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# Here Comes the SUN: Self-Assessed Unmet Need, Worsening Health Outcomes and Healthcare Inequity

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Grant Gibson
Michel Grignon
Jeremiah Hurley
Li Wang

McMaster University

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Authors: Grant Gibson, Michel Grignon, Jeremiah Hurley, Li Wang

Reseach conducted at McMaster University, Hamilton, Ontario, Canada

Corresponding Author: Michel Grignon, Department of Economics, KTH 232, McMaster University, 1280 Main Street West, Hamilton, Ontario, Canada, L8S 4M4

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The measurement of socio-economic inequity in health care utilization is mostly based on an indirect approach, comparing actual to "necessary" (needs-adjusted) utilization. As we show in this paper, this indirect approach can be misleading when preferences over health and health care vary along socio-economic status. An alternative approach to assessing inequity is to measure the existence of barriers to access directly, through self-assessment of unmet need, and then estimate how it co-varies with socio-economic status. Questions on unmet need are asked in many health surveys but have not been much used in analyses of health inequity. The subjective nature of responses to unmet need may explain this neglect. In this paper we test the external validity of self-assessed unmet need, based on longitudinal Canadian data. We find that reporting unmet need statistically predicts deterioration in health status, suggesting that responses to the question on unmet need capture some actual barriers to access to care, and that responses are not only the result of subjective perceptions.

# 1 Introduction

Equity is a stated objective of public health care payers in many OECD countries [eg. Council of the European Union [2006]] . In Canada, equity lies at the core of the 1984 Canada Health Act and of citizens' accounts of what they value about being Canadian (Giacomini et al. 2004). In their conceptual framework of equity, Culyer & Wagstaff (1993) distinguish two types of measure of inequity of health care: socio-economic differences in actual utilization (conditioning on need), and socio-economic differences in access to services. The latter seems to better translate the policy goals put forward by health care systems but is not easy to define (Le Grand 1991, Culyer & Wagstaff 1993) and is even harder to measure: access is essentially the ability to get a service if/when needed. In this paper, we propose to explore a way to measure access that complements our current understanding of inequity of health care, which is almost exclusively based on studies of utilization.

The utilization-based method consists of using micro-data containing a variety of health information as well as some measures of health care use. Utilization is regressed on the health information to create an indirect standardization of use on need, and the socio-economic gradient of this standardized use is measured (through a concentration index or similar measure). The main advantage of using such a method is that it is based on survey questions that are reliable (questions on utilization are well understood), valid (systematic variations in under or over-reporting of health problems is documented to be small), and standardized across surveys and countries to a large degree (Bago d'Uva et al. 2008); see O'Donnell et al. (2008) for a full discussion.

The inherent assumptions of this methodology, however, have been described as a over-simplistic (Mooney 2009). Utilization-based studies could be measuring inequity, but could just as easily be capturing socio-economic differences in seeking care for identical need because of differences of preferences. While this, by some definitions, may still be considered inequitable, the appropriate policy response depends on whether barriers or tastes are to blame (Culyer & Wagstaff 1993, Mooney 2009). Consider a fictitious example with two groups: Group 1 has a low taste for healthcare; a disease or injury must be severe before they will consider seeking treatment. The second, Group 2, individuals have relatively higher taste for healthcare; they seek care for most ailments. As a result, for a given level of health, group 1 will attempt to access the healthcare system less often than group 2. Assume also that this system is equitable and that both groups have perfect access to care. In this simple world, needs-adjusted utilization for given health characteristics will lie somewhere between what is chosen by groups 1 and 2. Group 1 will seem to use less care than predicted, and group 2 more care than predicted. The system will be deemed inequitable even though both groups have perfect and equal access to care.

An approach based on self-assessed access to care can overcome the issues with utilization-based methods in the face of heterogeneous preferences. Our proposed approach to measuring access directly relies on questions on self-assessed unmet need (SUN). The measure considered in this analysis has already been used by many researchers in cross-sectional analyses in Canada (for examples see Allin et al. (2010), Hurley et al. (2008), Allin (2008), Chen & Hou (2001)), and in one case, longitudinally (Jamal 2015).

The determinants of SUN are relatively well studied: low income (Himmelstein & Woolhandler 1995, Chen & Hou 2001, Newacheck et al. 2003, Shi & Stevens 2005, Koolman 2007), lack of insurance in the US (Reschovsky et al. 2000), unemployment (Westin et al. 2004), higher education (Åhs & Westerling 2006, Koolman 2007), and immigration (Koolman 2007) have all been found to increase the probability of reporting SUN in various western countries. SUN does not provide a way to measure access perfectly[1] and to compare it across socio-economic status, but it allows the researcher to identify an access problem in a binary way (there is or there is no barrier to access to care).

