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ABSTRACT

Since Brazil's adoption of universal health care in 1988, the country's health care system has consisted of a mix of private providers and free public providers. We test whether income-based disparities in medical visits and medications remain in Brazil despite universal coverage using a nationally representative sample of over 48,000 households. Additional income is associated with less public sector utilization and more private sector utilization, both using simple correlations and regressions controlling for household characteristics and local area fixed effects. Importantly, the increase in private care use is greater than the drop in public care use. Also, income and unmet medical needs are negatively associated. These results suggest that access limitations remain for low-income households despite the availability of free public care.

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1 Introduction and Background

In 1990, Brazil adopted a universal health care system that uses network of public providers to deliver a full range of health services free of charge. The public system – called *sistema único de saúde* (SUS) – is guided by three main principles of universality, integrality, and decentralization. Universality means that health care is a universal right; it is the state's duty to provide health care to all citizens free of charge. Integrality means that public health assistance must comprise primary, secondary, and tertiary levels of care. Decentralization means that the management and organization of health services is the responsibility of the municipalities. The SUS is one of the world's largest public health care systems. Its ambulatory system consists of 56,640 units and assists 350 million cases annually, while 6,493 hospitals and 487,058 hospital beds are part of the SUS network. In 2001, the SUS conducted 250 million consultations, 200 million laboratory tests, and 70 million high complexity procedures (Rehem de Souza, 2002). The SUS network consists of a mix of public, non-profit, and for-profit providers, but all services are paid by the federal, state, and municipal governments (Uga and Santos, 2007).

In 1994, the Brazilian government added the Family Health Program (*Programa Saúde da Família*; PSF) to the public system in an effort to improve primary health care access and reduce service inequality. The PSF assigns a geographical area inhabited by an average of 3,450 and a maximum of 4,500 people to a team composed of one physician, one nurse, one nurse assistant, and four or more community health workers. While PSF physicians and nurses typically provide care at health facilities, community workers provide prevention and education services during household visits.

Brazil's private health care system – called *sistema suplementar de saúde* (SSS) – comprises those private institutions that do not belong to the SUS. Patients are responsible for their own medical bills in the private system. Individual and group health insurance plans are available to help defray the costs, but coverage rates are low. Though only 20% of the population participates in the SSS, it accounts for approximately half of the country's medical expenditures.

According to SUS principles, essential medicines are also to be provided universally and free of charge. However, in practice a large proportion of Brazilians resort to private pharmacies. The main reasons indicated in the literature are the prescription of drugs not included in the list of essential medications, the lack of stock at local public pharmacies, and bureaucratic problems such as improper prescriptions (Bertoldi et al. 2009, Oliveira Silva Naves and Silver 2005).

Empirical research to date suggests that universal health care in Brazil has improved health care utilization and health but that barriers to access remain. Thume et al. (2010) and Rodriguez et al. (2009) found that the PSF increased health care utilization by the elderly. Macinko et al. (2006 and 2007), Rasella et al. (2010), and Morsch et al. (2001) documented a negative association between PSF coverage and infant or five-year mortality rates. Goldbaum et al. (2005) compared two areas of São Paulo City and found that disparities in health care utilization on the bases of income and education were more evident in the area that was not covered by the PSF. However, according to Xu et al. (2003) Brazil had the second-highest prevalence of catastrophic medical expenditures out of 59 countries despite the availability of free public care. Rodriguez et al. (2009) found that less than half of elderly individuals with chronic conditions had a medical visit in the preceding six months. Barros and Bertoldi (2008) examined a sample of 869 households and found that the proportion of income spent on private health services was similar across economic groups, though in a larger sample Uga and Santos (2007) found that this proportion falls with income. In a study of the northeastern state of Ceará, Maciel et al. (2010) show that the need of physicians to have multiple jobs is a major obstacle to SUS efficacy. Bos (2007) estimated a positive relationship between the number of public outpatient clinics in a municipality and residents' probability of using the public system, suggesting that utilization could be increased by further expansion.

