NBER WORKING PAPER SERIES

HIGH-POWERED INCENTIVES IN DEVELOPING COUNTRY HEALTH INSURANCE: EVIDENCE FROM COLOMBIA'S RÉGIMEN SUBSIDIADO

Grant Miller
Diana M. Pinto
Marcos Vera-Hernández

Working Paper 15456
http://www.nber.org/papers/w15456

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2009

We are grateful to Orazio Attanasio, Jay Bhattacharya, Adriana Camacho, Pedro Carneiro, Ken Chay, Will Dow, Alan Garber, Ursula Giedion, Mike Grossman, Ted Joyce, Robert Kaestner, Carlos Medina, Costas Meghir, Imran Rasul, and numerous seminar participants for helpful suggestions; to Luis Carlos Ortiz, Renata Pardo, Licerole Ruiz, Sandra Sterling, and Elisa Torrenegra for discussions about the health policy context in Colombia; to the Departamento Nacional de Planeación (DNP), the Departamento Administrativo Nacional de Estadística (DANE), and Profamilia for data and SISBEN index information; to Adriana Ruiz for GIS assistance; and to the Economic and Social Research Council (RES-167-25-0124), the Inter-American Development Bank, the National Institute of Child Health and Human Development (K01 HD053504), and the Stanford Center on the Demography and Economics of Health and Aging for financial support. Carolina Mejia, Nicole Smith, and Julieta Trias provided excellent research assistance. We alone are responsible for the views in this paper as well as all errors. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

High-Powered Incentives in Developing Country Health Insurance: Evidence from Colombia's Régimen Subsidiado
Grant Miller, Diana M. Pinto, and Marcos Vera-Hernández
NBER Working Paper No. 15456
October 2009, Revised March 2010
JEL No. I10,O10

ABSTRACT

Despite current emphasis on health insurance expansions in developing countries, inefficient consumer incentives for over-use of medical care are an important counterbalancing concern. However, three factors that are more acute in poor countries (credit constraints, principal-agent problems, and positive externalities) result in substantial under-use and misuse as well. This paper studies Colombia's Régimen Subsidiado, the first major developing country effort to expand insurance in a way that purposefully addresses these inefficiencies. Using a regression discontinuity design, we find that Colombia's insurance program has provided risk protection while substantially increasing the use of traditionally under-utilized preventive services (with measurable health gains) through high-powered supply-side incentives.

Grant Miller
CHP/PCOR
Stanford University
117 Encina Commons
Stanford, CA 94305-6019
and NBER
ngmiller@stanford.edu

Marcos Vera-Hernández
University College London
Department of Economics
Gower St.
London WC1E 6BT
United Kingdom
m.vera@ucl.ac.uk

Diana M. Pinto
Department of Clinical Epidemiology
and Biostatistics
Faculty of Medicine
Pontificia Universidad Javeriana
7th Avenue, #40-62, Second Floor
Bogotá, Colombia
dmpinto@gmail.com

1. Introduction

In developing countries, the inability to smooth consumption directly reduces welfare and leads to informal risk management strategies that stifle productive activity (Paxson 1993, Townsend 1994, Morduch 1995). Because unexpected illness is a leading source of economic risk, the expansion of health insurance is therefore a public policy priority in many parts of the developing world (Gertler and Gruber 2002, GTZ, WHO, and ILO 2005, Mohanan 2008).[1] At the same time, *ex post* moral hazard is a key counterbalancing concern (Zeckhauser 1970). Insured patients pay only a fraction of the full cost of their medical care at the point-of-service, so they have inefficient incentives to use too much of it (Arrow 1963, Pauly 1968, Newhouse et. al. 1993).[2]

Standard economic analyses of health insurance (and concerns about *ex post* moral hazard) may require modification in developing country settings for at least three reasons, however. First, in the presence of significant credit constraints, medical care use absent insurance may be inefficiently low (Pitt and Khandker 1998, Morduch 1999). Second, the alignment between patient and provider incentives in developing countries is notoriously poor (estimates of absenteeism rates among health care professionals in South Asia range from 25% to 75%, for example) (Chaudhury and Hammer 2004, Duflo, Banerjee, and Deaton 2004).[3] Third, in environments with highly prevalent infectious diseases, there may be large positive externalities associated with the use of some services – in particular, preventive services. Under-

[1] For example, one recent study finds that 5% of Latin American households spend 40% or more of 'non-subsistence' income on medical care each year (Xu et. al. 2003). As Gertler and Gruber (2002) note, there are two major costs of illness: medical care costs and reduced labor income. Health insurance (our focus) addresses the former, while disability insurance addresses the latter.

[2] This assumes that health care prices facing consumers reflect true resource costs in the absence of health insurance. Health care prices in developing countries are generally set administratively, so this is unlikely to be true, but the direction of the error is uncertain. For a thorough treatment of administrative pricing in medical care, see Newhouse (2002).

[3] Another striking example is the purposeful provision of services with no medical value because of their perceived benefit by uninformed patients (Das and Hammer 2007).

use and misuse are therefore sources of considerable inefficiency as well (WHO Commission on Macroeconomics and Health 2001, Black, Morris, and Bryce 2003).

This paper studies the first major developing country effort to expand health insurance in a way that addresses these inefficiencies – including the promotion of traditionally under-used preventive services – through high-powered contracts.[4] Strengthening supply-side incentives is a promising alternative to common demand-side approaches to increasing preventive service use (conditional cash transfer programs, for example), but little is known about its effectiveness (Filmer, Hammer, and Pritchett 2000, Gertler 2004, Bloom et. al. 2006, Lagarde, Haines, and Palmer 2007). In 1993, the Colombian government introduced the *Régimen Subsidiado* (or "Subsidized Regime," henceforth "SR"), a variant of the classical 'managed competition' model of insurance (Enthoven 1978a and 1978b). Colombians passing a means test are eligible for fully-subsidized health insurance. Insurers, in turn, have new contracting authority to form restrictive medical care networks, deny coverage for services deemed wasteful, and pay health care providers in ways that encourage higher quality and lower cost medical care. Importantly, we emphasize these more efficient supply-side incentives as the key innovation of the SR.[5]

To investigate how SR enrollment is associated with protection against financial risk and efficiency in health service use, we employ an empirical strategy that utilizes discrete breaks in eligibility along Colombia's continuous poverty-targeting index (called SISBEN, or *Sistema de*

[4] According to Article 153 of Law 100 (authorizing the creation of the SR), one of the SR's guiding principles is *Proteccion Integral*: "The System of Social Security in health will provide health care to the population in: education, information, health promotion and prevention, diagnostic, treatment and rehabilitation in quantity and quality according to the *Plan Obligatorio de Salud*." For studies of traditional health insurance programs in developing countries, see Abel-Smith (1992); Dow, Gertler, Schoeni, Strauss, and Thomas (1997); WHO (2000); WHO Commission on Macroeconomics and Health (2001); Gertler and Solon (2002); Dow, Gonzalez, Rosero-Bixby (2003); Dow and Schmeer (2003); Gakidou et. al. (2006); Pauly, Zweifel, Scheffler, Preker, and Bassett (2006); Hughes and Leethongdee (2007); Wagstaff (2007); Wagstaff and Yu (2007); Odonnell et. al. (2008); and Pauly, Blavin, and Meghan (2008).

[5] Very few Colombian counties actually had more than one insurer during the years we study, and heavily regulated premiums and benefit packages (a departure from textbook managed competition) leave few margins along which plans can compete. The lack of meaningful competition has also been corroborated through interviews with stakeholders in the Colombian health care system.

Identificación de Beneficiarios).[6] We address concerns about widespread manipulation of eligibility (BDO and CCRP 2000, DNP 2001, 2003a, and 2003b, Fresneda 2003, Camacho and Conover 2009) by instrumenting for SR enrollment with simulated eligibility (Hahn, Todd, and Van der Klaauw 2001). To construct this instrument, we calculate SISBEN scores in household surveys not used for actual eligibility determinations. We also estimate and utilize county-specific thresholds used in practice by each of Colombia's local governments (following Chay, McEwan, and Urquiola 2005). A variety of evidence bolsters the validity of our approach, and our estimates are generally robust across a number of parametric and non-parametric specifications.

Overall, we first find evidence that the SR has succeeded in protecting poor Colombians from financial risk associated with the medical costs of unexpected illness. In particular, SR enrollment appears to have successfully reigned-in large outliers in the right-skewed distribution of medical spending, reducing the variability of medical spending by one-third. Despite this reduction in risk, however, we observe little evidence of meaningful portfolio choice effects (changes in the composition of household assets, human capital investments, or household consumption), perhaps because the SR falls short of providing full insurance.

Our results also suggest that SR enrollment is associated with large increases in the use of traditionally under-utilized preventive services – some of which nearly doubled. Because preventive services are generally free regardless of insurance status, the SR's high-powered

[6] Several other recent papers also study the SR in Colombia. Giedion and Uribe (2009) utilize difference-in-difference and matched difference-in-difference frameworks; Camacho and Conover (2008) develop a regression discontinuity design using birth records and administrative data from Colombia's Census of the Poor; Gaviria, Medina, and Mejía (2007) use an IV strategy, instrumenting for SR enrollment using residence history; Trujillo, Portillo, and Vernon (2005) employ propensity score matching; and Panopoulus and Vélez (2001) use multivariate regression.

supply-side incentives presumably play a central role.[7] Moreover, we find evidence of health improvement under the SR as well – specifically, gains along margins sensitive to the increases in preventive care that we observe. There is more mixed evidence of changes in the use of curative services (although theoretical predictions about the use of curative care are ambiguous). We conclude by noting that the full potential of high-powered supply-side incentives in developing country health insurance has not yet been realized and that such incentives hold promise for further welfare improvement.

2. Colombia's Subsidized Health Insurance Regime for the Poor

2.1 Overview

Under Law 100 in 1993, Colombia introduced the *Régimen Subsidiado* (or SR), a novel form of publicly-financed health insurance for the poor (Gwatkin et al. 2005, Escobar 2005). Primarily through SR expansion, formal health insurance coverage in Colombia grew from 20% of the population in 1993 to 80% in 2007 (CENDEX 2008). The SR is organized as a variant of classical 'managed competition' (Enthoven 1978a and 1978b). Beneficiaries receive full public subsidies to purchase health insurance from one of multiple health insurance plans. These subsidies are financed by a combination of public resources including payroll taxes and national and local general revenue. These resources are transferred to county governments, which in turn are responsible for eligibility determination, enrollment, and contracting with health plans.

[7] *Ex ante* moral hazard under insurance acts in the opposite direction (Pauly 1968), so our results suggest that more efficient supply-side incentives dominate this effect. As Section 5.4 describes, we also fail to find any direct evidence of *ex ante* moral hazard.

Health plans charge government-regulated premiums and offer a standardized package of benefits (see Appendix 1 for the details of these benefits).[8] Out-of-pocket prices charged by providers for covered curative services are 10% of the full price (as opposed to 30% for poor uninsured Colombians – with public sector transfers to medical facilities offsetting the balance).[9] Moreover, total out-of-pocket spending per episode of illness each year is capped at half of the monthly minimum wage for SR beneficiaries – while the same cap is six times as large for uninsured patients (*Decreto 2357* and *Acuerdo 260* of the *Consejo Nacional de Seguridad Social en Salud*). Preventive services are essentially free regardless of insurance status.

Participating health plans also act as group purchasers of health services for their enrollees by contracting with a network of health facilities and clinicians (Section 3 describes key supply-side incentives embedded in these contracts). Because premiums and benefit packages are standardized by law (unlike the classical 'managed competition' model), health plans compete for enrollees on the margins of provider networks and service quality. In practice, however, very few cities had more than one insurer during the years that we study.

We note that the comparison between "uninsurance" and SR enrollment is actually a comparison between types of insurance and rationing methods: less generous insurance with exclusive reliance on demand-side cost sharing vs. more generous insurance with more efficient supply-side incentives.

2.2 Eligibility for the SR

[8] The benefits package of the SR (*Plan Obligatorio de Salud Subsidiado*) emphasizes coverage for primary and basic outpatient services, drugs, and some catastrophic care. There is limited coverage for specialist services, and there are substantial gaps in coverage for hospital care.

[9] The Colombian Ministry of Social Protection maintains a fee schedule (*soat*) regulating out-of-pocket prices. For example, the full price for a physician consultation was 17,300 pesos in 2007 and 18,460 pesos in 2008 (approximately US $10). Medical facility social workers assess uninsured patients' "ability to pay" and may charge them sliding scale out-of-pocket fees (greater than 10% of the full price) on a case-by-case basis. Operating losses are then recouped by facilities through direct public transfers.

Eligibility for the SR is determined using a poverty-targeting index called SISBEN (or *Sistema de Identificación de Beneficiarios*). The original SISBEN index consisted of fourteen components measuring different aspects of household well-being (such as housing material, access to public utilities, ownership of durable assets, demographic composition, educational attainment, and labor force participation – for a complete description, see Appendix 2).[10] On each dimension, households are classified according to mutually exclusive, collectively exhaustive categories with varying weights assigned to each category; these weights vary between urban and rural areas. A household's SISBEN score is then calculated by summing points across components. Possible scores range from 0 to 100 (with 0 being the most impoverished) and are divided into six strata. Households scoring in SISBEN strata 1 and 2 (the lowest strata) are eligible for the SR (below 48 in urban areas, below 31 in rural areas).[11]

2.3 Eligibility and Enrollment in Practice

Although eligibility for the SR increases the likelihood of enrollment, neither one necessarily implies the other for at least three reasons: misclassification or manipulation of SISBEN scores, shortfalls in local government revenue, and enrollment that preceded SISBEN enumeration.[12]

First, both local governments and households have incentives to manipulate SISBEN scores. Local governments receive fixed transfers from the national government for each

[10] Eligibility also varies within households because certain demographic groups (including pregnant women and young children) are prioritized for enrollment.
[11] SISBEN eligibility shifts abruptly at each county's *cabecera* boundary, an administrative demarcation formally distinguishing urban and rural parts of each county and loosely corresponding to the fringe of public utility infrastructure. Distinct urban and rural SISBEN scales are applied to households on corresponding sides of the boundary, differing both in component parts and in the weighting of response categories for each component. We implemented a research design exploiting these urban/rural index differences, but inconsistent application of the rural index and data limitations prevent us from drawing meaningful conclusions from it. In this paper we therefore focus on urban eligibility.
[12] Administrative mistakes in the enrollment process are also important.

resident they enroll, creating incentives to maximize enrollment. The selective enrollment of key constituents can also provide political benefits (Camacho and Conover 2009). Households prefer enrollment over "uninsurance" as well because co-insurance rates are lower for SR beneficiaries than for those lacking formal insurance. Consistent with both types of incentives, there is evidence of considerable SISBEN score manipulation between 1997 and 2003 (Camacho and Conover 2009).[13]

Second, most local governments lack sufficient revenue to finance the enrollment of all eligible residents. According to law, those with lower SISBEN scores and those belonging to certain targeted groups (such as children under five and pregnant women) are therefore prioritized for enrollment.[14] This means that many counties use *de facto* eligibility thresholds that fall below the uniform national threshold.

Third, some counties began enrolling residents in the SR before all of their residents had been classified using SISBEN. These counties instead used other means-test criteria such as residents' *estrato*, an alternative poverty measure used to establish electricity prices paid by local households.

In general, these practical considerations have two broad implications for our empirical analyses. One is the necessity of an empirical strategy that addresses manipulation of official SR eligibility and enrollment. Section 4.2 describes our instrumental variables approach of simulating eligibility with household data not used for eligibility determinations and then

[13] Using results from the 2005 population census, the Colombian newspaper *El Tiempo* reports that there are more SR enrollees than residents in some counties (*El Tiempo*, October 26, 2006). Camacho and Conover (2009) show that the distribution of official SISBEN scores exhibits both large leftward shifts in density over time and the formation of a mass point just to the left of the national eligibility threshold in urban areas. Neither are present in Colombian household surveys. The former suggests misrepresentation by households, while the latter suggests misrepresentation by enumerators or officials.

[14] The laws formalizing this prioritization are *Acuerdos 244* and *253* of the *Consejo Nacional de Seguridad Social en Salud*. This prioritization also means that although SISBEN scores are calculated at the level of family "nucleus," individuals within families can vary in enrollment status; we observe this in our household survey data.

instrumenting for enrollment using simulated eligibility. The other is that our first stage regressions (of enrollment on predicted eligibility, as presented in Section 4.2) will be weaker than if eligibility mapped cleanly onto enrolment. We address this latter issue by estimating and utilizing county-specific eligibility thresholds (and by controlling for other criteria used idiosyncratically for SR enrollment).