Using a panel survey of Canadians, this paper shows that SUN, though subjective, can be objectively linked to worse future health. This ability to predict deteriorating health is important for two reasons: first, it suggests that individuals can detect, even imperfectly, when they did not get all the care they needed in a given period and validates SUN as a meaningful measure of barriers to access; second, it implies that addressing socio-economic inequalities in SUN might reduce inequities in health outcomes and improve the average health of the population. We are aware of only a few other papers that investigate the impact of present unmet-need on future health. Using a sample of French residents, Dourgnon et al. (2010) find a detrimental impact on health four years after having declined care for financial reasons. Zhen et al. (2015) find an increase in three-year mortality among the elderly with unmet needs in China. While these important findings are mostly confirmed in our study, we believe that this study represents an improvement due to the shorter time between surveys (2 years), the more generic nature of the unmet need variable, and the length of the panel which, in some ways, better capture the effect of unmet need on future health. The remainder of the paper will proceed as follows: section 2 presents the theoretical differences between a model using unmet need and one using needs-adjusted utilization followed by a brief discussion of SUN; section 3 provides an introduction to the dataset and variables; section 4, the econometric models; section 5 results; and section 6 concludes.

[1] If survey questions asked about attempted access, we would be able to calculate the percentage of access attempts that were successful rather than simply a binary unmet need.

## 2 Formalizing measurement error in inequity

Returning to the two-group example from the introduction, we define the following: $\mu_i$, the probability of success in accessing the healthcare service[2] for person of type $i$, $N_i$ is the number of times that an individual of type $i$ attempts to access the healthcare service conditioning on health status (the between-group difference in attempted access is thus based only on preferences). Also, $\sigma$ is the fraction of group 1 (and $1-\sigma$ the fraction of group 2).The discrepancies calculated are comparing the outcomes of the low taste group (1) to the population average.

A utilization-based method uses a fitted model (predicted usage, which we call need-standardized usage) and subtracts actual usage to determine where inequity exists. The underuse, denoted $discrepancy_u$, would be

$$discrepancy_u = (1-\sigma)(\mu_1 N_1 - \mu_2 N_2) \tag{1}$$

In a study of an equitable system where $\mu$ is the same for both groups (ie. $\mu_1 = \mu_2$) there is still measurable overuse and underuse unless $N_2 = N_1$. Given the heterogeneity of preferences, this method finds an effect regardless of the value of $\mu$ when it is the same across groups (ie. there is equal access). Recall that overuse and underuse are defined because $N_2 > N_1$, ie. need-standardized usage omits the group's preferences.

By this logic, it may be tempting to consider only SUN since we have just shown that an equitable system can generate the perception of inequity under utilization-based methods. There is also, however, a discrepancy, denoted $discrepancy_s$, in a SUN-based analysis in an equitable system with constant probability of ability to access which can be expressed as:

$$discrepancy_s = (1-\sigma)((\mu_2)^{N2} - (\mu_1)^{N1}) \tag{2}$$

Recall that with $\mu_1 = \mu_2$ this system is *equitable* (ie. the "discrepancy" is entirely mechanical and due to the binary nature of the variable). The overuse and underuse discrepancies are generated by the heterogeneity in preferences, while the unmet need discrepancy is generated by a statistically higher likelihood of experiencing the failure state given more attempts with constant probability. The unmet need discrepancy equation simply represents the difference in the percent of low types who report unmet need and the percent of high types who do given the same access. Since $N_1$ is assumed to be less than $N_2$, and $\mu$ is bounded by zero and one, the discrepancy (expressed in this way) is always positive, and is, ceteris paribus, increasing in $\mu$, the difference between $N_1$ and $N_2$, and decreasing in the level of $N_1$ and $N_2$. Since utilization is a continuous measure and SUN is a binary measure,

[2]For the theoretical discussion, we make the assumption that each attempt to access healthcare's success probability is independent of any earlier attempt. This assumption is unlikely to hold in reality.