We contribute to this growing literature in three ways.[3] First, we use a large nationally representative sample to test for income-based disparities in health care utilization in Brazil. Second, we examine whether these disparities are purely driven by differences in the private sector or whether disparities exist in the public sector as well. Third, we test whether income influences the amount of medical needs left unmet because of cost despite the existence of free public care.

2 Data and Methods

2.1 Data

Our main data source is the 2002-2003 wave of the *Pesquisa de Orçamentos Familiares* (POF; Survey of Family Budget), a nationally representative dataset of about 50,000 households collected by the Brazilian Institute of Geography and Statistics. The POF contains detailed information on all types of expenditures (including doctor visits and medicines) and income sources by the families in a one year period, as well as socioeconomic and demographic characteristics of the household members.

We also use the next wave of the POF, 2008-2009, to confirm the results from 2002-2003. Because public (as opposed to private) doctor visits and the variables on unmet medical needs are not available, a complete analysis using 2008-2009 data is not feasible. The variables and summary statistics described below, as well as the main regressions in the next section, are therefore from 2002-2003.[1]

In the POF, total household income is reported per month; we multiply it by twelve to approximate annual income. Table 1 lists the independent variables for our empirical analysis, which include the natural log (because of the skewness of the income distribution) of income as well as sets of controls for demographics, religion, health, and living conditions. Table 1 also reports the variable means from our analysis sample of 48,225.

The (June 2006 version of the) 2002-2003 POF also collects data on services and products consumed by the surveyed individuals' households, either paid for by them or obtained free of charge, as well as those that they intended to consume but could not afford. In the medical visits instrument, each record corresponds to a narrowly defined service reported by the individual, its associated cost, a payment method including the option *donation*, its source of provision (e.g. private provider, different types of health insurance, or public provision), and the provider location (e.g. SUS or SSS)..We use this information to construct the following dependent variables reflecting household-level health care utilization: private doctor visits, public doctor visits, all doctor visits, doctor visits needed but not made because of cost, private medications, public medications, all medications, and medications needed but not consumed because of cost.

Private doctor visits are defined as reported services provided by a private doctor, health organization, or other health company for which the individual was charged a cost. Services provided by health organizations associated with labor unions are included as health companies and any payment made by the individuals to these organizations in exchange of services is included as an out-of-pocket expenditure. We define public visits as those classified as a *donation* whose provider location was coded as *SUS*.[2] Medical visits needed but not afforded are those with the payment method *restriction*. All doctor visits are simply the sum of private and public visits.[3] The POF reports the number of medical visits in the preceding three months; we multiply the responses by four to obtain an annualized measure. The following doctor specializations were included in our analysis: ob/gynecologist, pediatrician, cardiologist, ophtalmologist, orthopedist, neurologist, psychiatrist, dermatologist, allergist, gastroenterologist, generalist, geriatrician, homeopat, nefrologist, nutricionist, obstetric, oncologist, otolaryngologist, pneumologist, rheumatologist, endocrinologist, proctologist, urologist, angiologist, other medical specialization.

Pharmaceutical expenditures are collected in the POF through a separate instrument. Each record corresponds to a medicine reported by the individual, its associated cost, a payment method including the option *donation*, and the provider location. Both prescription and over-the-counter medications are included. Our procedure to classify private, public, all, and not afforded medications is analogous to the one applied for physician services. The POF's questions on medications are for the preceding month; we multiply by twelve to obtain approximations of annual consumption.

Table 2 gives, for each of the eight dependent variables, their means, correlations with ln(income), proportion of the population for whom utilization is non-zero, and the mean among those for whom utilization is nonzero. Only 16% of households report any private medical visits, while 24% report any public visits and only 38% report accessing either sector. These means suggest some limitations in access, and these limitations are correlated with income. The correlation between ln(income) and private visits is 0.27, while the correlation between income and overall visits is a smaller 0.127 but still positive and significant. The correlation is weaker for overall visits than for private visits because income is negatively correlated with public visits (-0.071).