3. Changes in Incentives under the SR

3.1 The SR's Supply-Side Incentives

In our analyses of the SR, we emphasize insurers' ability to contract with health care providers (hospitals and medical groups) for more efficient service use as the principal innovation. Insurers receive premiums (flat payments per enrollee per unit time) for all covered services, giving them strong incentives to constrain total spending. Insurers participating in the SR then transmit these incentives to provider organizations (hospitals and clinics) through high-powered contracts and the authority to deny coverage for services deemed inefficient.[15]

Specifically, there are two types of contracts between insurers and provider organizations under the SR: capitated primary care contracts and fee-for-service specialty care contracts. For primary care, insurers pay organizations fixed amounts per month for all services used by enrollees ("capitation"). These contracts create strong incentives for organizations to constrain total spending on primary care and have important implications for both preventive and curative health care (as discussed in the next section). Importantly, promotion of preventive care can be a central means of reducing total primary care expenditures. For specialty care, insurers pay

[15] Provider organizations, in turn, must transmit the incentives they face to individual clinicians whom they employ. Our interviews with stakeholders in the Colombian health care system suggest that organizations solve this agency problem through non-financial rather than payment-based incentives (systematic data on organizational incentives and clinician contracts is unavailable).

provider organizations a pre-determined fee for each covered service that they supply (i.e., on a "fee-for-service" basis). These contracts encourage the provision of all reimbursable services (both efficient and inefficient). However, SR insurers also have the authority to deny coverage on a case-by-case basis for inefficient specialty care (termed "utilization review"), allowing them to limit wasteful service use.[16]

3.2 The Interaction of Supply- and Demand-Side Incentives and Empirical Predictions

Although changes in supply-side incentives are the salient feature of the SR, out-of-pocket prices for covered services also fall. This section therefore considers the joint effect of supply- and demand-side changes in formulating predictions about changes in risk protection, the use of preventive and curative services, and health outcomes. Section 5 then provides empirical evidence on each.

Risk Protection and Out-of-Pocket Medical Spending. Protecting households against medical care costs associated with unexpected illness is the primary purpose of health insurance. As Appendix 1 shows, the SR covers many of the most expensive services including trauma care, dialysis, and major joint replacement (hips and knees, for example). It also caps total out-of-pocket spending per episode of illness each year at half of the monthly minimum wage. The SR should therefore reduce the variance of medical care spending relative to the mean as it reduces the probability of large right-tail outliers in the distribution of medical spending. Assuming that the price elasticity of demand for catastrophic services is less than one, the SR should also reduce total out-of-pocket spending for medical care.

Preventive Health Services. Most preventive services in Colombia are free regardless of insurance status (i.e., free both for SR enrollees and the uninsured), so the main difference in

[16] Utilization review does little to promote services traditionally used sub-optimally.

incentives for preventive care originate on the supply-side. Preventive services are categorized as primary care, and because primary care providers are paid on a capitated basis, they have strong incentives to limit total primary care spending. Increasing preventive service use can reduce the need for more costly curative care – and therefore be cost-saving – so capitated primary care incentives are likely to increase preventive care.[17] An opposing force at work is *ex ante* moral hazard: SR enrollees may have weaker incentives to use preventive services because they pay less for curative care (Nordquist and Wu 1976, Phelps 1978). Although we fail to find evidence of ex ante moral hazard (as shown in Section 5.4) and there is little evidence of it in other studies (Kenkel 2000), our preventive care estimates reflect the net effect of these forces.[18] Because preventive services generate important positive externalities (both pecuniary and infectious disease-related), increases in their use presumably improve welfare.[19]

Curative Medical Care. Three changes under the SR are likely to influence the use of curative medical care: reductions in out-of-pocket prices, increases in the use of preventive services, and supply-side incentives for limiting total medical spending. First, for negative price elasticities of demand, reductions in out-of-pocket prices will increase the use of curative care. This increase is inefficient under standard assumptions (*ex post* moral hazard); however, curative care in developing countries may produce positive externalities, and there may also be important credit constraints. Second, any increases in preventive service use may reduce the use of curative care (an efficient result). Third, both capitation and utilization review produce

[17] The RAND Health Insurance Experiment (Newhouse 1993) found that enrollees in health maintenance organizations (or HMOs, which employ capitation) used relatively more preventive care. More recent observational studies in the United States report congruent results (Keenan, Elliott, Cleary, Zaslavsky, and Landon 2009).
[18] The RAND Health Insurance Experiment reports no sizeable or significant effect of insurance coverage on health behaviors (smoking, drinking, and exercise) (Newhouse and the Insurance Experiment Group 1993). Medicare coverage has also been reported not to increase unhealthy behaviors (Dave and Kaestner 2006).
[19] Popular conditional cash transfer programs – including the Familias en Acción program in Colombia – aim to increase the use of preventive services even though they are otherwise available for free. At least eleven developing countries have introduced such conditional cash transfer programs (Fiszbein and Schady 2009).

incentives for providers to limit the use of curative medical care. Some reductions may be efficient (counterbalancing demand-side incentives leading to *ex post* moral hazard) while others may be inefficient (leading to "stinting" – see for example Ellis and McGuire (1990), Holmstrom and Milgrom (1991), Ma and McGuire (1997), and McGuire (2000)). Predictions about changes in curative care under the SR as well as their welfare implications are therefore ambiguous.

Health Outcomes. Both preventive and curative services are inputs into health production, so changes in health under the SR will depend on the precise pattern of changes in their use. However, if one considers preventive care to influence health on the extensive margin (whether or not an individual becomes sick) and curative care to influence health on the intensive margin (duration or severity of illness conditional on becoming sick), it is reasonable to believe that preventive care have larger health consequences than curative care. In our empirical analyses, we are also able to examine specific illnesses linked to types of preventive service use that we observe.

4. Data and Empirical Strategy

4.1 Data

Our empirical approach requires household survey data containing three types of information: (1) enrollment in the SR, (2) components of the SISBEN index (enabling us to simulate SR eligibility), and (3) potential behavioral responses and outcomes of interest (both welfare-improving and distortionary). There are two candidate Colombian household surveys that meet these criteria: the *Encuestas de Calidad de Vida* (ECV) and the Demographic and Health Surveys (DHS).[20] The ECVs are nationally-representative household surveys designed to

[20] Official SISBEN classification data (used for eligibility determination) do not contain outcomes of interest and are unattractive for our purposes given manipulation evidence of manipulation (Camacho and Conover 2009).

measure socio-economic well-being and "quality of life," broadly defined. The DHS data reports detailed fertility, health, and socio-economic information for nationally-representative samples of fertile age women (defined as ages 15-49) and their households. Because the *de facto* implementation of the SR occurred in 1996/1997, we use the 2003 ECV and the 2005 DHS for our analyses.[21] Table 1 shows descriptive statistics by type of behavior/outcome for the full samples as well as those with and without SR coverage.

As our empirical strategy requires, we calculate household-level SISBEN scores to simulate SR eligibility because simulated eligibility should not reflect misrepresentation of household characteristics as official SISBEN scores do (Camacho and Conover 2009). However, not all household surveys contain all necessary components of the SISBEN index. Appendix 2 provides a complete description of the SISBEN components present in each survey.[22] We impute values using ordered probit models for the few variables that are missing.

4.2 Empirical Strategy

Instrumenting for Enrollment with Simulated Eligibility

In principle, the SISBEN index's SR eligibility threshold (at score 48 in urban areas) can be used to study behavioral responses associated with SR enrollment. This discontinuity induces an abrupt shift in eligibility (and enrollment) along otherwise smooth distributions of household characteristics; coincident shifts in behaviors and outcomes can reasonably be linked to the program. However, selection into eligibility (and enrollment) according to unobserved

[21] There was also a Colombian DHS survey conducted in 2000, but it is much smaller and contains few outcome variables of interest. We do not use the 1997 wave of the ECV because SR enrollment was still very low in that year.

[22] In theory, SISBEN scores should be calculated at the family (or "nucleus") level. However, we treat entire households as families given reports that SISBEN enumerators adopted this definition in practice due to difficulties in conforming to the technical definition.

household characteristics as discussed in Section 2.3 is likely to bias the estimates of interest (McCrary 2008).

To circumvent this difficulty, we employ an instrumental variables strategy closely resembling one proposed by Hahn, Todd, and Van der Klaauw (2001). Conceptually, we seek to reconstruct 'true' SISBEN scores when both official SISBEN scores and observed SR enrollment reflect manipulation. To do so, we calculate SISBEN scores for each household in the ECV and DHS data and then use calculated scores to instrument for SR enrollment (for prominent examples of simulated instruments, see Currie and Gruber (1996), and Cutler and Gruber (1996), and Hoxby (2001)).[23] A virtue of this approach is that neither ECV nor DHS data is used for eligibility determinations.

Using urban households with simulated SISBEN scores near the urban eligibility threshold,[24] we could in principle begin by estimating the following first-stage equation for individuals i in household h:

$$(1) \qquad enroll_{ih} = \alpha + \gamma below_h + \beta SISBEN_h + \Sigma_k \delta_k estrato_{hk} + \varepsilon_{ih},$$

where *enroll* is an indicator for whether or not household i is enrolled in the SR, *below* is an indicator for simulated SISBEN score lying below the eligibility threshold, *SISBEN* is simulated SISBEN score, and *estrato* is a dummy variable for an *estrato* category. Using Two-Stage Least Squares (2SLS), we could then estimate the following second-stage equation:

$$(2) \qquad outcome_{ih} = \varphi + \lambda enroll_{ih} + \theta SISBEN_h + \Sigma_k \pi_k estrato_{hk} + \xi_{ih},$$

instrumeting for *enroll* with *below*. The relationship between behavioral outcomes of interest (*outcome*) and SR enrollment would then be captured by estimates of the parameter λ.

[23] We emphasize "old" SISBEN scores – those calculated using the official scale in effect between the beginning of the SR and 2003. Enrollees eligible only under the old scale were not disenrolled with the introduction of the "new scale," and the old (but not the new) eligibility discontinuity is evident in the 2005 DHS.

[24] We do not use rural households to examine the rural threshold between SISBEN strata 2 and 3 because of inconsistent application of the rural scale.

Estimating County-Specific Eligibility Thresholds

As described in Section 2.3, financial shortfalls led many Colombian counties to use SR eligibility thresholds at SISBEN scores below the official national threshold.[25] The implication of this for estimating equations (1) and (2) using the official threshold is that our first stage relationship will be weaker than necessary, compounding limitations to first stage strength posed by the other issues raised in Section 2.3. We therefore use county-specific eligibility thresholds. In addition to improving the strength of our first stage, this approach offers another key benefit: because some local governments use the official national threshold for other public benefits, changes in outcomes observed at county-specific SR thresholds will not reflect behavioral responses to other public programs. (Section 5.5 shows that participation in other public programs is not discontinuous at county-specific thresholds.)

Exact county-specific eligibility thresholds are unknown, so we estimate them following Chay, McEwan, and Urquiola (2005). Specifically, using our full samples, we establish county-specific breaks in SR eligibility at the SISBEN score that maximize the goodness-of-fit of a model of SR enrollment as a function of a dichotomous indicator for whether or not a household's score falls below the threshold.[26] This approach establishes thresholds that maximize the percentage of individuals correctly classified as eligible in each county.

We then use county-specific thresholds to re-code the variable *below* for each individual *i* in households *h* and Colombian counties *c* and estimate the following first stage equation:

[25] Bogotá adopted a threshold above the uniform national one, first using SISBEN score 50 and later SISBEN score 52.

[26] We also constrain estimated thresholds to fall below the uniform national threshold (given that our measure of SR enrollment reflects both true eligibility and manipulation), and we exclude individuals from a few counties using two criteria related to having very few observations in some counties. One is having the first percentile in the SISBEN score distribution lie above the national threshold or the 99th percentile score lie below the national threshold. The other is having an estimated threshold with those below it having relatively lower SR enrollment rates than those above it. The total number of observations excluded for these reasons is minor (3.8% of the sample in the ECV data and 5% of the sample in the DHS data). The mean threshold in the ECV sample is 45 with a standard deviation of 4.25; the DHS sample mean is the same but with a smaller standard deviation (3.26).

(3) $enroll_{ihc} = \alpha + \gamma below_{hc} + \beta SISBEN_h + \varphi SISBEN\_diff_{hc} + \Sigma_k \delta_k estrato_{hk} + \mu_c + \varepsilon_{ihc}$,

where *below* is now an indicator for whether or not individual *i*'s simulated SISBEN score falls below the eligibility threshold in the individual's county *c*, *SISBEN_diff* is the difference between an individual's simulated SISBEN score and the estimated eligibility threshold in the individual's county (i.e., relative SISBEN score), μ_c represents county fixed effects (allowing us to focus on within-county variation in simulated eligibility across county-specific thresholds), and all other variables are defined as in equation (1). To adhere transparently to the identifying assumption that individuals with simulated SISBEN scores very near the threshold are comparable with the exception of their eligibility, we conservatively focus on individuals whose calculated scores lie within two index points of the county-specific cutoff (our main estimates persist across various bandwidths, as shown in Section 5.5).[27]

Figure 1 uses ECV and DHS data to show SR enrollment and "uninsurance" by simulated SISBEN score relative to county-specific eligibility thresholds. Each county's threshold is normalized to zero, and the figure then shows means and 95% confidence intervals for each SISBEN index integer relative to the threshold as well as non-parametric kernel density plots on either side. The figure illustrates large discrete increases in the probability of enrollment and concomitant decreases in the probability of uninsurance at the threshold ranging between 25 to 30 percentage points.

Using our re-coded variable *below* to instrument for *enroll*, we then estimate the following equation by 2SLS:

(4) $outcome_{ihc} = \varphi + \lambda enroll_{ihc} + \theta SISBEN_h + \psi SISBEN\_diff_{hc} + \Sigma_k \pi_k estrato_{hk} + \mu_c + \xi_{ihc}$,

[27] Because eligibility thresholds vary by county, our approach essentially averages across sample respondents with different absolute SISBEN scores. To investigate how our estimates vary with absolute SISBEN score, we also estimate variants of equation (4) below with interactions between SR enrollment and absolute SISBEN score (instrumenting for this term with interactions between an indicator for falling below county-specific thresholds and absolute SISBEN score) and find insignificant estimates for this interaction term.

where the estimate of interest is the estimate of λ.[28] Section 5.5 shows that our results are robust across bandwidths to including higher-order relative SISBEN score polynomials, using interactions between relative SISBEN scores and simulated eligibility, and excluding county fixed effects. We also estimate λ using local linear regression without any covariates other than relative SISBEN score.

5. Results

This section presents empirical evidence on a variety of important behavioral responses to the SR. We begin by investigating the effectiveness of health insurance in accomplishing its primary objective: protecting households against financial risk (and possibly changing the optimal composition of household spending and assets). Next, we study changes in the use of traditionally under-utilized preventive services in light of the SR's emphasis on allocative efficiency. In doing so, we also examine changes in health outcomes that are sensitive to the use of important preventive services. We then analyze how SR enrollment is related to changes in the use of curative medicine. To investigate possible behavioral distortions, we also test for reductions in private health investments associated with SR enrollment (*ex ante* moral hazard) and insurance 'crowd-out.' Finally we present evidence on the validity of our empirical strategy and show that our estimates are generally robust across a number of parametric and non-parametric specifications.

5.1 Financial Risk Protection and Portfolio Choice

[28] We estimate equations (3) and (4) using linear models; marginal probabilities computed using bivariate probit models yield similar results to the 2SLS estimates for dichotomous outcomes examined throughout the paper. We calculate our standard errors by relaxing the assumption that disturbance terms are independent and identically-distributed within households, the level at which the treatment of interest (eligibility based on the SISBEN index) is assigned.

Although a key innovation of the SR is its emphasis on allocative efficiency, the primary objective of health insurance is to provide protection against financial risk. We therefore begin by investigating the relationship between SR enrollment and both the level and variability of out-of-pocket medical spending. To construct our variability measure, we first calculate mean individual spending separately among those enrolled and those not enrolled in the SR. For each individual, we then measure the difference between individual spending and the mean among those with the same enrollment status. Our variability measure is then the absolute value of this difference. We analyze outpatient and inpatient spending (within the past twelve months) separately and emphasize the latter because of idiosyncrasies in how outpatient spending is reported.[29]

We first graphically examine shifts in the distribution of medical spending associated with simulated eligibility for the SR. Panel A of Figure 2 shows the distribution of outpatient medical spending in the preceding month separately for those falling above and those falling below county-specific thresholds (using our sample of those within two index points of the cutoff). Both distributions are heavily right-skewed, but mass in the distribution for those who are eligible (those below the threshold) falls to the left of the distribution for those who are ineligible. Panel B shows the difference between the two distributions (density among those below the threshold minus density among those above the threshold at every level of spending), confirming this result. Figure 3 then shows the same distributions for inpatient spending in the past year; there is again more mass at greater spending values (between 300,000 and 400,000 pesos in particular) among the ineligibles. Both figures suggest that SR eligibility is associated with reductions in right-tail medical spending – a pattern consistent with risk protection.