it is meaningless to try to compare $discrepancy_u$ and $discrepancy_s$. We can, however, think about the performance of the two measures. First, in an equitable system, utilization based methods will predict inequity in favor of the high types, while unmet need will predict inequity in favor of the low types. Second, in the case where access is perfect ($\mu = 1$) unmet need methods provide no discrepancy while utilization methods do (more practically as the system approaches perfect access the discrepancy from unmet need disappears asymptotically while the discrepancy from utilization does not). Eliminating the assumption that $\mu_1 = \mu_2$ results in discrepancies which are impossible to sign. Inequity could be masked in utilization studies if a group with *higher* taste for healthcare experienced less access. In the unmet need study, the effect can disappear when the group with *low* taste has less access. In general, the two methods tend to 'find inequity' in different directions. We reiterate that in a theoretical sense, when agents do not share common preferences, utilization-based measures are always biased towards finding inequity in system *even when none exists.*

# 3 Data

Data for this analysis come from the National Population Health Survey (NPHS) [Statistics Canada, 2014a]. The data consist of a series of biennial surveys beginning in 1994-95 and continuing to 2011. Due to adjustments made in the survey wording and content, and the inclusion of additional respondents in the second wave, we do not use the observations from 1995, and thus have 8 years of observations.

## 3.1 Sample & Attrition

In the 1997 survey 16,032 respondents drawn from the non-institutionalized population aged 12 or more formed our first year of observation. Attrition reduced the sample by approximately four percentage points per wave until in 2011 75.1% (12,041) of the initial respondents were still in the sample. We investigate the possibility of attrition introducing bias in the estimates of our coefficient of interest. The most important source of bias would be if SUN were correlated with sample exit[3]. If those with unmet need disappear at unequal rates across the distribution of health changes, the coefficient on unmet need will be biased. The correlation between lagged unmet need and exit from the sample is not statistically different from zero at the 95% level (P-Value 0.07). Since, however, it is significant at the 90% level we further point out that the increased probability of exit for those reporting

[3]We do observe selective attrition based on gender, and income. We are not specifically interested in the values of these coefficients and point out that any bias would favour finding a non-effect.

unmet need is very small (<.01). Furthermore, missing responses are more likely to be from those in worse health (on the left tail of the health-change distribution) and thus will attenuate any negative impact of unmet need.

## 3.2 Variables

In the NPHS survey, the specific question addressing unmet need is as follows "During the past 12 months, was there ever a time when you felt that you/he/she needed health care but you/he/she didn't receive it?" [Statistics Canada, 2014(b)]. Responses to unmet need are binary, 1(yes) or 0(no), but some follow up questions attempt to ascertain the reason for the unmet need. Since physician and hospital services are provided free to patients, SUN should not arise as a result of being unable to pay for these services. SUN may occur, however, as a result of being unable to afford complementary services (such as parking or public transit), difficulty with scheduling time off work/family commitments, or if services are underprovided in an area and wait times are exceedingly long (see Allin et al. (2010) for more details). SUN may also be reported due to the many non-covered services (e.g. chiropractor, prescription drugs) since the question is generally about health care, and not about covered services specifically.

We examine SUN's impact on four health outcomes: The two of primary interest are Health Utility Index (HUI3), and self-assessed health (SAH). Two supporting measures are restriction of activities, and number of chronic conditions.

HUI3 (health utilities index) (Horsman et al. 2003) is a well validated preference-based health-related quality of life score measured over the ability to function in eight different aspects of everyday life (hearing, vision, speech, mobility, dexterity, pain, emotion and cognition). HUI3 is defined over the interval [- 0.36, 1.0] where 1 represents perfect health and zero represents death.

SAH is a five valued measure of the respondent's perceptions of their own health ranging from excellent (5) to poor (1). It has been proven to be a good predictor of future mortality and a relatively reliable indicator of health at the individual level (Crossley & Kennedy 2002, Huisman et al. 2007).

Two final supporting measures are restriction of activities, and number of chronic conditions. These measures complement HUI3 and SAH by providing information on more specific components of health, are very often used as health outcomes in analyses of population health, and can be considered more "clinically objective" than HUI3 or SAH. These measures can be affected by reporting issues but what they are measuring is objectively defined, which is not the case for HUI3 and SAH.

Activity restriction is a binary measure of inability to do tasks at home or at work (with 1 representing no restrictions). Specifically, the survey question asks about the ability to complete tasks related to work, school, or home life due to health problems.

There are 14 chronic conditions that are common to all waves of the survey: allergies, asthma, arthritis/rheumatism, back problems, high blood pressure, migraine headaches, chronic bronchitis/emphysema, diabetes, epilepsy, heart disease, cancer, stomach ulcers, stroke effects, and urinary incontinence. While chronic conditions are usually considered incurable, it should be noted that for every two new diagnoses that appear in the sample between waves, one condition disappears. The survey taker is careful to ensure that the condition really was 'cured' by asking follow up questions to ensure the accuracy of both the response in the previous wave and the current response.

