# Bounds on Average Treatment Effects with an Invalid Instrument: An Application to the Oregon Health Insurance Experiment

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**Abstract**

This paper derives nonparametric sharp bounds on the population average treatment effect ($ATE$) and the average treatment effect on the treated ($ATT$) employing an instrumental variable (IV) that does not satisfy the exclusion restriction assumption (i.e., an invalid IV). This critical assumption of IV methods, which is usually difficult to justify in practice, requires that the instrument affects the outcome only through its effect on the treatment. We allow the instrument to affect the outcome through channels other than the treatment, and employ assumptions requiring weak monotonicity of average potential outcomes within or across subpopulations defined by the values of the potential treatment status under each value of the instrument. There are two key features of the approach we use to derive bounds on the $ATE$ and $ATT$. First, we write the parameters as weighted averages of the local average treatment effects of the different principal strata, and construct bounds by first bounding each of these local treatment effects. Second, we employ a causal mediation analysis framework to disentangle the part of the effect of the instrument on the outcome that works through the treatment from the part that works through the other channels. This enables us to use the (invalid) instrument to learn about the causal effect of the treatment on the outcome. To illustrate the identifying power of the bounds and the usefulness of the methods developed herein, the bounds are employed to analyze the effect of Medicaid insurance on health care utilization, self-reported health status, and financial strain, taking into account the possibility that Medicaid lottery may serve as an invalid instrument in the Oregon Health Insurance Experiment.

Key words and phrases: Causal inference; Nonparametric bounds; Instrumental variables; Mediation analysis; Principal stratification

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# 1 Introduction

Instrumental variable (IV) methods exploit exogenous variation of an IV to address endogeneity of the treatment when evaluating the treatment effect on an outcome of interest. A widely used framework for studying IV methods was developed in Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996) (hereafter IA and AIR, respectively). They show that in the presence of heterogeneous effects, IV estimators point identify the local average treatment effect ($LATE$), i.e., the average treatment effect for a subpopulation whose treatment status is affected by the instrument (i.e., compliers). Their results imply that only under strong and typically untenable assumptions IV methods point identify the average treatment effect for the population, such as assuming a constant treatment effect. Additionally, a critical assumption of IV methods is the exclusion restriction, which in the $LATE$ framework requires that the instrument affects the outcome only through its effect on the treatment. However, it is often debatable in empirical studies whether the instrument satisfies the exclusion restriction, which is a largely untestable assumption, and thus researchers have to resort to careful argumentation of the validity of the instrument in applications.[1]

This paper addresses those two crucial aspects of IV estimation. It derives nonparametric sharp bounds for the population average treatment effect ($ATE$) and the average treatment effect on the treated ($ATT$) while allowing the instrument to directly affect the outcome of interest through channels other than the treatment (i.e., with an invalid instrument). Intuitively, to employ an invalid instrument, its overall effect on the outcome is decomposed into the part of the effect that works through the treatment—the part that aids directly in identification and is uniquely present in a valid IV—and the part that works through channels other than the treatment. This is a distinctive feature of our approach that links violations of the exclusion restriction to the causal mediation literature (e.g., Robins and Greenland, 1992; Pearl, 2001; Rubin, 2004; Flores and Flores-Lagunes, 2009; Imai et al., 2010; Flores and Flores-Lagunes, 2010; Huber, 2013). More specifically, the part of the effect of the invalid IV on the outcome that works through the treatment is conceptualized as a mechanism or indirect effect, while the part of the effect of the invalid IV that works through the other channels is conceptualized as a net or direct effect.

A second distinctive feature of our approach is that the sharp nonparametric bounds on the $ATE$ and $ATT$ are obtained under weak monotonicity assumptions on mean potential outcomes of subpopulations defined by the values of the potential treatment status under each value of the instrument, called principal strata. Principal stratification (Frangakis and Rubin, 2012), with its roots in IA, AIR, and Hirano et al. (2000), partitions the population of interest into

[1] Just recently, some papers have suggested ways in which to gauge the validity of the exclusion restriction assumption under certain conditions (Hirano et al., 2000; Huber and Mellace, 2010, 2013; Mealli and Pacini, 2012; Flores and Flores-Lagunes, 2013), but their use is yet to become widespread.

principal strata of individuals that, by definition, are affected in the same way by treatment assignment. Thus, comparisons of individuals within the same stratum yield causal effects. Our identification strategy is then to achieve partial identification of the local causal effect of each stratum through the weak monotonicity assumptions, and subsequently to obtain partial identification on the $ATE$ and $ATT$ by a weighted average of the partially identified local causal effects. In practice, those weak monotonicity assumptions can be substantiated with economic theory, combined with each other depending on their plausibility, and some of them can be falsified from the data by employing their testable implications.

Current partial identification literature on IV models usually obtain bounds on the $ATE$ in the presence of a valid IV (Manski, 1990, 1997; Balke and Pearl, 1997; Heckman and Vytlacil, 2000; Bhattacharya et al., 2008; Kitagawa, 2009; Shaikh and Vytlacil, 2011; Chen et al. 2014; Huber and Mellace, 2013), while a few papers consider invalid instruments. Conley et al. (2012) use prior information regarding the coefficient of the IV in the reduced-form regression of the outcome to measure the extent of violations of the exclusion restriction and to present practical inference strategies on the parameter of interest. Nevo and Rosen (2012) derive analytic bounds on treatment effects by allowing correlation between the IV and the error term in linear models, but restricting the sign and extent of that correlation. Manski and Pepper (2000) derive nonparametric bounds on the $ATE$ based on the monotone instrumental variable (MIV) assumption, which consists of weak inequalities on mean potential outcomes of subpopulations defined by the observed values of a possibly invalid IV. As in this paper, Manski and Pepper (2000) do not model the extent of violation of the exclusion restriction nor use prior information. A key different in the two approaches is the reliance on different subpopulations and our link to causal mediation. In turn, the setup in Manski and Pepper (2000) allows for multivalued treatments and instruments, while ours is currently limited to binary versions of the same variables. Lastly, the two identification approaches are not nested; thus, the informativeness of the estimated bounds under each approach may be different in different applications.

Our general approach is related to Hirano et al. (2000) and Mealli and Pacini (2012), who extended the $LATE$ framework to allow for violation of the exclusion restriction. However, in both of those papers the focus is on effects of the IV on the outcome for different principal strata, i.e., on local intention-to-treat ($ITT$) effects; while the focus on this paper is on average treatment effects of the actual treatment of interest on the outcome using an IV. Specifically, Hirano et al. (2000) adopt a Bayesian analysis to point identify those effects and assess sensitivity to violations of the exclusion restriction. Mealli and Pacini (2012) propose nonparametric bounds on the same effects by exploiting the restrictions implied by the randomly assigned treatment on the joint distribution of the primary outcome and an auxiliary covariate, and their bounds can be used to detect the extent of violations of the exclusion restriction. Another related work is Flores and Flores-Lagunes (2013), who employed the same general approach used here to partially identify a local average treatment effect ($LATE$) for compliers under exposure to the active instrument status–a more specific subpopulation than the original IA and AIR $LATE$—in the absence of the exclusion restriction. Thus, the present work is a useful generalization of their results. In addition, our bounds on the local net effects for noncompliers, whose treatment status are not affected by the instrument, provide a straightforward test for the exclusion restriction.

Throughout the paper, we consider the setup consisting of a binary and randomly assigned instrument and a binary treatment. This is a canonical setting that is important in practice. A large amount of the program evaluation literature focuses on the binary instrument and treatment case (e.g., Angrist, 1990; Oreopoulous, 2006; Schochet et al., 2008). Moreover, randomized experiments (e.g., Heckman et al., 1999; Duflo et al., 2008) and quasi-experiments (e.g., Angrist and Pischke, 2009) have gained economists' attention as a way of estimating causal effects. In both cases, two common occurrences are non-compliance and violations of the exclusion restriction by the randomized variable. The methods presented herein allow conducting statistical inference on the population $ATE$ in those cases. More generally, our bounds can be employed to use existing experiments to make inference on the $ATE$ of treatments other than the ones the experiments were designed to address. Intuitively, in certain cases, the random assignment in those experiments can be used as an invalid IV for another (non-randomized) treatment of interest. This can be important when it is not possible or it is too costly to randomize a treatment of interest. For instance, though individuals are randomly selected to be encouraged to participate in a job training program, they are able to choose whether or not to actually enroll in the program. And a job training program usually provides a comprehensive package of job training to its participants, including vocational training courses, job search and counselor services. The randomly assigned IV would violate the exclusion restriction if the focus is on the treatment effect of one training module, whose effect is usually difficult to disentangle from those of the other modules but important for public policy.

To illustrate our methodology, we use public-use data from the Oregon Health Insurance Experiment (OHIE) to investigate the effect of Medicaid coverage on health care and preventative care utilization, self-reported health status and financial strain. In 2008, a group of uninsured low-income adults in Oregon was selected by lottery to be given a chance to apply for Medicaid, which is the public health insurance program in the U.S. for low-income adults and children. As pointed out by Finkelstein et al. (2012), it is possible that Medicaid lottery violates the exclusion restriction of the IV assumption. Thus, it is important to examine the results of OHIE without imposing the exclusion restriction. And their results apply to compliers that account for less than 30% of the target population. Instead, we bound the $ATE$ for the entire target population and $ATT$ for the treated individuals covered by Medicaid, which are of great importance from the point of view of public policy. Therefore, we examine the bounds on the $ATE$ and $ATT$ of Medicaid coverage on health care and preventative care utilization, self-reported health status and financial strain, taking into account the possibility that medicaid lottery may violate the exclusion restriction assumption. We find decent evidence that the exclusion restriction may had indeed been violated, at least for some outcome measures. Our bounds on $ATE$ and $ATT$ are informative under the two sets of weak monotonicity assumptions of average potential outcomes we propose, and the bounds on local net effects for never takers and always takers under the the weak monotonicity across strata provide a straightforward test for the exclusion restriction. In addition, compared with the bounds derived by imposing the exclusion restriction, we find that the exclusion restriction largely shrinks the width of the bounds.

The rest of the paper is organized as follows. Section 2 presents the setup and the partial identification results on the $ATE$ and $ATT$, with proofs relegated to the Appendix. Section 3 employs those bounds to analyze the effect of the Medicaid insurance, while Section 4 concludes.

# 2 Econometric Framework

## 2.1 Set-up and Link with Causal Mediation Analysis

Assume we have a random sample of size $n$ from a large population. For each unit $i$ in the sample, let $D_i \in \{0,1\}$ indicate whether the unit received the active treatment ($D_i = 1$) or the control treatment ($D_i = 0$). The outcome of interest is $Y$. Let $Y_{1i}$ and $Y_{0i}$ denote the two potential outcomes as a function of the treatment, that is, the outcome individual $i$ would get if she received the treatment or not, respectively. We consider employing exogenous variation in a binary variable $Z$ to learn about the effect of $D$ on $Y$, with $Z_i \in \{0,1\}$. Let $D_{1i}$ and $D_{0i}$ denote the potential treatment status; that is, the treatment status individual $i$ would receive depending on the value of $Z_i$. Accordingly, we incorporate $Z$ in the definition of the potential outcomes. Let $Y_i\,(z,d)$ denote the potential composite outcome individual $i$ would obtain if she received values of the instrument and the treatment of $z$ and $d$, respectively. For each unit $i$, we observe the vector $(Z_i, D_i, Y_i)$, where $D_i = Z_i D_{1i} + (1 - Z_i)\, D_{0i}$ and $Y_i = D_i Y_{1i} + (1 - D_i)\, Y_{0i}$. Our parameter of interest is the average effect of $D$ on $Y$ while allowing $Z$ to have a net or direct effect on $Y$. By the Law of Iterated Expectations we write them as $E[Y_{1i} - Y_{0i}] \equiv E[E[Y_i\,(z,1) - Y_i\,(z,0)\,|Z = z]] \equiv E[\Delta(z)]$, for $z = 0,1$. To simplify notation, we write the subscript $i$ only when deemed necessary.[2]

We partition the population into groups such that all individuals within the same group share the same values of the vector $\{D_{0i}, D_{1i}\}$, as in AIR. Frangakis and Rubin (2002) call such

[2] Our notation implicitly imposes the stable unit treatment value assumption (SUTVA) of Rubin (1980), an assumption also imposed in AIR. This assumption implies that the individual potential outcomes are not affected by the treatment received by other individuals, and that there are no different versions of the treatment.

a partition a "basic principal stratification" and demonstrate that comparisons of potential outcomes within these strata yield causal effects because the stratum an individual belongs to is not affected by the value of the instrument received. Our setting gives rise to four principal strata: $\{1,1\}$, $\{0,0\}$, $\{0,1\}$ and $\{1,0\}$. These strata are commonly referred to as always takers ($at$), never takers ($nt$), compliers ($c$), and defiers ($d$), respectively. As in AIR, we impose the following assumptions:

**Assumption 1** (*Randomly Assigned Instrument).*

$$\{Y(1,1), Y(0,0), Y(0,1), Y(1,0), D_0, D_1\} \text{ is independent of } Z.$$

**Assumption 2** *(Nonzero Average Effect of $Z$ on $D$). $E[D_1 - D_0] \neq 0$.*

**Assumption 3** *(Individual-Level Monotonicity of $Z$ on $D$). $D_{1i} \geq D_{0i}$ for all $i$.*

Assumption 2 requires the instrument to have an effect on the treatment status while Assumption 3 rules out the existence of defiers.

In addition, IA and AIR impose the *Exclusion Restriction Assumption: $Y_i(0,d) = Y_i(1,d)$ for all $i$ and $d \in \{0,1\}$*, which requires that all of the effect of $Z$ on $Y$ works through $D$. They show that adding the exclusion restriction to Assumptions 1 through 3, the local average treatment effect ($LATE$) is point identified as:

$$E[Y(z,1) - Y(z,0)|D_1 - D_0 = 1] = \frac{E[Y|Z=1] - E[Y|Z=0]}{E[D|Z=1] - E[D|Z=0]}. \quad (1)$$

$LATE$ refers to the average effect of $D$ on $Y$ for those individuals whose treatment status is affected by the instrument (i.e., compliers). Vytlacil (2002) shows that the IV assumptions imposed in the framework of IA and AIR are equivalent to those imposed in nonparametric selection models.

In contrast to AIR, we allow the instrument to have a causal effect on the outcome through channels other than the treatment. To employ such an instrument to learn about the treatment effect, we disentangle the part of the effect of the instrument ($Z$) on the outcome ($Y$) that works through the treatment ($D$) (i.e., the mechanism effect) from the part that works through the other channels (i.e., the net effect). Let $Y_i(1)$ and $Y_i(0)$ denote the potential outcomes as a function of the instrument, that is, the outcome individual $i$ would obtain if she were or were not exposed to the instrument, respectively. Hence, the average effect of the instrument on the outcome is given by $ATE_{ZY} \equiv E[Y(1) - Y(0)]$. Note that by definition $Y_i(1) = Y_i(1, D_1)$ and $Y_i(0) = Y_i(0, D_0)$. Then, let the counterfactual outcome $Y_i(z, D_{1-z})$ represent the outcome individual $i$ would obtain if she were exposed to the value of $z$ of the instrument, but her treatment status were under the effect of the instrument at the alternative value $1-z$. Intuitively, $Y_i(z, D_{1-z})$ is the outcome from a counterfactual experiment in which the individual is exposed to $Z_i = z$ but the effect of $Z$ on $D$ is held at $D_{1-z}$. Note also that $Y_i\left(z, D_{1-z}\right)$ represents an entirely counterfactual or hypothetical outcome (i.e., never observed in the data, in principle) and constitutes a modification of the original principal stratification framework (Flores and Flores-Lagunes, 2013). Following Flores and Flores-Lagunes (2010), the mechanism average treatment effect ($MATE^z$) is given by

$$MATE^z = E[Y\left(z, D_1\right) - Y(z, D_0)], \text{ for } z = 0, 1, \tag{2}$$

and the net average treatment effect ($NATE^z$) is given by

$$NATE^z = E[Y(1, D_z) - Y\left(0, D_z\right)], \text{ for } z = 0, 1. \tag{3}$$

Since $Y\left(z\right) = Y\left(z, D_z\right)$, $MATE^z$ gives the average effect on the outcome from a change in the treatment status that is due to the instrument, holding the value of the instrument at $z$, while $NATE^z$ gives the average effect of the instrument on the outcome when the treatment status is held constant at $D_z$. By construction, $ATE_{ZY} = MATE^z + NATE^{1-z}$, for $z = 0, 1$. Hence, $MATE^z$ and $NATE^{1-z}$ decompose the total average effect of the instrument on the outcome into the part that works through the treatment status ($MATE^z$) and the part that is net of the treatment-status channel ($NATE^{1-z}$). Note that $ATE_{ZY} = MATE^z$ if all the effect of $Z$ on $Y$ works through $D$, that is, if the exclusion restriction is satisfied. And $ATE_{ZY} = NATE^z$ if none of the effect of $Z$ on $Y$ works through $D$ (either because $Z$ does not affect $D$ or because $D$ does not affect $Y$).