[29] The ECV 2003 question about outpatient spending in the preceding 30 days excludes outpatient costs associated with illness ultimately leading to hospitalization; the inpatient expenditure question asks about all inpatient spending in the past 12 months.

The first four columns of Panel A in Table 2 then present econometric results obtained by estimating equations (3) and (4) for the level and variability of medical spending by type. The first row presents IV estimates for SR enrollment, and the second row reports intent-to-treat (ITT) estimates for simulated SR eligibility (estimates for a dummy variable coding whether or not an individual falls below the eligibility threshold obtained by OLS regressions of outcomes on this dummy and the other covariates in equation 4). The first column suggests that SR enrollment lowers mean inpatient spending by about 60,000 pesos, a 31% reduction among those using any inpatient services (Table 4 suggests no selection into inpatient service use associated with SR enrollment). Perhaps more importantly, consistent with insurance through the SR reigning-in large outliers in the right-skewed distribution of medical spending, the variability of inpatient spending fell by roughly 62,000 pesos, a reduction of 34%.[30] (Despite suggestive evidence in Figure 2, our estimates for level and variability of outpatient spending are statistically insignificant.) To further probe the association between SR enrollment and protection against catastrophic medical care costs, columns 5 through 7 show estimates obtained by using dichotomous indicators for inpatient spending exceeding 600,000, 900,000, and 1,200,000 pesos as dependent variables. The resulting estimates suggest that outlier inpatient spending falls by 3, 2, and 2 percentage points (respectively) with SR enrollment. Appendix 3 Figure 1 graphically shows all outcomes examined in Panel A across county-specific eligibility thresholds (essentially, graphical versions of our intent-to-treat analyses).

Overall, the results shown in the top panel of Table 2 suggest that SR enrollment is associated with meaningful risk protection benefits. By reducing household exposure to financial risk, SR enrollment could also produce meaningful changes in the composition of

[30] Using the estimates in Panel A of Table 2 and mean inpatient spending and mean inpatient variability among those in our bandwidth of 2 sample enrolled in the SR, 60,371/194,858≈0.31 and 62,109/185,424≈0.34.

household assets, human capital investments, and household consumption (i.e., portfolio choice effects). Specifically, it may increase investments not previously undertaken because of costly informal risk-management activities (such as precautionary saving). Panel B of Table 2 presents estimates for durable goods not used to construct the SISBEN index (car and radio ownership) as well as household education and consumption expenditures. In general, it implies that SR enrollment is not associated with discernable portfolio choice effects (perhaps because the SR falls short of providing full insurance).[31]

5.2 Preventive Service Use and Health Status

In addition to providing protection against financial risk, the twin objective of the SR is to improve allocative efficiency in medical care. In particular, increasing the use of highly beneficial preventive services – many of which produce large positive externalities – is likely to improve welfare. These externalities are due both to reduced rates of infectious disease transmission and to reduced curative care costs borne by others through risk pools. Because preventive services are generally free for Colombians regardless of their insurance status, changes in use must necessarily reflect high-powered supply-side incentives (capitation in particular) under the SR.

The first two columns of Table 3 report estimates for different types of preventive care use obtained from equations (3) and (4) (Appendix 3 Figure 2 shows graphical versions of the intent-to-treat analyses). In general, they suggest substantial increases in the use of preventive

[31] In our robustness tables (Appendix 4, Table A1 – described in Section 5.5), however, we find evidence of statistically significant increases in car and radio ownership when estimating equations (3) and (4) using bandwidths of 3 and 4 SISBEN index units.

health care services.[32] Specifically, SR enrollment is associated with a 29 percentage point increase in the probability of a preventive physician visit in the past year (a 50% increase). Importantly, children enrolled in the SR also had 1.24 more growth-monitoring and well-care visits in the past year than their uninsured peers, an increase of nearly twofold. These well-care visits are a principal way of addressing important childhood health problems including iron deficiency anemia and other micronutrient deficiencies. Corrective action is a key way to strengthen immune system function and reduce vulnerability to leading childhood illnesses such as diarrheal diseases and acute respiratory infections (Fogel 1994).[33]

We then investigate whether or not health status has improved under the SR – in particular, dimensions of health that are sensitive to the use of preventive care (which we conceptualize as influencing the extrinsic margin of illness incidence rather than the intrinsic margin of duration or severity). Although our preventive physician visit variable is not service-specific, our measure of childhood growth and development checks should correspond directly with infectious disease incidence. Columns three through six of Table 3 show estimates for child health outcomes linked to preventive care use. SR enrollment is associated with 1.3 fewer child days absent from usual activities due to illness in the past month. Moreover, enrollment is also associated with a 35 percentage point reduction in the self-reported incidence of cough, fever, or diarrhea among children in the preceding two weeks (a 62% reduction). Appendix 3 Figure 2 shows graphical versions of the intent-to-treat analyses for these health outcomes as well. Because these health gains can be linked to increases in preventive service use, they are also likely to improve welfare.

[32] This increase in preventive care use dominates any *ex ante* moral hazard (which would reduce prevention – although Section 5.4 suggests no evidence of *ex ante* moral hazard).
[33] For more information about Colombia's growth monitoring and well-care programs (which include the provision of iron supplements and de-worming medication as well as nutrition education and supervision), see http://www.minproteccionsocial.gov.co/vbecontent/library/documents/DocNewsNo16062DocumentNo4071.PDF.

5.3 Use of Curative Medical Care

As Section 3.2 discusses, there are a variety of competing incentives and other forces influencing curative medical care under the SR. These include reductions in out-of-pocket prices, increases in the use of preventive services, and supply-side incentives for limiting total medical spending. Predictions about changes in curative service use under the SR – and their welfare implications – are therefore ambiguous, but they remain a central interest of policymakers and are important for assessing the costs of Colombia's innovative health insurance reform.

Table 4 reports estimates for various categories of curative care obtained from equations (3) and (4) (Appendix 3 Figure 3 shows graphical versions of the intent-to-treat analyses). We find that SR enrollment is associated with a 13 percentage point increase in physician visits because of health problems within the past 30 days. There is no change in use of curative care among children, which may be due to improvements in child health associated with SR enrollment. Finally, there is no meaningful relationship at conventional significance levels between participation in the SR and hospitalizations or medical visits for chronic diseases.

5.4 *Ex Ante* Moral Hazard and Insurance Crowd-Out

Protection from financial risk associated with unexpected illness weakens private incentives for costly health protection (*ex ante* moral hazard) (Pauly 1968). Because we find evidence of greater risk protection, we investigate how protective private health behaviors not directly linked to medical care change with SR enrollment.[34] As Table 5 and Appendix 3 Figure

[34] More generally, private health behaviors and public health services could theoretically be either complements or substitutes for publicly provided health services. While reductions in the price of medical care may raise the return to private health investments given competing risks, cheaper health services could also instead 'crowd-out' costly private health behaviors (Dow, Holmes, Philipson, and Sala-i-Martin 1999, Murphy and Topel 2003).

4 show, however, we find no meaningful change in handwashing, breastfeeding, or maternal investments in fetal health (alcohol, drug, or tobacco use during pregnancy; or prenatal dietary supplementation with iron, calcium, or folic acid), suggesting little *ex ante* moral hazard associated with SR enrollment.

Manipulation of official SISBEN scores suggests that Colombians perceive benefits of SR enrollment, so we also investigate the possibility that SR enrollment displaces other forms of explicit health insurance.[35] Table 5 also presents intent-to-treat (ITT) estimates obtained by estimating equation (3) with dichotomous indicators for *Regimen Contributivo* enrollment, other forms of health insurance (those for the military, police officers, and certain industrial groups like oil industry workers, for example), and "uninsurance" as dependent variables. The estimates for *Regimen Contributivo* and other insurance are generally small and not statistically meaningful, suggesting that the SR does not generally "crowd-out" other forms of insurance (and that our comparisons throughout the paper are truly between SR enrollees and the "uninsured").

5.5 Balance across Discontinuities and Robustness

Our interpretation of the results above requires that absent the SR, eligible and ineligible individuals (according to our simulated SISBEN scores) in the vicinity of each county's threshold are comparable. To probe this assumption further, Table 6 first presents results obtained by estimating equations (3) and (4) for individual characteristics that could not reasonably change in response to SR enrollment (such as age or educational attainment for

[35] Formal sector employees are mandated to enroll in an employment-based health insurance system called *Regimen Contributivo*. This mandate holds even for individuals with SISBEN scores falling below the SISBEN eligibility threshold for the SR.

adults). Consistent with our assumption, we generally find no estimates that are meaningfully different from zero.[36]

Next, we consider whether or not our SR enrollment estimates might be attributable to participation in other public programs for which some counties also use the SISBEN index. Before investigating this possibility directly, we first note that it is doubtful because these programs use the uniform national eligibility threshold, while we estimate and utilize *de facto* county-specific thresholds for the SR – many of which fall below the uniform national threshold. To confirm this, we re-estimate equations (3) and (4) using a dichotomous indicator for participation in these other programs as the dependent variable in equation (4). We analyze participation in a wide range of publicly financed programs, including job training, home mortgage subsidies, education vouchers, *Hogares Comunitarios* (a large child care program), and services provided by the *Instituto Colombiano de Bienestar Familiar* (the largest social welfare agency in Colombia).[37] Table 6 presents these results, suggesting that participation in other programs is balanced across county-specific SR eligibility thresholds.

Finally, to investigate the robustness of our results, we estimate a variety of alternative parametric and non-parametric specifications based on our main estimating equations. First, we re-estimate equations (3) and (4) using SISBEN score bandwidths ranging between two and four. Second, at each bandwidth we estimate specifications that include higher order polynomials of relative SISBEN scores (including squared, cubic, and fourth power terms) as well as models

[36] The single exception is whether or not the household head completed elementary education at the 10% level (and in the 2003 ECV, but not the 2005 DHS) and if anything would suggest our results to be slight underestimates.
[37] The program *Empleo en Acción* (a workfare program active in 2003 but abolished in 2004) is the only public program of which we are aware that used the uniform national SISBEN eligibility threshold for which the 2003 ECV does not contain data. Participation in the program was low (only 7.4% of those in SISBEN strata 1 or 2) and provided no benefits directly related to health (it paid 60% of the minimum wage to individuals who worked on official program projects an average of at least 30 hours per week for 2.4 months) (IFS-SEI-Econometria 2005). We do not have information about participation in *Familias en Acción*, a Colombian conditional cash transfer program, but the eligibility threshold for this program (36) is considerably lower, falling more than two standard deviations below the mean of our estimated SR thresholds.

that include interactions between relative SISBEN scores and simulated eligibility (although allowing SISBEN gradients to vary on either side of the eligibility threshold should matter little given our narrow bandwidth). Third, we re-estimate specifications that do not include county fixed effects (at each bandwidth), allowing us also to make cross-county comparisons among individuals with identical simulated SISBEN scores but that fall on opposite sides of county-specific eligibility thresholds. Finally, we also estimate models using non-parametric local linear regression.[38] As Tables A1-A4 of Appendix 4 suggest, our results are generally robust (with some variation in precision) across these alternative bandwidths and specifications.

6. Conclusion

Distinct from traditional analyses of health insurance in wealthy countries (and standard concerns about *ex post* moral hazard), under-use and misuse are particularly important sources of inefficiency in developing country medical care. This paper presents new evidence that the SR has successfully protected poor Colombians against financial risk while also addressing these inefficiencies in health service use. Specifically, the SR has successfully increased the use of key preventive services with large positive externalities through high-powered contracting with medical care providers. We find evidence of health improvement under the SR as well – in particular, gains that can be linked to the increases in preventive care that we observe. This approach to strengthening supply-side incentives for the provision of key preventive services warrants further research because it may be a potent alternative (or perhaps an effective

[38] Specifically, we use local linear regression functions with triangle kernels to estimate conditional means of outcome variables (conditioning on *SISBEN_diff*) on either side of the eligibility threshold, and we repeat this estimation process for enrollment in the SR. We then construct Wald statistics using differences in the estimated conditional means of the outcome variable on either side of the threshold as numerators and the difference in the conditional mean of SR enrollment on either side of the threshold as the denominator. We estimate our standard errors using 250 bootstrap replications. Details of the implementation can be found in Nichols (2007).

complement) to common demand-side approaches embodied in conditional cash transfer programs.

We conclude by observing that the welfare-improving potential of high-powered incentives in health insurance has yet to be fully realized. In the specific context of Colombia, a variety of political concessions followed the creation of the SR – including exemptions from the end of government subsidies as well as requirements that insurers contract with public facilities for a minimum share of the services that they finance. These concessions have presumably limited the ability of health plans to pay medical care providers in ways that encourage better quality and lower cost services. More generally, the performance of contracts that explicitly reward health improvement has yet to be assessed.

References

Abel-Smith, B. 1992. "Health Insurance in Developing Counties: Lessons from Experience," *Health Policy and Planning* 7(3): 215-226.

Arrow, K. 1963. "Uncertainty and the Welfare Economics of Medical Care," *The American Economic Review* 53: 941-973.

BDO International and CCRP (Corporación Centro Regional de Población). 2000. *Evaluation of SISBEN: Efficiency, Institutional Efficacy of the Identification, Classification and Beneficiary Selection Processes*. Bogotá: Ministry of Health.

Black, R., Morris, S., and J. Bryce. 2003. "Where and Why are 10 Million Children Dying Every Year?" *The Lancet* 361: 2226-2234.

Bloom, E., Bhushan, I., Clingingsmith, D., Hong, R., King, E., Kremer, M., Loevinsohn, B., and B. Schwartz. 2006. "Contracting for Health: Evidence from Cambodia," Unpublished Manuscript, Harvard University.

Camacho, A. and E. Conover. 2008. "Effects of Subsidized Health Insurance on Newborn Health in a Developing Country," *CEDE Working Paper 14*, Universidad de Los Andes.

Camacho, A and E. Conover. 2009. "Manipulation of Social Program Eligibility: Detection, Explanations, and Consequences for Empirical Research," *CEDE Working Paper 19*, Universidad de Los Andes.

CENDEX. 2008. Resultados de la Encuesta Nacional de Salud 2007. Power point presentation available online at: http://www.cendex.org.co.

Chaudhury, N. and J. Hammer. 2004. "Ghost Doctors: Absenteeism in Rural Bangladeshi Health Facilities," *World Bank Economic Review* 18(3): 423-441.

Chay, K., McEwan, P. and M. Urquiola. 2005. "The Central Role of Noise in Evaluating Interventions that Use Test Scores to Rank Schools," *American Economic Review* 95(4): 1237-1258.

Currie, J. and J. Gruber. 1996. "Health Insurance Eligibility, Utilization of Medical Care, and Child Health," *Quarterly Journal of Economics* 111(2): 431-466.

Cutler, D. 2002. "Health Care and the Public Sector." In *Handbook of Public Economics Vol. 4*, edited by A. Auerbach and M. Feldstein. Amsterdam: Elsevier.

Cutler, D. and J. Gruber. 1996. "Does Public Insurance Crowd Out Private Insurance?" *Quarterly Journal of Economics* 111(2): 391-430.

Cutler, D. and R. Zeckhauser. 2000. "The Anatomy of Health Insurance." In *Handbook of Health Economics Vol. 1*, edited by A. J. Culyer & J. P. Newhouse. Edition 1, Chapter 11. Amsterdam: Elsevier.

Das, J. and J. Hammer. 2007. "Money for Nothing: The Dire Straits of Medical Practice in Delhi, India," *Journal of Development Economics* 83(1): 1-36.

Dave, D. and R. Kaestner. 2006. "Health Insurance and Ex-ante Moral Hazard: Evidence from Medicare," *NBER Working Paper #12764,* Cambridge, MA.

Department of National Planning (DNP) and Ministry of Social Protection. 2001. *Proceedings of the National Conference on SISBEN: Integral Evaluation.* Bogotá: Department of National Planning.

Department of National Planning (DNP) and Ministry of Social Protection. 2003a. *Who Benefits from SISBEN? Integral Evaluation.* Bogotá: Department of National Planning.

Department of National Planning (DNP) and Ministry of Social Protection. 2003b. *Results of the Evaluation Survey of SISBEN at the Municipal Level.* Bogotá: Department of National Planning.

Department of National Planning (DNP). 2004. *Programa Familias en Acción – Condiciones Iniciales de los Beneficiarios e Impactos Preliminares*. Bogotá: Department of National Planning.