Utilization patterns are quite different for medications, as 78% of households obtain at least one medication and the vast majority of medications are purchased in the private sector. The higher participation rate for medicines relative to medical visits could be explained by over-the-counter medications not requiring physician access. The private sector accounting for a much larger share of medications than medical visits is consistent with the fact that not all prescriptions or recommendations for over-the-counter drugs issued by SUS doctors are filled for free at a public facility. Despite these differences in overall utilization, we observe the same patterns for income-based disparities: ln(income) is positively correlated with overall and private medications and negatively correlated with public medications.

The means and correlations for our most direct measures of access limitations – visits and medications needed but not consumed because of cost – also reveal interesting patterns. First, only 5.6% and 7% of households report any unmet needs for visits and medications, respectively. These relatively low rates are consistent with improved access as a result of universal coverage. However, the correlations show that unmet medical needs because of cost fall with income, suggesting that some pro-rich disparities still remain.

3 Methods

We use regression analysis to estimate the association between ln(income) and the health care utilization measures controlling for the demographic, religion, health, and living condition factors summarized in Table 1 plus fixed effects for all 3,979 local geographic areas utilized in the POF. Brazil consists of 5,560 municipalities, so the POF's geographic areas are on average slightly larger than a municipality (Pan American Health Organization, 2008).The dependent variables are non-negative counts with a significant number of zeros, and the process governing the transition from non-participation to the first visit/medication is likely different than the process governing successive visits/medications after a household is already participating in the system. We therefore estimate two-part hurdle models where the first part predicts participation and the second part predicts number of visits/medications conditional on participation. We estimate a separate two-part model for each of the eight visit/medication dependent variables from Table 2. There is controversy over whether Heckman's sample selection model or the two-part model is the most appropriate when *potential* health care utilization/expenditure is the outcome of interest (see Madden, 2008 and Jones, 2000). However, our outcome of interest is *actual* utilization – Brazilians' actual consumption of health services, rather the services they would have consumed had they sought health care. Dow and Norton (2003) underscore that the appropriate model in this case is the two-part model as no selection bias is actually present in the sample. Sometimes this is referred as the "true zeros" case in the literature, since a zero observation represents no consumption, and not an unobserved value.

The first part estimates the conditional association between income and probability of a household having any visits/medications. We estimate linear probability models because probit and logistic fixed effects estimators are known to potentially suffer from bias – even with the number of observations per group as large as it is in our dataset – because of the incidental parameters problem (Kalbfleisch and Sprott, 1970; Hsiao, 1996; Greene, 2004). While the linear probability model has the drawback of predicting outside of the 0-1 range, its coefficient estimates are reliable (Angrist, 2001), and the purpose of our analysis is to accurately estimate average effects of the covariates rather than predict outcomes. Effect sizes obtained from probit and logistic regressions (available upon request) were similar. The regression equation is

$$P(y_{ij} > 0|income_{ij}, \mathbf{X_{ij}}) = \alpha_0 + \alpha_1 \ln(income_{ij}) + \mathbf{X'_{ij}}\boldsymbol{\alpha} + \gamma_j \tag{1}$$

where i and j are indices for household and local area, y is the number of visits/medications, *income* is household income, $\mathbf{X}$ is the vector of control variables, and γ is the local area fixed effect. Taking the log of income gives the coefficient a straightforward interpretation: the approximate percentage point effect on participation of a 100% increase in income. We compute heteroskedasticity-robust standard errors clustered by local area.[4]