Importantly, instead of focusing on the subpopulation of compliers, as IA and AIR do, we focus on the average treatment effect for the population, i.e., $E[\Delta(z)]$, for $z = 0, 1$. Following the notation above, under Assumption 1, we have $\Delta(z) \equiv E[Y_i\left(z, 1\right) - Y_i\left(z, 0\right)|Z = z] = E[Y_i\left(z, 1\right) - Y_i\left(z, 0\right)]$. Let $\pi_k$ denote the proportion of the stratum $k$ in the population, for $k = at, nt, c$. Under Assumptions 1 through 3, we write $\Delta(z)$ as a weighed average of the (local) average effects of the strata:

$$\begin{aligned} \Delta(z) &= E_k[E[Y(z,1) - Y(z,0)|k]], \text{ for } k = at, nt, c \text{ and } z = 0, 1 \\ &= \pi_{at} E[Y(z,1) - Y(z,0)|at] + \pi_{nt} E[Y(z,1) - Y(z,0)|nt] + \pi_c E[Y(z,1) - Y(z,0)|c] \end{aligned} \tag{4}$$

Using equation (4), partial identification of $\Delta(z)$ will be attained from the level of the strata up. Thus, we also define local versions of the causal mechanism and causal net effects as the corresponding average effects of the strata. Under Assumptions 1 through 3, let

$$LMATE^z_k = E[Y(z, D_1)|k] - E[Y(z, D_0)|k], \text{ for } k = at, nt, c \text{ and } z = 0, 1; \tag{5}$$

and

$$LNATE^z_k = E[Y(1, D_z)|k] - E[Y(0, D_z)|k], \text{ for } k = at, nt, c \text{ and } z = 0, 1. \tag{6}$$

Since $D_{0i} = D_{1i}$ for always takers and never takers, $LNATE_k^z = E[Y(1) - Y(0)|k]$ for $z = 0, 1$ and $k = at, nt$. It also implies that for these two strata $Y_i(z, D_z) = Y_i(z, D_{1-z})$, so $LMATE_k^z = 0$, for $z = 0, 1$ and $k = at, nt$; and by implication the observed data contain information on $Y_i(z, D_{1-z})$ for individuals in these two strata. Therefore, under Assumptions 1 through 3, $MATE^z = \pi_c LMATE_c^z$, for $z = 0, 1$. It is worth nothing that $LATE$ in (1) is equal to the local mechanism effect for compliers ($LMATE_c^z$), for $z = 0, 1$, when the instrument $Z$ is allowed to have effects on $Y$ through channels other than the treatment $D$ ($LMATE_c^z = E[Y(z, D_1) - Y(z, D_0)|c] = E\{[D_1 - D_0] \cdot [Y(z, 1) - Y(z, 0)]|c\} = E[Y(z, 1) - Y(z, 0)|c]$). Here, the value of $Z$ specifies whether the effects of the instrument through the other channels are blocked or exposed, and thus it may affect average treatment effects differently. In contrast, under the exclusion restriction of AIR, whether the treatment effect is under exposure to the instrument is irrelevant (Flores and Flores-Lagunes, 2013).

To further motivate our bound analysis on $\Delta(z)$, consider the following table that shows the distribution of the strata by the observed instrument exposure and treatment status $\{Z_i, D_i\}$:

Table 1. Principal Strata by Observed $Z_i$ and $D_i$

| | | $Z_i$ | |
|---|---|---|---|
| | | 0 | 1 |
| $D_i$ | 0 | $nt, c$ | $nt$ |
| | 1 | $at$ | $at, c$ |

Let $p_{d|z} \equiv \Pr(D_i = d|Z_i = z)$ and $\overline{Y}^{zd} \equiv E[Y|Z = z, D = d]$ for $z, d = 0, 1$. Under Assumptions 1 through 3, the stratum proportions in the population are point identified as $\pi_{nt} = p_{0|1}$, $\pi_{at} = p_{1|0}$, and $\pi_c = p_{1|1} - p_{1|0} = p_{0|0} - p_{0|1}$. The following average outcomes are also point identified: $E[Y(0)|at] = \overline{Y}^{01}$ and $E[Y(1)|nt] = \overline{Y}^{10}$. Furthermore, bounds on $E[Y(1)|at]$, $E[Y(0)|nt]$, $E[Y(0)|c]$ and $E[Y(1)|c]$ can be constructed by employing a trimming procedure similar to that used in Zhang et al. (2008) and Lee (2009). For instance, consider the bounds for $E[Y(0)|nt]$. The average outcome for the individuals in the $\{Z, D\} = \{0, 0\}$ group can be written as:

$$\overline{Y}^{00} = \frac{\pi_{nt}}{\pi_{nt} + \pi_c} \cdot E[Y(0)|nt] + \frac{\pi_c}{\pi_{nt} + \pi_c} \cdot E[Y(0)|c]. \tag{7}$$

The proportion of never takers in the observed group $\{Z, D\} = \{0, 0\}$ is point identified as $\pi_{nt}/(\pi_{nt} + \pi_c) = p_{0|1}/p_{0|0}$. Thus, $E[Y(0)|nt]$ can be bounded from above by the expected value of $Y$ for the $p_{0|1}/p_{0|0}$ fraction of *largest values* of $Y$ for those in the observed group $\{Z, D\} = \{0, 0\}$. The bounds on $E[Y(0)|c]$ can also be obtained by equation (7), while the bounds on $E[Y(1)|at]$ and $E[Y(1)|c]$ can be derived similarly based on the observed group $\{Z, D\} = \{1, 1\}$.

A key step in deriving bounds on $\Delta(z)$ (and thus $E[\Delta(z)]$) by means of causal mediation analysis is to write $\Delta(z)$ in different ways as a function of terms that can be either point identified or partially identified. Under Assumptions 1 through 3, for $z = 0, 1$, $\Delta(z)$ in (4) can be written as:

$$
\begin{aligned}
& \Delta(z) \\
= \;& \pi_{at}(E[Y(z)|at] - E[Y(z,0)|at]) + \pi_{nt}(E[Y(z,1)|nt] - E[Y(z)|nt]) + \pi_c LMATE_c^z \quad (8)\\
= \;& \pi_{at}(E[Y(z)|at] - E[Y(z,0)|at]) + \pi_{nt}(E[Y(z,1)|nt] - E[Y(z)|nt]) \\
& + E[Y(1)] - E[Y(0)] - \pi_{at} LNATE_{at}^{1-z} - \pi_{nt} LNATE_{nt}^{1-z} - \pi_c LNATE_c^{1-z} \quad (9)\\
= \;& p_{1|z}\overline{Y}^{z1} - p_{0|z}\overline{Y}^{z0} - \pi_{at}E[Y(z,0)|at] + \pi_{nt}E[Y(z,1)|nt] \\
& + (-1)^z \pi_c E[Y(z, D_{1-z})|c] \quad (10)
\end{aligned}
$$

Each of the equations above exploits different information in the data and, depending on the additional assumptions imposed below, generates different bounds on $\Delta(z)$. Equation (8) employs $LMATE_c^z$ to obtain the bounds. Equation (9) exploits point identification of $ATE_{ZY}$ by using the fact that $MATE^z = ATE_{ZY} - NATE^{1-z}$, and assumptions on $LNATE_k^{1-z}$ below, for $k = at, nt, c$. Equation (10) takes advantage of point identification of the conditional average outcomes $\overline{Y}^{zd}$. Since the data contain no information on the counterfactual potential outcomes, $Y(z,0)$ for always takers, $Y(z,1)$ for never takers, and $Y(z, D_{1-z})$ for compliers, we consider different assumptions below to provide information on those terms and derive the nonparametric bounds on $\Delta(z)$.

The most basic assumption considered in the partial identification literature is the bounded support of the outcome (e.g., Manski, 1990; Balke and Pearl, 1997; Heckman and Vytlacil, 2000; Sjölander, 2009).

**Assumption 4** *(Bounded Outcome)* $Y(z,d) \in [y^l, y^u]$, for $z, d = 0, 1$.

Assumption 4 states that the composite potential outcome has a bounded support. Because this assumption does not impose direct restrictions on $LMATE_c^z$ or $LNATE_k^z$, for $k = at, nt, c$, we can directly obtain the bounds on $\Delta(z)$ by equation (10), presented in Proposition 1.

**Proposition 1** *If Assumptions 1 through 4 hold, then the bounds* $LB^0 \le \Delta(0) \le UB^0$ *and* $LB^1 \le \Delta(1) \le UB^1$ *are sharp. And for* $z = 0, 1$,

$$\Pr(Z=0)LB^0 + \Pr(Z=1)LB^1 \le E[\Delta(z)] \le \Pr(Z=0)UB^0 + \Pr(Z=1)UB^1,$$

*where*

$$
\begin{aligned}
LB^0 &= p_{1|0}(\overline{Y}^{01} - y^u) + p_{0|0}(y^l - \overline{Y}^{00}) \\
LB^1 &= p_{1|1}(\overline{Y}^{11} - y^u) + p_{0|1}(y^l - \overline{Y}^{10}) \\
UB^0 &= p_{1|0}(\overline{Y}^{01} - y^l) + p_{0|0}(y^u - \overline{Y}^{00}) \\
UB_1 &= p_{1|1}(\overline{Y}^{11} - y^l) + p_{0|1}(y^u - \overline{Y}^{10}).
\end{aligned}
$$

By Assumption 4, the lower bounds in Proposition 1 are negative while the upper bounds are positive. Such bounds are often uninformative in practice. Examples in which bounds involving IVs under a bounded-outcome assumption are wide include Manski (1990), Balke and Pearl (1997), Heckman and Vytlacil (2000), Kitagawa (2009), Chen et al. (2014), and Huber and Mellace (2013).

## 2.2 Bounds under Weak Monotonicity Assumptions

In this subsection, we derive bounds on $E[\Delta(z)]$ under two sets of weak monotonicity assumptions that relate the unidentified terms in equations (8) through (10) to other point or partially identified terms. Once bounds for each of the non-point-identified terms in equations (8) through (10) are obtained, they are plugged in the corresponding equations, and the resulting bounds are compared to rule out lower (upper) bounds that are always smaller (greater) than the others. For simplicity, the weak monotonicity assumptions are presented below using weak inequalities in one particular direction; however, this direction can be changed depending on the empirical setting. Furthermore, each set of monotonicity assumptions below could be substantiated by economic theory pertinent to the empirical setting. The first set of assumptions considered are weak monotonicity of mean potential outcomes within strata.

**Assumption 5.** (*Weak Monotonicity of Mean Potential Outcomes Within Strata).*

*5.1* $LMATE_c^z \geq 0$; *5.2.* $LNATE_k^z \geq 0$, for $k = nt, at, c$;

*5.3* $E[Y(z)|at] \geq E[Y(z,0)|at], E[Y(z,1)|nt] \geq E[Y(z)|nt]$; where $z = 0, 1$.

Assumption 5.1 states that the treatment has a non-negative average effect on the outcome for compliers, regardless of exposure status to the instrument. Assumption 5.2 states that, within each stratum, the instrument has a non-negative average effect on the outcome that works through channels other than the treatment. When combined with Assumption 3, Assumption 5.1 implies $MATE^z \geq 0$ while Assumption 5.2 implies that $NATE^z \geq 0$; for $z = 0, 1$. Therefore, under Assumptions 3, 5.1 and 5.2, we have $ATE_{ZY} \geq 0$. Assumption 5.3 imposes non-negative average treatment effects on always takers and never takers by considering their respective counterfactual treatment status. Note that Assumption 5.2 is not strictly necessary to derive bounds on $\Delta(z)$, but it is helpful in tightening the bounds.[3] In contrast, Assumptions 5.1 and 5.3 are indispensable. Assumption 5.1 allows partial identification of $Y(z, D_{1-z})$ for compliers. As for the two inequalities in Assumption 5.3, not only $Y(z,0)$ for always takers and $Y(z,1)$ for never takers are counterfactually hypothetical, just as $Y(z, D_{1-z})$ for compliers, but additional information is unavailable by their local mechanism and net effects because of their compliance

[3] For example, the upper bound for $E[Y(0)|nt]$ is the minimum of the upper bound derived using the trimming procedure described above and $\overline{Y}^{10}$, which is derived by the equation $E[Y(1,D_0)|nt] = E[Y(1)|nt] = \overline{Y}^{10}$ implied by Assumption 5.2.

behavior. Finally, note that since Assumption 5.3 only provides one-sided bounds for these counterfactual outcomes, the bounded-support assumption is also necessary to derive their bounds.

Similar assumptions regarding weak monotonicity of outcomes have been considered to partially identify average treatment effects in IV models (e.g., Manski and Pepper, 2000) and in other settings (Manski, 1997; Sjölander, 2009). For instance, Manski and Pepper (2000) employ the monotone treatment response (MTR) assumption that postulates the *individual-level* treatment effect is non-negative, i.e., $Y_{1i} \geq Y_{0i}$ for all $i$. Different from the MTR assumption, Assumptions 5.1 and 5.3 allow some individuals to experience negative treatment effects by imposing the monotonicity restriction on the average treatment effects at the *stratum level*.

To present the identification result, let $y_r^{zd}$ be the $r$-th quantile of $Y$ conditional on $Z = z$ and $D = d$. For ease of exposition, suppose $Y$ is continuous so that $y_r^{zd} = F^{-1}_{Y|Z=z,D=d}(r)$, with $F_{\cdot}(\cdot)$ the conditional density of $Y$ given $Z = z$ and $D = d$. We denote by $U^{z,k}$ and $L^{z,k}$ the upper and lower bounds, respectively, on the mean potential outcome $Y(z)$ for the stratum $k$ derived using the trimming procedure described above, where $z = 0, 1$ and $k = at, nt, c$. The following proposition presents the bounds on $E[\Delta(z)]$ under Assumptions 1 through 5.

**Proposition 2** *If Assumptions 1 through 5 hold, then* $0 \leq \Delta(0) \leq \min\{UB_a^0, UB_b^0\}$ *and* $0 \leq \Delta(1) \leq \min\{UB_a^1, UB_b^1\}$ *are sharp. And for* $z = 0, 1$,

$$0 \leq E[\Delta(z)] \leq \Pr(Z = 0) \min\{UB_a^0, UB_b^0\} + \Pr(Z = 1) \min\{UB_a^1, UB_b^1\},$$

*where*

$$\begin{aligned}
UB_a^0 &= p_{1|0}(\overline{Y}^{01} - y^l) + p_{0|1}(y^u - L^{0,nt}) + E[Y|Z=1] - E[Y|Z=0] \\
&\quad -p_{1|0} \max\{0, L^{1,at} - \overline{Y}^{01}\} - p_{0|1} \max\{0, \overline{Y}^{10} - U^{0,nt}\} \\
UB_b^0 &= p_{1|0}(\overline{Y}^{01} - y^l) - p_{0|0}\overline{Y}^{00} + p_{0|1}y^u + (p_{0|0} - p_{0|1})U^{1,c} \\
UB_a^1 &= p_{1|0}(U^{1,at} - y^l) + p_{0|1}(y^u - \overline{Y}^{10}) + E[Y|Z=1] - E[Y|Z=0] \\
&\quad -p_{1|0} \max\{0, L^{1,at} - \overline{Y}^{01}\} - p_{0|1} \max\{0, \overline{Y}^{10} - U^{0,nt}\} \\
UB_b^1 &= p_{1|1}\overline{Y}^{11} + p_{0|1}(y^u - \overline{Y}^{10}) - p_{1|0}y^l - (p_{1|1} - p_{1|0})L^{0,c};
\end{aligned}$$

$$\begin{aligned}
U^{1,at} &= E[Y|Z=1, D=1, Y \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] \\
L^{1,at} &= E[Y|Z=1, D=1, Y \leq y^{11}_{(p_{1|0}/p_{1|1})}] \\
U^{0,nt} &= E[Y|Z=0, D=0, Y \geq y^{00}_{1-(p_{0|1}/p_{0|0})}] \\
L^{0,nt} &= E[Y|Z=0, D=0, Y \leq y^{00}_{(p_{0|1}/p_{0|0})}] \\
U^{1,c} &= E[Y|Z=1, D=1, Y \geq y^{11}_{(p_{1|0}/p_{1|1})}] \\
L^{0,c} &= E[Y|Z=0, D=0, Y \leq y^{00}_{1-(p_{0|1}/p_{0|0})}].
\end{aligned}$$

**Proof.** See Appendix.

The lower bound 0 for $\Delta(z)$, for $z = 0, 1$, is derived from equation (8), which produces the largest analytical lower bounds across the three equations. $UB_a^z$ and $UB_b^z$ are derived from equations (9) and (10), respectively.