We divide our control variables into two categories, health and demographic. The health variables include: alcohol consumption (average number of drinks/week), smoking (former/never/current), deviation from normal BMI (which is the absolute value of the difference between the respondents BMI and the midpoint of normal BMI - 21.5)[4], and whether or not the respondent has a general practitioner. Demographic controls include dummy variables for sex, immigrants and marital status (divorced[5]/single/married). Also included are education level (less than high school/high school graduate/some post-secondary/post secondary graduate), age[6], and urban variables (rural area, city $\leq 30,000$, city $\leq 100,000$, city $\leq 500,000$, city $\geq 500,000$). Income is categorized into 5 income groups and is scaled to household size using the income cutoffs in Van Doorslaer et al. (2004). A single (five) person household is low income, group 1, if income is less than $10,000$(15,000) and high income, group 5, if income is greater than 60,000 (80,000).

In our sample, unmet need is reported by about 10% of respondents in 2005 (the middle year of our sample) and, consistent with other studies, is reported more by young people, women, and people with low income. While the rate of unmet need varies over the course of the panel, the patterns of reporting do not (e.g. men always report at a lower rate than women). Growth rates of unmet need are also similar across groups. For example, between 1997 and 2011 unmet need reported by males grew by 54% (4.6% to 7.1%), while women's reports similarly grew 56.5% (6.9% to 10.8%). Other variables such as income or smoking status vary in the growth of reporting rates over the sample period, however it should be noted that movement in and out of smoking and a general trend of rising income over time intro-

[4]Since we expect there to be no effect at very small values, we explored robustness with a complement of dummy variables for obese, overweight and underweight as well as $BMI^2$ with no change in the SUN coefficients

[5]Divorced subsumes also separated and widowed categories.

[6]Both sex and age are included only in regressions without fixed effects (OLS specifications and ordered logit).

duces a great deal of complexity in interpreting these differences. The last, and most important consideration is the difference in health between those reporting unmet need and those not reporting unmet need. Individuals reporting unmet need had an average HUI3 of 0.80 while those not reporting unmet need have an average HUI3 of 0.89.

## 4 Empirical Strategy

Estimation of the empirical models seeks to identify the change in health status associated with reporting unmet need. A simple econometric model could take the form:

$$\Delta Health_{it} = \alpha_0 + \beta_1 SUN_{it-1} + \beta_2 X_{it} + \epsilon_{it} \tag{3}$$

Where $\Delta Health_{it}$ represents the change in the relevant health status measure of person $i$ between time $t-1$ and $t$, $\epsilon_{it}$ represents the classic error with expectation zero, and $X_{it}$ represents a vector of characteristics. For SUN to be considered a useful proxy for unmet need, $\beta_1$ is expected to be negative and significant.

While we have a mixture of discrete and continuous variables, an immediate problem exists with this framework that is not specific to the data type. As in many panel data applications in health, we are concerned with a problem of initial conditions (Heckman 1981). Simply, this model assumes that health today is a function of health in all previous periods. Since our sample does not include observations of health in all periods of the individual's life and instead observes the individual only after a certain number of iterations of the health process, we cannot control for the possible nonrandomness in the initial observation state. Given an increased likelihood of reporting unmet need for lower values of the initial state and a higher rate of health decay in the initial state, the coefficient on SUN will be more negative than its true value, making unmet need appear to be a more important predictor than it is[7]. Additionally, we should be concerned about the possibility of preference heterogeneity and its effects on the estimate of $\beta_1$. Given estimation of equation 4, individuals with a higher propensity to report unmet need will see their specific rate of health decay (if it differs from the global rate $\alpha_0$) come to dominate the coefficient $\beta_1$. Put simply, if hypochondriacs are healthier than average, reporting unmet need will appear to be associated with health that deteriorates less quickly, thus under-estimating the effect of unmet need for others. These problems are addressed in different ways based on the outcome variable being investigated

[7] While it is tempting to appeal to use of the lagged health control variable, it should be recognized that it too is dependent on the non-random component of the initial value of health status.

as outlined in the next section[8]. We estimate each model four times: using only lagged SUN and the lag of the health variable (Base), using only demographic controls (Demo), using only health-based controls (Health), and using the full complement of controls (Full). Results are generally robust to specification.