The second part of the model estimates the relationships between the covariates and the number of visits/medications among those who cleared the participation "hurdle" in the first step. Since the dependent variables are counts, we estimate zero-truncated Poisson models of the form

$$E[y_{ij}|income_{ij}, \mathbf{X_{ij}}, y_{ij} > 0] = \frac{\exp(\beta_0 + \beta_1 \ln(income_{ij}) + \mathbf{X'_{ij}}\boldsymbol{\beta} + \delta_j)}{1 - P(y_{ij} = 0|income_{ij}, \mathbf{X_{ij}})} \tag{2}$$

where the sample is restricted to participators. Further details of this econometric model can be found in Greene (2007, p. 37-38). Greene (2004) notes that this model is not susceptible to the incidental parameters problem. In unreported regressions we also considered truncated negative binomial models and found that the coefficient estimate and standard error for β_1 were virtually identical to the truncated Poisson in all cases. Since the coefficient estimates are difficult to interpret, we computed the effect sizes of ln(income) on visits among participators, which we define as η_1. Combining the results from the two parts allows us to approximate the overall marginal effect of $ln(income)$ on y for the whole sample as follows:

$$\frac{dE[y]}{d\ln(income)} = \alpha_1 \left(\overline{y}|y > 0\right) + \eta_1 \left(P[y > 0]\right) \tag{3}$$

where $\overline{y}|y > 0$ is the mean number of visits/medications among participators and $P[y > 0]$ is the sample participation rate, given in the last two columns of Table 2.

The control variables, listed in Table 1, are chosen to capture as many factors that may be correlated with both income and health care utilization as the data allow. The underweight, overweight, and obesity indicators capture certain aspects of health, while the private health insurance indicator also reflects health as demand for insurance is a function of expected medical needs. Since the POF does not contain more detailed health information, we also add the extensive set of controls for living conditions, which could influence health and be correlated with income. The fixed effects for local areas push further toward isolating a causal relationship by capturing unobserved factors related to health care demand or supply that vary systematically across communities.

The variance inflation factor (VIF) for ln(income) is only 3, well below the maximum accepted level of 10 at which the extent of multicollinearity is considered to be problematic (Wooldridge, 2006:99). Moreover, there is little to no loss in precision if we drop the fixed effects from the model, suggesting that the efficiency gained from their predictive power is sufficient to offset the lost degrees of freedom.

Finally, the SUS and, in particular, the PSF, have continued to grow since our sample period of 2002-2003. This poses the question of whether our results would still hold today. We therefore re-estimated our models using the subsequent 2008-2009 POF for the four out of our eight dependent variables that are available in the later survey: private medical visits and private, public, and all medications.[5] The estimated effects of income remained very similar to those obtained using 2002-2003 for those four variables (results available upon request), which suggests that the results would likely also remain similar for the other four utilization measures for which 2008-2009 data is not available.

4 Findings

Tables 3 and 4 present the results for medical visits and medications, respectively. The top half of each Table reports the coefficient estimates and standard errors for the $ln(income)$ variable from the linear probability models (equation (1)), while the bottom half gives the effect sizes and standard errors for the income variable from the zero-truncated Poisson regressions (equation (2)). To conserve space, we do not report the full regression output for the control variables but instead present F statistics from tests of the joint statistical significance of the variables in each group. The next-to-last line of each Table reports the overall marginal effects as computed by equation (3), while the last line scales these overall marginal effects by the dependent variables' sample means from Table 2 to express them as percentages.

The first column of Table 3 shows that income is positively and statistically significantly ($p < 0.001$) associated with the probability of household members having any private medical visits, as well as the number of private visits conditional on having any. Specifically, a 100% increase in income raises the probability of participation by approximately 5.1 percentage points and the number of visits conditional on participation by approximately 0.43. The overall effect size suggests that a 100% increase in income leads to 0.36 more private visits per household across the entire sample (including both participators and non-participators), or 40% of the sample mean of 0.91 visits.