The second set of assumptions we consider does not impose restrictions on the sign of the local average effects of each stratum as Assumption 5 does, which may be objectionable in certain applications Instead, it involves weak monotonicity of mean potential outcomes across strata.

**Assumption 6.** (*Weak Monotonicity of Mean Potential Outcomes Across Strata).*

*6.1* $E[Y(z)|at] \geq E[Y(z, D_{1-z})|c] \geq E[Y(z)|nt]$;

*6.2* $E[Y(z)|at] \geq E[Y(z)|c] \geq E[Y(z)|nt]$;

*6.3* $E[Y(z,0)|at] \geq E[Y(z, D_0)|c], E[Y(z, D_1)|c] \geq E[Y(z,1)|nt]$, where $z = 0, 1$.

Assumption 6 states that the mean potential outcomes of the always takers are greater than or equal to those of the compliers, and that these in turn are greater than or equal to those of the never takers. Thus, Assumption 6 formalizes the notion that some strata are likely to have more favorable characteristics and thus better mean potential outcomes than others. Assumption 6.1 provides bounds for $E[Y(z, D_{1-z})|c]$ by employing the fact that $Y(z, D_{1-z}) = Y(z)$ for never takers and always takers. Assumption 6.2 considers the average outcomes of the instrument across strata and, although not strictly necessary to derive the bounds, it plays an important role in tightening them. For example, combining Assumption 6.2 with equation (7) yields $\overline{Y}^{00} \geq E[Y(0)|nt]$, where by definition $\overline{Y}^{00}$ is less than or equal to $U^{0,nt}$ in Proposition 2. Assumption 6.3 provides one-sided bounds to the counterfactual potential outcomes of never takers and always takers by employing those of compliers under the same potential values of the instrument and the treatment.

Two attractive features of Assumption 6 are (1) it may be substantiated with economic theory in practice and (2) it contains testable implications. Regarding the first feature, for instance, depending on the application, we may expect from theory that individuals in each stratum have (average) traits that will imply that their mean potential outcomes vary weakly monotonically across strata. As for the second feature, the combination of Assumptions 1, 3 and 6.2 implies that $\overline{Y}^{01} \geq \overline{Y}^{00}$ and $\overline{Y}^{11} \geq \overline{Y}^{10}$. These two inequalities follow from equation (7) and the corresponding equation for the observed group $\{Z, D\} = \{1, 1\}$, respectively. They can be used in practice to falsify the assumptions. Moreover, it is possible to get indirect evidence about the plausibility of Assumption 6 by looking at relevant average baseline characteristics (e.g., pre-treatment outcomes) of the different strata. These tools will be implemented and further discussed in the context of our empirical analysis.

Assumption 6 is different from the monotone instrumental variable (MIV) assumption in Manski and Pepper (2000). The MIV assumption states that mean potential outcomes as a function of the treatment vary weakly monotonically in groups defined by observed values of the instrument: e.g., $E[Y_d|Z=1] \geq E[Y_d|Z=0]$ for $d=\{0,1\}$. It relaxes the traditional mean independence assumption in IV models by allowing the instrument to monotonically affect the average potential outcome of the treatment. Assumption 6 also relaxes the mean independence assumption but differs from the MIV assumption in two important ways. First, Assumption 6 refers to potential outcomes that explicitly allow the instrument to have a causal effect on the outcome (through $D$ and other channels), by writing them as a function of the treatment and the instrument. Second, Assumption 6 imposes weak monotonicity on the mean potential outcomes across subpopulations defined by the principal strata, as opposed to the observed values of the instrument. None of the MIV assumption and our Assumption 6 appear to be weaker than the other. The following proposition presents the bounds on $E[\Delta(z)]$ employing Assumption 6.

**Proposition 3** *If Assumptions 1 through 4, and 6 hold, then the bounds $LB^0 \leq \Delta(0) \leq UB^0$ and $LB^1 \leq \Delta(1) \leq UB^1$ are sharp. And for $z=0,1$,*

$$\Pr(Z=0)LB^0 + \Pr(Z=1)LB^1 \leq E[\Delta(z)] \leq \Pr(Z=0)UB^0 + \Pr(Z=1)UB^1,$$

*where*

$$\begin{aligned}
LB^0 &= p_{1|0}(\overline{Y}^{01} - y^u) + p_{0|1}(y^l - L^{0,nt}) + p_{0|0}(L^{0,nt} - \overline{Y}^{00}) \\
LB^1 &= p_{0|1}(y^l - \overline{Y}^{10}) + p_{1|0}(U^{1,at} - y^u) + p_{1|1}(\overline{Y}^{11} - U^{1,at}) \\
UB^0 &= \overline{Y}^{01} - \overline{Y}^{00} \\
UB^1 &= \overline{Y}^{11} - \overline{Y}^{10};
\end{aligned}$$

$$\begin{aligned}
U^{1,at} &= E[Y|Z=1, D=1, Y \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] \\
L^{0,nt} &= E[Y|Z=0, D=0, Y \leq y^{00}_{(p_{0|1}/p_{0|0})}].
\end{aligned}$$

**Proof.** See Appendix.

The lower and upper bounds for $\Delta(z)$ (for $z=0,1$) are derived from equation (10). The fact that none of the bounds in Proposition 3 comes from equations (8) and (9) is intuitive because these two equations exploit assumptions on the signs of $LMATE^z_c$, and $LNATE^z_k$, for $z=0,1$ and $k=nt,at,c$, which are not imposed in Assumption 6. The lower bound on $E[\Delta(z)]$ in Proposition 3 is always less than or equal to zero because $LB^0$ and $LB^1$ are non-positive by the bounded-outcome assumption and the nature of the trimming bounds ($L^{0,nt}$ and $U^{1,at}$). Thus, the lower bounds in Proposition 3 cannot be used to rule out a negative $E[\Delta(z)]$.

Finally, we combine Assumptions 5 and 6 to construct bounds on $E[\Delta(z)]$. Combining Assumptions 5 and 6 yields an additional testable implication: $\overline{Y}^{11} \geq \overline{Y}^{00}$.[4] As shown in Proposition 4, once these two assumptions are combined, the bounded-outcome assumption (Assumption 4) is no longer necessary.

**Proposition 4.** *If Assumptions 1 through 3, 5 and 6 hold, then* $0 \leq \Delta(0) \leq \min\{UB_a^0, UB_b^0\}$ *and* $0 \leq \Delta(1) \leq \min\{UB_a^1, UB_b^1\}$ *are sharp. And for* $z = 0, 1$*,*

$$0 \leq E[\Delta(z)] \leq \Pr(Z=0)\min\{UB_a^0, UB_b^0\} + \Pr(Z=1)\min\{UB_a^1, UB_b^1\},$$

*where*

$$\begin{aligned}
UB_a^0 &= E[Y|Z=1] - \overline{Y}^{00} - p_{0|1}(L^{0,nt} - \overline{Y}^{11} + \max\{0, \overline{Y}^{10} - \overline{Y}^{00}\}) \\
UB_b^0 &= p_{1|0}\overline{Y}^{01} - \overline{Y}^{00} + p_{0|0}\min\{\overline{Y}^{11}, \overline{Y}^{01}\} \\
UB_a^1 &= \overline{Y}^{11} - E[Y|Z=0] + p_{1|0}(U^{1,at} - \overline{Y}^{00} - \max\{0, \overline{Y}^{11} - \overline{Y}^{01}\}) \\
UB_b^1 &= \overline{Y}^{11} - p_{0|1}\overline{Y}^{10} - p_{1|1}\max\{\overline{Y}^{10}, \overline{Y}^{00}\};
\end{aligned}$$

$$\begin{aligned}
U^{1,at} &= E[Y|Z=1, D=1, Y \geq y^{11}_{1-(p_{1|0}/p_{1|1})}] \\
L^{0,nt} &= E[Y|Z=0, D=0, Y \leq y^{00}_{(p_{0|1}/p_{0|0})}].
\end{aligned}$$

**Proof.** See Appendix.

The lower bound 0 is derived from equation (8) under Assumption 5, while the upper bounds $UB_a^z$ and $UB_b^z$ (both for $z = 0, 1$) come from equations (9) and (10), respectively.

We close this section with a few final remarks pertaining to the fact that the particular conditions imposed in Assumptions 5 and 6 can be adjusted depending on their plausibility, identifying power, and the economic theory behind any particular application. First, some particular assumptions can be dropped if they are not plausible or needed in a given application. As previously mentioned, Assumptions 5.2 and 6.2 for the *nt* and *at* strata are not strictly necessary to derive bounds on $\Delta(z)$. Similarly, other assumptions can be dropped if interest lies only on a lower or upper bound for $\Delta(z)$. Second, the direction of the weak inequalities, including that in Assumption 3, can be reversed depending on the empirical setting. Third, some specific potential outcomes in the assumptions can be changed. For instance, Assumption 6.1 could be changed to $E[Y(1, D_0)|c] \geq E[Y(0)|nt]$, which may be easier to justify in some empirical settings.

[4] Note that Assumptions 5 and 6 imply $E[Y(1)|at] \geq E[Y(0)|at] \geq E[Y(0)|c] \geq E[Y(0)|nt]$ and $E[Y(1)|c] \geq E[Y(0)|c] \geq E[Y(0)|nt]$. The result follows from combining these inequalities with equation (7) and the corresponding equation for the observed group $\{Z, D\} = \{1, 1\}$.

### 2.3 Bounds on the $ATT$

This subsection uses the same approach as above to derive the bounds on the average effect on the treated ($ATT$) while allowing the IV to have a direct effect on the outcome. The average effect $D$ on $Y$ for the treated is defined as $E[Y_{1i} - Y_{0i}|D = 1] \equiv E[E[Y_i(z,1) - Y_i(z,0)|Z = z, D = 1]]$. Let us denote $\Pr(Z = z) = w_z$ and $\Pr(D = 1) = r_1$. By Table 1, the $ATT$ can be written as:

$$ATT = \frac{w_1 p_{1|1}}{r_1}(\overline{Y}^{11} - E[Y(1,0)|at,c]) + \frac{w_0 p_{1|0}}{r_1}(\overline{Y}^{01} - E[Y(0,0)|at]) \quad (11)$$

$$= E[Y|D = 1] - \frac{w_1 p_{1|1}}{r_1}E[Y(1,0)|at,c] - \frac{w_0 p_{1|0}}{r_1}E[Y(0,0)|at] \quad (12)$$

Equation (11) employs the Bayesian rule, $\Pr(Z = z|D = 1) = \frac{w_z p_{1|z}}{r_1}$, and the definition of the principal strata, while equation (12) is derived from $r_1 E[Y|D = 1] = w_1 p_{1|1}\overline{Y}^{11} + w_0 p_{1|0}\overline{Y}^{01}$. According to equation (11), we further write the $ATT$ as $ATT = \frac{w_1}{r_1}\Gamma(1) + \frac{w_0}{r_1}\Gamma(0)$, with $\Gamma(1) = p_{1|1}\overline{Y}^{11} - \pi_{at}E[Y(1,0)|at] - \pi_c E[Y(1,D_0)|c]$ and $\Gamma(0) = p_{1|0}(\overline{Y}^{01} - E[Y(0,0)|at])$. In particular, $E[Y(1,D_0)|c]$ in $\Gamma(1)$ is bounded by linking with the causal mechanism effects, while $E[Y(1,0)|at]$ and $E[Y(0,0)|at]$ are bounded by Assumptions 4, 5.3 and 6.3.

Similar to deriving bounds on $\Delta(z)$, we write $\Gamma(1)$ as functions involving local causal mechanism and net effects that can be either point identified or partially identified:

$$\Gamma(1) = \pi_{at}(E[Y(1)|at] - E[Y(1,0)|at]) + \pi_c LMATE_c^1 \quad (13)$$

$$= \pi_{at}(E[Y(0)|at] - E[Y(1,0)|at]) + E[Y|Z = 1] - E[Y|Z = 0]$$
$$-\pi_{nt}LNATE_{nt}^0 - \pi_c LNATE_c^0 \quad (14)$$

$$= p_{1|1}\overline{Y}^{11} - \pi_{at}E[Y(1,0)|at] - \pi_c E[Y(1,D_0)|c] \quad (15)$$

Each of the equations above exploits different information in the data and would generate different bounds on $\Gamma(1)$ (and thus on the $ATT$). The rest of this subsection briefly presents the bounds on the $ATT$ under each set of the assumptions above, with their proofs provided in the Appendix.

Under the assumptions in Proposition 1 (A4), the bounds $lb \leq ATT \leq ub$ are sharp, where

$$lb = E[Y|D = 1] - y^u$$
$$ub = E[Y|D = 1] - y^l.$$

Under the assumptions in Proposition 2 (A5), the bounds $0 \leq ATT \leq \min\{ub_a, ub_b\}$ are sharp, where

$$ub_a = E[Y|D = 1] - \frac{w_1}{r_1}(p_{0|0}\overline{Y}^{00} - p_{0|1}\overline{Y}^{10}) - \frac{p_{1|0}}{r_1}y^l$$
$$ub_b = E[Y|D = 1] - \frac{w_1}{r_1}(p_{1|1} - p_{1|0})L^{0,c} - \frac{p_{1|0}}{r_1}y^l.$$

Under the assumptions in Proposition 3 (A6), the bounds $lb \leq ATT \leq ub$ are sharp, where

$$\begin{aligned} lb &= E[Y|D=1] - \frac{w_1}{r_1}(p_{1|1} - p_{1|0})U^{1,at} - \frac{p_{1|0}}{r_1}y^u \\ ub &= E[Y|D=1] - \frac{w_1 p_{1|1}}{r_1}\overline{Y}^{10} - \frac{w_0 p_{1|0}}{r_1}\overline{Y}^{00}. \end{aligned}$$

Under the assumptions in Proposition 4 (A5 & A6), the bounds $0 \leq ATT \leq \min\{ub_a, ub_b\}$ are sharp, where

$$\begin{aligned} ub_a &= E[Y|D=1] - \frac{w_1}{r_1}(p_{0|0}\overline{Y}^{00} - p_{0|1}\overline{Y}^{10}) - \frac{p_{1|0}}{r_1}\overline{Y}^{00} \\ ub_b &= E[Y|D=1] - \frac{w_1 p_{1|1}}{r_1}\overline{Y}^{10} - \frac{w_0 p_{1|0}}{r_1}\overline{Y}^{00}. \end{aligned}$$

### 2.4 Estimation and Inference

Most of our bounding functions can be estimated by plug-in estimators, for example, the bounds on $ATE$ in Propositions 1, 2, and 3, and the bounds on $ATT$. The bounds on $ATE$ in Proposition 4 involve minimum (min) or maximum (max) operators, which create complications for estimation and inference. First, because of the concavity (convexity) of the min (max) function, sample analog estimators of the bounds can be severely biased in small samples. Second, closed-form characterization of the asymptotic distribution of estimators for parameters involving min or max functions are very difficult to derive and, thus, usually unavailable. Furthermore, Hirano and Porter (2012) show that there exist no locally asymptotically unbiased estimators and no regular estimators for parameters that are nonsmooth functionals of the underlying data distribution, such as those involving min or max operators. These issues have generated a growing literature on inference methods for partially identified models of this type (see Tamer, 2010, and the references therein).

We employ the methodology proposed by Chernozhukov, Lee and Rosen (2011) (hereafter CLR) to obtain confidence regions for the true parameter value, as well as half-median unbiased estimators for the bounds on $ATE$ in Proposition 4. The half-median-unbiasedness property means that the upper (lower) bound estimator exceeds (falls below) the true value of the upper (lower) bound with probability at least one half asymptotically. This is an important property because achieving local asymptotic unbiasedness is not possible, implying that "bias correction procedures cannot completely eliminate local bias, and reducing bias too much will eventually cause the variance of the procedure to diverge" (Hirano and Porter, 2012). For details on our implementation of CLR's method see Flores and Flores-Lagunes (2013). We also employ CLR's methodology to the bounds without min or max operators due to its asymptotically validity.