Dow, W., Gertler, P., Schoeni, R., Strauss, J., and D. Thomas. 1997. "Health Care Prices, Health, and Labor Outcomes: Experimental Evidence," *Labor and Population Working Paper 97-01*, RAND.

Dow, W., Philipson, T., and X. Sala-i-Martin. 1999. "Longevity Complementarities under Competing Risks," *American Economic Review* 89(5): 1358-1371.

Dow, W., Gonzalez, K. and L. Rosero-Bixby. 2003. "Aggregation and Insurance-Mortality Estimation," *NBER Working Paper #9827*.

Dow, W. and K. Schmeer. 2003. "Health Insurance and Child Mortality in Costa Rica," *Social Science and Medicine* 57(6): 975-986.

Duflo, E., Banerjee, A., and A. Deaton. 2004. "Wealth, Health, and Health Services in Rural Rajasthan," *American Economic Review Papers and Proceedings* 94(2): 326-330.

Ellis, R. and T. McGuire. 1990. "Optimal Payment Systems for Health Services," *Journal of Health Economics* 9: 375-396.

Ellis, R., and T. McGuire. 1993. "Supply-Side and Demand-Side Cost Sharing In Health Care," *Journal of Economic Perspectives*, 7(4): 135-151.

Enthoven, A. 1978. "Consumer Choice Health Plan (first of two parts). Inflation and Inequity in Health Care Today: Alternatives for Cost Control and an Analysis of Proposals for National Health Insurance" *New England Journal of Medicine* 298: 650-658.

Enthoven, A. 1978. "Consumer Choice Health Plan (second of two parts). A National Health Insurance Proposal Based on Regulated Competition in the Private Sector," *New England Journal of Medicine* 298: 709-720.

Escobar, M. 2005. "Health sector reform in Colombia," in *Development Outreach Magazine*. Washington, D.C.: World Bank Institute.

Filmer, D., Hammer, J. and L. Pritchett. 2000. **"**Weak Links in the Chain: A Diagnosis of Health Policy in Poor Countries," *World Bank Research Observer* 15(2): 199-224.

Fiszbein, A. and N. Schady. 2009. *Conditional Cash Transfers – Reducing Present and Future Poverty.* Washington D.C: The World Bank.

Fogel, R. 1994. "Economic Growth, Population Theory, and Physiology: The Bearing of Long-term Processes on the Making of Economic Policy," *American Economic Review* 84(3): 369-395.

Fresneda O. 2003. "Focusing on the subsidized health regime in Colombia," *Revista Salud Pública* 5(3): 209-245.

Fuchs, V. 1974. *Who Shall Live? Health, Economics, and Social Choice.* Singapore: World Scientific Publishing Company.

Gakidou, E. et. al. 2006. "Assessing the Effect of the 2001-2006 Mexican Health Reform: An Interim Report Card," *The Lancet* 368: 1920-1935.

Gaviria, A., Medina, C., and C. Mejía. 2007. "Assessing Health Reform in Colombia: From Theory to Practice," *Economía* 7(1): 29-72.

Gertler, P. 2004. "Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment," *American Economic Review Papers and Proceedings* 94(2): 336-341.

Gertler, P. and J. Gruber. 2002. "Insuring Consumption against Illness," *American Economic Review* 92(1): 51–70.

Gertler, P. and Solon, O. 2002. "Who Benefits From Social Health Insurance? Evidence from the Philippines," Unpublished Manuscript, University of California, Berkeley and the University of the Philippines.

Giedion, U. and M. Uribe. 2009. "Colombia's Universal Health Insurance System," *Health Affairs* 28(3): 853-863.

GTZ, ILO and WHO. 2005. *Recommendations for Action. The International Conference on Social Health Insurance in Developing Countries.* Geneva: GTZ, ILO, and WHO.

Gwatkin, D. R., Wagstaff, A. and A. S. Yazbeck. 2005. *Reaching the Poor with Health, Nutrition and Population Services: What Works, What Doesn't, and Why*. Washington, D. C.: World Bank.

Hahn, J., Todd, P. and W. Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design," *Econometrica* 69(1): 201-209.

Holmstrom, B. and P. Milgrom, P. 1991. "Multitask Principal-Agent Analyses: Incentive Contracts, Asset Ownership, and Job Design," *Journal of Law, Economics and Organization* 7: 24-52.

Hoxby, C. 2001. "All School Finance Equalizations Are Not Created Equal," *Quarterly Journal of Economics* 116(4): 1189-1231.

Hughes, D. and S. Leethongdee. 2007. "Universal Coverage in the Land of Smiles: Lessons from Thailand's 30 Baht Health Reforms," *Health Affairs* 26(4): 999-1008.

IFS-SEI-Econometria. 2005. *Evaluación de Impacto del Programa Empleo en Acción (Volume VI).* Bogotá: National Planning Department.

Keenan, P., Elliott, M., Cleary, P., Zaslavsky, A., and M. Landon. 2009. "Quality Assessments by Sick and Healthy Beneficiaries in Traditional Medicare and Medicare Managed Care," *Medical Care* 47(8): 882-888.

Kenkel, D. 2000. "Prevention." In *Handbook of Health Economics Vol. 1*, edited by A. J. Culyer & J. P. Newhouse. Edition 1, Chapter 31. Amsterdam: Elsevier.

Lagarde, M., Haines, A., and N. Palmer. 2007. "Conditional Cash Transfers for Improving Uptake of Health Interventions in Low- and Middle-Income Countries: A Systematic Review," *Journal of the American Medical Association* 298(16): 1900-1910.

Levy, H. and D. Meltzer. 2004. "What Do We Really Know about Whether Health Insurance Affects Health?" In *Health Policy and the Uninsured: Setting the Agenda*, edited by C. McLaughlin. Washington, DC: Urban Institute Press.

Ma, A.C. and T. McGuire. 1997. "Optimal Health Insurance and Provider Payment," *American Economic Review* 87: 685-704.

McGuire, T. 2000. "Physician Agency." In *Handbook of Health Economics Vol. 1*, edited by A. J. Culyer & J. P. Newhouse. Edition 1, Chapter 9. Amsterdam: Elsevier.

Mohanan, M. 2008. "Causal Effects of Health Shocks on Consumption and Debt: Quasi-Experimental Evidence from Bus Accident Injuries," Unpublished Manuscript, Harvard University.

Morduch, J. 1995. "Income Smoothing and Consumption Smoothing," *Journal of Economic Perspectives* 9(3): 103 - 114.

Morduch, J. 1999. "The Microfinance Promise," *Journal of Economic Literature* 37(4): 1569-1614.

Murphy, K. M. and R. H. Topel. 2006. "The Value of Health and Longevity," *Journal of Political Economy* 114: 871-904, October.

Newhouse, J. and the Insurance Experiment Group. 1993. *Free for All? Lessons from the RAND Health Insurance Experiment*. Cambridge, MA: Harvard University Press.

Newhouse, J. P. 2002. *Pricing the Priceless – A Health Care Conundrum*. Cambridge, MA: MIT Press.

Nichols, A. 2007. *rd: Stata Module for Regression Discontinuity Estimation.* Available online at: http://ideas.repec.org/c/boc/bocode/s456888.html.

Nordquist, G., and S.Y. Wu. 1976. "The Joint Demand for Health Insurance and Preventive Medicine." In *The Role of Health Insurance in the Health Services Sector*, edited by R. Rosett. New York: National Bureau of Economic Research.

Odonnell, O. et. al. 2008. "Who Pays for Health Care in Asia?" *Journal of Health Economics* 27: 460-475.

Panopoulus, G. and C. Vélez. 2001. "Subsidized Health Insurance, Proxy Means Testing, and the Demand for Health Care among the Poor in Colombia," in *Colombia Poverty Report* (Volume 2). Washington: World Bank.

Pauly, M. 1968. "The Economics of Moral Hazard," *American Economic Review* 49: 531-537.

Pauly, M., Blavin, F., and Meghan, S. 2008. "Is There a Market for Voluntary Health Insurance in Developing Countries?" *NBER Working Paper #14095*, Cambridge, MA.

Pauly, M., Zweifel, P., Scheffler, R., Preker, A., and M. Bassett. 2006. "Private Health Insurance in Developing Countries," *Health Affairs* 25(2): 369-379.

Paxson, C. 1993. "Consumption and Income Seasonality in Thailand," *Journal of Political Economy* 101(1): 39-72.

Phelps, C. 1978. "Illness Prevention and Medical Insurance," *Journal of Human Resources* 13: 183-207.

Pitt, M. and S. Khandker. 1998. "The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter?" *Journal of Political Economy* 106(5): 958-996.

Townsend, R. 1994. "Risk and Insurance in Village India," *Econometrica* 62(3): 539-591.

Trujillo, A., Portillo, J., and J. Vernon. 2005. "The Impact of Subsidized Health Insurance for the Poor: Evaluating the Colombian Experience Using Propensity Score Matching," *International Journal of Health Care Finance and Economics* 5(3): 211-239.

Wagstaff, A. 2007. "Health Insurance for the Poor: Initial Impacts of Vietnam's Health Care Fund for the Poor," Policy Research Paper Series Working Paper # 4134, The World Bank.

Wagstaff, A. and S. Yu. 2007. "Do Health Sector Reforms Have Their Intended Impacts? The World Bank's Health VIII Project in Gansu Province, China," *Journal of Health Economics* 26: 505-535.

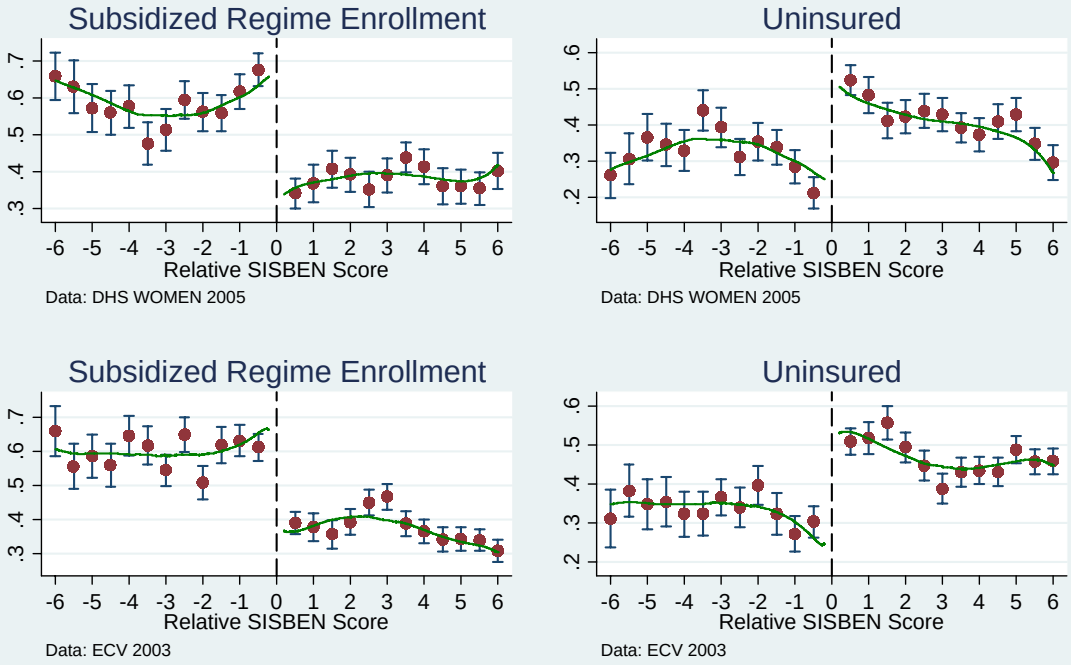
World Health Organization. 2000. *World Health Report 2000: Health Systems – Improving Performance.* Geneva, Switzerland: World Health Organization.

WHO Commission on Macroeconomics and Health. 2001. *Macroeconomics and Health: Investing in Health for Economic Development*. Geneva, Switzerland: World Health Organization.

Xu, K., Evans, D., Kawabata, K., Zeramdini, R., Klavus, J., and C. Murray. 2003. "Household Catastrophic Health Expenditure: A Multicountry Analysism" *The Lancet* 362(9378): 111-117.

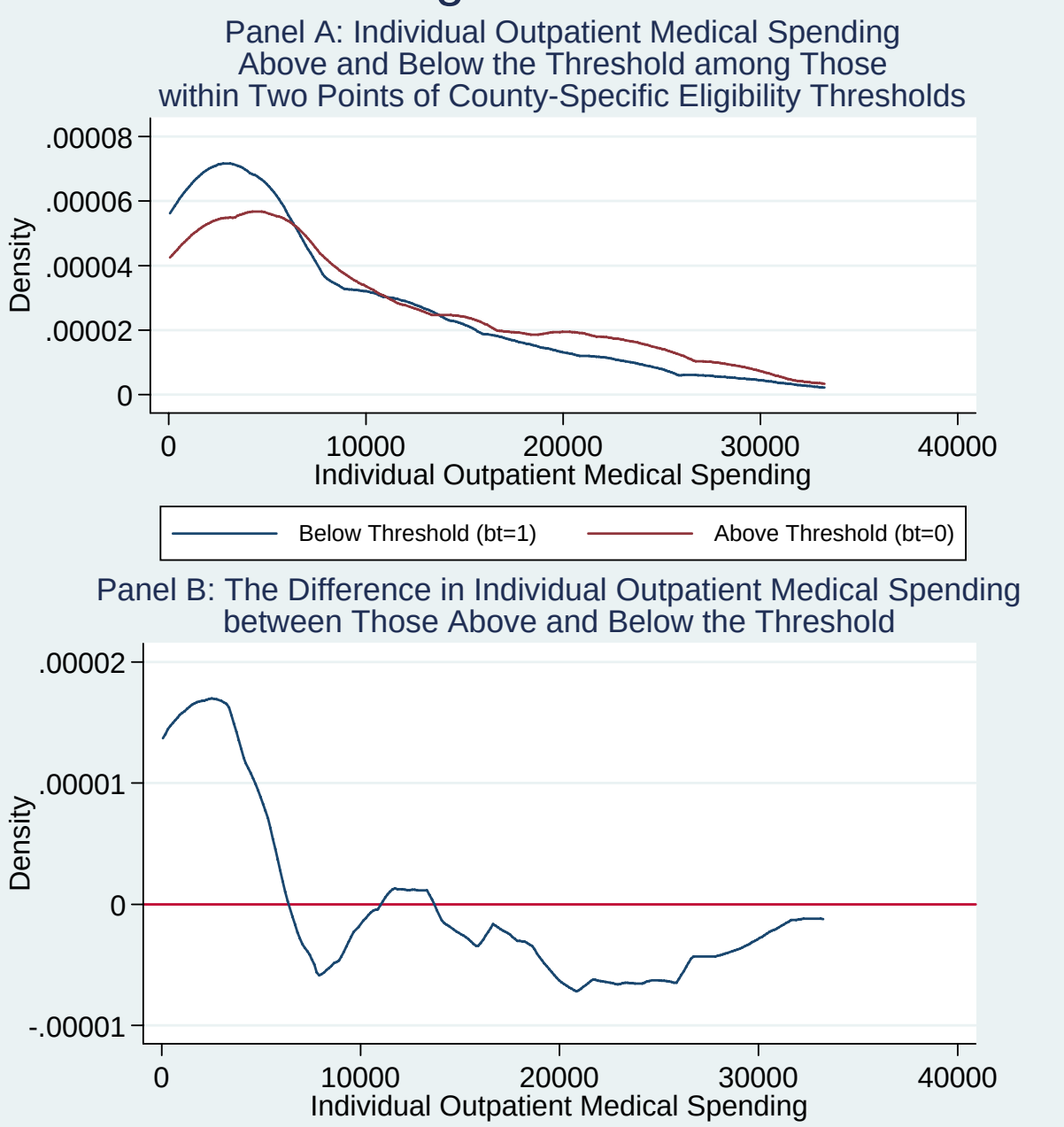
Zeckhauser, R. 1970. "Medical Insurance: A Case Study of the Tradeoff between Risk Spreading and Appropriate Incentives," *Journal of Economic Theory* 2(1): 10-26.