The second column of Table 3 gives the results for public visits. Additional income leads to a statistically significant ($p < 0.001$) reduction in the probability of participating in the public sector for medical visits, but has no statistically detectable effect on the number of public visits conditional on participation. In other words, a pay raise makes an individual more likely to opt out of the public sector, but if she chooses to remain in the public sector her level of utilization does not change. A 100% increase in income is associated with a 2.5 percentage point reduction in the probability of having any medical visits. Combining the negative effect on participation with the non-effect on utilization, the overall effect of a 100% increase in income is to reduce public visits by 0.14, or 10% of the sample mean of 1.38.

The fact that the positive effect on private visits is larger than the negative effect on public visits suggests that income should increase the total number of visits. This is what we find in the third column of Table 3. A 100% increase in income raises the probability of having any medical visits (public or private) by 2.3 percentage points ($p < 0.001$) and the number of visits conditional on having any by 0.22 ($p < 0.001$), for an overall effect of 0.223 or 9.7% of the mean of 2.3.

The final column of Table 3 shows that an additional 100% of income reduces the probability of having any medical visits needed but not made because of cost by 0.7 percentage points ($p < 0.001$) and the number of these unaffordable visits conditional on having any by 0.31 ($p < 0.05$). Together, these estimates imply an overall reduction in visits not made because of cost of -0.06, which represents a sizeable 19% of the sample mean of 0.32.

The results for medication, shown in Table 4, reveal a similar pattern. In the first column, a 100% increase in income raises the probability of purchasing at least one medication in the private sector by 5.8 percentage points ($p < 0.001$) and the number of medications purchased among those who purchase at least one by 3.4 ($p < 0.001$). Overall, the effect is 4.5 medications, or 18% of the sample mean of 25.6. The second column shows that a higher income leads to fewer medications obtained for free at SUS facilities. An additional 100% of income reduces the probability of public sector participation by 2.3 percentage points ($p < 0.001$) and the number of free medications obtained by participators by 0.5, although the latter is not quite statistically significant at conventional levels. The overall effect is 0.57 fewer free medications, or 19% of the mean of 3.06.

Since income leads to a greater increase in medications bought than the decrease in medications given for free, its association with total medications – given in the third column

– is positive. A 100% increase in income leads to a 4.2 percentage point increase in the probability of obtaining any medicines ($p < 0.001$) and 3.1 additional medicines for those who obtain at least one ($p < 0.001$). In all, 100% more income increases the number of medications consumed by the average household by approximately 3.9, or 14% of the sample mean of 28.7.

Finally, the fourth column of Table 4 shows that 100% of additional income reduces the probability of having any unmet needs for medications because of cost by 2.1 percentage points ($p < 0.001$), while also reducing the number of unmet needs among those with any by a statistically insignificant 0.44. Together, these estimates imply that a 100% increase in income is associated with 0.4 fewer medicines needed but unable to obtain because of cost per household per year, or 32% of the sample mean of 1.25. Note that, while we also observed a negative relationship between income and medical visits not made because of cost, the magnitude is larger for medications. This may reflect the fact that many prescriptions and recommendations for over-the-counter drugs issued by SUS doctors still have to be bought in the private sector, so using the public sector for doctor visits does not automatically translate to getting free medicines. Increasing the stock of medicines SUS facilities have available to distribute may therefore ameliorate this disparity to some extent.

5 Discussion

Brazil adopted a universal health care policy in 1990 with the aim at reducing important health access disparities. By analyzing the utilization of a nationally representative sample of Brazilians, our study sheds light on the degree to which income-based disparities in Brazilians' use of health services and medication still remained over a decade after the enactment of universal coverage.

Our estimated correlations reveal four key results relating to income-based disparities. First, income is positively associated with private sector utilization. Second, income is negatively associated with public sector utilization. Third, income is positively associated with overall health care consumption. These first three points together suggest that additional income leads to a substitution away from the public sector and toward the private sector. Most of this is likely due to the fact that the private sector offers more desirable (although non-essential) characteristics compared to the public sector, such as lower wait times for appointments and upscale facilities. However, our results show that the increase in private care from higher income outweighs the decrease in public care. This raises the question of whether higher income families overuse health care resources or, more worrisome, lower income families do not have enough access. Our fourth key result, namely the negative association between income and unmet medical needs because of cost, provides evidence supporting the latter.