# 3 Empirical Application

We illustrate our bound analysis using the public-use data from the Oregon Health Insurance Experiment (OHIE) to examine the effects of Medicaid coverage on health care and preventative care utilization, self-reported health status and financial strain, taking into account the possibility that Medicaid lottery may violate the exclusion restriction of the IV assumption.

## 3.1 Data from the OHIE

In January 2008, Oregon initiated a Medicaid expansion program for low-income adults, the Oregon Health Plan (OHP) Standard. Its eligible adults should be aged 19-64, Oregon residents, U.S. citizens or legal immigrants, without health insurance for at least six months, not otherwise eligible for public insurance. And their income should be below the federal poverty level (FPL, \$10,400 for an individual and \$21,200 for a family of four in 2008), and assets below \$2000. OHP Standard provides relatively comprehensive medical benefits (except vision and nonemergency dental services) with no cost sharing and low monthly premiums (varying between \$0-\$20 dependent on income). The state conducted eight lottery drawings randomly selected from a waiting list from March through September 2008. Selected individuals won the opportunity for any household member (whether listed or not) to apply for Medicaid coverage. Thus the lottery (i.e. the IV) is random conditional on the number of household members (i.e. household size) on the waiting list. Selected individuals who completed the application process and met the eligibility requirements were enrolled in either OHP Plus or OHP Standard.[5] (Finkelstein et al., 2012; Taubman et al., 2014).

We use the 12-month survey data from OHIE to examine the effects of Medicaid coverage. The survey was mailed out in seven waves over July and August 2009, and the average survey response occurs roughly one year after insurance approval (mean = 13.1 months, with std. dev. = 2.9 months). The three sets of outcomes we consider include health care and preventative care utilization, self-reported health status, and financial strain. Consistent with Finkelstein et al. (2012), the IV indicates whether household was selected by the lottery, and the treatment denotes whether the individual of that household was ever on Medicaid (including both OHP Standard and OHP Plus) during the study period. Our paper, however, investigates the bounds on $ATE$ and $ATT$ of Medicaid coverage, taking into account the possibility that the lottery may violate the exclusion restriction. Due to the sampling strategy, the probability of winning the lottery varies by survey waves and within household size. Thus, we construct a weight, which predicts the probability of winning the lottery conditional on household size, survey waves,

[5] When reviewing applications, the state first examined eligibility for OHP Plus and then, if not eligible for Plus, examined eligibility for OHP Standard. OHP Plus serves the categorically eligible Medicaid population, including children, pregnant women, the disabled and families enrolled in Temporary Assistance to Needy Families (TANF) (Finkelstein et al., 2012).

their interaction terms, and the 12-month sampling weight. Additionally, because winning the lottery occurs at the household level, we calculate cluster standard errors.

Table 1 shows the summary statistics of demographic variables from the OHIE. The upper panel shows the means of the treatment and control groups and their differences for pre-treatment variables, as well as the proportions of missing values in our sample size of 23741 observations. Approximately 60% of individuals are female, 68% aged 19-49, over 90% choose English as a preferred language, and three-quarters live in a metropolitan area (MSA). Over one-half have ever participated in food stamps, while approximately 1% participated in TANF. Except for English as a preferred language, the survey sample shows balanced characteristics on these pre-treatment variables. The lower panel shows the statistics of other demographic variables from the initial survey of OHIE. The initial survey was conducted between June 2008 and November 2008, shortly after randomization. Over 80% of individuals are white, over one-half have high school diploma or GED, have household income above 150% of the federal poverty line, and don't have jobs at that time. Most of these demographic variables are balanced, though the treatment group have higher average household income and less of them have income below 50% of the poverty line.

Table 2 shows the $ITT$ estimates of relevant variables from the initial and 12-month surveys. 8.5% of individuals enrolled in Medicaid shortly after randomization. Individuals who had ever been diagnosed with chronic diseases take up similar proportions in the two groups, with differences equal to around 1%. The two groups show similar patterns on health care utilization, except that the treatment group take number of prescription drugs less than the control group by 5.5%. The treatment group report better health status by themselves and less financial strain due to medical expenses than the control group do, however, the disparities between the two groups are generally very small, around 1%. Outcomes on financial strain are defined as one minus of each measure in Finkelstein et al. (2012), that is, "less or no financial strain".In the following we will use the pre-treatment variables and the variables in the initial survey from Tables 1 and 2 to estimate average baseline characteristics of different strata to inform the ranking of the weak monotonicity assumption of mean potential average outcomes across strata (A6).

Table 3 shows the proportions of different strata in our samples. In the 12-month survey, never takers take up 57.6%, compliers 28.9%, and always takers 13.5%. 28% of individuals in the target population are actually covered by Medicaid. In addition, for the effects of Medicaid coverage on preventative care, we focus on women larger than 40 years old to examine the effect on mammogram, and women to examine the effect on pap test. The stratum proportions in these two samples show similar patterns to the ones in our main sample. The results of Tables 1, 2, and 3 are close to those in Finkelstein et al. (2012).

### 3.2 Assessment of Assumptions

The randomly assigned IV (A1) holds by the design of OHIE. The individual-level monotonicity of lottery indicator on Medicaid coverage (A2) implies that no individuals would get Medicaid coverage if lost the lottery and would not get coverage if won the lottery. It seems plausible in this application. Non-zero average effect of lottery on Medicaid coverage (A3) also holds in our data. As shown in Table 3, the average effect (i.e., $\pi_c$), is 0.289, which is positive and statistically significant. For the bounded-outcome support assumption (A4), we use the minimum and maximum values in the empirical data to bound the outcome measures we consider.

The weak monotonicity assumptions of mean potential outcomes within strata (A5) and across strata (A6) are of our great concern. A5.1 implies that for each stratum, Medicaid coverage has a non-negative average effect on the outcomes we consider, including health care and preventative care utilization, self-reported health, and alleviating financial strain. Medicaid coverage decreases the price of medical care and thus increases the quantity demanded for health care and preventative care. The increased quantity of health care may translate into improved health status.[6] As mentioned by Finkelstein et al. (2012), the positive effect on self-reported health status may also reflect a general sense of improved wellbeing due to Medicaid coverage, since data from the initial survey suggests no evidence of an increase in health care utilization. From the point of view of risk-spreading, Medicaid coverage may play positive roles in reducing financial strain due to medical expenses. Thus, we expect positive local mechanism effects on each outcome we consider.

A5.2 implies that for each stratum, winning lottery has non-negative average effects on the above outcomes through channels other than Medicaid coverage. For example, winning lottery may raise individuals' awareness of health and make themselves adjust towards heathy lifestyle, which may increase health care and preventative care utilization and improve their health status. Furthermore, as pointed out by Finkelstein et al. (2012), individuals who apply for public health insurance may also be encouraged to apply for other public programs, such as food stamps and TANF. These cash transfer programs may improve selected individuals' nutritional intake through income effect and thus strengthen their health status. These public assistance programs may also improve households' financial situation, and thus play positive roles in reducing financial strain due to medical expense. Finkelstein et al. (2012) find that lottery selection is associated with a statistically significant increase in the probability of food stamp receipt and in total food stamp benefits. The bottom of Table 2 shows insurance coverage and public assistance programs in the 12-month survey. Winning Medicaid lottery make more individuals in the treatment group covered by Medicaid by 29.0% than the control group. Consistent with the findings in Finkelstein et al. (2012), more individuals in the treatment

[6] Though self-reported health measures might be less accurate than physical health measures, diagnosis of mental health, by its nature, relies on such self-reports, for instance, depression (Finkelstein et al., 2012).

group participate in TANF and the food stamp, and their average benefits are also higher than those in the control group. In particular, Medicaid lottery increases the probability of food stamp receipt by 2.7%, and increases total benefits by $96.99. All of the above implies that it is possible that Medicaid lottery may violate the exclusion restriction of a valid IV.

A5.3 implies that non-negative average treatment effects for always takers and never takers. If we could force always takers to be absent of Medicaid coverage and never takers to be covered by Medicaid insurance, then we expect Medicaid coverage to increase their health care and preventative care utilization, to improve self-reported health status, and to increase the probability of having no financial strain due to medical expenses.

Since the $ITT$ effect is decomposed as the sum of local mechanism and net effects, combination of A5.1 and A5.2 imply non-negative $ITT$ effects. Table 4 shows the $ITT$ effects of Medicaid lottery on health care utilization and preventative care. The $ITT$ effects on prescription drugs currently and outpatient visits last six months (both extensive and intensive measures) are all positive and statistically significant. The $ITT$ effects on preventative care (blood cholesterol checked and blood tested for high blood sugar/diabetes ever, mammogram and pap test within last 12 months) are also positive and significant. Table 5 shows the $ITT$ effects on seven measures of self-reported health status (not fair/poor and not poor currently; health about the same or gotten better over last six months; did not screen positive for depression last two weeks; numbers of days physical health good, mental health good, poor physical or mental health did not impair usual activity past 30 days), all of which are positive and statistically significant. Table 6 shows the positive and statistically significant $ITT$ effects on alleviating financial strain (not owe for medical expenses currently; no out of pocket medical expenses, not borrow to pay medical bills and not be refused treatment due to medical debt last six months). In addition, these tables also show the point estimate of $LATE_c$ if Medicaid lottery serves as a valid IV. These $LATE_c$ estimates are all positive and significant, which are very close to those in Finkelstein et al. (2012).

The basic notion behind the weak monotonicity assumption of mean potential outcomes across strata (A6) is that always takers are likely to have more favorable characteristics and thus better mean potential outcomes than others. The adverse selection theory predicts that people in poor health are more likely to select health insurance than healthy people, and thus they may also demand more medical care. By the definition of principal stratum, always takers are individuals who are covered by Medicaid regardless of lottery selection while never takers are individuals who are never covered by Medicaid. Thus, it is reasonable to presume that always takers are in the poorest health among the three while never takers are the most healthy group.

The bottom parts of Tables 4, 5, and 6 show the point estimates of average outcomes of different strata, which indirectly suggest the ranking of the weak monotonicity across strata.

For health care and preventative care utilization, we have $\overline{Y}^{11} > E[Y(1)|nt]$ and $E[Y(0)|at] > \overline{Y}^{00}$, which is consistent with the implication of A6.2, and $\overline{Y}^{11} > \overline{Y}^{00}$, which holds under the combined A5 and A6. These inequalities indirectly supports our presumption that always takers generally have the poorest health status, and thus use more health care services, which is consistent with the ranking of A6.

The bottom of Table 5 shows the estimated average outcomes on self-reported health status. Based on our presumption that never takers have the best health status among the three and always takers have the poorest, the ranking of the weak monotonicity assumption across strata is reversed for self-reported status. We show the reversed assumption (A6') and the bounds under the reversed assumption (Propositions 3' and 4') in the Appendix. Under the reversed A6.2', the testable implications are $\overline{Y}^{11} \leq E[Y(1)|nt]$ and $E[Y(0)|at] \leq \overline{Y}^{00}$, while combining A5 and A6' implies that $E[Y(0)|at] < E[Y(1)|nt]$. All of these inequalities hold for the seven measures of health status.

The bottom of Table 6 shows the estimated average outcomes on alleviating financial strain. For no out of pocket medical expenses and not borrowing money to pay medical bills, we have $\overline{Y}^{11} > \overline{Y}^{00}$, $\overline{Y}^{11} > E[Y(1)|nt]$, and $E[Y(0)|at] > \overline{Y}^{00}$. For not owing for medical expenses and not being refused treatment due to medical debt, we have $\overline{Y}^{11} > \overline{Y}^{00}$ and $\overline{Y}^{11} > E[Y(1)|nt]$, and $E[Y(0)|at] - \overline{Y}^{00}$ is not statistically different from zero. Thus, these inequalities are consistent with the testable implications of our Assumption A6. As we will show in Table 7, never takers have the most favorable economic situation among the three while always takers have the worst. It is probably that never takers are more concerned with the quality of health care services and thus would rather pay for out of pocket medical expenses (for instance, vision care and dental care, which are not covered by Medicaid), and that they borrow money to pay for medical bills because of their capability to make repayments. In that sense, never takers might tend to owe money for medical expenses during the survey period and to be refused treatment due to medical debt. Furthermore, as we will show in Table 7, always takers are generally covered by Medicaid insurance, which has no cost sharing and only low monthly premiums. Always takers also obtain more benefits from public assistance programs compared with the other groups, which might let them less likely suffer financial strain due to medical expenses. Given the poor health status and vulnerable economic situation of always takers, they are more likely to choose cheap medical care services they could afford. Therefore, the weak monotonicity assumption across strata on alleviating financial strain is the same as the one we use for health care and preventative care utilization (A6).

To further inform the ranking of weak monotonicity across strata, we could obtain their average baseline characteristics of different strata by estimating a non-parametric GMM problem. The details on the GMM problem is provided in the Appendix. Table 7 shows average baseline characteristics of the 12-month survey sample using pre-treatment variables and some demographic variables from the initial survey. The initial survey was conducted on average 2.6 months after randomization, and about 1 month after coverage approval. Taking into account the short time span and the results in Table 2, we argue that Medicaid lottery generally has no effects on demographic variables, and the small effects on the outcomes we consider are negligible (most of which are around 1%).

As shown in Table 7, always takers are more likely to be females, younger, and more of them live in a metropolitan area. They are more likely to enroll in food stamp and TANF, and obtain more benefits. Additionally, always takers have lower level of education, less household income, and most of them do not work during the initial survey period. In contrast, never takers tend to have the highest level of education and of household income among the three strata, and more of them work more than 30 hours per week. During the period of the initial survey, more of never takers enrolled in private insurance while more of compliers and always takers enrolled in Medicaid. Therefore, never takers have the most favorable economic situation among the three group while always takers have the worst. From this perspective, never taker may also demand larger quantity of health care services than always takers do due to their higher income, which contradicts to the prediction of adverse selection. However, the differences between the two groups on health care utilization in Table 7 show that always takers demand the highest quantity among the three, and thus indirectly supports the weak monotonicity assumption across strata A6 on health care and preventative care utilization. Furthermore, Table 7 shows that always takers are more likely to have been ever diagnosed with chronic diseases, and never takers are generally the most healthy group, which is consistent with the prediction of adverse selection theory. For the financial strain, compliers are less likely to have out of pocket medical expenses, always takers are more likely to borrow money to pay for medical bills, and never takers are less likely to owe money for medical expenses or to be refused treatment due to medical debt. Therefore, the average baseline characteristics further support our weak monotonicity assumptions across strata on the different outcomes we consider.

### 3.3 Empirical Bounds in OHIE

Table 8 shows the bounds of average effects on health care utilization. The bounds on $ATE$ and $ATT$ are usually uninformative under the bounded support assumption (A4). Under the monotonicity assumption of mean potential outcomes within strata (A5), the bounds are restricted to non-negative regions. Under the weak monotonicity assumption of mean potential outcomes across strata that always takers use the largest quantity of health care services among the three groups (A6), the bounds are narrower than those under A4, especially the upper bounds. The bounds under the combined assumptions are narrower than those under A5, though we cannot rule out zero effect. The lower panel of Table 8 shows the bounds on the local net effects of never takers and always takers as well as on the local average effect of compliers, which is a weighted average of the local mechanism effects for compliers at $Z = 1$ and $Z = 0$. The bounds of $LNATE_{nt}$ on outpatient visits are in the positive region under A6 without assuming the signs of the local effects (A5). Winning the lottery increases the probability of outpatient visits for never takers by between 2.3% and 25.6%, and its 95% confidence interval also stays in the positive region. Winning the lottery also increases the number of outpatient visits by between .086 and 1.377, and its 90% confidence interval, $[0.008, 1.445]$, rules out zero effect. This decent evidence indicates that winning the lottery may violate the exclusion restriction. Under the combined assumptions, the bounds of $LNATE_{nt}$ and $LNATE_{at}$ on prescription drugs stay in non-negative regions. The point estimate of $LATE_c$ on the probability of outpatient visits under the exclusion restriction falls outside the bounds of $LATE_c$ under the combined assumptions. To test it more formally, we employ CLR's method to calculate the half-median-unbiased estimators and the confidence intervals for the difference between the bounds of $LATE_c$ and the point estimate of $LATE_c$ under the exclusion restriction. Though its confidence interval contains zero, the difference on the probability of outpatient visits is bounded between $-.215$ and $-.026$, reinforcing that Medicaid lottery may violate the exclusion restriction for this outcome.

