market inclusion, we exploit the variation introduced by a public policy that made health insurance free for children under 6 years of age in Vietnam in 2005. By exploiting the age eligibility criteria of the program and the survey timing of the VHLSS, we are able to show that, first, policy can play an important role in improving access to markets, especially when affordability considerations create large barriers to market access. We find that parents are more likely to take their children to the doctor and that visits for eligible children increase significantly over the ineligible group. Second, we show that policy, when appropriately designed, not only allows the consumers to engage in the market but also increases spending on transformative services such as health. We find that parents increase spending on children who are eligible than on children who are not. Third, we show that not only do consumers visit doctors more, but they also access complementary services, such as referral care. Parents are more likely to take their children to see doctors in government hospitals, access to which requires visits to and referrals from the local health center. Thus, overall, we show that by reducing the entry barrier for participation, a policy tool such as health insurance can increase access to transformative services at the economy level.

Macromarketing scholars have tended to view the aggregate marketing system as society's provisioning system and have shown a profound interest in addressing the ethical challenge of market inclusion in base-of-the-pyramid markets (Fisk 1981; Viswanathan et al. 2009). Fostering inclusive markets represents the ultimate ethical challenge for marketers (Hart 2005) because the aggregate marketing system is a principal vehicle through which essential needs such as food, nutrition, finance, healthcare, and education are met in society. The ability to access these essential products and services holds the potential to enhance well-being of billions of low-income individuals globally (Viswanathan and Sridharan 2009). In this empirical work, we demonstrate that the aggregate marketing system can be suitably shaped through public policy to bring about freedom from deprivation of transformative services, such as healthcare, in society. Such a systemic perspective advances fresh insight that has the potential to galvanize new avenues of research on base-of-the-pyramid markets.

Funding No funding was received for this project.

Compliance with Ethical Standards

Conflict of interest The authors have no conflicts of interest to disclose.

Ethical Approval The paper employs secondary data gathered from human subjects by General Statistics Office, Government of Vietnam. The dataset preserves participant confidentiality by eliminating all identifying information.

Informed Consent For this type of secondary data analysis, formal consent is not required.

Appendix

Selection Criteria for HCFP Households

To determine the selecting households to exclude, we used the following estimation strategy:

$$Y_{ht} = \beta_0 + \beta_k \times X_k + E_{ht}, \quad (2)$$

Table 6 Household selection criteria

	(1) HCFP 1 (b/se)	(2) HCFP 2 (b/se)
Female household	0.092** (0.0377)***	−0.044 (0.0304)
Higher educated male (years)	−0.063*** (0.0046)	−0.082*** (0.0034)
Higher educated female (years)	−0.052*** (0.0056)	−0.079*** (0.0041)
Household size	0.095*** (0.0096)	0.167*** (0.0072)
Urban	−0.237*** (0.0743)	−0.530*** (0.0424)
Ethnic majority	0.839*** (0.1006)	0.005 (0.0697)
R^2	0.458	0.422
Obs.	36,734	36,734
Province FE	Y	Y
Cluster	Y	Y

Probit Estimates, *HCFP* heath care fund for the poor

*0.10, **0.05, ***0.01

where h is the household with children between the ages of one and 12 and t is the survey year (2002 and 2008). The outcome variable Y_{ht} is a binary variable that takes the value 1 (0 otherwise) if the households were reported as being a part of the bottom-income quintile in their respective provinces or if households belonged to the provinces of Thai Nguyen, Cao Bang, Bac Kan, Lao Kai, Ha Giang, Son La, Lai Chau, Dien Bien, Son La, Hoa Binh, Kon Tum, and Soc Trang (where more than 50% of the communes were selected for the 135 programs of the Vietnam government). The controls (X_k) included gender of head of household, education of the highest educated man and woman in the household, household size, whether the household is urban, and survey year, and province fixed effects. The regression is clustered at the level of the province and run as a probit model.

We use the results from the probit regression to estimate a predicted probability of being covered by the HCFP program after conditioning on covariates. In the main regression results, we consider children whose households received the cutoff of less than 40%. In Table 6, we provide the marginal effects of two models and their predicted probabilities to show how the chosen model works relative to others.