Figure 1: Subsidized Regime Enrollment and 'Uninsurance' across SISBEN Eligibility Thresholds



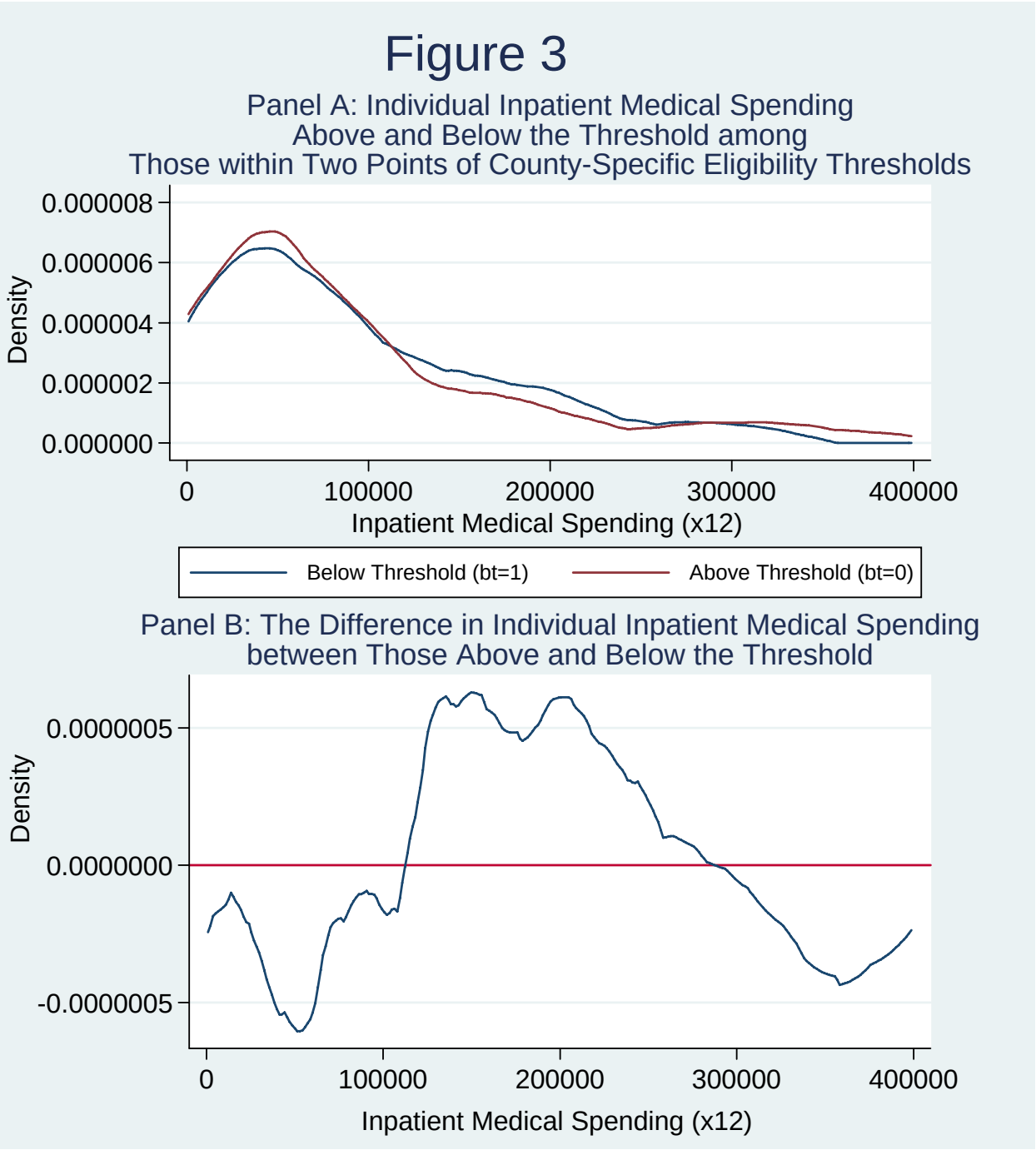
Individual-level “urban” data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Each point (and corresponding 95% confidence interval bars) represents means for individuals grouped into half-integer bins relative to county-specific thresholds. Non-parametric kernel density plots also fitted separately using individuals on either side of county-specific thresholds.

Figure 2



Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV The top panel shows non-parametric kernel density plots fitted separately using individuals on either side of county-specific thresholds; the bottom panel shows the difference between the two plots.

Figure 3



Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV The top panel shows non-parametric kernel density plots fitted separately using individuals on either side of county-specific thresholds; the bottom panel shows the difference between the two plots.

TABLE 1:
DESCRIPTIVE STATISTICS

	Total			Not Enrolled in the Subsidized Regime			Enrolled in the Subsidized Regime			
Variable:	Mean	SD	N	Mean	SD	N	Mean	SD	N	Data Source
Risk Protection, Consumption Smoothing, and Portfolio Choice										
Individual Inpatient Medical Spending	10,577	129,443	4,211	10,971	128,490	2,248	10,125	130,558	1,963	ECV
Individual Outpatient Medical Spending	2,118	22,565	4,218	2,513	20,170	2,249	1,666	25,019	1,969	ECV
Variability of Individual Inpatient Medical Spending	20,167	127,861	4,211	20,983	126,764	2,248	19,234	129,133	1,963	ECV
Variability of Individual Outpatient Medical Spending	3,971	22,209	4,218	4,698	19,615	2,249	3,140	24,822	1,969	ECV
Individual Inpatient Medical Spending >= 600,000	0.004	0.06	4,211	0.004	0.06	2,248	0.004	0.06	1,963	ECV
Individual Inpatient Medical Spending >=900,000	0.002	0.04	4,211	0.002	0.04	2,248	0.002	0.04	1,963	ECV
Individual Inpatient Medical Spending >= 1,200,000	0.001	0.04	4,211	0.001	0.04	2,248	0.002	0.04	1,963	ECV
Individual Education Spending	7,588	20,279	3,567	7,501	23,234	1,874	7,684	16,407	1,693	ECV
Household Education Spending	35,145	48,468	4,222	34,089	52,464	2,253	36,352	43,428	1,969	ECV
Total Spending on Food	274,881	217,392	4,096	279,128	201,735	2,171	270,091	233,756	1,925	ECV
Total Monthly Expenditure	654,552	599,861	966	688,065	716,382	479	621,590	455,559	487	ECV
Has Car	0.03	0.16	3,276	0.03	0.17	1,676	0.02	0.15	1,600	DHS
Has Radio	0.60	0.49	3,276	0.60	0.49	1,676	0.61	0.49	1,600	DHS
Medical Care Use										
Preventive Physician Visit	0.47	0.50	4,222	0.39	0.49	2,253	0.57	0.49	1,969	ECV
Number of Growth Development Checks Last Year	1.17	1.85	1,186	0.96	1.75	678	1.45	1.93	508	DHS
Curative Care Use (Not Conditional on Health Status)	0.06	0.24	4,222	0.06	0.23	2,253	0.07	0.26	1,969	ECV
Hospital Stay	0.08	0.26	4,222	0.07	0.25	2,253	0.08	0.28	1,969	ECV
Medical Visit for Chronic Disease	0.58	0.49	564	0.52	0.50	262	0.64	0.48	302	ECV
Curative Care Use among Children (Not Conditional on Health Status)	0.32	0.46	1,184	0.29	0.46	678	0.34	0.47	506	DHS
Health Status										
Child Days Lost to Illness	0.59	1.96	1,184	0.67	2.21	678	0.49	1.57	506	DHS
Cough, Fever Diarrhea	0.56	0.50	1,188	0.56	0.50	679	0.57	0.50	509	DHS
Any Health Problem	0.64	0.48	1,184	0.63	0.48	678	0.65	0.48	506	DHS
Birthweight (KG)	3.26	0.56	901	3.23	0.52	527	3.28	0.61	374	DHS
Behavioral Distortions										
Drank Alcohol during Pregnancy	0.11	0.31	1,013	0.12	0.33	500	0.10	0.29	513	DHS
Number of Drinks per Week during Pregnancy	3.59	11.70	109	3.68	12.40	60	3.47	10.91	49	DHS
Months Child Breastfed	12.41	10.57	962	11.11	10.21	572	14.32	10.81	390	DHS
Folic Acid During Pregnancy	0.55	0.50	1,003	0.55	0.50	495	0.55	0.50	508	DHS
Number Months Folic Acid during Pregnancy	4.03	2.45	528	4.10	2.41	260	3.97	2.49	268	DHS
Handwashing	0.60	0.49	652	0.52	0.50	406	0.72	0.45	246	DHS
Contributory Regime Enrollment	0.09	0.29	4,222	0.17	0.38	2,253	0.00	0.00	1,969	ECV
Other Health Insurance	0.00	0.06	4,222	0.01	0.08	2,253	0.00	0.00	1,969	ECV
Uninsured	0.44	0.50	4,222	0.82	0.39	2,253	0.00	0.00	1,969	ECV
Contributory Regime Enrollment	0.12	0.33	3,276	0.24	0.43	1,676	0.00	0.00	1,600	DHS
Other Health Insurance	0.01	0.08	3,276	0.01	0.11	1,676	0.00	0.00	1,600	DHS
Uninsured	0.38	0.49	3,276	0.74	0.44	1,676	0.00	0.00	1,600	DHS
Balance										
Household Head Age	46.79	14.74	3,276	47.25	15.28	1,676	46.31	14.14	1,600	DHS
Household Head Age	46.58	14.79	4,222	45.71	15.12	2,253	47.57	14.35	1,969	ECV
Completed Elementary School	0.19	0.39	3,275	0.18	0.38	1,675	0.20	0.40	1,600	DHS
Completed Elementary School	0.19	0.39	3,764	0.18	0.39	1,985	0.19	0.39	1,779	ECV
Completed Secondary School	0.19	0.39	3,275	0.20	0.40	1,675	0.18	0.39	1,600	DHS
Completed Secondary School	0.07	0.26	3,764	0.08	0.27	1,985	0.07	0.25	1,779	ECV
Household Head Completed Elementary School	0.29	0.45	3,276	0.28	0.45	1,676	0.31	0.46	1,600	DHS
Household Head Completed Elementary School	0.28	0.45	4,222	0.27	0.45	2,253	0.28	0.45	1,969	ECV
Household Head Completed Secondary School	0.02	0.15	3,276	0.02	0.13	1,676	0.03	0.16	1,600	DHS
Household Head Completed Secondary School										
Student Received School Grant	0.08	0.26	1,305	0.05	0.22	651	0.10	0.30	654	ECV
Benefits to Buy House	0.01	0.08	4,222	0.00	0.06	2,253	0.01	0.10	1,969	ECV
Attended Training	0.05	0.21	3,010	0.06	0.23	1,593	0.04	0.19	1,417	ECV
Household in Hogar Comunitario program	0.10	0.30	4,222	0.09	0.29	2,253	0.11	0.31	1,969	ECV
Services from Bienstar Familiar	0.18	0.38	4,222	0.16	0.37	2,253	0.20	0.40	1,969	ECV

All data summarized is from samples of "urban" individuals within two SISBEN index points of county-specific eligibility thresholds in either the 2003 *Encuesta de Calidad de Vida* (ECV) or the 2005 Demographic and Health Survey (DHS) (as indicated in the right column). SISBEN index calculations are described in detail in Appendix 2, and estimation of county-specific eligibility thresholds is presented in Section 4.2. The first group of three columns present summary statistics for the entire bandwidth of 2 sample, the second group for those not enrolled in the Subsidized Regime, and the third group for those enrolled in the Subsidized Regime.

TABLE 2:
RISK PROTECTION AND PORTFOLIO CHOICE

Panel A: Risk Protection

Outcome:	Individual Inpatient Medical Spending	Individual Outpatient Medical Spending	Variability of Individual Inpatient Medical Spending	Variability of Individual Outpatient Medical Spending	Individual Inpatient Medical Spending ≥ 600,000	Individual Inpatient Medical Spending ≥ 900,000	Individual Inpatient Medical Spending ≥ 1,200,000
2SLS Estimate, Subsidized Regime Enrollment	-60,371*	3,562	-62,109*	2,620	-0.03*	-0.02**	-0.02**
	(33,166)	(3,307)	(32,860)	(3,160)	(0.01)	(0.01)	(0.01)
Intent to Treat Estimate	-15,628*	918	-16,078**	676	-0.01**	-0.004***	-0.003***
	(8,138)	(827)	(8,046)	(793)	(0.004)	(0.002)	(0.002)
First Stage Estimate, Below Eligibility Threshold	0.26***	0.26***	0.26***	0.26***	0.26***	0.26***	0.26***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
First Stage F-Statistic	25.75	25.53	25.75	25.53	25.75	25.75	25.75
Observations	4,211	4,218	4,211	4,218	4,211	4,211	4,211
Data Source	ECV	ECV	ECV	ECV	ECV	ECV	ECV

Panel B: Portfolio Choice

Outcome:	Individual Education Spending	Household Education Spending	Total Spending on Food	Total Monthly Expenditure	Has Car	Has Radio
2SLS Estimate, Subsidized Regime Enrollment	-342	30,366	32,136	-33,826	0.07	0.14
	(4,963)	(25,733)	(104,871)	(305,878)	(0.04)	(0.11)
Intent to Treat Estimate	-84.72	7,815	8,790	-14,036	0.03*	0.05
	(1,230)	(6,412)	(28,271)	(127,170)	(0.02)	(0.04)
First Stage Estimate, Below Eligibility Threshold	0.25***	0.26***	0.27***	0.41***	0.40***	0.40***
	(0.05)	(0.05)	(0.05)	(0.11)	(0.04)	(0.04)
First Stage F-Statistic	23.16	25.45	27.82	13.53	110	110
Observations	3,567	4,222	4,096	966	3,276	3,276
Data Source	ECV	ECV	ECV	ECV	DHS	DHS

Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column. The first row shows 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The second row shows reduced-form intent-to-treat estimates for simulated SISBEN score falling below the county-specific eligibility threshold; for dichotomous dependent variables, marginal probabilities obtained from probit estimates calculated at the mean of the independent variables are reported . The third row shows first stage estimates from OLS regressions of Subsidized Regime enrollment on an indicator for falling below the county-specific eligibility threshold. All specifications also include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

TABLE 3:
USE OF PREVENTIVE MEDICAL CARE AND HEALTH STATUS

	Use of Preventive Care		Health Status (Children)			
Outcome:	Preventive Physician Visit	Number of Growth Dev. Checks Last Year	Child Days Lost to Illness	Cough, Fever, Diarrhea	Any Health Problem	Birthweight (KG)
2SLS Estimate, Subsidized Regime Enrollment	0.29*	1.24*	-1.30*	-0.35*	-0.26	-0.38
	(0.17)	(0.74)	(0.71)	(0.21)	(0.19)	(0.33)
Intent to Treat Estimate	0.08*	0.39*	-0.41**	-0.11*	-0.08	-0.10
	(0.05)	(0.23)	(0.21)	(0.06)	(0.06)	(0.08)
First Stage Estimate, Below Eligibility Threshold	0.26***	0.31***	0.31***	0.32***	0.32***	0.28***
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
First Stage F-Statistic	25.45	25.19	25.11	25.53	25.11	14.36
Observations	4,222	1,186	1,184	1,188	1,184	901
Data Source	ECV	DHS	DHS	DHS	DHS	DHS

Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column. The first row shows 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The second row shows reduced-form intent-to-treat estimates for simulated SISBEN score falling below the county specific eligibility threshold; for dichotomous dependent variables, marginal probabilities obtained from probit estimates calculated at the mean of the independent variables are reported . The third row shows first stage estimates from OLS regressions of Subsidized Regime enrollment on an indicator for falling below the county-specific eligibility threshold. All specifications also include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

TABLE 4:
USE OF CURATIVE MEDICAL CARE

Outcome:	Curative Use (Not Conditonal on Health Status)	Curative Use among Children (Not Conditional on Health Status)	Medical Visit for Chronic Disease	Hospital Stay
2SLS Estimate, Subsidized Regime Enrollment	0.13**	-0.05	0.51	-0.04
	(0.06)	(0.19)	(0.34)	(0.06)
Intent to Treat Estimate	0.04**	-0.02	0.20*	-0.01
	(0.02)	(0.06)	(0.10)	(0.02)
First Stage Estimate, Below Eligibility Threshold	0.26***	0.31***	0.35***	0.26***
	(0.05)	(0.06)	(0.10)	(0.05)
First Stage F-Statistic	25.45	25.11	11.58	25.45
Observations	4,222	1,184	564	4,222
Data Source	ECV	DHS	ECV	ECV

Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column. The first row shows 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The second row shows reduced-form intent-to-treat estimates for simulated SISBEN score falling below the county specific eligibility threshold; for dichotomous dependent variables, marginal probabilities obtained from probit estimates calculated at the mean of the independent variables are reported . The third row shows first stage estimates from OLS regressions of Subsidized Regime enrollment on an indicator for falling below the county-specific eligibility threshold. All specifications also include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