Because these correlations may be driven by confounders we also conducted regression analysis. The results were consistent with the correlations. Controlling for a large, although not fully comprehensive, set of characteristics, we find that income increases the number of private visits and decreases the number of public visits. The net effect remains that higher income increases the total number of doctor visits. Importantly, we find that an increase in income is associated with a reduction in the overall number in visits that were needed but could not be afforded. The size of this effect is substantial: a 100% income increase reduces the mean number of unaffordable visits by 19%. This is an important contribution. It shows that, in spite of the financial and human resources invested in the public health system, low income families claim to have unmet health care needs. Whether this occurs because the access is insufficient (e.g., lack of facilities in rural remote areas) or because the families do not have the resources (time and money) to have their needs met, is an important question that will have to be addressed in future work.

Our research also points to the fact that higher income levels are associated with a much larger number of acquired medications. At first glance, one may think that this phenomenon is similar to (and occurs for the same reasons as) the association between income and doctor visits. However, this is not necessarily true. In particular, there may be an important causal connection between difficulties in accessing doctor visits and difficulties in accessing medications. For example, low income families may have substantially lower number of doctor visits because of their perception that doctors are likely to prescribe medications that they will not be able to afford. This hypothesis (which cannot be tested with our data) has critical policy implications. Knowing the answer is critical to understanding whether unmet needs for doctor visits can be most effectively reduced by increasing the number of facilities (or extending PSF) or by priorizing universal access to medications.

Importantly, our results should not be interpreted to suggest that universal health care in Brazil has not led to reductions in income-based disparities or improvements in access to health care services for low income families. The pro-poor disparity in public utilization and the overall relatively low rate of unmet medical needs imply that disparities and access problems would be worse without the public sector. Furthermore, the finding that income increases the likelihood of individuals opting out of the public sector could conceivably be considered equity-improving. By switching to the private sector in seek of comfort and non-essential services, richer individuals free public resources for those in need of essential medical care.

Our research has limitations. First, our ability to control for health characteristics of the individuals is limited given our dataset. We included underweight, overweight, obesity, and health insurance indicators as well as a number of variables related to sanitation and living conditions, but these may not suffice to capture all important aspects of health. Second, there may be potential endogeneity of the health and living condition controls and local area fixed effects. Income could influence health, living conditions, and area of residence, which in turn could influence health care consumption. In this case, our specification might "control away" part of the overall effect of income on utilization. To address this concern, we estimated additional models dropping each of these three sets of covariates separately as well as dropping all three together. This did not affect our findings; the results are available upon request. Third, the Brazilian economy, and the SUS in particular, are in constant change. Our analysis are based on 2002-2003 data and complemented by 2008-2009 data. However, there still remains the concern that the current expenditure patterns may depart from those captured in our datasets.

6 Conclusion

In its Constitution of 1988, Brazil adopted a universal health care policy with the goal of guaranteeing public health care to the most vulnerable sectors of the population. Previous research suggests that Brazil's universal health care system has improved access to health care but that gaps in access for vulnerable populations remain. We test for income-based disparities in medical visits and medications and find results that are consistent with this assessment.

We find that more income leads individuals to substitute paid private sector care for free public sector care. This may be due to perceived higher quality, shorter queues, or greater convenience of the private sector. However, we also find that the increase in private care doctor visits is greater than the decrease in public care doctor visits. This suggests that the overall disparity in health care utilization remains pro-rich. We also show that income is negatively associated with medical needs unmet because of cost, pointing to continued access problems among at least some low-income households. In all, our findings add to the growing body of evidence that limitations in health care access still exist for low-income households in Brazil despite the availability of the free public sector.

Overall, our findings suggest that universal coverage does not automatically lead to universal care. Even if a population is shielded from medical bills, high transportation costs, long waits, or perceived difficulties in securing the medications can still prevent the poor from obtaining care.