Table 9 shows the bounds of average effects on preventative care. Similar to the case of health care utilization, the bounds on $ATE$ and $ATT$ are usually uninformative under A4. Under A5, the bounds are restricted to non-negative regions. Under the same weak monotonicity assumption across strata as the one for health care utilization (A6), the bounds are narrower than those under A4, especially the upper bounds. As a result, the bounds under the combined assumptions are also narrower than those under A5. In particular, the upper bounds on the probability of blood cholesterol checked and blood tested for high blood sugar or diabetes are very informative. For example, the $ATE$ and $ATT$ of Medicaid coverage on the probability of blood cholesterol checked are no larger than 4.2% and 5.3%, respectively. The lower panel shows the local effects of different strata. The bounds of $LNATE_{nt}$ are in the positive region under A6 for all of the four measures. And the 90% confidence interval of $LNATE_{nt}$ for mammogram test and the 95% confidence interval for pap test are also positive, $[.004, .309]$ and $[.007, .337]$, respectively. In addition, the bounds of $LNATE_{at}$ on the probability of blood cholesterol checked is bounded between 5.4% and 36.4%. All of the above indicates that medicaid lottery may violate the exclusion restriction at least for the preventative care measures. The bounds on $LATE_c$ are lying in the non-negative regions under the combined assumptions. The point estimates of $LATE_c$ under the exclusion restriction fall outside the bounds of $LATE_c$ under the combined assumptions. Their differences are bounded in the negative region, and the corresponding 95% confidence intervals also rules out zero. In particular, the difference on blood cholesterol check without assuming the signs of local effects (A5) also stay in the negative region. Therefore, violation of exclusion restriction may result in up-biased point estimates of

$LATE_c$ on preventative care measures.

Table 10 shows the bounds of average effects on binary outcomes of self-reported health status. The bounds on $ATE$ and $ATT$ are very wide under A4. A5 restricts our bounds to non-negative regions. The weak monotonicity assumption across strata on health status says that never takers are the group in best health among the three (A6'). The lower bounds under A6' is substantially larger than those under A4. The bounds are non-negative under the combined assumptions and largely narrower than those under A4. Unfortunately, the bounds of local net effects for never takers and always takers cannot rule out zero effect. Their upper bounds, however, are informative. For instance, winning the lottery increases the probability that health got the same or gotten better for never takers and always takers by no larger than 4.0% and 2.8%, respectively. Except for whether the individual is screened negative for depression, the point estimates of $LATE_c$ under the exclusion restriction fall outside the bounds of $LATE_c$ under the combined assumptions. Their differences are bounded in negative regions, as well as the 95% confidence intervals for health is not fair or poor and for health got the same or gotten better and the 90% confidence interval for health is not poor. Therefore, the point estimates of $LATE_c$ might be up-biased when the exclusion restriction is violated.

Table 11 shows the bounds of average effects on number of days in good health last 30 days. The bounds of $ATE$ and $ATT$ are uninformative under A4. The weak monotonicity assumption within strata (A5) transforms the negative lower bounds under A4 into zero. Under the weak monotonicity assumption across strata that never takers are the most healthy group (A6'), the majority of the identification regions of the bounds stays in positive regions. And the bounds under the combined assumption are much narrower than those under A4. Though the bounds on local net effects for never takers and always takers cannot rule out zero effect, the small upper bounds are informative. For example, Medicaid lottery increases the number of days in good mental health for never takers and always takers by no larger than 1.023 and .857, respectively. All the point estimates of $LATE_c$ under the exclusion restriction fall within the bounds of $LATE_c$ on the three measures.

Table 12 shows the bounds of average effects on alleviating financial strain. The monotonicity assumption within strata (A5) substantially shrinks the width of the bounds under the bounded supported assumption (A4), and restricts the bounds to non-negative identification regions. Under the weak monotonicity assumption across strata that never takers are more likely to suffer financial strain due to medical expenses (A6), the bounds are narrower compared with those under A4. The combined assumption produces informative bounds in the non-negative region. For example, Medicaid coverage increases the probability of not owing for medical expenses during the survey period for the entire population by no larger than 1.9%, and for the treated individuals by no larger than 3.2%. Except for no out of pocket medical expenses, the bounds of $LNATE_{nt}$ stay in positive regions under A6. For instance, Medicaid lottery increases the probability of not owing for medical expenses for never takers by between 3.5% and 33.1%, and its 95% confidence interval is $[.019, .357]$. And the $LNATE_{nt}$ on not being refused treatment due to medical debt is bounded between .8% and 4.8%, and its 90% confidence interval is $[.0004, .058]$. In addition, Medicaid lottery also increases the probability of alleviating financial strain for always takers, as shown by the bounds of $LNATE_{at}$ and their 95% confidence intervals lying in the positive region, except the confidence interval for not being refused treatment. All of the above provides decent evidence that the medicaid lottery may violate the exclusion restriction on alleviating financial strain. Furthermore, the point estimates of $LATE_c$ on the four measures under the exclusion restriction fall outside the bounds of $LATE_c$ without assuming the signs of local effects (A5) and under the combined assumptions. Their differences and the corresponding 95% confidence intervals are bounded in negative regions, except that the 90% confidence interval for not being refused treatment is negative. Thus, the point estimate of $LATE_c$ on alleviating financial strain may be up-biased without the exclusion restriction.

We also computes the bounds on the average effects when Medicaid lottery serves as a valid IV.[7] Table A1 shows the bounds on health care utilization. Compared with the bounds in Table 8, the width of the bounds substantially shrink under the exclusion restriction and most of the bounds are contained by the ones in Table 8. Furthermore, most of the bounds on $ATE$ and $ATT$ under the exclusion restriction identify the positive effects under the combined assumptions. The bounds on outpatient visits (both extensive and intensive measures) show different identification regions from the corresponding ones in Table 8. Table A2 shows the bounds on preventative care. Generally, the width of the bounds shrink substantially under the exclusion restriction, and the majority of their identification regions crosses over with the ones in Table 9. The exception happens to the bounds on blood cholesterol check. The identification regions of the bounds on $ATT$ and on $LATE_{at}$ stay on the right of the respective ones in Table 9, and the upper bound on $LATE_{nt}$ is much smaller than the one in Table 9. Tables A3 and A4 show that the bounds on self-reported health status. The bounds on $ATE$ and $ATT$ are similar to those without the exclusion restriction, but the former identify positive effects without assuming the signs of local effects for never taker and always takers. The upper bounds on local effects are usually substantially larger than the corresponding ones without the exclusion restriction. Table A5 shows the bounds on alleviating financial strain. The identification regions of the bounds of $ATE$ and $ATT$ cross over with the ones in Table 12 for no out of pocket medical expenses and not borrowing money to pay medical bills, while the

[7] We follow the methodology in Chen et al. (2014) to calculate the bounds on $ATE$ and $ATT$, and local effects on never takers and always takers. With the exclusion restriction, weak monotonicity within strata (A5) here would reduce to the monotonicity assumption A6 in that paper, and weak monotonicity across strata (A6) here would reduce to the mean dominance assumption A7c with the reversed direction. As a result, the bounds here would reduce to the bounds under the exclusion restriction because the equations to derive our bounds are simplified to the one under the exclusion restriciton.

identification regions are on the right of the ones in Table 12 for not owing for medical expenses and not being refused treatment due to medical debt. The identification regions of local effects generally cross over with the ones in Table 12, and their upper bounds are substantially larger in Table A5, except that the identification regions of $LATE_{at}$ are on the left of the ones for $LNATE_{at}$ in Table 12 for no out of pocket medical expenses and not owing for medical expenses.

## 4 Conclusion

We derive nonparametric sharp bounds on the population average treatment effect ($ATE$) and the average treatment effect on the treated ($ATT$) with an invalid instrumental variable (IV) that may violate the exclusion restriction. We accomplish our bound analysis by linking two key features. First, we write the $ATE$ or $ATT$ as a weighted average of the local average treatment effects in each of the principal strata, which are subpopulations that are defined by the joint potential values of the treatment status under each value of the instrument. Bounds are obtained after (point or partially) identifying each one of those local treatment effects. Second, we employ a causal mediation analysis framework to separate the total average effect of the instrument on the outcome into the part of the effect that works through the treatment, i.e., the mechanism effect ($MATE$) and the part of the effect through the channels net of the treatment, i.e., the net effect ($NATE$). When the exclusion restriction holds, the net effect of the instrument on the outcome equals zero and its mechanism effect equals the treatment effect of interest. Otherwise the none-zero net effect implies the violation of the exclusion restriction. We propose weak monotonicity assumptions relating the mean potential outcomes within and across different strata to add to the basic $LATE$ framework (Imbens and Angrist, 1994; Angrist, Imbens, and Rubin, 1996) to partially identify the $ATE$ and $ATT$. The two sets of weak monotonicity assumptions provide testable inequalities on the average pointed identified outcomes, which can be used to falsify assumptions. These assumptions can also be modified according to empirical applications and the economic theory behind them. Furthermore, our bounds on local net effects for non-compliers, whose treatment status are not affected by the instrument, provide a straightforward test for the exclusion restriction. The methods developed herein can be extended to the context where the IV is random conditional on covariates, which often holds in observational studies.

We employ the public-use data from the Oregon Health Insurance Experiment (OHIE) to illustrate the informativeness of our bound analysis. We investigate the $ATE$ and $ATT$ of Medicaid coverage on health care and preventative care utilization, self-reported health status and financial strain. The randomly assigned Medicaid lottery may serve as an invalid IV on the outcomes we consider. We find decent evidence that the exclusion restriction may had indeed been violated under our weak monotonicity assumptions across strata, at least for outpatient visits, preventative care, and financial strain. Our bounds on $ATE$ and $ATT$ are informative under the two sets of weak monotonicity assumptions we use. Compared with the bounds derived by imposing the exclusion restriction, we find that the exclusion restriction would largely shrink the width of the bounds.

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Table 1: Summary Statistics of Demographic Variables

| | Missing Prop. | $Z=1$ | $Z=0$ | Diff.(Std.Err.) |
|---|---|---|---|---|
| *Pre-treatment Variables* | | | | |
| Female | 0 | .579 | .591 | -.012 (.007)* |
| Older (50-64) | 0 | .317 | .316 | .001 (.007) |
| Younger (19-49) | 0 | .683 | .684 | -.001 (.007) |
| English preferred | 0 | .907 | .917 | -.010 (.005)** |
| MSA | 0 | .747 | .751 | -.005 (.007) |
| Ever on food stamps | 0 | .547 | .550 | -.003 (.008) |
| Food stamps benefits | 0 | 1136.3 | 1123.6 | 12.73 (25.31) |
| Ever on TANF | 0 | .009 | .008 | .001 (.001) |
| TANF benefits | 0 | 43.08 | 33.22 | 9.860 (5.478)* |
| *Initial Survey* | | | | |
| White | 0 | .806 | .817 | -.011 (.006)* |
| Black | .000 | .033 | .037 | -.004 (.003) |
| Hispanic | .005 | .131 | .125 | .006 (.006) |
| Education | | | | |
| Less than high school | .333 | .170 | .171 | -.000 (.004) |
| High school diploma or GED | .333 | .503 | .506 | -.003 (.006) |
| Vocational training/2-year degree | .333 | .216 | .212 | .004 (.005) |
| 4-year college degree or more | .333 | .111 | .111 | -.001 (.003) |
| Employment | | | | |
| don't currently work | .311 | .521 | .527 | -.005 (.006) |
| work <20 hours per week | .311 | .094 | .093 | .001 (.003) |
| work 20-29 hours per week | .311 | .109 | .110 | -.002 (.003) |
| work >30 hours per week | .311 | .276 | .270 | .006 (.005) |
| Average household income (2008) | 0 | 7968.1 | 7560.0 | 408.1 (162.5)** |
| Income (% federal poverty line) | | | | |
| <50% | 0 | .230 | .247 | -.018 (.006)** |
| 50-75% | 0 | .074 | .074 | .000 (.004) |
| 75-100% | 0 | .088 | .088 | .000 (.004) |
| 100-150% | 0 | .101 | .097 | .005 (.004) |
| Above 150% | 0 | .506 | .494 | .012 (.008) |
| Observations | 23741 | 11808 | 11933 | |

Table 2: *ITT* Estimates of Relevant Variables

| | Missing Prop. | Diff.(Std.Err.) | | Missing Prop. | Diff.(Std.Err.) |
|---|---|---|---|---|---|
| *Initial Survey* | | | | | |
| Insurance Coverage | | | Ever Diagnosed with | | |
| Any insurance | .366 | .082 (.005)** | Diabetes | .302 | -.008 (.004)** |
| OHP/Medicaid | .366 | .085 (.004)** | Asthma | .302 | -.010 (.004)** |
| Private insurance | .302 | -.002 (.003) | High blood pressure | .302 | -.008 (.005) |
| Other types of insurance | .302 | .002 (.003) | Emphysema or chronic bronchitis | .302 | -.002 (.003) |
| Number of months on insurance | .311 | .121 (.024)** | Depression | .302 | -.018 (.006)** |
| Health Care Utilization | | | Self-reported Health | | |
| Any prescription drugs | .354 | .001 (.006) | Not fair or poor | .323 | .022 (.006)** |
| # of prescription drugs | .355 | -.055 (.029)* | Same or gotten better | .315 | .017 (.005)** |
| Any outpatient visits | .307 | .004 (.006) | # of days Physical health good | .364 | .357 (.123)** |
| # of outpatient visits | .308 | .000 (.035) | # of days Mental Health good | .364 | .358 (.131)** |
| Less Financial Strain | | | # of days Poor physical or mental | .360 | .338 (.116)** |
| No out of pocket medical expenses | .358 | .012 (.005)** | health did not impair usual activity | | |
| Not owe for medical expenses | .308 | .015 (.006)** | | | |
| Not borrow money to pay medical bills | .312 | .015 (.006)** | | | |
| Not be refused treatment due to medical debt | .335 | .007 (.003)** | | | |
| *12-month Survey* | | | | | |
| Insurance Coverage | | | Public Assistance Programs | | |
| Any insurance | .016 | .176 (.008)** | Ever on TANF | 0 | .002 (.003) |
| Treatment: OHP/Medicaid | 0 | .290 (.007)** | TANF benefits | 0 | 4.281 (11.96) |
| Private insurance | .016 | -.008 (.005) | Ever on food stamp | 0 | .027 (.005)** |
| Other types of insurance | .016 | -.007 (.005) | Food stamp benefits | 0 | 96.99 (46.48)** |
| Number of months on insurance | .012 | 1.140 (.043)** | | | |
| Observations | 23741 | | | 23741 | |

Table 3: Estimates of Stratum Proportions

| | Main Sample | Women≥40 | Women |
|---|---|---|---|
| $\pi_{nt}$ | .576** | .587** | .559** |
| | (.006) | (.008) | (.007) |
| $\pi_c$ | .289** | .296** | .281** |
| | (.007) | (.010) | (.008) |
| $\pi_{at}$ | .135** | .117** | .160** |
| | (.004) | (.006) | (.005) |
| $\Pr(Z=1)$ | .5** | .5** | .5** |
| | (.000) | (.000) | (.000) |
| $\Pr(D=1)$ | .280** | .265** | .301** |
| | (.003) | (.005) | (.004) |
| $N$ | 23741 | 8274 | 14086 |

Table 4: Point Estimates of Average Health Care and Preventative Care Utilization

| | Health Care Utilization | | | | Preventative Care | | | |
|---|---|---|---|---|---|---|---|---|
| | Prescription Drugs | | Outpatient Visits | | Blood Cholesterol Checked | Blood Tested for High Blood Suger/Diebetes | Mammogram (Women ≥40) | Pap Test (Women) |
| | Any | Number | Any | Number | | | | |
| $E[Y\|Z=1]$ | .656** | 2.382** | .634** | 2.205** | .656** | .629** | .353** | .456** |
| | (.006) | (.037) | (.005) | (.039) | (.005) | (.005) | (.009) | (.007) |
| $E[Y\|Z=0]$ | .628** | 2.269** | .572** | 1.890** | .624** | .604** | .300** | .405** |
| | (.006) | (.036) | (.006) | (.038) | (.006) | (.005) | (.009) | (.007) |
| $E[Y\|D=1]$ | .738** | 2.937** | .749** | 2.969** | .677** | .677** | .456** | .552** |
| | (.007) | (.054) | (.006) | (.060) | (.007) | (.007) | (.012) | (.009) |
| $ITT$ | .028** | .113* | .062** | .315** | .032** | .025** | .053** | .051** |
| | (.008) | (.052) | (.008) | (.054) | (.008) | (.008) | (.012) | (.010) |
| $LATE_c$ | .097** | .390* | .215** | 1.089** | .112** | .086** | .178** | .181** |
| | (.029) | (.178) | (.026) | (.186) | (.027) | (.026) | (.041) | (.034) |
| Point Identified Average Outcomes | | | | | | | | |
| $E[Y(0)\|at]$ | .775** | 3.208** | .794** | 3.392** | .636** | .695** | .523** | .599** |
| | (.013) | (.101) | (.012) | (.130) | (.015) | (.014) | (.027) | (.018) |
| $E[Y(1)\|nt]$ | .603** | 2.027** | .560** | 1.744** | .631** | .598** | .294** | .395** |
| | (.008) | (.043) | (.007) | (.047) | (.007) | (.007) | (.011) | (.009) |
| $\overline{Y}^{11}$ | .726** | 2.849** | .734** | 2.835** | .690** | .672** | .438** | .534** |
| | (.008) | (.062) | (.007) | (.068) | (.007) | (.008) | (.014) | (.010) |
| $\overline{Y}^{00}$ | .603** | 2.111** | .537** | 1.658** | .622** | .590** | .272** | .368** |
| | (.007) | (.038) | (.006) | (.038) | (.006) | (.006) | (.009) | (.007) |
| $\overline{Y}^{11} - E[Y(1)\|nt]$ | .123** | .821** | .174** | 1.090** | .060** | .074** | .144** | .139** |
| | (.011) | (.074) | (.010) | (.084) | (.010) | (.010) | (.018) | (.014) |
| $E[Y(0)\|at] - \overline{Y}^{00}$ | .172** | 1.098** | .257** | 1.734** | .015 | .105** | .251** | .231** |
| | (.015) | (.107) | (.013) | (.134) | (.016) | (.015) | (.028) | (.019) |