TABLE 5:
BEHAVIORAL DISORTIONS - *EX ANTE* MORAL HAZARD AND INSURANCE CROWD-OUT

	***Ex-Ante* Moral Hazard**						**Insurance Crowd-Out**					
Outcome:	Drank Alcohol during Pregnancy	Number of Drinks per Week during Pregnancy	Months Breastfed as Child	Folic Acid During Pregnancy	Number Months Folic Acid during Pregnancy	Hand Washing	Contributory Regime Enrollment	Uninsured	Other Health Insurance	Contributory Regime Enrollment	Uninsured	Other Health Insurance
2SLS Estimate, Subsidized Regime Enrollment	-0.05	-21.59	-0.82	0.15	0.52	-0.24						
	(0.12)	(136)	(5.27)	(0.17)	(1.46)	(0.37)						
Intent to Treat Estimate	-0.02	-1.89	-0.22	0.06	0.17	-0.05	-0.025	-0.23***	-0.002	-0.043*	-0.36***	-0.001
	(0.04)	(10.56)	(1.41)	(0.06)	(0.47)	(0.08)	(0.03)	(0.05)	(0.003)	(0.02)	(0.04)	(0.008)
First Stage Estimate, Below Eligibility Threshold	0.35***	0.09	0.27***	0.36***	0.33***	0.36***						
	(0.06)	(0.32)	(0.06)	(0.06)	(0.09)	(0.06)						
First Stage F-Statistic	31.29	0.07	17.56	32.49	11.91	8.44						
Observations	1,013	109	962	1,003	528	652	4,222	4,222	4,222	3,276	3,276	3,276
Data Source	DHS	DHS	DHS	DHS	DHS	DHS	ECV	ECV	ECV	DHS	DHS	DHS

Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column. The first row shows 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The second row shows reduced-form intent-to-treat estimates for simulated SISBEN score falling below the county-specific eligibility threshold; for dichotomous dependent variables, marginal probabilities obtained from probit estimates calculated at the mean of the independent variables are reported. Intent-to-treat estimates only are reported for the crowd-out analyses of other insurance types (the last six columns). The third row shows first stage estimates from OLS regressions of Subsidized Regime enrollment on an indicator for falling below the county-specific eligibility threshold. All specifications also include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

TABLE 6:
BALANCE ACROSS ELIGIBILITY THRESHOLDS

Outcome:	Household Head Age	Household Head Age	Completed Elementary School	Completed Elementary School	Completed Secondary School	Completed Secondary School	Household Head Completed Elementary School	Household Head Completed Elementary School	Household Head Completed Secondary School	Household Head Completed Secondary School	Student Received School Grant	Benefits to Buy House	Attended Training	Household in Hogar Comunitario Program	Services from Bienestar Familiar
2SLS Estimate, Subsidized Regime Enrollment	1.29	3.05	-0.09	-0.15	0.09	0.05	-0.16	-0.37*	0.001	-0.04	-0.06	0.02	0.01	0.03	-0.04
	(3.15)	(6.53)	(0.06)	(0.10)	(0.07)	(0.06)	(0.11)	(0.22)	(0.03)	(0.07)	(0.14)	(0.04)	(0.05)	(0.16)	(0.20)
Intent to Treat Estimate	0.52	0.79	-0.04	-0.04	0.04	0.02	-0.06	-0.11*	0.00	-0.01	-0.02	0.01	0.002	0.01	-0.01
	(1.26)	(1.68)	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.06)	(0.01)	(0.01)	(0.04)	(0.02)	(0.02)	(0.06)	(0.06)
First Stage Estimate, Below Eligibility Threshold	0.40***	0.26***	0.40***	0.25***	0.40***	0.25***	0.40***	0.26***	0.40***	0.26***	0.21***	0.26***	0.27***	0.26***	0.26***
	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
First Stage F-Statistic	110	25.45	111	24.72	111	24.72	110	25.45	110	25.45	8.71	25.45	28.79	25.45	25.45
Observations	3,276	4,222	3,275	3,764	3,275	3,764	3,276	4,222	3,276	4,222	1,305	4,222	3,010	4,222	4,222
Data Source	DHS	ECV	DHS	ECV	DHS	ECV	DHS	ECV	DHS	ECV	ECV	ECV	ECV	ECV	ECV

Individual-level "urban" data for those within two SISBEN index points of county-specific eligibility thresholds from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column. The first row shows 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The second row shows reduced-form intent-to-treat estimates for simulated SISBEN score falling below the county-specific eligibility threshold; for dichotomous dependent variables, marginal probabilities obtained from probit estimates calculated at the mean of the independent variables are reported . The third row shows first stage estimates from OLS regressions of Subsidized Regime enrollment on an indicator for falling below the county-specific eligibility threshold. All specifications also include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

Appendix 1: Subsidized Regime Benefits

<table>
<tr><th rowspan="2">AGE / POPULATION GROUP</th><th colspan="8">TYPE OF BENEFIT</th></tr>
<tr><th>Preventive care</th><th>Primary care (basic medical consultations, procedures and diagnostic tests)</th><th>Secondary care (specialist care, hospitalizations)</th><th>Tertiary care</th><th>Catastrophic care</th><th>Medications</th><th>Transportation</th><th>Excluded interventions</th></tr>
<tr><td>< 1 YEAR</td><td>Neonatal care and screening (Vit K, anemia, TSH), immunizations, well child care</td><td rowspan="6">All</td><td>All</td><td>All</td><td rowspan="6">Treatment with radiotherapy and chemotherapy for cancer, dialysis and organ transplant for renal failure, Surgical treatment of heart, cerebrovascular, neurological and congenital conditions, treatment of major trauma, intensive care unit, hip and knee replacement, major burns, treatment for AIDS</td><td rowspan="6">All medications in national formulary</td><td rowspan="6">For referrals, catastrophic care cases</td><td rowspan="6">Aesthetic surgery
Infertility treatment
Treatment for sleep disorders
Organ transplants (except renal, heart, chornea and bone marrow)
Psychotherapy and psychoanalysis
Treatments for end stage disease</td></tr>
<tr><td>1-4 years</td><td>Well child care, immunizations, anemia screening</td><td rowspan="3">Cataract and strabismus surgery, herniorraphy, appendectomy, cholecystectomy, orthopedics, rehabilitation services and procedures</td><td rowspan="3">Not covered</td></tr>
<tr><td>5-19 years</td><td>Well child care, immunizations, anemia screening</td></tr>
<tr><td>20-60 years</td><td>Cardiovascular and renal disease risk screening, cervical and breast cancer screening</td></tr>
<tr><td>>60 years</td><td>Cardiovascular and renal disease risk screening, cervical and breast cancer screening</td><td></td></tr>
<tr><td>PREGNANT WOMEN</td><td>High risk screening, STD, prenatal care</td><td>Same as above plus obstetric care</td><td>Obstetric care</td></tr>
</table>

Appendix 2: Components of the SISBEN Index and SISBEN Score Calculations

This appendix describes the components of SISBEN index, details the index information available in each household survey, and explains how we calculate SISBEN scores in each data source.

1. Components of the SISBEN Index

As explained in the text of the paper, our study focuses on the original urban SISBEN index There are four general types of information used in calculating the SISBEN index: (A) human capital, employer characteristics, and benefits; (B) demographics, income, and labor force participation; (C) housing characteristics; and (D) access to public utilities. The index is composed of 14 components across these categories. For each component, respondents are categorized according to mutually exclusive, collectively exhaustive polychotomous response categories. Each response category for each component corresponds to a weight or "points," and index scores are calculated by summing across points. Scores range between 0 and 100; higher scores denote higher socio-economic status.

The specific components of the index are:

(A) Human Capital; Employer Characteristics and Benefits

- (1) Educational attainment of the household head
- (2) Mean Schooling for household members twelve years old and older
- (3) Firm size and provision of Social Security benefits for the household head

(B) Demographics, Income, and Labor Force Participation

- (4) Proportion of children six years old and under (as share of children under age eighteen)
- (5) Proportion of household members employed (as a share of those older than twelve)
- (6) Per capita income indexed to the minimum wage (all types of income are counted)

(C) Housing Characteristics

- (7) Number of rooms per person
- (8) Primary wall material
- (9) Primary roof material
- (10) Primary floor material
- (11) Number of appliances (among those on a pre-determined list)

(D) Access to Public Utilities

- (12) Water source
- (13) Sewage disposal
- (14) Garbage disposal

2. SISBEN Components Available in Each Household Survey

Our analyses use the 2003 ECV and the 2005 DHS. The table below shows which SISBEN components are available in each survey.

Variable	DHS 2005	ECV 2003
Educational Attainment	Available	Available
Employment Status	Available	Available
Social Security Benefits		
Health Insurance	Available	Available
Pension	Not Available	Available
Firm Size (Number of Employees)	Not Available	Available
Age	Available	Available
Income	Not available	Available
Number of Rooms	Available	Available
Primary Wall Material	Available	Available
Primary Roof Material	Not available	Not available
Primary Floor Material	Available	Available
Number of Appliances		
TV	Available	Available
Refrigerator	Available	Available
Air Conditioner	Available	Available
Blender	Available	Available
Washing Machine	Available	Available
Water Source	Available	Available
Sewage Disposal	Available	Available
Garbage Disposal	Available	Available

Most SISBEN components are available in the household surveys we use in our primary analyses (nearly all in the 2003 ECV and the great majority in the 2005 DHS). For missing components, we use an ordered probit procedure to predict the most likely response category for each missing component using a large number of observable household characteristics. The section below describes how we performed our SISBEN score calculations.

3. SISBEN Score Calculations

In this section we report SISBEN index weights for each response category for each component and describe how we impute scores for components not represented in our household surveys. SISBEN index scores are then calculated by summing weights or points across all components.

A. Human Capital; Employer Characteristics and Benefits

1. Educational attainment of the household head

1	No education	0
2	Some elementary	1.6239
3	Complete elementary	3.4435
4	Some secondary	5.0039
5	Complete secondary	7.3434
6	Some of higher education	9.7833
7	Complete higher education	11.546
8	Graduate studies	12.4806

To compute educational attainment, we use information of level of schooling completed and number of years of schooling. Levels of schooling correspond to the following number of years of education:

- Complete elementary school: 5 years
- Complete secondary education: 11 years
- Complete higher education: 16 years
- Graduate studies: 16 or more years

Sufficient information on level and years of schooling is available to compute this variable in all household surveys.

2. Mean Schooling for household members twelve years old and older

1	0 years	0
2	Between 0 and 4 years	1.657
3	Between 4 and 5 years	2.9947
4	Between 5 and 10 years	4.969
5	Between 10 and 11 years	7.6387
6	Between 11 and 15 years	9.4425
7	Between 15 and 16 years	10.69
8	16 years or more	11.1396

Using the coding scheme described for calculating educational attainment for the household head, we calculate mean years of schooling for all household members 12 and older. Sufficient information is available to compute this variable in all household surveys.

3. Firm size and provision of Social Security benefits for the household head

1	Without benefits and either works alone or does not work	0
2	Without benefits and works in firm with 2 to 9 employees	1.166
3	Without benefits and works in firm with 10 or more employees	2.6545
4	With benefits and either works alone or does not work	3.9539
5	Without benefits and works in firm with 2 to 9 employees	5.8427
6	Without benefits and works in firm with 10 or more employees	6.9718

Assigning response categories for this index component requires information about employment status, social security benefits (health insurance and pension benefits), and firm size:

- *Employment status* is available in all household surveys.
- *Firm size* is not available in the 2005 DHS. We therefore use ordered probit models to predict the probability of falling into each of the three firm size categories (1 employee, 2-9 employees, 10 or more employees). We then select the category with the highest predicted probability. To obtain parametric estimates of the relationship between a variety of observable household characteristics (demographic characteristics, education,

and regional controls among urban residents) and firm size, we estimate these ordered probit models using the 2003 ECV

- *Social Security benefits* consist of two components: health insurance benefits and pension benefits:
  - *Health Insurance Benefits.* Health insurance status is judged in each household survey in the following way:
    ECV 2003: Has health insurance if affiliated with "ISS," "Caja de Prevision," "army/police" insurance scheme, "Ecopetrol" scheme, the "educational system" scheme, or an "EPS – different to ISS or Caja de Prevision." Those with insurance through an "ARS" or "Empresa solidaria" are excluded.
    DHS 2005: Has health insurance if affiliated with "ISS," "EPS," "Public Agency," "army/police" insurance scheme, "Ecopetrol" scheme, the "educational system"scheme, or "Foncolpuertos." Those with insurance through an "ARS" are excluded.
  - *Pension Benefits.* Pension benefits are judged according to affiliation with the public or private pension system. This information is available in the 2003 ECV but not in the 2005 DHS.

In the 2003 ECV, Social Security benefits are judged according to having health insurance and/or pension benefits. In the 2005 DHS, Social Security benefits are judged according to health insurance benefits.

(B) Demographics, Income, and Labor Force Participation

(4) Proportion of children six years old and under (as share of children under age eighteen)

1	Greater than 0.65	0
2	From 0 to 0.65	0.2237
3	Zero	1.4761

Sufficient information is available to compute this variable in all household surveys.

(5) Proportion of household members employed (as a share of those older than twelve)

1	Less than 0.30	0
2	From 0.30 to 0.60	0.6717
3	From 0.60 to 0.90	1.739
4	Greater than 0.90	4.0149

For constructing this proportion, employment is defined as having worked in the preceding week, not having worked but having regular job, or receiving payment for working more than one hour. Sufficient information is available to compute this variable in all household surveys.

(6) Per capita income indexed to the minimum wage (all types of income are counted)

1	Up to 0.15	0
2	Above 0.15 up to 0.25	0.8476
3	Above 0.25 up to 0.35	2.1828
4	Above 0.35 up to 0.50	3.5362
5	Above 0.50 up to 0.75	5.3636
6	Above 0.75 up to 1.00	7.0827
7	Above 1.00 up to 1.25	8.2489
8	Above 1.25 up to 1.50	9.4853
9	Above 1.50 up to 2.00	10.2098
10	Above 2.00 up to 3.00	11.3999
11	Above 3.00 up to 4.00	13.0872
12	Above 4.00	13.7378

To calculate per capita income for a family, we define income to include labor income from primary and secondary jobs (both for the employed and self-employed) and pension benefits for retirees. In-kind subsides are excluded. We obtained nominal minimum wage information (summarized below) from The Colombian Central Bank's *Monetary and Financial Statistics*:

Year	**Minimum wage (in Colombian pesos)**
2003	332,000.0
2005	381,500.0

Income variables are available only in the 2003 ECV. For the 2005 DHS, we use ordered probit models to predict the probability of falling into each of 12 discrete categories; we then select the category with the highest predicted probability. To obtain parametric estimates of the relationship between a variety of observable household characteristics (demographic characteristics, education, and regional controls among urban residents) and firm size, we estimate these ordered probit models using the 2003 ECV.

(C) Housing Characteristics

(7) Number of rooms per person

1	Less than 0.20	0
2	0.20 to 0.30	0.5584
3	0.30 to 0.40	1.6535
4	0.40 to 0.70	2.5727
5	0.70 to 1.00	4.3886
6	1.00 to 4.00	6.0042
7	Greater than 4.00	8.3828

To assign response categories for this index component, rooms are defined as rooms exclusively used by household members (including living rooms but excluding kitchens, bathrooms, garages, and rooms used for business). This information is available in the 2003 ECV. For the 2005 DHS, we use number of rooms used by household members for sleeping.

(8) Primary wall material

1	Without walls or with bamboo or other organic materials	0
2	Zinc, cloth, cardboard, cans	0.2473
3	Raw wood	2.0207
4	Mud and cane wall	4.8586
5	Adobe, wide mud wall	6.2845
6	Block, bricks, stone, prefabricated material, polished wood	7.7321

Information on wall material is available in both the 2003 ECV and the 2005 DHS.

(9) Primary roof material

1	Straw or palm leaves	0
2	Recycled household materials (cardboard, cans, burlap sacks, etc)	2.1043
3	Zinc, asbestos, cement, without ceiling	3.7779
4	Clay tile, zinc, asbestos, cement, with ceiling	5.0973

Information on primary roof material is available only in the 1997 ECV. We therefore use parametric estimates of the relationship between observable characteristics (number of rooms, floor material and regional dummies among urban households) and roof material obtained from an ordered probit model fit with the 1997 ECV to predict the probability of falling into each roof material category shown above. We assign the category with the highest predicted probability.

(10) Primary floor material

1	Dirt	0
2	Raw wood, boards	2.9037
3	Cement	3.6967
4	Floor tile (clay, vinyl), brick or paving tile	5.8712
5	Wall to wall carpet, marble, polished wood	6.8915

Sufficient information is available to compute this variable in all household surveys.