Table 7 Testing for parallel trends

	(1) Any Vst	(2) Tot Vst	(3) G.HC Vst	(4) G.Hosp Vst	(5) OOPs ('000DNG)
Main specification					
Treated × Post05	0.21*** (0.04)	0.15*** (0.01)	0.10** (0.05)	0.13*** (0.05)	45.17*** (10.16)
R^2	0.12p	0.12	0.12p	0.06p	0.02
Obs.	37,099	37,099	37,099	37,099	37,099
Only 2002 data					
Treated	0.04 (0.04)	0.03** (0.01)	0.10** (0.05)	0.01 (0.06)	8.84* (5.03)
R^2	0.12[p]	0.05	0.12[p]	0.06[p]	0.02
Obs.	18,513	18,513	18,513	18,513	18,513
Falsification test					
False treated*Post05	0.04 (0.03)	0.04*** (0.01)	0.06 (0.05)	0.01 (0.04)	−6.32 (7.09)
R^2	0.08[p]	0.07	0.10[p]	0.04[p]	0.01
Obs.	40,666	40,666	40,666	40,666	40,666
Controls	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y
Cluster	Y	Y	Y	Y	Y

Columns 1, 3, 4, 5 = probit estimates. Columns 2 and 6 = ordinary least squares estimates. Numbers in parentheses are robust standard errors Only the coefficients of interest in the regressions has been presented, Controls include child controls—treatment status, post dummy, female dummy, age. Household controls—gender, education levels, urban, household size

Vst visit, *G* government, *HC* health center, *Hosp* hospital, *OOPs* out-of-pocket spending, *15,000DNG* 1USD. False treated refers to children who are between the ages of 9 and 12 who have been assigned to treatment. In the falsification test, sample includes children between 9 and 16

*0.10, **0.05, ***0.01. *P* is the value of the pseudo-R^2

Column 1 uses information from those who reported having HCFP or not as the outcome of interest. Column 2 is our preferred specification as it uses information from the program to construct the probability of being covered, thus avoiding any misreporting by households that were ineligible but received the program. We drop close to 20% of households (and thus children) by using this criterion in our main specification.

See Tables 6, 7, 8, 9, 10, 11, and 12.

Table 8 Testing sensitivity to children treated in the past

	(1) Any Vst	(2) Tot Vst	(3) G.HC Vst	(4) G.Hosp Vst	(5) OOPs ('000DNG)
Main specification					
Treated × Post05	0.21***	0.15***	0.10**	0.13***	44.10***
	(0.04)	(0.01)	(0.05)	(0.05)	(10.14)
R^2	0.12[p]	0.12	0.12[p]	0.06[p]	0.02
Obs.	37,029	37,029	37,029	37,029	37,029
Without 6- to 8-year Olds					
Treated × Post05	0.26***	0.17***	0.12**	0.18***	51.33***
	(0.04)	(0.02)	(0.06)	(0.06)	(10.14)
R^2	0.12[p]	0.12	0.12[p]	0.06[p]	0.02
Obs.	26,558	26,558	26,558	26,558	26,558
Dummy for past treated					
Treated × Post05	0.27***	0.17***	0.14**	0.17***	51.71***
	(0.04)	(0.02)	(0.06)	(0.06)	(10.14)
Dummy partially treated (D)	0.01	−0.02	0.02	−0.03	−23.33***
	(0.03)	(0.01)	(0.05)	(0.05)	(8.67)
D × Post	0.13***	0.07***	0.07	0.10**	21.26***
	(0.03)	(0.01)	(0.06)	(0.04)	(6.73)
R^2	0.12[p]	0.12	0.12[p]	0.06[p]	0.02
Obs.	37,029	37,029	37,029	37,029	37,029
Controls	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y
Cluster	Y	Y	Y	Y	Y

Columns 1, 3, 4, 5 = probit estimates, Columns 2 and 6 = ordinary least squares estimates. Numbers in parentheses are robust standard errors Only the coefficients of interest in the regressions has been presented. Control include child controls—treatment status, post dummy, female dummy, age. Household controls—gender, education levels, urban, household size

Vst visit, *G* government, *HC* health center, *Hosp* hospital, *OOPs* out-of-pocket spending, *15,000DNG* 1USD