### 4.1 Fixed Effects Specifications

In consideration of our concern for unobserved heterogeneity biasing the estimate of $\beta_1$, we employ fixed-effect specifications where possible. HUI3, activity restrictions, and number of chronic conditions admit fixed-effects specifications, although, in the binary case (activity restriction), we lose a number of observations to a complete lack of within observational unit variability[9]. If the unobserved heterogeneity that affects the initial condition is not time-variant, estimates from the fixed effects regression will be unbiased.

$$Health_{it} = \alpha_i + \beta_1 SUN_{it-1} + \beta_2 Health_{it-1} + \beta_3 X_{it} + \epsilon_{it} \tag{4}$$

Since this model estimates $\beta_1$ by using deviations from individual means, the process by which the initial condition is arrived at is of less concern unless there exists a mechanism by which the conditions leading to the initial rate of decline affect the change in the second derivative of this rate[10]. We attempted to relax the assumptions of the fixed-effect model by implementing the dynamic panel model for small $t$ large $n$ developed by Arellano, Blundell, Bover and Bond (Arellano & Bond 1991, Arellano & Bover 1995, Blundell & Bond 1998)[11]. Because the coefficient estimates do not differ meaningfully between the models, on the basis of parsimony, the fixed effect model is preferred. Since fixed effects are able to capture all of the additive time-invariant heterogeneity we employ this strategy whenever possible, recognizing that, in so doing, the ability to assess time invariant effects on changes to health becomes impossible and the information on those whose propensity to report unmet need is 1 or 0 (e.g. $SUN_{it-1} = 1 \forall t$) is lost.

[8]We also ran the regressions exluding those who report unmet need in the first year, there was no significant change in the coefficient or significance of the results, but the resulting loss of one year of SUN expanded the confidence interval of the estimates.

[9]What the coefficient from the fixed effect specification measures in this case is the conditional average change in reporting activity restrictions based on moving from no unmet need to unmet need in the previous period among those who ever report activity restrictions (but don't report in all periods). While there are differences between those who never report and those who report occasionally, this model will either support or deny the hypothesis that unmet need is a predictor of future health.

[10]In equation 4, controlling for lagged health, we allow there to be a global rate of health decay; including fixed effects allows an individual's specific average rate of health decay to differ by a constant amount over time.

[11]Results from these estimations are available by request to the authors.

### 4.2 Wooldridge Specification

SAH estimates by ordered logit will be biased if we do not consider a correction for the initial conditions problem. Following the method of Wooldridge (2005) the simple solution to the initial conditions problem is employed. While this estimator is known to be inadequate for small $t$ large $n$ type datasets, Monte-Carlo simulations by several authors indicate that it should work well in our case (8 observation periods) (Arulampalam & Stewart 2009, Akay 2012). We follow a hybrid of the original Wooldridge method and a constrained version by Akay (2012) detailed in Rabe-Hesketh & Skrondal (2013) and estimate the following equation by random effects ordered probit:

$$SAH_{it} = \beta_1 SUN_{it-1} + \beta_2 vSAH_{it-1} + \beta_3 vSAH_{i0} + \beta_4 \bar{z}_{ij} + \beta_5 z_{i0} + \epsilon_{it} + \alpha_i$$

Where, as in Contoyannis et al. (2004) $vSAH_{ij}$ represents a vector of dummy variables for the health statuses in year $j$ to permit flexibility in the transition probabilities matrix and $z_{i0}$ and $\bar{z}_{ij}$ are the initial level of the exogenous variables and average of all stricly exogenous variables for $t \leqslant j$ respectively[12]. Estimating this model with only the initial health condition (i.e. excluding $vSAH_{it-1}$) assumes that the unobserved individual specific heterogeneity is uncorrelated with the health determining process. We expect the coefficient to be biased in this case.

# 5 Results

Across all the outcome measures, we observe a deleterious effect of unmet need on the next period's health outcome. In HUI3 (see table 3) we observe a negative effect of SUN which, although small, is statistically significant. Samsa et al. (1999) suggests that an effect which is 20% of a standard deviation represents a moderately important effect. The change we observe is half this size; 11% of a standard deviation. While this change is not large, we can still detect the negative change in health status. It is also important to note that the estimate of the OLS coefficient and the FE coefficients are not directly comparable since one measures the conditional average change from the conditional average rate of health-decline due to SUN, while the other measures a conditional average change to the individual's specific average rate of health-decline given a fixed propensity to report unmet need. There is insufficient precision to indicate that these two measures differ from one another, however the theoretical concerns about the estimating equation detailed earlier suggest placing more confidence in the estimates resulting from the fixed effects model.