(11) Number of appliances (among those on a pre-determined list)

1	No appliances	0
2	1-3 basic appliances basics	2.1435
3	4 basic appliances without laundry machine	3.0763
4	3 or more basic appliances with laundry machine	4.7194

For this SISBEN index component, four appliances are considered “basic” (TVs, refrigerators, blenders, and air conditioners) and a washing/laundry machine is treated separately as shown in the table above. All necessary information about appliances is present in both the 2003 ECV and the 2005 DHS.

(D) Access to Public Utilities

(12) Water source

1	River or spring	0
2	Public fountain or other source	1.1606
3	Well without water pump, container or rain water	2.6497
4	Well with water pump	4.6037
5	Container truck	6.1693
6	Aqueduct	7.2554

All necessary information for assigning response categories is available in the 2003 ECV. In the 2005 DHS, we classify "bottled water" as "aqueduct."

(13) Sewage disposal

1	No sewage	0
2	Latrine	2.4519
3	Toilet without connection to sewer or septic tank	3.3323
4	Toilet with connection to septic tank	3.9615
5	Toilet with connection to sewer	6.8306

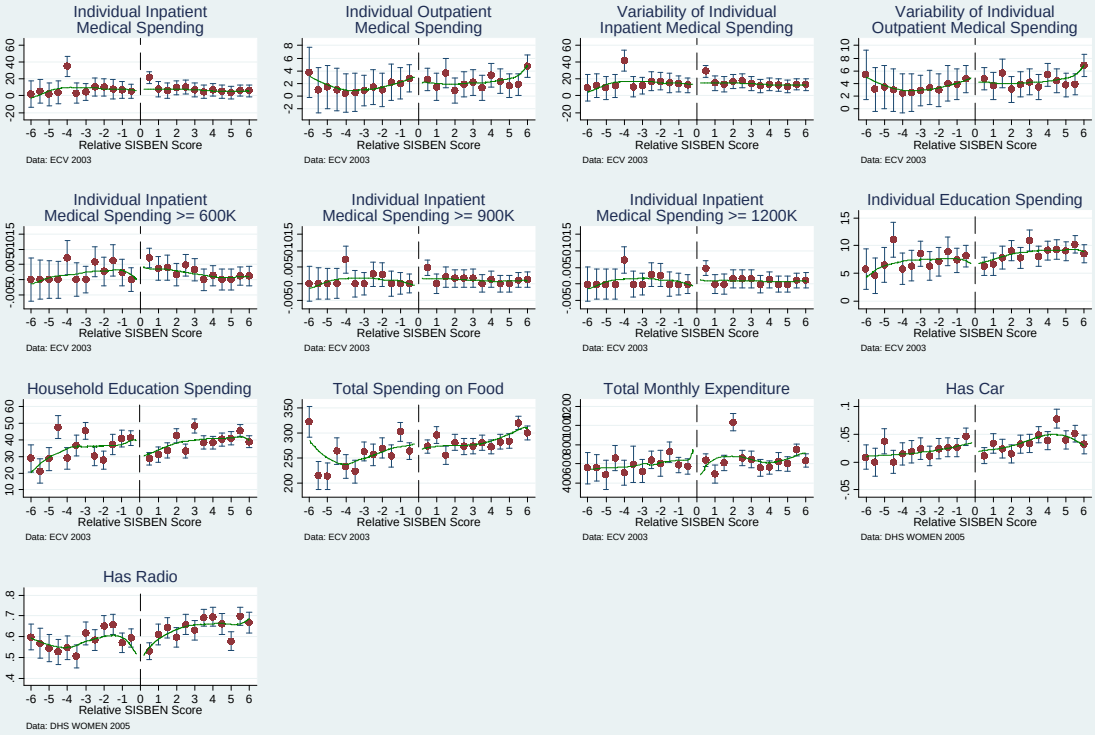
Information on sewage disposal is available in all household surveys, but in the 2005 DHS, some minor coding assumptions were necessary. In the 2005 DHS, we code both "traditional pit toilet" and "traditional toilet to sea/river" as "latrine."

(14) Garbage disposal

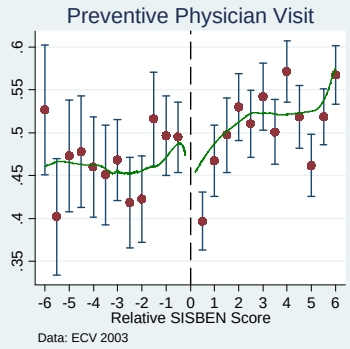
1	Yard, lot, river, etc.	0
2	Local container or public trashcan	2.1291
3	Picked up by public services	3.2701

Information on garbage disposal is available in both the 2005 DHS and the 2003 ECV.

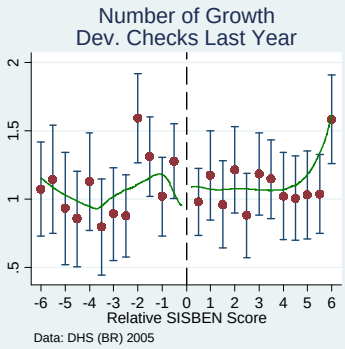
Appendix 3, Figure 1: Risk Protection and Portfolio Choice



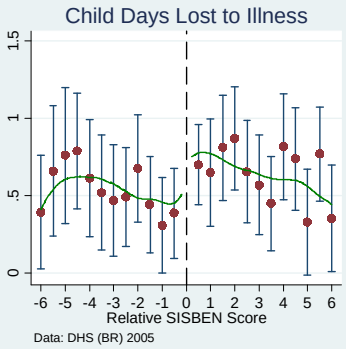
Appendix 3, Figure 2: Use of Preventive Care and Health Status



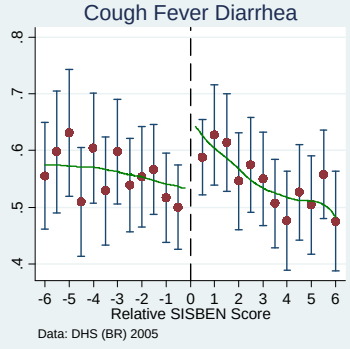
Data: ECV 2003



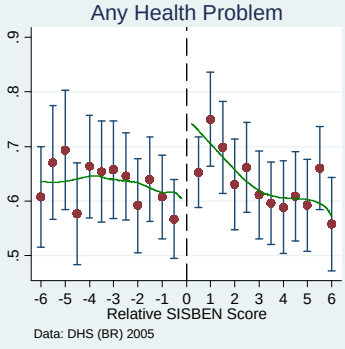
Data: DHS (BR) 2005



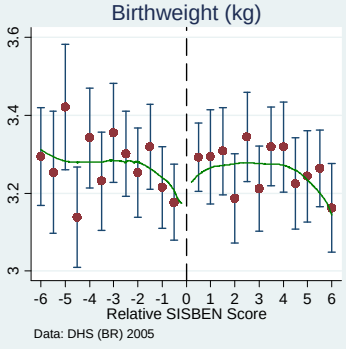
Data: DHS (BR) 2005



Data: DHS (BR) 2005

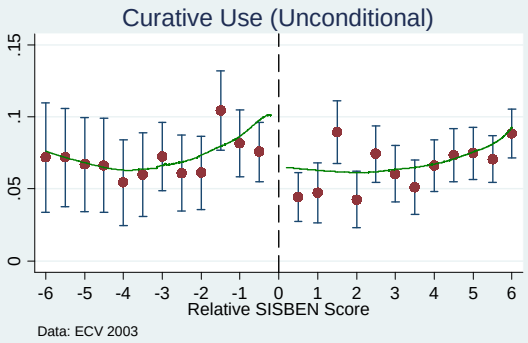


Data: DHS (BR) 2005

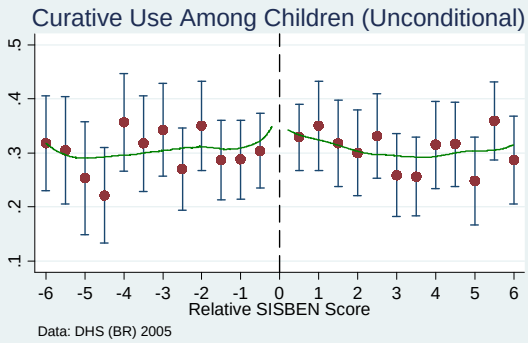


Data: DHS (BR) 2005

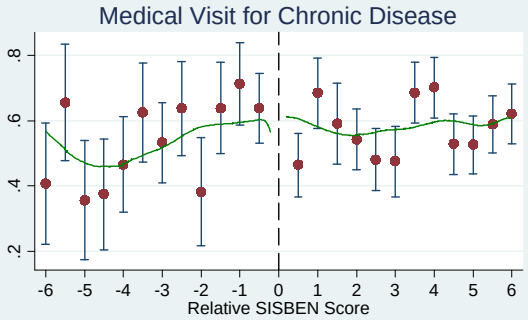
Appendix 3, Figure 3: Use of Curative Care



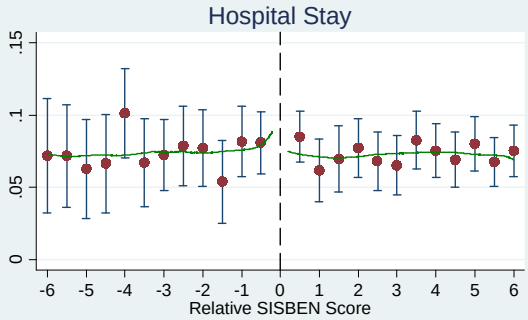
Data: ECV 2003



Data: DHS (BR) 2005

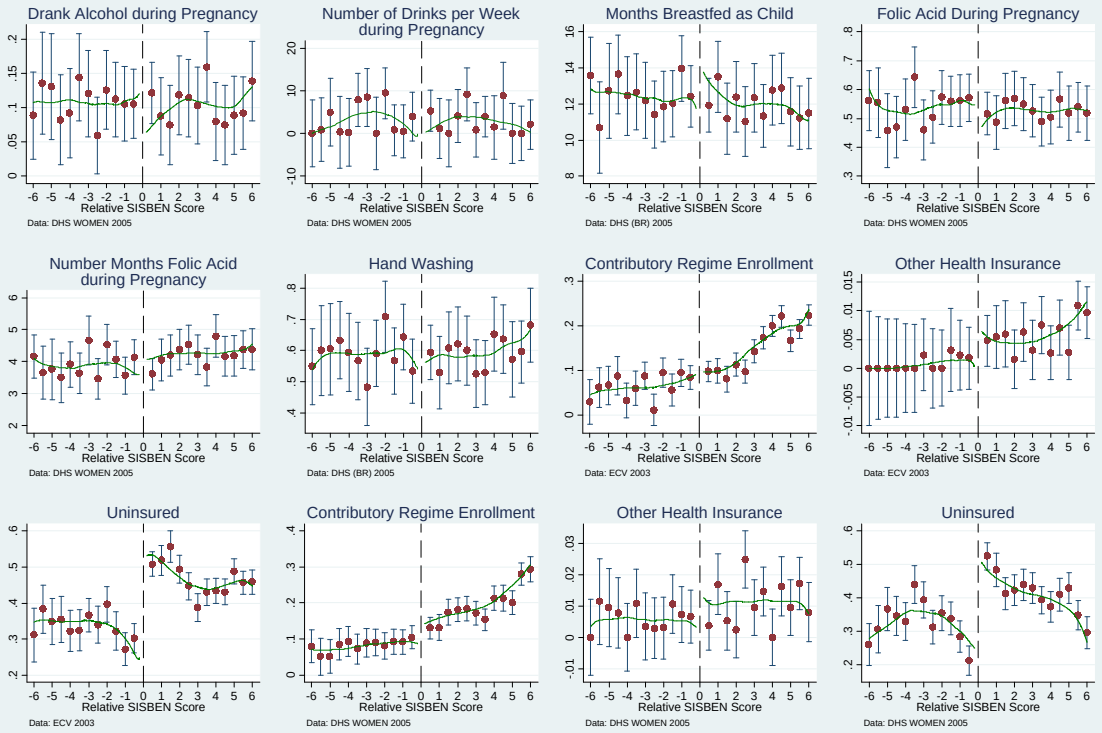


Data: ECV 2003



Data: ECV 2003

Appendix 3, Figure 4: Behavioral Distortions



APPENDIX 4 TABLE A1:
ROBUSTNESS OF RISK PROTECTION AND PORTFOLIO CHOICE RESULTS

Panel A: Risk Protection

Model:	Individual Inpatient Medical Spending	Individual Outpatient Medical Spending	Variability of Individual Inpatient Medical Spending	Variability of Individual Outpatient Medical Spending	Individual Inpatient Medical Spending ≥ 600,000	Individual Inpatient Medical Spending ≥ 900,000	Individual Inpatient Medical Spending ≥ 1,200,000
Bandwidth 2	-60,371*	3,562	-62,109*	2,620	-0.03*	-0.02**	-0.02**
	(33,166)	(3,307)	(32,860)	(3,160)	(0.01)	(0.01)	(0.01)
Bandwidth 3	-46,561*	704	-48,237*	-501	-0.02	-0.01*	-0.01
	(27,208)	(3,939)	(26,931)	(3,856)	(0.01)	(0.01)	(0.01)
Bandwidth 4	-62,047**	2,544	-62,512**	1,207	-0.03*	-0.02**	-0.02*
	(30,387)	(4,018)	(30,152)	(3,921)	(0.01)	(0.01)	(0.01)
Bandwidth 2 with Higher Order SISBEN Polynomial	-149,854*	10,029	-151,503*	9,092	-0.06*	-0.04*	-0.05*
	(84,609)	(7,946)	(84,197)	(7,639)	(0.03)	(0.02)	(0.02)
Bandwidth 3 with Higher Order SISBEN Polynomial	-84,792**	-93.4	-86,252**	-1,169	-0.03**	-0.03**	-0.03**
	(43,097)	(5,831)	(42,730)	(5,708)	(0.02)	(0.01)	(0.01)
Bandwidth 4 with Higher Order SISBEN Polynomial	-42,084	-1,483	-42,573	-2,700	-0.02*	-0.01*	-0.01
	(30,161)	(5,155)	(29,886)	(5,088)	(0.01)	(0.01)	(0.01)
Bandwidth 2 with SISBEN×Eligible Interactions	-54,935*	4,128	-56,529*	3,238	-0.02*	-0.02**	-0.02**
	(29,227)	(3,518)	(28,916)	(3,375)	(0.01)	(0.01)	(0.01)
Bandwidth 3 with SISBEN×Eligible Interactions	-43,404*	1,634	-45,170*	429	-0.02	-0.01*	-0.01*
	(25,327)	(3,802)	(25,052)	(3,709)	(0.01)	(0.01)	(0.01)
Bandwidth 4 with SISBEN×Eligible Interactions	-64,040**	4,071	-64,544**	2,712	-0.02*	-0.02**	-0.02**
	(31,648)	(3,723)	(31,423)	(3,611)	(0.01)	(0.01)	(0.01)
Bandwidth 2 without County Fixed Effects	-63,012	-1,996	-64,910	-2,957	-0.03*	-0.02	-0.02
	(41,233)	(6,967)	(40,957)	(6,894)	(0.02)	(0.01)	(0.01)
Bandwidth 3 without County Fixed Effects	-44,629	-56.4	-46,507	-1,216	-0.02	-0.01	-0.01
	(29,815)	(5,175)	(29,589)	(5,112)	(0.01)	(0.01)	(0.01)
Bandwidth 4 without County Fixed Effects	-61,221*	1,343	-61,853*	24.02	-0.03*	-0.02*	-0.01*
	(33,074)	(5,477)	(32,859)	(5,403)	(0.01)	(0.01)	(0.01)
Bandwidth 2 Local Linear Regression	-89,480	-121	-91,184	-1,032	-0.04**	-0.02*	-0.03
	(65,470)	(9,519)	(58,670)	(9,535)	(0.02)	(0.01)	(0.02)
Bandwidth 3 Local Linear Regression	-60,282	156	-62,112	-925	-0.03**	-0.02	-0.02*
	(37,413)	(6,094)	(41,334)	(5,492)	(0.01)	(0.01)	(0.01)
Bandwidth 4 Local Linear Regression	-52,559	856	-53,164	-354	-0.02*	-0.02*	-0.01*
	(33,833)	(5,153)	(33,447)	(5,364)	(0.01)	(0.01)	(0.01)
Data Source	ECV	ECV	ECV	ECV	ECV	ECV	ECV