Table 5: Point Estimates of Average Self-Reported Health

| | Binary Outcomes | | | | # of Days | | |
|---|---|---|---|---|---|---|---|
| | Not Fair or Poor | Not Poor | Same or Gotten Better | Not Screen Positive For depression | Physical Health Good | Mental Health Good | Poor Physical or Mental Health Did not Impair Usual Activity |
| $E[Y\|Z=1]$ | .590** (.005) | .889** (.003) | .749** (.005) | .698** (.005) | 20.88** (.122) | 19.45** (.133) | 22.36** (.120) |
| $E[Y\|Z=0]$ | .552** (.006) | .860** (.004) | .716** (.005) | .674** (.005) | 20.46** (.127) | 18.86** (.134) | 22.03** (.115) |
| $E[Y\|D=1]$ | .567** (.007) | .867** (.005) | .729** (.006) | .651** (.007) | 19.71** (.162) | 18.42** (.175) | 20.70** (.165) |
| $ITT$ | .038** (.008) | .029* (.005) | .032** (.007) | .024** (.008) | .416* (.177) | .588** (.193) | .332* (.164) |
| $LATE_c$ | .131** (.027) | .099* (.017) | .112** (.024) | .083** (.026) | 1.438* (.615) | 2.032** (.670) | 1.147* (.569) |
| Point Identified Average Outcomes | | | | | | | |
| $E[Y(0)\|at]$ | .524** (.015) | .827** (.012) | .708** (.013) | .626** (.014) | 18.73** (.331) | 17.77** (.345) | 19.56** (.334) |
| $E[Y(1)\|nt]$ | .598** (.007) | .896** (.004) | .758** (.006) | .726** (.007) | 21.51** (.159) | 20.06** (.175) | 23.31** (.149) |
| $\overline{Y}^{11}$ | .580** (.008) | .879** (.005) | .736** (.007) | .660** (.008) | 20.02** (.190) | 18.63** (.200) | 21.06** (.186) |
| $\overline{Y}^{00}$ | .557** (.006) | .866** (.004) | .718** (.005) | .681** (.006) | 20.73** (.138) | 19.03** (.145) | 22.41** (.123) |
| $\overline{Y}^{11} - E[Y(1)\|nt]$ | -.018 (.011) | -.017* (.007) | -.022* (.009) | -.067** (.010) | -1.489** (.248) | -1.431** (.264) | -2.249** (.231) |
| $E[Y(0)\|at] - \overline{Y}^{00}$ | -.033* (.017) | -.038** (.012) | -.009 (.014) | -.056** (.015) | -1.992** (.358) | -1.266** (.375) | -2.848** (.357) |

Table 6: Point Estimates of Average "Less Financial Strain"

| | No out of pocket medical expenses | Not owe for medical expenses currently | Not borrow money to pay medical bills | Not be refused treatment due to medical debt |
|---|---|---|---|---|
| $E[Y\|Z=1]$ | .505** | .460** | .683** | .930** |
| | (.005) | (.005) | (.005) | (.003) |
| $E[Y\|Z=0]$ | .449** | .408** | .637** | .920** |
| | (.006) | (.005) | (.005) | (.003) |
| $E[Y\|D=1]$ | .579** | .463** | .738** | .932** |
| | (.007) | (.007) | (.006) | (.004) |
| $ITT$ | .056** | .052** | .045** | .010* |
| | (.008) | (.007) | (.007) | (.004) |
| $LATE_c$ | .195** | .180** | .157** | .034* |
| | (.026) | (.026) | (.025) | (.014) |
| Point Identified Average Outcomes | | | | |
| $E[Y(0)\|at]$ | .502** | .401** | .701** | .925** |
| | (.015) | (.015) | (.014) | (.007) |
| $E[Y(1)\|nt]$ | .433** | .444** | .633** | .927** |
| | (.007) | (.007) | (.007) | (.004) |
| $\overline{Y}^{11}$ | .603** | .483** | .749** | .934** |
| | (.008) | (.008) | (.007) | (.004) |
| $\overline{Y}^{00}$ | .441** | .410** | .627** | .920** |
| | (.006) | (.006) | (.006) | (.003) |
| $\overline{Y}^{11} - E[Y(1)\|nt]$ | .170** | .038** | .116** | .006 |
| | (.011) | (.011) | (.010) | (.006) |
| $E[Y(0)\|at] - \overline{Y}^{00}$ | .061** | -.009 | .074** | .006 |
| | (.016) | (.016) | (.015) | (.008) |

Table 7: Average Baseline Characteristics of the Main Sample

| Variable | $nt$ | $c$ | $at$ | $nt-c$ | $c-at$ | $nt-at$ |
|---|---|---|---|---|---|---|
| *Pre-treatment Variables* | | | | | | |
| Female | .563** | .569** | .694** | -.006 | -.125** | -.131** |
| | (.006) | (.011) | (.013) | (.015) | (.020) | (.013) |
| Older (50-64) | .328** | .324** | .251** | .005 | .073** | .078** |
| | (.006) | (.011) | (.013) | (.014) | (.020) | (.013) |
| Younger (19-49) | .672** | .676** | .749** | -.005 | -.073** | -.078** |
| | (.006) | (.011) | (.013) | (.014) | (.020) | (.013) |
| English | .897** | .947** | .906** | -.050** | .041** | -.010 |
| | (.004) | (.007) | (.009) | (.009) | (.014) | (.009) |
| MSA | .748** | .738** | .767** | .010 | -.029 | -.019 |
| | (.006) | (.010) | (.013) | (.013) | (.019) | (.013) |
| Ever enrolled in food stamp | .431** | .695** | .746** | -.264** | -.051* | -.315** |
| | (.006) | (.011) | (.013) | (.015) | (.020) | (.014) |
| Total benefits from food stamp | 782.4** | 1457.3** | 1944.3** | -674.9** | -487.0** | -1161.9** |
| | (18.94) | (42.92) | (64.83) | (52.94) | (94.05) | (63.52) |
| Ever enrolled in TANF | .004** | .006** | .030** | -.003 | -.024** | -.026** |
| | (.001) | (.002) | (.004) | (.003) | (.006) | (.004) |
| Total benefits from TANF | 23.59** | 23.68** | 113.8** | -.091 | -90.12** | -90.21** |
| | (3.451) | (8.729) | (15.25) | (10.72) | (20.78) | (14.80) |
| *Initial Survey* | | | | | | |
| White, Non-hispanic | .795** | .850** | .804** | -.056* | .047* | -.009 |
| | (.005) | (.009) | (.012) | (.012) | (.018) | (.012) |
| Black, Non-Hispanic | .034** | .031** | .049** | .002 | -.018* | -.015** |
| | (.002) | (.004) | (.006) | (.006) | (.009) | (.006) |
| Hispanic | .146** | .078** | .151** | .068** | -.073** | -.005 |
| | (.005) | (.008) | (.011) | (.011) | (.016) | (.011) |
| Education | | | | | | |
| Less than high school | .165** | .172** | .187** | -.007 | -.016 | -.023** |
| | (.003) | (.007) | (.008) | (.009) | (.013) | (.008) |
| High school diploma or GED | .494** | .524** | .511* | -.030** | .013 | -.017 |
| | (.005) | (.009) | (.010) | (.012) | (.016) | (.011) |
| Vocational training/2-year degree | .217** | .206** | .218** | .011 | -.012 | -.001 |
| | (.004) | (.007) | (.008) | (.009) | (.013) | (.009) |
| 4-year college or more | .124** | .098** | .084** | .025** | .015 | .040** |
| | (.003) | (.005) | (.005) | (.006) | (.008) | (.005) |
| Employment | | | | | | |
| Don't currently work | .461** | .599** | .637** | -.137** | -.039* | -.176** |
| | (.004) | (.009) | (.010) | (.011) | (.016) | (.011) |
| Work <20 hours per week | .090** | .104** | .085** | -.014* | .020* | .006 |
| | (.002) | (.005) | (.006) | (.007) | (.009) | (.006) |
| Work 20-29 hours per week | .114** | .112** | .083** | .002 | .029** | .030** |
| | (.003) | (.005) | (.006) | (.007) | (.010) | (.006) |
| Work 30+ hours per week | .335** | .185** | .195** | .150** | -.010 | .140** |
| | (.004) | (.007) | (.008) | (.009) | (.014) | (.009) |
| Average household income | 9158.2** | 5701.4** | 6106.3** | 3456.8** | -404.9 | 3051.9** |
| | (136.7) | (214.8) | (285.2) | (288.9) | (435.4) | (290.3) |
| Income (% federal poverty line) | | | | | | |
| <50% | .166** | .359** | .299** | -.193** | .060** | -.133** |
| | (.005) | (.010) | (.012) | (.012) | (.019) | (.012) |
| 50%−75% | .067** | .081** | .077** | -.013 | .004 | -.010 |
| | (.003) | (.006) | (.008) | (.008) | (.012) | (.007) |
| 75%−100% | .097** | .082** | .070** | .015 | .012 | .027** |
| | (.004) | (.006) | (.007) | (.008) | (.011) | (.008) |
| 100%−150% | .122** | .064** | .068** | .058** | -.005 | .054** |
| | (.003) | (.006) | (.007) | (.008) | (.012) | (.007) |
| Above 150% | .547** | .417** | .485** | .129** | -.068** | .062** |
| | (.006) | (.011) | (.015) | (.015) | (.022) | (.015) |

Table 7 (Con't): Average Baseline Characteristics of the Main Sample

| Variable | $nt$ | $c$ | $at$ | $nt-c$ | $c-at$ | $nt-at$ |
|---|---|---|---|---|---|---|
| *Initial Survey* | | | | | | |
| Insurance Coverage | | | | | | |
| Any | .259** | .321** | .478** | -.062** | -.157** | -.219** |
| | (.004) | (.008) | (.012) | (.010) | (.017) | (.012) |
| OHP/Medicaid | .071** | .115** | .392** | -.044** | -.277** | -.320** |
| | (.002) | (.007) | (.015) | (.007) | (.019) | (.015) |
| Private insurance | .115** | .033** | .060** | .082** | -.027** | .055** |
| | (.003) | (.004) | (.005) | (.005) | (.008) | (.005) |
| Other types of insurance | .087** | .060** | .098** | .027** | -.038** | -.010* |
| | (.002) | (.005) | (.006) | (.006) | (.010) | (.006) |
| # of months with insurance | .993** | .750** | 1.800** | .244 | -1.050** | -.806** |
| | (.018) | (.037) | (.061) | (.046) | (.086) | (.060) |
| Ever Diagnosed with | | | | | | |
| Diabetes | .112** | .107** | .128** | .005 | -.021* | -.016* |
| | (.003) | (.006) | (.007) | (.007) | (.011) | (.007) |
| Asthma | .149** | .157** | .190** | -.007 | -.033* | -.040** |
| | (.003) | (.007) | (.009) | (.009) | (.013) | (.009) |
| High blood pressure | .278** | .285** | .290** | -.006 | -.005 | -.011 |
| | (.004) | (.008) | (.009) | (.010) | (.014) | (.009) |
| Emphysema or chronic bronchitis | .068** | .075** | .089** | -.007 | -.014 | -.021** |
| | (.002) | (.005) | (.006) | (.006) | (.010) | (.006) |
| Depression (screen positive) | .398** | .439** | .475** | -.041** | -.036* | -.076** |
| | (.005) | (.009) | (.011) | (.012) | (.017) | (.011) |
| Health Care Utilization | | | | | | |
| Any prescription drugs | .481** | .483** | .573** | -.001 | -.090** | -.091** |
| | (.004) | (.009) | (.011) | (.012) | (.017) | (.011) |
| # of prescription drugs | 1.620** | 1.604** | 2.109** | .017 | -.505** | -.489** |
| | (.022) | (.048) | (.060) | (.061) | (.091) | (.061) |
| Any outpatient visits | .569** | .554** | .676** | .015 | -.122** | -.107** |
| | (.004) | (.009) | (.010) | (.011) | (.016) | (.011) |
| # of outpatient visits | 1.742** | 1.734** | 2.563** | .007 | -.829** | -.822** |
| | (.025) | (.059) | (.081) | (.073) | (.121) | (.079) |
| Self-reported Health | | | | | | |
| Not fair or poor | .625** | .583** | .561** | .041** | .023 | .064** |
| | (.004) | (.009) | (.011) | (.012) | (.017) | (.011) |
| Same or gotten better | .754** | .712** | .707** | .042** | .005 | .048** |
| | (.004) | (.009) | (.009) | (.011) | (.015) | (.009) |
| # of days Physical health good | 21.28** | 20.16** | 18.85** | 1.124** | 1.310** | 2.434** |
| | (.094) | (.209) | (.245) | (.262) | (.388) | (.247) |
| # of days Mental Health good | 19.83** | 18.42** | 17.59** | 1.417** | .827** | 2.244** |
| | (.104) | (.221) | (.239) | (.279) | (.385) | (.246) |
| # of days Poor physical or mental | 22.83** | 21.10** | 20.02** | 1.732** | 1.080** | 2.812** |
| | (.087) | (.202) | (.233) | (.248) | (.369) | (.230) |
| health did not impair usual activity | | | | | | |
| Less Financial Strain | | | | | | |
| No out of pocket medical expenses | .302** | .353** | .299** | -.051** | .054** | .004 |
| | (.004) | (.009) | (.009) | (.011) | (.015) | (.009) |
| Not owe for medical expenses | .411** | .381** | .356** | .030** | .024 | .055** |
| | (.004) | (.009) | (.010) | (.012) | (.016) | (.011) |
| Not borrow to pay for medical bills | .560** | .569** | .533** | -.009 | .036** | .027** |
| | (.005) | (.009) | (.011) | (.012) | (.017) | (.011) |
| Not be refused treatment due to debt | .925** | .905** | .909** | .019** | -.003 | .016** |
| | (.002) | (.005) | (.006) | (.007) | (.009) | (.006) |