[12]The assumptions required for unbiased estimation are detailed in Wooldridge (2005).

While the coefficients from the ordered logit models (SAH as the dependent variable - table 4) are not directly comparable to each other, in examining the health status regressions we can frame the results in reference to the threshold differences[13]. The coefficient on SUN represents approximately one-tenth of the size of a cutpoint range; again, a small, but statistically significant amount. The scale of this effect does not vary much when looking at different specifications for the model.

Turning to chronic conditions, we again see evidence of the deleterious effect of SUN on health with approximately six net new chronic conditions in the next period per 100 people reporting SUN. Finally, the activity restrictions variable also exposits a negative relationship with unmet need. We can interpret this estimate to mean that if, among 100 of those without an activity restriction but who report SUN in the previous wave, 5 develop a new activity restriction, we will need 119 non-SUN respondents to have five who develop a new activity restriction. Taken together, the four regression analyses indicate that SUN is correlated with worse health in future periods across outcomes.

## 6 Conclusion & Discussion

This paper has demonstrated self-reported unmet need's predictive power for a variety of health-related outcomes. Recall that the objective is not to predict health status, but rather to assess the external validity of SUN through its ability to correlate with health decline. We infer back from an effect on health that reporting unmet need reflects a true access problem and not a mere subjective perception. The results of this analysis support its use in assessments of equity of access. Further study focusing on more targeted unmet need questions[14] would better inform whether covered services suffer from inequity in access. The primary weakness of unmet need as defined in the NPHS follows from the question's failure to identify the unmet need as being from a type of healthcare that is funded by the government (ie. "covered services"). While it may be interesting to know that a non-covered service is frequently indicated as an unmet need, analysis of whether funded services are being equitably provided is rendered impossible without separating the two categories of services. It is worth noting that most unmet need in this sample is classified by the respondent as being a physical ailment and therefore encompasses some unknown percentage of both covered and non-covered services. Similarly, we retained observations where the 'unmet need' was a missed annual checkup. This was the most

[13] Since the prediction of the model depends on $X_i\beta+\epsilon$ falling into some range ($\alpha_{ui}$,$\alpha_{ui+1}$ where these $\alpha_{ui}$ denote estimated cutpoints, the size of the coefficient $\beta$ relative to the size of the intercutpoint range is somewhat telling of the magnitude of the effect.

[14] An obvious example would be more specific questions about type of unmet need.

common reason given by those who did not have a family physician. In the event of a more immediate and specific medical concern these respondents may not have had any unmet need. Investigating annual, instead of biennial, responses would permit a greater understanding of the more immediate impact of unmet need (remembering that the coefficient estimates in this paper reflect a two-year change.) The observed effect in a one-year panel study could be larger if individuals seek care for their unmet need in the intervening year, or smaller if there is no change in the rate of decline between years. A data gathering project which connects SUN and an objective health assessment (eg. a physician's perception of the interviewed's health) would cement SUN as a predictor of worse health even to those most convinced that the physician is best able to determine health status, while such a project would also be useful in testing whether there are systematic differences in propensity to report between people from different socio-economic statuses. Finally, as other research in equity has mentioned, the *reason* for unmet need can be as important as the existence of unmet need. We caution that the conditional mean presented in this work is for a global measure of SUN, and thus is unlikely to correctly capture the effect of different types of unmet need. Future data gathering projects should be more purposeful about requiring details of unmet need (perhaps attempting to collect a continuous measure for unmet need rather than binary) for the purpose of estimating these impacts. Until such time as these data become available, practicioners should be mindful that inclusion of SUN alongside usage data in investigations of health inequity presents a more complete picture.