Panel B: Portfolio Choice

Model:	Individual Education Spending	Household Education Spending	Total Spending on Food	Total Monthly Expenditure	Has Car	Has Radio
Bandwidth 2	-342	30,366	32,136	-33,826	0.07	0.14
	(4,963)	(25,733)	(104,871)	(305,878)	(0.04)	(0.11)
Bandwidth 3	2,599	28,059	-1,495	-320,415	0.08**	0.14
	(4,554)	(23,228)	(88,770)	(356,917)	(0.04)	(0.11)
Bandwidth 4	2,613	25,670	18,654	-348,373	0.09**	0.23**
	(4,667)	(23,564)	(93,938)	(374,759)	(0.04)	(0.10)
Bandwidth 2 with Higher Order SISBEN Polynomial	-7,023	7,150	3,136	-776,577*	0.09*	0.19
	(11,759)	(46,641)	(185,284)	(457,479)	(0.05)	(0.13)
Bandwidth 3 with Higher Order SISBEN Polynomial	-2,350	27,095	27,129	-362,101	0.08*	0.10
	(6,365)	(33,363)	(128,735)	(396,651)	(0.05)	(0.12)
Bandwidth 4 with Higher Order SISBEN Polynomial	1,842	28,281	14,132	-319,591	0.08*	0.10
	(4,564)	(25,329)	(94,706)	(348,511)	(0.04)	(0.11)
Bandwidth 2 with SISBEN×Eligible Interactions	-1384	30,593	32,790	-63,185	0.07	0.11
	(5,298)	(27,146)	(103,778)	(330,739)	(0.04)	(0.11)
Bandwidth 3 with SISBEN×Eligible Interactions	1,884	25,782	-837	-326,491	0.08**	0.12
	(4,711)	(25,127)	(92,515)	(367,999)	(0.04)	(0.11)
Bandwidth 4 with SISBEN×Eligible Interactions	2,243	24,374	27,751	-230,269	0.09**	0.22**
	(4,844)	(26,122)	(96,361)	(356,416)	(0.04)	(0.10)
Bandwidth 2 without County Fixed Effects	3,595	40,950	-76	7,924	0.06	0.13
	(5,148)	(26,487)	(125,627)	(448,543)	(0.05)	(0.14)
Bandwidth 3 without County Fixed Effects	4,882	31,351	-5,734	-315,118	0.08*	0.17
	(4,473)	(22,030)	(105,874)	(404,085)	(0.05)	(0.13)
Bandwidth 4 without County Fixed Effects	4,495	29,234	8,948	-466,629	0.08**	0.27**
	(4,832)	(23,487)	(114,144)	(439,817)	(0.04)	(0.12)
Bandwidth 2 Local Linear Regression	8,457	60,037	-29,196	-365,953	0.10	0.16
	(12,796)	(39,775)	(148,304)	(10,778,000)	(0.07)	(0.15)
Bandwidth 3 Local Linear Regression	7,512	50,717	-10,243	-202,306	0.09*	0.15
	(6,542)	(37,386)	(122,781)	(2,543,245)	(0.05)	(0.13)
Bandwidth 4 Local Linear Regression	7,619	46,440	3,083	-238,253	0.09*	0.18*
	(5,418)	(28,926)	(115,261)	(666,015)	(0.05)	(0.11)
Data Source	ECV	ECV	ECV	ECV	DHS	DHS

Individual-level "urban" data used from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column; all estimates are 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The first three rows report estimates using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The fourth through sixth rows control for squared, cubic, and fourth power terms of SISBEN scores using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The seventh through ninth rows include interactions between SISBEN scores and an indicator variable for eligibility according to our calculations. The tenth through twelfth rows do not condition on county fixed effects and use samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The thirteenth through fifteenth rows report non-parametric local linear regression estimates obtained through the following process. First, local linear regression functions (with triangle kernels) are used to estimate conditional means (conditioning only on SISBEN_diff) of outcome variables on either side of the eligibility threshold. Second, this estimation process is repeated for enrollment in the SR. Finally, Wald statistics are constructed using differences in outcome variable means on either side of the threshold as numerators and the difference in SR enrollment means on either side of the threshold as the denominator; 250 bootstrap replications are used to estimate standard errors. All specifications otherwise include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

APPENDIX 4 TABLE A2:
ROBUSTNESS OF USE OF PREVENTIVE MEDICAL CARE AND HEALTH STATUS RESULTS

	Use of Preventive Care		Health Status (Children)			
Model:	Preventive Physician Visit	Number of Growth Dev. Checks Last Year	Child Days Lost to Illness	Cough, Fever, Diarrhea	Any Health Problem	Birthweight (KG)
Bandwidth 2	0.29*	1.24*	-1.30*	-0.35*	-0.26	-0.38
	(0.17)	(0.74)	(0.71)	(0.21)	(0.19)	(0.33)
Bandwidth 3	0.20	1.75**	-1.14	-0.44**	-0.45**	-0.31
	(0.15)	(0.73)	(0.73)	(0.21)	(0.20)	(0.31)
Bandwidth 4	0.24	2.06***	-1.43*	-0.59**	-0.60***	-0.16
	(0.15)	(0.76)	(0.80)	(0.23)	(0.22)	(0.42)
Bandwidth 2 with Higher Order SISBEN Polynomial	0.40	1.63**	-1.05*	-0.31	-0.21	-0.44
	(0.31)	(0.80)	(0.61)	(0.21)	(0.20)	(0.34)
Bandwidth 3 with Higher Order SISBEN Polynomial	0.48**	1.18	-1.13	-0.35	-0.26	-0.28
	(0.22)	(0.81)	(0.70)	(0.23)	(0.21)	(0.36)
Bandwidth 4 with Higher Order SISBEN Polynomial	0.26*	1.67**	-0.96	-0.39*	-0.39*	-0.33
	(0.15)	(0.78)	(0.74)	(0.23)	(0.22)	(0.36)
Bandwidth 2 with SISBEN×Eligible Interactions	0.27	1.16	-1.30*	-0.34*	-0.26	-0.37
	(0.17)	(0.73)	(0.70)	(0.20)	(0.19)	(0.32)
Bandwidth 3 with SISBEN×Eligible Interactions	0.21	1.73**	-1.13	-0.43**	-0.45**	-0.32
	(0.15)	(0.72)	(0.72)	(0.21)	(0.20)	(0.31)
Bandwidth 4 with SISBEN×Eligible Interactions	0.23	2.06***	-1.43*	-0.58**	-0.60***	-0.18
	(0.16)	(0.76)	(0.80)	(0.23)	(0.22)	(0.42)
Bandwidth 2 without County Fixed Effects	0.46***	0.86	-1.24*	-0.42*	-0.37*	-0.51
	(0.18)	(0.73)	(0.72)	(0.23)	(0.22)	(0.44)
Bandwidth 3 without County Fixed Effects	0.32**	1.39*	-1.48**	-0.49**	-0.54**	-0.55
	(0.14)	(0.71)	(0.75)	(0.23)	(0.23)	(0.42)
Bandwidth 4 without County Fixed Effects	0.31*	1.37*	-1.72**	-0.59**	-0.61***	-0.47
	(0.16)	(0.71)	(0.77)	(0.24)	(0.23)	(0.52)
Bandwidth 2 Local Linear Regression	0.72**	0.99	-1.21	-0.41*	-0.39	2.72*
	(0.32)	(0.80)	(0.80)	(0.24)	(0.26)	(1.54)
Bandwidth 3 Local Linear Regression	0.55***	1.00	-1.47**	-0.43*	-0.43*	2.70*
	(0.19)	(0.75)	(0.60)	(0.26)	(0.25)	(1.57)
Bandwidth 4 Local Linear Regression	0.45***	1.21*	-1.53**	-0.45**	-0.48**	2.49*
	(0.16)	(0.73)	(0.64)	(0.23)	(0.24)	(1.37)
Data Source	ECV	DHS	DHS	DHS	DHS	DHS

Individual-level "urban" data used from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column; all estimates are 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The first three rows report estimates using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The fourth through sixth rows control for squared, cubic, and fourth power terms of SISBEN scores using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The seventh through ninth rows include interactions between SISBEN scores and an indicator variable for eligibility according to our calculations. The tenth through twelfth rows do not condition on county fixed effects and use samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The thirteenth through fifteenth rows report non-parametric local linear regression estimates obtained through the following process. First, local linear regression functions (with triangle kernels) are used to estimate conditional means (conditioning only on SISBEN_diff) of outcome variables on either side of the eligibility threshold. Second, this estimation process is repeated for enrollment in the SR. Finally, Wald statistics are constructed using differences in outcome variable means on either side of the threshold as numerators and the difference in SR enrollment means on either side of the threshold as the denominator; 250 bootstrap replications are used to estimate standard errors. All specifications otherwise include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

APPENDIX 4 TABLE A3:
ROBUSTNESS OF USE OF CURATIVE MEDICAL CARE RESULTS

Model:	Curative Use (Not Conditonal on Health Status)	Curative Use among Children (Not Conditional on Health Status)	Medical Visit for Chronic Disease	Hospital Stay
Bandwidth 2	0.13**	-0.05	0.51	-0.04
	(0.06)	(0.19)	(0.34)	(0.06)
Bandwidth 3	0.15***	-0.08	0.26	-0.05
	(0.06)	(0.18)	(0.24)	(0.06)
Bandwidth 4	0.16***	-0.15	0.60**	-0.03
	(0.06)	(0.19)	(0.30)	(0.06)
Bandwidth 2 with Higher Order SISBEN Polynomial	0.23*	-0.05	1.79	-0.08
	(0.14)	(0.19)	(2.13)	(0.13)
Bandwidth 3 with Higher Order SISBEN Polynomial	0.18**	0.03	0.83	-0.07
	(0.08)	(0.20)	(0.65)	(0.08)
Bandwidth 4 with Higher Order SISBEN Polynomial	0.14**	-0.05	0.21	-0.03
	(0.06)	(0.20)	(0.23)	(0.06)
Bandwidth 2 with SISBEN×Eligible Interactions	0.12**	-0.05	0.56*	-0.03
	(0.06)	(0.18)	(0.34)	(0.06)
Bandwidth 3 with SISBEN×Eligible Interactions	0.15***	-0.08	0.28	-0.04
	(0.06)	(0.18)	(0.24)	(0.06)
Bandwidth 4 with SISBEN×Eligible Interactions	0.16***	-0.15	0.60**	-0.03
	(0.06)	(0.19)	(0.30)	(0.06)
Bandwidth 2 without County Fixed Effects	0.14**	-0.18	0.41	0.00
	(0.06)	(0.21)	(0.27)	(0.06)
Bandwidth 3 without County Fixed Effects	0.14***	-0.21	0.22	-0.01
	(0.05)	(0.20)	(0.20)	(0.05)
Bandwidth 4 without County Fixed Effects	0.16***	-0.23	0.55**	0.00
	(0.06)	(0.20)	(0.24)	(0.06)
Bandwidth 2 Local Linear Regression	0.13	-0.20	0.99	0.01
	(0.08)	(0.22)	(4.34)	(0.09)
Bandwidth 3 Local Linear Regression	0.13**	-0.19	0.55*	0.00
	(0.07)	(0.22)	(0.32)	(0.06)
Bandwidth 4 Local Linear Regression	0.14**	-0.22	0.48*	0.01
	(0.06)	(0.19)	(0.27)	(0.06)
Data Source	ECV	DHS	ECV	ECV

Individual-level "urban" data used from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column; all estimates are 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The first three rows report estimates using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The fourth through sixth rows control for squared, cubic, and fourth power terms of SISBEN scores using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The seventh through ninth rows include interactions between SISBEN scores and an indicator variable for eligibility according to our calculations. The tenth through twelfth rows do not condition on county fixed effects and use samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The thirteenth through fifteenth rows report non-parametric local linear regression estimates obtained through the following process. First, local linear regression functions (with triangle kernels) are used to estimate conditional means (conditioning only on SISBEN_diff) of outcome variables on either side of the eligibility threshold. Second, this estimation process is repeated for enrollment in the SR. Finally, Wald statistics are constructed using differences in outcome variable means on either side of the threshold as numerators and the difference in SR enrollment means on either side of the threshold as the denominator; 250 bootstrap replications are used to estimate standard errors. All specifications otherwise include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.

APPENDIX 4 TABLE A4:
ROBUSTNESS OF BEHAVIORAL DISORTION RESULTS (*EX ANTE* MORAL HAZARD)

Model:	Drank Alcohol during Pregnancy	Number of Drinks per Week during Pregnancy	Months Breastfed as Child	Folic Acid During Pregnancy	Number Months Folic Acid during Pregnancy	Hand Washing
Bandwidth 2	-0.05	-21.59	-0.82	0.15	0.52	-0.05
	(0.12)	(136.39)	(5.27)	(0.17)	(1.46)	(0.09)
Bandwidth 3	0.02	1.20	-0.66	0.27	0.72	-0.11
	(0.10)	(289.72)	(4.63)	(0.17)	(1.17)	(0.08)
Bandwidth 4	-0.01	6.39	-0.18	0.16	0.47	-0.02
	(0.10)	(264.01)	(4.91)	(0.16)	(1.17)	(0.08)
Bandwidth 2 with Higher Order SISBEN Polynomial	-0.09	39.35	-0.79	0.04	2.82	-0.07
	(0.14)	(428.76)	(5.64)	(0.20)	(2.20)	(0.11)
Bandwidth 3 with Higher Order SISBEN Polynomial	-0.02	0.10	-0.63	0.12	1.84	-0.02
	(0.12)	(130.13)	(5.55)	(0.18)	(1.63)	(0.10)
Bandwidth 4 with Higher Order SISBEN Polynomial	0.00	4.59	-0.46	0.18	1.08	-0.06
	(0.11)	(106.85)	(4.97)	(0.17)	(1.25)	(0.09)
Bandwidth 2 with SISBEN×Eligible Interactions	-0.05	-11.75	-0.54	0.14	0.34	-0.06
	(0.11)	(123.55)	(5.20)	(0.17)	(1.45)	(0.09)
Bandwidth 3 with SISBEN×Eligible Interactions	0.02	24.31	-0.34	0.27	0.63	-0.11
	(0.10)	(230.17)	(4.60)	(0.17)	(1.16)	(0.08)
Bandwidth 4 with SISBEN×Eligible Interactions	-0.01	20.30	-0.07	0.16	0.50	-0.02
	(0.10)	(198.63)	(4.90)	(0.16)	(1.17)	(0.08)
Bandwidth 2 without County Fixed Effects	-0.06	-19.65	4.79	0.18	1.46	0.00
	(0.12)	(56.39)	(5.24)	(0.19)	(1.47)	(0.10)
Bandwidth 3 without County Fixed Effects	-0.01	-10.84	3.66	0.25	0.86	-0.07
	(0.11)	(20.51)	(4.57)	(0.18)	(1.18)	(0.09)
Bandwidth 4 without County Fixed Effects	-0.01	-3.43	2.91	0.14	0.91	0.00
	(0.11)	(14.97)	(4.63)	(0.17)	(1.18)	(0.08)
Bandwidth 2 Local Linear Regression	-0.06	-23.92	6.37	0.16	3.06	-0.20
	(0.16)	(353.52)	(5.90)	(0.23)	(2.50)	(0.53)
Bandwidth 3 Local Linear Regression	-0.01	-17.09	5.32	0.21	1.79	-0.07
	(0.12)	(257.54)	(4.99)	(0.20)	(2.09)	(0.55)
Bandwidth 4 Local Linear Regression	0.00	-13.82	4.14	0.17	1.21	0.01
	(0.10)	(130.82)	(4.60)	(0.16)	(1.22)	(0.46)
Data Source	DHS	DHS	DHS	DHS	DHS	DHS

Individual-level "urban" data used from the 2003 ECV and 2005 DHS. Dependent variables are shown at the top of each column; all estimates are 2SLS estimates for enrollment in the Subsidized Regime (SR), instrumenting for SR enrollment using simulated eligibility. The first three rows report estimates using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The fourth through sixth rows control for squared, cubic, and fourth power terms of SISBEN scores using samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The seventh through ninth rows include interactions between SISBEN scores and an indicator variable for eligibility according to our calculations. The tenth through twelfth rows do not condition on county fixed effects and use samples of individuals within two, three, and four SISBEN index points of county-specific eligibility thresholds (respectively). The thirteenth through fifteenth rows report non-parametric local linear regression estimates obtained through the following process. First, local linear regression functions (with triangle kernels) are used to estimate conditional means (conditioning only on SISBEN_diff) of outcome variables on either side of the eligibility threshold. Second, this estimation process is repeated for enrollment in the SR. Finally, Wald statistics are constructed using differences in outcome variable means on either side of the threshold as numerators and the difference in SR enrollment means on either side of the threshold as the denominator; 250 bootstrap replications are used to estimate standard errors. All specifications otherwise include SISBEN score, distance from the county-specific threshold, estrato dummy variables, and county fixed effects. Standard errors (clustered by household) are shown in parentheses below each estimate. *p<0.10, **p<0.05, ***p<0.01.