Table 8: Bounds of Average Effects on Health Care Utilization

| | Prescription Drugs | | | | Outpatient Visits | | | |
|---|---|---|---|---|---|---|---|---|
| | Any | | Number | | Any | | Number | |
| | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ |
| *Bounds on* $ATE$ | | | | | | | | |
| Proposition 1 | -.508 | .492 | -7.386 | 16.61 | -.464 | .536 | -8.778 | 21.22 |
| Bounded Outcome (A4) | (-.515, .500) | | (-7.753, 17.54) | | (-.470, .542) | | (-8.926, 21.37) | |
| Proposition 2 | 0 | .421 | 0 | 13.65 | 0 | .480 | 0 | 17.49 |
| Mono. within Strata (A5) | (0, .432) | | (0, 14.42) | | (0, .490) | | (0, 17.74) | |
| Proposition 3 | -.449 | .148 | -4.776 | .959 | -.420 | .216 | -5.314 | 1.412 |
| Mono. across Strata (A6) | (-.455, .163) | | (-4.974, 1.068) | | (-.425, .230) | | (-5.455, 1.540) | |
| Proposition 4 | 0 | .132 | 0 | .822 | 0 | .194 | 0 | 1.212 |
| A4 & A5 &A6 | (0, .146) | | (0, .927) | | (0, .207) | | (0, 1.324) | |
| *Bounds on* $ATT$ | | | | | | | | |
| Proposition 1 | -.262 | .738 | -21.06 | 2.937 | -.251 | .749 | -27.03 | 2.968 |
| Bounded Outcome (A4) | (-.273, .749) | | (-22.33, 3.026) | | (-.262, .759) | | (-27.13, 3.067) | |
| Proposition 2 | 0 | .425 | 0 | 1.759 | 0 | .494 | 0 | 2.199 |
| Mono. within Strata (A5) | (0, .453) | | (0, 1.924) | | (0, .519) | | (0, 2.365) | |
| Proposition 3 | -.262 | .135 | -12.02 | .890 | -.251 | .194 | -14.83 | 1.245 |
| Mono. across Strata (A6) | (-.273, .150) | | (-12.69, .993) | | (-.262, .208) | | (-15.20, 1.362) | |
| Proposition 4 | 0 | .140 | 0 | .789 | 0 | .198 | 0 | 1.277 |
| A4 & A5 &A6 | (0, .153) | | (0, .960) | | (0, .211) | | (0, 1.384) | |
| *Bounds on* $LNATE_{nt}$ | | | | | | | | |
| Proposition 2 | 0 | .199 | 0 | 1.452 | 0 | .256 | 0 | 1.377 |
| Mono. within Strata (A5) | (0, .224) | | (0, 1.537) | | (0, .279) | | (0, 1.462) | |
| Proposition 3 | -.000 | .199 | -.083 | 1.452 | .023 | .256 | .086 | 1.377 |
| Mono. across Strata (A6) | (-.018, .224) | | (-.180, 1.537) | | (.007, .279) | | (-.013, 1.463) | |
| Proposition 4 | -.000 | .199 | -.000 | 1.452 | .023 | .256 | .086 | 1.377 |
| A4 & A5 &A6 | (-.000, .224) | | (-.000, 1.537) | | (.007, .279) | | (-.000, 1.462) | |
| *Bounds on* $LNATE_{at}$ | | | | | | | | |
| Proposition 2 | 0 | .225 | 0 | 3.329 | 0 | .205 | 0 | 3.043 |
| Mono. within Strata (A5) | (0, .248) | | (0, 3.661) | | (0, .227) | | (0, 3.480) | |
| Proposition 3 | -.049 | .225 | -.359 | 3.329 | -.060 | .205 | -.557 | 3.043 |
| Mono. across Strata (A6) | (-.076, .248) | | (-.557, 3.659) | | (-.085, .227) | | (-.815, 3.474) | |
| Proposition 4 | -.000 | .225 | -.000 | 3.329 | -.000 | .205 | -.000 | 3.043 |
| A4 & A5 &A6 | (-.000, .248) | | (-.000, 3.661) | | (-.000, .227) | | (-.000, 3.480) | |
| *Bounds on* $LATE_c$ | | | | | | | | |
| Proposition 2 | 0 | 1 | 0 | 4.111 | 0 | 1 | 0 | 4.079 |
| Mono. within Strata (A5) | (0, 1) | | (0, 4.272) | | (0, 1) | | (0, 4.258) | |
| Proposition 3 | -.387 | .148 | -3.585 | .959 | -.443 | .216 | -3.908 | 1.412 |
| Mono. across Strata (A6) | (-.403, .163) | | (-3.763, 1.068) | | (-.457, .230) | | (-4.103, 1.540) | |
| Proposition 4 | 0 | .128 | 0 | .774 | 0 | .190 | 0 | 1.173 |
| A4 & A5 &A6 | (0, .143) | | (0, .894) | | (0, .204) | | (0, 1.291) | |

Table 9: Bounds of Average Effects on Preventative Care

| | Blood Cholesterol Checked | | Blood Tested for High Blood Sugar/Diabetes | | Mammogram (Women ≥40) | | Pap Test (Women) | |
|---|---|---|---|---|---|---|---|---|
| | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ |
| *Bounds on $ATE$* | | | | | | | | |
| Proposition 1 | -.540 | .459 | -.517 | .483 | -.350 | .650 | -.400 | .600 |
| Bounded Outcome (A4) | (-.547, .466) | | (-.524, .489) | | (-.360, .660) | | (-.408, .608) | |
| Proposition 2 | 0 | .398 | 0 | .420 | 0 | .592 | 0 | .570 |
| Mono. within Strata (A5) | (0, .409) | | (0, .431) | | (0, .610) | | (0, .584) | |
| Proposition 3 | -.478 | .037 | -.462 | .089 | -.350 | .198 | -.393 | .185 |
| Mono. across Strata (A6) | (-.484, .053) | | (-.467, .105) | | (-.360, .225) | | (-.400, .205) | |
| Proposition 4 | 0 | .042 | 0 | .085 | 0 | .169 | 0 | .164 |
| A4 & A5 &A6 | (0, .059) | | (0, .098) | | (0, .193) | | (0, .182) | |
| *Bounds on $ATT$* | | | | | | | | |
| Proposition 1 | -.323 | .677 | -.322 | .677 | -.544 | .456 | -.448 | .552 |
| Bounded Outcome (A4) | (-.334, .689) | | (-.334, .689) | | (-.564, .477) | | (-.464, .567) | |
| Proposition 2 | 0 | .365 | 0 | .380 | 0 | .330 | 0 | .404 |
| Mono. within Strata (A5) | (0, .391) | | (0, .405) | | (0, .370) | | (0, .431) | |
| Proposition 3 | -.323 | .049 | -.322 | .082 | -.544 | .167 | -.448 | .164 |
| Mono. across Strata (A6) | (-.334, .063) | | (-.334, .096) | | (-.564, .193) | | (-.464, .183) | |
| Proposition 4 | 0 | .053 | 0 | .086 | 0 | .175 | 0 | .169 |
| A4 & A5 &A6 | (0, .067) | | (0, .099) | | (0, .198) | | (0, .187) | |
| *Bounds on $LNATE_{nt}$* | | | | | | | | |
| Proposition 2 | 0 | .199 | 0 | .214 | 0 | .294 | 0 | .344 |
| Mono. within Strata (A5) | (0, .221) | | (0, .236) | | (0, .312) | | (0, .377) | |
| Proposition 3 | .009 | .199 | .008 | .214 | .022 | .294 | .027 | .344 |
| Mono. across Strata (A6) | (-.007, .221) | | (-.008, .236) | | (-.001, .313) | | (.007, .377) | |
| Proposition 4 | .009 | .199 | .008 | .214 | .022 | .294 | .027 | .344 |
| A4 & A5 &A6 | (-.000, .221) | | (-.000, .236) | | (-.000, .312) | | (.007, .377) | |
| *Bounds on $LNATE_{at}$* | | | | | | | | |
| Proposition 2 | 0 | .364 | 0 | .305 | 0 | .477 | 0 | .401 |
| Mono. within Strata (A5) | (0, .389) | | (0, .329) | | (0, .523) | | (0, .431) | |
| Proposition 3 | .054 | .364 | -.023 | .305 | -.085 | .477 | -.065 | .401 |
| Mono. across Strata (A6) | (.026, .389) | | (-.049, .329) | | (-.136, .522) | | (-.098, .431) | |
| Proposition 4 | .054 | .364 | -.000 | .305 | -.000 | .477 | -.000 | .401 |
| A4 & A5 &A6 | (.027, .389) | | (-.000, .329) | | (-.000, .523) | | (-.000, .431) | |
| *Bounds on $LATE_c$* | | | | | | | | |
| Proposition 2 | 0 | 1.000 | 0 | .985 | 0 | .611 | 0 | .838 |
| Mono. within Strata (A5) | (0, 1.010) | | (0, 1.009) | | (0, .650) | | (0, .878) | |
| Proposition 3 | -.290 | .037 | -.359 | .089 | -.624 | .198 | -.581 | .185 |
| Mono. across Strata (A6) | (-.306, .053) | | (-.374, .105) | | (-.645, .225) | | (-.602, .205) | |
| Proposition 4 | 0 | .042 | 0 | .083 | 0 | .163 | 0 | .159 |
| A4 & A5 &A6 | (0, .058) | | (0, .097) | | (0, .188) | | (0, .178) | |

Table 10: Bounds of Average Effects on Self-Reported Health (Binary)

| | Not Fair or Poor | | Not Poor | | Same or Gotten Better | | Not Screen Positive for Depression | |
|---|---|---|---|---|---|---|---|---|
| | *LB* | *UB* | *LB* | *UB* | *LB* | *UB* | *LB* | *UB* |
| *Bounds on $ATE$* | | | | | | | | |
| Proposition 1 | -.534 | .466 | -.670 | .330 | -.604 | .396 | -.601 | .399 |
| Bounded Outcome (A4) | (-.541, .472) | | (-.676, .336) | | (-.610, .402) | | (-.607, .405) | |
| Proposition 2 | 0 | .410 | 0 | .227 | 0 | .325 | 0 | .336 |
| Mono. within Strata (A5) | (0, .422) | | (0, .235) | | (0, .334) | | (0, .348) | |
| Proposition 3' | -.026 | .442 | -.028 | .241 | -.016 | .371 | -.061 | .399 |
| Mono. across Strata (A6') | (-.042, .448) | | (-.039, .245) | | (-.029, .379) | | (-.076, .405) | |
| Proposition 4' | 0 | .334 | 0 | .197 | 0 | .260 | 0 | .270 |
| A4 & A5 &A6' | (0, .341) | | (0, .204) | | (0, .267) | | (0, .277) | |
| *Bounds on $ATT$* | | | | | | | | |
| Proposition 1 | -.433 | .567 | -.133 | .867 | -.271 | .729 | -.349 | .651 |
| Bounded Outcome (A4) | (-.446, .579) | | (-.141, .875) | | (-.281, .740) | | (-.360, .663) | |
| Proposition 2 | 0 | .321 | 0 | .450 | 0 | .399 | 0 | .345 |
| Mono. within Strata (A5) | (0, .346) | | (0, .471) | | (0, .424) | | (0, .371) | |
| Proposition 3' | -.022 | .567 | -.022 | .546 | -.019 | .642 | -.064 | .651 |
| Mono. across Strata (A6') | (-.037, .579) | | (-.032, .555) | | (-.032, .665) | | (-.078, .663) | |
| Proposition 4' | 0 | .300 | 0 | .442 | 0 | .366 | 0 | .331 |
| A4 & A5 &A6' | (0, .313) | | (0, .459) | | (0, .382) | | (0, .346) | |
| *Bounds on $LNATE_{nt}$* | | | | | | | | |
| Proposition 2 | 0 | .264 | 0 | .098 | 0 | .182 | 0 | .205 |
| Mono. within Strata (A5) | (0, .288) | | (0, .112) | | (0, .202) | | (0, .227) | |
| Proposition 3' | -.239 | .041 | -.104 | .031 | -.242 | .040 | -.274 | .045 |
| Mono. across Strata (A6') | (-.263, .057) | | (-.111, .041) | | (-.252, .054) | | (-.285, .060) | |
| Proposition 4' | 0 | .041 | 0 | .031 | 0 | .040 | 0 | .045 |
| A4 & A5 &A6' | (0, .059) | | (0, .042) | | (0, .056) | | (0, .062) | |
| *Bounds on $LNATE_{at}$* | | | | | | | | |
| Proposition 2 | 0 | .476 | 0 | .173 | 0 | .292 | 0 | .374 |
| Mono. within Strata (A5) | (0, .502) | | (0, .193) | | (0, .313) | | (0, .398) | |
| Proposition 3' | -.523 | .057 | -.207 | .052 | -.540 | .028 | -.626 | .034 |
| Mono. across Strata (A6') | (-.549, .085) | | (-.250, .075) | | (-.604, .053) | | (-.650, .060) | |
| Proposition 4' | 0 | .057 | 0 | .052 | 0 | .028 | 0 | .034 |
| A4 & A5 &A6' | (0, .089) | | (0, .077) | | (0, .056) | | (0, .065) | |
| *Bounds on $LATE_c$* | | | | | | | | |
| Proposition 2 | 0 | .850 | 0 | .402 | 0 | .844 | 0 | .919 |
| Mono. within Strata (A5) | (0, .878) | | (0, .426) | | (0, .884) | | (0, .981) | |
| Proposition 3' | -.026 | .456 | -.028 | .225 | -.016 | .442 | -.061 | .550 |
| Mono. across Strata (A6') | (-.042, .473) | | (-.040, .245) | | (-.029, .473) | | (-.076, .563) | |
| Proposition 4' | 0 | .080 | 0 | .075 | 0 | .050 | 0 | .104 |
| A4 & A5 &A6' | (0, .110) | | (0, .096) | | (0, .076) | | (0, .131) | |

Table 11: Bounds of Average Effects on Self-Reported Health (# of days)

| | Physical Health Good | | Mental Health Good | | Poor Physical or Mental Health Did not Impair Usual Activity | |
|---|---|---|---|---|---|---|
| | *LB* | *UB* | *LB* | *UB* | *LB* | *UB* |
| *Bounds on ATE* | | | | | | |
| Proposition 1 | -18.03 | 11.97 | -17.24 | 12.76 | -19.00 | 11.00 |
| Bounded Outcome (A4) | (-18.19, 12.12) | | (-17.41, 12.92) | | (-19.15, 11.15) | |
| Proposition 2 | 0 | 9.546 | 0 | 10.53 | 0 | 8.445 |
| Mono. within Strata (A5) | (0, 9.826) | | (0, 10.83) | | (0, 8.709) | |
| Proposition 3' | -1.740 | 10.84 | -1.348 | 11.70 | -2.547 | 9.832 |
| Mono. across Strata (A6') | (-2.088, 10.97) | | (-1.730, 11.84) | | (-2.900, 9.973) | |
| Proposition 4' | 0 | 8.079 | 0 | 8.803 | 0 | 7.216 |
| A4 & A5 &A6' | (0, 8.263) | | (0, 8.994) | | (0, 7.420) | |
| *Bounds on ATT* | | | | | | |
| Proposition 1 | -10.29 | 19.71 | -11.58 | 18.42 | -9.295 | 20.70 |
| Bounded Outcome (A4) | (-10.56, 19.98) | | (-11.87, 18.71) | | (-9.569, 20.98) | |
| Proposition 2 | 0 | 9.782 | 0 | 9.623 | 0 | 10.03 |
| Mono. within Strata (A5) | (0, 10.43) | | (0, 10.31) | | (0, 10.68) | |
| Proposition 3' | -1.608 | 16.95 | -1.390 | 16.55 | -2.388 | 17.22 |
| Mono. across Strata (A6') | (-1.941, 17.22) | | (-1.756, 16.83) | | (-2.711, 17.50) | |
| Proposition 4' | 0 | 9.927 | 0 | 9.326 | 0 | 10.17 |
| A4 & A5 &A6' | (0, 10.52) | | (0, .9.739) | | (0, 10.77) | |
| *Bounds on $LNATE_{nt}$* | | | | | | |
| Proposition 2 | 0 | 5.413 | 0 | 6.467 | 0 | 4.718 |
| Mono. within Strata (A5) | (0, 5.922) | | (0, 7.016) | | (0, 5.186) | |
| Proposition 3' | -6.009 | .778 | -6.246 | 1.023 | -5.377 | .900 |
| Mono. across Strata (A6') | (-6.333, 1.130) | | (-6.647, 1.407) | | (-5.677, 1.222) | |
| Proposition 4' | 0 | .778 | 0 | 1.023 | 0 | .900 |
| A4 & A5 &A6' | (0, 1.178) | | (0, 1.455) | | (0, 1.262) | |
| *Bounds on $LNATE_{at}$* | | | | | | |
| Proposition 2 | 0 | 11.19 | 0 | 12.09 | 0 | 10.44 |
| Mono. within Strata (A5) | (0, 11.75) | | (0, 12.68) | | (0, 10.99) | |
| Proposition 3' | -13.41 | 1.281 | -14.15 | .857 | -12.82 | 1.498 |
| Mono. across Strata (A6') | (-14.39, 1.931) | | (-15.11, 1.519) | | (-13.87, 2.130) | |
| Proposition 4' | 0 | 1.281 | 0 | .857 | 0 | 1.498 |
| A4 & A5 &A6' | (0, 2.019) | | (0, 1.619) | | (0, 2.206) | |
| *Bounds on $LATE_c$* | | | | | | |
| Proposition 2 | 0 | 19.64 | 0 | 21.04 | 0 | 17.81 |
| Mono. within Strata (A5) | (0, 20.48) | | (0, 21.81) | | (0, 18.54) | |
| Proposition 3' | -1.740 | 12.48 | -1.348 | 12.49 | -2.547 | 12.89 |
| Mono. across Strata (A6') | (-2.088, 13.01) | | (-1.731, 13.06) | | (-2.900, 13.47) | |
| Proposition 4' | 0 | 2.848 | 0 | 2.383 | 0 | 3.817 |
| A4 & A5 &A6' | (0, 3.476) | | (0, 3.069) | | (0, 4.443) | |