Notes

1. An alternative survey, the Pesquisa Nacional por Amostra de Domicilios 2008 (PNAD 2008), contains more detailed health information, but it is not suitable for our purposes since it does not allow for separate counts of medical visits (or medicines) based on whether the provision is public or private. Additionally, the PNAD's health system utilization data has a reference period of only two weeks.

2. As robustness checks we also defined public visits in two alternative ways: 1) any visit whose payment method was coded as *donation,* and 2) any *donation* whose source of provision was coded as *public* and whose provider location was coded as *SUS.* The results were virtually identical and are available upon request. This robustness reflects the fact that virtually all free visits in Brazil are publically provided by the SUS.

3. Occasionally, medical visits reported by the POF do not fall into either our private or public classifications, so our classification of "all" visits does not include every single POF visit. However, these "other" visits are rare and may in many cases simply reflect reporting error. We therefore elect not to include them when computing "all visits". This exclusion is of little consequence for the sample means, correlations, or regression estimates, and results using the broader classification are available upon request. A similar caveat applies to the following discussion of medications.

4. We do not use the POF sampling weights since some of the Stata modules used in the analysis do not support them. In unreported regressions (available upon request), we verified that the results from the regressions for which sampling weights are supported are not sensitive to their use.

5. The 2008-2009 POF questionnaire on individual expenditures contains the variable payment method but not source of provision or provider location. Hence, publicly provided medicines were identified only through the code that corresponds to *donation* as payment method, introducing a degree of noise in the variable. We nonetheless obtain similar results as with the 2002-2003 data.

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Table 1 – Independent Variables

Category	Description	Mean
ln(Income)	Natural log of total household income (annualized)*	6.749
Demographics	Share of females 15 to 60 years old in household	0.326
	Share of children ($\leq$10 years old) in household	0.176
	Share of elderly ($\geq$60 years old) in household	0.124
	Highest education years of any household member	7.843
	Household size dummies (omitted category is one)	
	Two	0.172
	Three	0.222
	Four	0.232
	Five	0.142
	Six	0.068
	Seven	0.034
	Eight	0.018
	Nine	0.011
	Ten or more	0.012
	Race dummies (omitted category is other)	
	Modal race of household members is white	0.463
	Modal race of household members is black	0.047
	Modal race of household members is mixed	0.483
Religion	Dummy for Catholic	0.779
	Dummy for Evangelical	0.151
	Dummy for other (omitted category is atheist)	0.021
Health	Dummy for anyone in household underweight (BMI$\leq$ 18.5)	0.180
	Dummy for anyone in household overweight (25 $\leq$BMI$<$ 30)	0.444
	Dummy for anyone in household obese (BMI$\geq$ 30)	0.156
	Dummy for anyone in household has private insurance	0.210

(continued on next page)

*The mean household income before taking the log is 18303.53. Before taking the log, we add one to prevent it from being undefined for households with no income.

Table 1 – Independent Variables (continued)

Category	Description	Mean
Living Conditions	Number of rooms in home	5.815
	Number of rooms in home squared	38.930
	Dummy for not having electricity	0.057
	Dwelling type dummies (omitted category is other type of house)	
	Dummy for rudimentary house	0.062
	Dummy for apartment or single-room dwelling	0.061
	Water source dummies (omitted category is water system)	
	Dummy for well	0.193
	Dummy for other source	0.081
	Sewage dummies (omitted category is sewage network)	
	Septic tank	0.202
	Rudimentary tank	0.334
	Other source	0.054
	No sewage	0.094
	Floor type dummies (omitted category is carpet)	
	Ceramic, tile, or stone	0.382
	Treated wood	0.113
	Cement	0.426
	Other floor type	0.064