# 7 Appendices

Table 1: Survival Analysis - Exit from sample

| | Coefficient | S.E | P-Value | Min 95 C.I. | Max 95 C.I. |
|---|---|---|---|---|---|
| Newfoundland | 1.00 | | | | |
| PEI | 1.26** | 0.12 | 0.02 | 1.04 | 1.51 |
| Nova Scotia | 1.09 | 0.10 | 0.33 | 0.91 | 1.31 |
| New Brunswick | 1.25** | 0.11 | 0.01 | 1.04 | 1.49 |
| Quebec | 0.92 | 0.07 | 0.30 | 0.78 | 1.07 |
| Ontario | 1.59*** | 0.12 | 0.00 | .1.38 | 1.86 |
| Manitoba | 1.41*** | 0.12 | 0.00 | 1.18 | 1.67 |
| Saskatchewan | 1.20** | 0.11 | 0.04 | 1.01 | 1.43 |
| Alberta | 1.10 | 0.09 | 0.28 | 0.92 | 1.31 |
| British Columbia | 1.36*** | 0.12 | 0.00 | 1.15 | 1.61 |
| Rural Area | 1.00 | | | | |
| <30,000 people | 0.90** | 0.05 | 0.03 | 0.81 | 0.99 |
| 30,000 - 100,000 people | 1.00 | 0.06 | 0.96 | 0.89 | 1.12 |
| 100,000-500,000 people | 1.13** | 0.06 | 0.02 | 1.02 | 1.25 |
| 500,000+ people | 1.19*** | 0.06 | 0.00 | 1.08 | 1.32 |
| <secondary | 1.00 | | | | |
| secondary graduate | 0.75*** | 0.04 | 0.00 | 0.69 | 0.84 |
| Some post-secondary | 0.73*** | 0.03 | 0.00 | 0.67 | 0.80 |
| Post-Secondary grad | 0.64*** | 0.03 | 0.00 | 0.59 | 0.70 |
| Income Group 1 | 1.00 | | | | |
| Income Group 2 | 0.87 | 0.07 | 0.09 | 0.75 | 1.02 |
| Income Group 3 | 0.57*** | 0.04 | 0.00 | 0.49 | 0.66 |
| Income Group 4 | 0.40*** | 0.03 | 0.00 | 0.34 | 0.46 |
| Income Group 5 | 0.26*** | 0.02 | 0.00 | 0.22 | 0.31 |
| Immigrant | 1.05 | 0.05 | 0.27 | 0.96 | 1.15 |
| No GP | 1.3*** | 0.07 | 0.00 | 1.21 | 1.47 |
| BMI | 0.96*** | 0.00 | 0.00 | 0.95 | 0.97 |

*p<0.10 **p<0.05 ***p<0.01

Table 2: Regression of Lagged Unmet Need on Premature Exit from Sample

| | Coefficient | S.E. | P-Value |
|---|---|---|---|
| Lag SUN | 0.01 | 0.00 | 0.07 |
| Constant | 0.08 | 0.00 | 0.00 |

Table 3: HUI3 Regression Results

| | OLS | Base | Health | Demo | Full |
|---|---|---|---|---|---|
| SUN (t-1) | -0.02*** | -0.02*** | -0.01*** | -0.02*** | -0.01*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| HUI3 (t-1) | 0.61*** | 0.48*** | 0.07*** | 0.46*** | 0.06*** |
| | (0.01) | (0.00) | (0.01) | (0.00) | (0.01) |
| No GP | Yes | No | Yes | No | Yes |
| Smoking | Yes | No | Yes | No | Yes |
| Alcohol | Yes | No | Yes | No | Yes |
| BMI | Yes | No | Yes | No | Yes |
| Sex | Yes | No | No | No | No |
| Education | Yes | No | No | No | No |
| Age | Yes | No | No | No | No |
| Province | Yes | No | No | No | No |
| Urban Area Size | Yes | No | No | Yes | Yes |
| Income | Yes | No | No | Yes | Yes |
| Marital Status | Yes | No | No | Yes | Yes |
| Constant | 0.33*** | 0.46*** | 0.84*** | 0.47*** | 0.85*** |
| | (0.01) | (0.00) | (0.01) | (0.01) | (0.01) |
| _ n | 75,209 | 77,957 | 75,533 | 77,618 | 75,209 |

*p<0.10 **p<0.05 ***p<0.01
Base, health, demo and full contain individual fixed-effects