*0.10, **0.05, ***0.01. *P* is the value of the pseudo-R^2

Table 9 Comparing different criteria for choosing non-HCFP children

	(1) Any Vst	(2) Tot Vst	(3) G.HC Vst	(4) G.Hosp Vst	(6) OOPs ('000DNG)
Main specification					
Treated × Post05	0.21***	0.15***	0.10**	0.13***	45.17***
	(0.04)	(0.01)	(0.05)	(0.05)	(10.16)
R^2	0.12[p]	0.12	0.12[p]	0.06[p]	0.02
Obs.	37,099	37,099	37,099	37,099	37,099
Father's education > median education (8 years)					
Treated × Post05	0.24***	0.15***	0.15***	0.13**	34.94***
	(0.04)	(0.02)	(0.05)	(0.05)	(11.46)
R^2	0.12[p]	0.12	0.12[p]	0.06[p]	0.02
Obs.	29,775	29,775	29,775	29,775	29,775
Household income quintile ≥ 3					
Treated × Post05	0.22***	0.14***	0.12**	0.14**	44.21***
	(0.05)	(0.02)	(0.06)	(0.06)	(14.04)
R^2	0.12[p]	0.11	0.12[p]	0.06[p]	0.02
Obs.	26,105	26,105	26,105	26,105	26,105

Table 9 continued

	(1) Any Vst	(2) Tot Vst	(3) G.HC Vst	(4) G.Hosp Vst	(6) OOPs ('000DNG)
Controls	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y
Cluster	Y	Y	Y	Y	Y

Columns 1, 3, 4, 5 = probit estimates. Columns 2 and 6 = ordinary least squares estimates. Numbers in parentheses are robust standard errors Only the coefficients of interest in the regressions has been presented. Control include child controls—treatment status, post dummy, female dummy, age. Household controls—gender, education levels, urban, household size

Vst visit, *G* government, *HC* health center, *Hosp* hospital, *OOPs* out-of-pocket spending, *15,000DNG* 1USD

*0.10, **0.05, ***0.01. *P* is the value of the pseudo-R^2

Table 10 Robustness checks for private health center visits

	(1) PC	(2) PC
Treated	0.20***	−0.05
	(0.03)	(0.04)
Post2005	0.42***	0.34***
	(0.06)	(0.08)
Treated × Post05	0.05	0.05
	(0.04)	(0.05)
Female		0.01
		(0.02)
Age (years)		−0.05***
		(0.01)
Female HoH		0.02
		(0.03)
Father's educ (years)		0.01
		(0.00)
Mother's educ (years)		0.01***
		(0.00)
Household size		−0.07***
		(0.01)
Urban		0.21***
		(0.05)
Ethnic majority		−0.06
		(0.06)
Pseudo-R^2	0.030	0.108
Obs.	37,099	37,099
Sample mean	0.06	0.06
Province FE		Y
Cluster Y	Y	Y

PC visit to private health center, *HoH* head of household

Probit estimates, *0.10, **0.05, ***0.01

Table 11 Results from multinomial regressions

By service provider	Raw estimate	*Z*	*P*	Relative	OR
GHC	Versus no visit	0.28***	2.77	0.006	1.32
GHosp	Versus no visit	0.41***	4.33	0.000	1.51
PHC	Versus no visit	0.22**	2.51	0.012	1.24
GHC & PHC	Versus no visit	2.59**	1.98	0.048	13.26
GHC & PHC	Versus GHC	2.30*	1.76	0.078	13.26
GHC & PHC	Versus GHosp	2.17*	1.66	0.097	13.26
GHC & PHC	Versus PHC	2.37*	1.82	0.070	13.26
GHC & PHC	Versus GHC & Ghosp	3.34**	2.17	0.030	13.26

Only the estimates from the coefficients of interest have been presented. Controls include treatment status, post dummy, female dummy, age gender of head, education levels, urban, and household size

Vst visit, *G* government, *HC* health center, *Hosp* hospital, *P* private

*0.10, **0.05, ***0.01. Multinomial logit raw estimates, *OR* odds ratio

Table 12 Other definitions for outcomes

	(1) Log visits b/se	(2) Log OOPs b/se	(3) Cond OOPs b/se	(4) Cond Log OOPs b/se
Treated	−0.01* (0.01)	−0.10** (0.05)	5.61 (26.47)	0.03 (0.06)
Post2005	0.13*** (0.01)	0.66*** (0.09)	−83.88*** (26.65)	−0.42*** (0.11)
Treated × Post05	0.09*** (0.01)	0.50*** (0.05)	42.32 (33.74)	−0.03 (0.07)
R^2	0.13	0.12	0.03	0.09
Obs.	37,099	37,099	9495	9495
Controls	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Cluster	Y	Y	Y	Y

All controls included and regressions are clustered. Controls include treatment status, post dummy, female dummy, age gender of head, education levels, urban, and household size. Columns 3 and 4 are estimates of conditional values (i.e., conditional on individuals making a visit)

OOPs out-of-pocket spending, *15,000DNG* 1USD, *Cond* conditional

*0.10, **0.05, ***0.01. Ordinary least squares estimates

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