Table 2 – Dependent Variables

Variable Description	Mean	Corr. w/ ln(Income)	Fraction Nonzero	Mean if Nonzero
Private medical visits	0.913	0.270***	0.159	5.727
Public medical visits	1.383	−0.071***	0.241	5.744
All medical visits	2.296	0.127***	0.375	6.125
Medical visits prevented by cost	0.321	−0.032***	0.056	5.719
Private medications	25.603	0.291***	0.740	34.577
Public medications	3.058	−0.071***	0.143	21.327
All medications	28.661	0.253***	0.784	36.543
Medications prevented by cost	1.249	−0.074***	0.070	17.742

Notes: *** correlation is significant at 0.1% level; ** 1% level; * 5% level. All variables are annualized and at the household level.

Table 3 – Results for Medical Visits

		Private	Public	All	Unaffordable
Participation	ln(Income)	0.051	−0.025	0.023	−0.007
		(0.003)***	(0.003)***	(0.004)***	(0.002)***
	Demographic Controls	15.82***	54.68***	49.28***	12.00***
	Health Controls	47.58***	101.23***	4.42**	13.11***
	Living Condition Controls	7.48***	7.00***	1.82*	3.04***
	Religion Controls	1.85	0.47	0.94	2.52
	Local Area Fixed Effects	YES	YES	YES	YES
	Observations	48,225	48,225	48,225	48,225
	Number of Local Areas	3,979	3,979	3,979	3,979
Visits	ln(Income)	0.431	0.002	0.220	−0.308
		(0.068)***	(0.057)	(0.046)***	(0.130)*
(Participators	Demographic Controls	57.17***	361.43***	466.98***	119.43***
Only)	Health Controls	80.27***	29.97***	26.89***	5.97
	Living Condition Controls	30.40*	21.34	23.68	10.71
	Religion Controls	10.95**	7.93*	15.40**	1.46
	Local Area Fixed Effects	YES	YES	YES	YES
	Observations	7,685	11,613	18,076	2,708
	Number of Local Areas	2,998	2,879	3,623	1,220
Overall Marginal Effect of ln(Income)		0.361	-0.143	0.223	-0.060
Overall Marginal Effect as % of Mean		39.5%	-10.3%	9.7%	-18.6%

Notes: Heteroskedasticity-robust standard errors, clustered by local area, are in parentheses. *** significant at 0.1% level; ** 1% level; * 5% level. Marginal effects are reported in the zero-truncated Poisson visits regressions. F statistics from tests of joint significance are reported for the sets of controls.

Table 4 – Results for Medications

		Private	Public	All	Unaffordable
Participation	ln(Income)	0.058 (0.003)***	−0.023 (0.002)***	0.042 (0.003)***	−0.021 (0.002)***
	Demographic Controls	57.35***	50.98***	66.32***	19.20***
	Health Controls	22.89***	19.19***	18.22***	1.78
	Living Condition Controls	5.79***	1.99*	3.63***	3.56***
	Religion Controls	3.64*	1.56	5.48***	1.62
	Local Area Fixed Effects	YES	YES	YES	YES
	Observations	48,225	48,225	48,225	48,225
	Number of Local Areas	3,979	3,979	3,979	3,979
Visits	ln(Income)	3.394 (0.214)***	−0.526 (0.339)	3.109 (0.214)***	−0.438 (0.412)
(Participators	Demographic Controls	1187.83***	89.90***	1545.14***	61.62***
Only)	Health Controls	127.76***	4.81	113.77***	3.92
	Living Condition Controls	37.90***	14.12	30.71**	25.48*
	Religion Controls	16.87***	5.10	19.85***	9.89*
	Local Area Fixed Effects	YES	YES	YES	YES
	Observations	35,709	6,914	37,823	3,396
	Number of Local Areas	3,976	2,548	3,978	1,685
Overall Marginal Effect of ln(Income)		4.513	-0.566	3.932	-0.403
Overall Marginal Effect as % of Mean		17.6%	-18.5%	13.7%	-32.3%

See notes for Table 3.