Table 4: Self-Assessed Health Regression Results

| | No Correction | Base | Health | Demo | Full |
|---|---|---|---|---|---|
| SUN (t-1) | -0.27*** | -0.19*** | -0.21*** | -0.27*** | -0.27*** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Health Status (t-1) | | Reference: | Poor | | |
| Fair | 0.80*** | 0.83*** | 0.76*** | 0.80*** | 0.80*** |
| | (0.10) | (0.09) | (0.09) | (0.10) | (0.10) |
| Good | 1.57*** | 1.63*** | 1.52*** | 1.52*** | 1.55*** |
| | (0.10) | (0.09) | (0.10) | (0.10) | (0.10) |
| Very Good | 2.05*** | 2.11*** | 1.97*** | 1.98*** | 2.01*** |
| | (0.11) | (0.10) | (0.10) | (0.11) | (0.11) |
| Excellent | 2.61*** | 2.64*** | 2.47*** | 2.50*** | 2.55*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Initial Health | Yes | Yes | Yes | Yes | Yes |
| No GP | Yes | No | Yes | No | Yes |
| Smoking | Yes | No | Yes | No | Yes |
| Alcohol | Yes | No | Yes | No | Yes |
| BMI | Yes | No | Yes | No | Yes |
| Sex | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | No | No | Yes | Yes |
| Age | Yes | No | No | Yes | Yes |
| Province | Yes | No | No | Yes | Yes |
| Urban Area Size | Yes | No | No | Yes | Yes |
| Income | Yes | No | No | Yes | Yes |
| Marital Status | Yes | No | No | Yes | Yes |
| Cutpoints | | | | | |
| 1/2 | -1.60*** | -0.42*** | -0.35** | -1.65*** | -1.98*** |
| | (0.16) | (0.12) | (0.17) | (0.33) | (0.34) |
| 2/3 | 0.82*** | 1.99*** | 2.09*** | 0.80** | 0.47 |
| | (0.16) | (0.12) | (0.17) | (0.33) | (0.34) |
| 3/4 | 3.55*** | 4.71*** | 4.83*** | 3.53*** | 3.21*** |
| | (0.17) | (0.13) | (0.17) | (0.33) | (0.34) |
| 4/5 | 6.45*** | 7.60*** | 7.74*** | 6.43*** | 6.12*** |
| | (0.17) | (0.13) | (0.18) | (0.33) | (0.34) |
| sigma square | 1.24*** | 1.55*** | 1.52*** | 1.35*** | 1.26*** |
| | (0.05) | (0.06) | (0.06) | (0.05) | (0.05) |
| _ n | 53,160 | 60,402 | 56,043 | 53,524 | 51,826 |

*p<0.10 **p<0.05 ***p<0.01

Table 5: Activity Restriction Regression Results

| | Base | Health | Demo | Full |
|---|---|---|---|---|
| Lagged Activity Restriction | 0.50*** (0.03) | 0.46*** (0.03) | 0.47*** (0.03) | 0.44*** (0.03) |
| SUN(t-1) | -0.22*** (0.05) | -0.23*** (0.05) | -0.21*** (0.05) | -0.22*** (0.06) |
| No GP | No | Yes | No | Yes |
| Smoking | No | Yes | No | Yes |
| Weekly alcohol consumption | No | Yes | No | Yes |
| BMI | No | Yes | No | Yes |
| Sex | No | No | No | No |
| Education | No | No | Yes | Yes |
| Age | No | No | No | No |
| Province | No | No | No | No |
| Urban Area Size | No | No | Yes | Yes |
| Income | No | No | Yes | Yes |
| Marital Status | No | No | Yes | Yes |
| _ n | 25,065 | 23,910 | 24,980 | 23,827 |

*p<0.10 **p<0.05 ***p<0.01

Table 6: Number of Chronic Conditions Regression Results

| | OLS | Base | Health | Demo | Full |
|---|---|---|---|---|---|
| Lagged Number of Conditions | 0.78*** (0.00) | 0.19*** (0.01) | 0.18*** (0.01) | 0.18*** (0.01) | 0.18*** (0.01) |
| Lagged SUN | 0.07*** (0.02) | 0.07*** (0.02) | 0.06*** (0.02) | 0.06*** (0.02) | 0.06*** (0.02) |
| No GP | Yes | No | Yes | No | Yes |
| Smoking | Yes | No | Yes | No | Yes |
| Alcohol | Yes | No | Yes | No | Yes |
| BMI | Yes | No | Yes | No | Yes |
| Sex | Yes | No | No | No | No |
| Education | Yes | No | No | Yes | Yes |
| Age | Yes | No | No | No | No |
| Province | Yes | No | No | No | No |
| Urban Area Size | Yes | No | No | Yes | Yes |
| Income | Yes | No | No | Yes | Yes |
| Marital Status | Yes | No | No | Yes | Yes |
| Constant | 0.38*** (0.03) | 1.32*** (0.01) | 1.05*** (0.03) | 0.91*** (0.07) | 0.73*** (0.07) |
| _ n | 59,980 | 62,127 | 60,108 | 61,992 | 59,980 |

*p<0.10 **p<0.05 ***p<0.01
Base, health, demo and full contain individual fixed-effects

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