Table 12: Bounds of Average Effects on Less Financial Strain

| | No out of pocket medical expenses | | Not owe for medical expenses currently | | Not borrow money to pay medical bills | | Not be refused treatment due to medical debt | |
|---|---|---|---|---|---|---|---|---|
| | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ | $LB$ | $UB$ |
| *Bounds on $ATE$* | | | | | | | | |
| Proposition 1 | -.433 | .567 | -.455 | .545 | -.527 | .473 | -.684 | .316 |
| Bounded Outcome (A4) | (-.440, .573) | | (-.462, .551) | | (-.534, .480) | | (-.690, .322) | |
| Proposition 2 | 0 | .521 | 0 | .497 | 0 | .406 | 0 | .192 |
| Mono. within Strata (A5) | (0, .532) | | (0, .508) | | (0, .415) | | (0, .199) | |
| Proposition 3 | -.410 | .115 | -.439 | .015 | -.463 | .095 | -.556 | .006 |
| Mono. across Strata (A6) | (-.416, .132) | | (-.444, .031) | | (-.469, .110) | | (-.565, .014) | |
| Proposition 4 | 0 | .119 | 0 | .019 | 0 | .100 | 0 | .009 |
| A4 & A5 &A6 | (0, .135) | | (0, .037) | | (0, .114) | | (0, .017) | |
| *Bounds on $ATT$* | | | | | | | | |
| Proposition 1 | -.421 | .579 | -.537 | .463 | -.262 | .738 | -.068 | .932 |
| Bounded Outcome (A4) | (-.433, .591) | | (-.549, .475) | | (-.273, .748) | | (-.074, .938) | |
| Proposition 2 | 0 | .343 | 0 | .286 | 0 | .419 | 0 | .466 |
| Mono. within Strata (A5) | (0, .367) | | (0, .310) | | (0, .443) | | (0, .486) | |
| Proposition 3 | -.421 | .144 | -.537 | .027 | -.262 | .106 | -.068 | .006 |
| Mono. across Strata (A6) | (-.433, .159) | | (-.549, .042) | | (-.273, .120) | | (-.074, .014) | |
| Proposition 4 | 0 | .138 | 0 | .032 | 0 | .110 | 0 | .009 |
| A4 & A5 &A6 | (0, .163) | | (0, .047) | | (0, .122) | | (0, .016) | |
| *Bounds on $LNATE_{nt}$* | | | | | | | | |
| Proposition 2 | 0 | .274 | 0 | .331 | 0 | .194 | 0 | .048 |
| Mono. within Strata (A5) | (0, .299) | | (0, .357) | | (0, .216) | | (0, .060) | |
| Proposition 3 | -.007 | .274 | .035 | .331 | .006 | .194 | .008 | .048 |
| Mono. across Strata (A6) | (-.023, .299) | | (.019, .357) | | (-.010, .216) | | (-.001, .060) | |
| Proposition 4 | -.000 | .274 | .034 | .331 | .006 | .194 | .008 | .048 |
| A4 & A5 &A6 | (-.000, .299) | | (.019, .357) | | (-.000, .216) | | (-.000, .060) | |
| *Bounds on $LNATE_{at}$* | | | | | | | | |
| Proposition 2 | 0 | .498 | 0 | .599 | 0 | .299 | 0 | .075 |
| Mono. within Strata (A5) | (0, .523) | | (0, .624) | | (0, .322) | | (0, .088) | |
| Proposition 3 | .101 | .498 | .082 | .599 | .049 | .299 | .008 | .075 |
| Mono. across Strata (A6) | (.073, .523) | | (.053, 624) | | (.022, .322) | | (-.007, .088) | |
| Proposition 4 | .101 | .498 | .081 | .599 | .048 | .299 | .008 | .075 |
| A4 & A5 &A6 | (.073, .523) | | (.053, .624) | | (.023, .322) | | (-.000, .088) | |
| *Bounds on $LATE_c$* | | | | | | | | |
| Proposition 2 | 0 | .884 | 0 | .707 | 0 | 1 | 0 | .241 |
| Mono. within Strata (A5) | (0, .912) | | (0, .733) | | (0, 1) | | (0, .259) | |
| Proposition 3 | -.458 | .115 | -.422 | .015 | -.317 | .095 | -.061 | .006 |
| Mono. across Strata (A6) | (-.475, .132) | | (-.440, .031) | | (-.331, .110) | | (-.069, .014) | |
| Proposition 4 | 0 | .117 | 0 | .020 | 0 | .100 | 0 | .009 |
| A4 & A5 &A6 | (0, .134) | | (0, .037) | | (0, .114) | | (0, .017) | |

# 5 Appendix

## 5.1 Appendix Tables

Table A1: Bounds of Average Effects on Health Care Utilization with a Valid IV

| | Prescription Drugs | | Outpatient Visits | |
|---|---|---|---|---|
| | Any | Number | Any | Number |
| *Bounds on ATE* | | | | |
| Proposition 1 | -.348 .362 | -3.854 13.20 | -.288 .423 | -4.278 17.04 |
| Bounded Outcome (A5) | (-.362, .374) | (-4.087, 13.97) | (-.300, .433) | (-4.474, 17.30) |
| Proposition 2 | .028 .362 | .113 13.20 | .062 .423 | .316 17.04 |
| Monotonicity (A6) | (.014, .374) | (.027, 13.97) | (.050, .433) | (.226, 17.30) |
| Proposition 3c' | -.348 .110 | -3.854 .619 | -.288 .187 | -4.278 1.049 |
| Mean Dominance (A7c') | (-.362, .133) | (-4.088, .780) | (-.300, .208) | (-4.474, 1.227) |
| Proposition 6' | .028 .110 | .113 .619 | .062 .187 | .316 1.049 |
| A5 & A6 &A7c' | (.012, .136) | (.017, .799) | (.049, .210) | (.217, 1.245) |
| *Bounds on ATT* | | | | |
| Proposition 1 | -.058 .424 | -9.826 1.749 | .012 .495 | -12.27 2.200 |
| Bounded Outcome (A5) | (-.085, .452) | (-10.59, 1.911) | (-.011, .520) | (-12.82, 2.364) |
| Proposition 2 | .050 .424 | .202 1.749 | .112 .495 | .564 2.200 |
| Monotonicity (A6) | (.025, .452) | (.044, 1.915) | (.089, .520) | (.398, 2.370) |
| Proposition 3c' | -.058 .133 | -9.826 .651 | .012 .258 | -12.27 1.482 |
| Mean Dominance (A7c') | (-.087, .182) | (-10.59, .910) | (-.013, .300) | (-12.82, 1.741) |
| Proposition 6' | .050 .133 | .202 .651 | .112 .258 | .564 1.482 |
| A5 & A6 &A7c' | (.021, .184) | (.027, .945) | (.086, .301) | (.385, 1.770) |
| *Bounds on* $LATE_{nt}$ | | | | |
| Proposition 2 | 0 .397 | 0 21.97 | 0 .440 | 0 28.26 |
| Monotonicity (A6) | (0, .410) | (0, 23.25) | (0, .452) | (0, 28.33) |
| Proposition 3' | -.602 .101 | -2.027 .653 | -.560 .146 | -1.744 .830 |
| Mean Dominance (A7c') | (-.615, .126) | (-2.099, .837) | (-.572, .170) | (-1.824, 1.052) |
| Proposition 6' | 0 .101 | 0 .653 | 0 .146 | 0 .830 |
| A5& A6 &A7c' | (0, .129) | (0, .855) | (0, .171) | (0, 1.070) |
| *Bounds on* $LATE_{at}$ | | | | |
| Proposition 2 | 0 .775 | 0 3.208 | 0 .794 | 0 3.392 |
| Monotonicity (A6) | (0, .797) | (0, 3.376) | (0, .815) | (0, 3.608) |
| Proposition 3' | -.225 .171 | -20.79 .931 | -.205 .303 | -.26.61 1.904 |
| Mean Dominance (A7c') | (-.249, .222) | (-22.06, 1.225) | (-.227, .346) | (-26.82, 2.225) |
| Proposition 6' | 0 .171 | 0 .931 | 0 .303 | 0 1.904 |
| A5 & A6 &A7c' | (0, .225) | (0, 1.259) | (0, .348) | (0, 2.250) |

Table A2: Bounds of Average Effects on Preventative Care with a Valid IV

| | Blood Cholesterol Checked | | Blood Tested for High Blood Sugar/Diabetes | | Mammogram (Women ≥40) | | Pap Test (Women) | |
|---|---|---|---|---|---|---|---|---|
| *Bounds on ATE* | | | | | | | | |
| Proposition 1 | -.380 | .331 | -.360 | .350 | -.176 | .528 | -.234 | .485 |
| Bounded Outcome (A5) | (-.392, .342) | | (-.372, .361) | | (-.194, .546) | | (-.249, .499) | |
| Proposition 2 | .032 | .331 | .025 | .350 | .053 | .528 | .051 | .485 |
| Monotonicity (A6) | (.020, .342) | | (.013, .361) | | (.033, .546) | | (.035, .499) | |
| Proposition 3c' | -.380 | .085 | -.360 | .078 | -.176 | .151 | -.234 | .154 |
| Mean Dominance (A7c') | (-.392, .107) | | (-.372, .099) | | (-.195, .188) | | (-.249, .184) | |
| Proposition 6' | .032 | .085 | .025 | .078 | .053 | .151 | .051 | .154 |
| A5 & A6 &A7c' | (.018, .110) | | (.011, .102) | | (.030, .191) | | (.033, .186) | |
| *Bounds on ATT* | | | | | | | | |
| Proposition 1 | -.118 | .365 | -.102 | .380 | -.111 | .331 | -.129 | .404 |
| Bounded Outcome (A5) | (-.142, .391) | | (-.126, .405) | | (-.154, .371) | | (-.159, .431) | |
| Proposition 2 | .058 | .365 | .045 | .380 | .100 | .331 | .085 | .404 |
| Monotonicity (A6) | (.035, .391) | | (.022, .405) | | (.059, .372) | | (.058, .431) | |
| Proposition 3c' | -.118 | .073 | -.102 | .102 | -.111 | .230 | -.129 | .236 |
| Mean Dominance (A7c') | (-.144, .118) | | (-.128, .144) | | (-.156, .295) | | (-.160, .286) | |
| Proposition 6' | .058 | .073 | .045 | .102 | .100 | .230 | .085 | .236 |
| A5 & A6 &A7c' | (.031, .121) | | (.019, .147) | | (.056, .299) | | (.055, .289) | |
| *Bounds on* $LATE_{nt}$ | | | | | | | | |
| Proposition 2 | 0 | .369 | 0 | .402 | 0 | .706 | 0 | .605 |
| Monotonicity (A6) | (0, .381) | | (0, .414) | | (0, .724) | | (0, .620) | |
| Proposition 3c' | -.631 | .085 | -.598 | .064 | -.294 | .109 | -.395 | .103 |
| Mean Dominance (A7c') | (-.642, .109) | | (-.609, .088) | | (-.313, .153) | | (-.410, .137) | |
| Proposition 6 | 0 | .085 | 0 | .064 | 0 | .109 | 0 | .103 |
| A5 & A6 &A7c' | (0, .111) | | (0, .091) | | (0, .156) | | (0, .141) | |
| *Bounds on* $LATE_{at}$ | | | | | | | | |
| Proposition 2 | 0 | .636 | 0 | .695 | 0 | .523 | 0 | .599 |
| Monotonicity (A5) | (0, .661) | | (0, .718) | | (0, .569) | | (0, .628) | |
| Proposition 3 | -.364 | .032 | -.305 | .119 | -.477 | .296 | -.401 | .283 |
| Mean Dominance (A6) | (-.390, .081) | | (-.329, .164) | | (-.523, .370) | | (-.431, .337) | |
| Proposition 4 | 0 | .032 | 0 | .119 | 0 | .296 | 0 | .283 |
| A4 & A5 &A6 | (0, .086) | | (0, .168) | | (0, .375) | | (0, .341) | |

Table A3: Bounds of Average Effects on Health (Binary) with a Valid IV

| | Not Fair or Poor | | Not Poor | | Same or Gotten Better | | Not Screen Positive for Depression | |
|---|---|---|---|---|---|---|---|---|
| *Bounds on ATE* | | | | | | | | |
| Proposition 1 | -.370 | .340 | -.511 | .200 | -.443 | .267 | -.444 | .266 |
| Bounded Outcome (A5) | (-.383, .351) | | (-.521, .209) | | (-.455, .278) | | (-.456, .277) | |
| Proposition 2 | .038 | .340 | .029 | .200 | .032 | .267 | .024 | .266 |
| Monotonicity (A6) | (.025, .351) | | (.020, .209) | | (.021, .278) | | (.012, .277) | |
| Proposition 3c | .050 | .340 | .036 | .200 | .037 | .267 | -.001 | .266 |
| Mean Dominance (A7c) | (.027, .352) | | (.020, .209) | | (.016, .278) | | (-.023, .278) | |
| Proposition 6 | .044 | .340 | .032 | .200 | .038 | .267 | .025 | .266 |
| A5 & A6 &A7c | (.024, .352) | | (.019, .209) | | (.024, .278) | | (.009, .277) | |
| *Bounds on ATT* | | | | | | | | |
| Proposition 1 | -.162 | .321 | -.032 | .450 | -.083 | .399 | -.137 | .345 |
| Bounded Outcome (A5) | (-.187, .345) | | (-.047, .471) | | (-.104, .424) | | (-.161, .371) | |
| Proposition 2 | .068 | .321 | .051 | .450 | .058 | .399 | .043 | .345 |
| Monotonicity (A6) | (.044, .346) | | (.036, .471) | | (.037, .424) | | (.020, .371) | |
| Proposition 3 | .092 | .321 | .062 | .450 | .092 | .399 | .059 | .345 |
| Mean Dominance (A7c) | (.048, .347) | | (.036, .472) | | (.055, .425) | | (.018, .372) | |
| Proposition 6 | .087 | .321 | .059 | .450 | .088 | .399 | .055 | .345 |
| A5 & A6 &A7c | (.045, .347) | | (.035, .472) | | (.052, .425) | | (.017, .372) | |
| *Bounds on* $LATE_{nt}$ | | | | | | | | |
| Proposition 2 | 0 | .402 | 0 | .104 | 0 | .242 | 0 | .274 |
| Monotonicity (A6) | (0, .413) | | (0, .111) | | (0, .252) | | (0, .285) | |
| Proposition 3c | .008 | .402 | .007 | .104 | -.009 | .242 | -.051 | .274 |
| Mean Dominance (A7c) | (-.017, .413) | | (-.012, .112) | | (-.032, .252) | | (-.076, .285) | |
| Proposition 6 | .008 | .402 | .007 | .104 | -.000 | .242 | -.000 | .274 |
| A5 & A6 &A7c | (-.000, .413) | | (-.000, .111) | | (-.000, .252) | | (-.000, .285) | |
| *Bounds on* $LATE_{at}$ | | | | | | | | |
| Proposition 2 | 0 | .523 | 0 | .827 | 0 | .708 | 0 | .626 |
| Monotonicity (A6) | (0, .549) | | (0, .847) | | (0, .729) | | (0, .649) | |
| Proposition 3c | .049 | .523 | .023 | .827 | .071 | .708 | .034 | .626 |
| Mean Dominance (A7c) | (.001, .550) | | (-.009, .847) | | (.031, .730) | | (-.011, .649) | |
| Proposition 6 | .048 | .523 | .023 | .827 | .071 | .708 | .033 | .626 |
| A5 & A6 &A7c | (.003, .549) | | (-.000, .847) | | (.032, .730) | | (-.000, .649) | |

Table A4: Bounds of Average Effects on Health (# of days) with a Valid IV

| | Physical Health Good | | Mental Health Good | | Poor Physical or Mental Health Did not Impair Usual Activity | |
|---|---|---|---|---|---|---|
| *Bounds on ATE* | | | | | | |
| Proposition 1 | -13.48 | 7.832 | -12.61 | 8.706 | -14.49 | 6.820 |
| Bounded Outcome (A5) | (-13.80, 8.119) | | (-12.93, 9.012) | | (-14.79, 7.093) | |
| Proposition 2 | .417 | 7.832 | .589 | 8.706 | .333 | 6.820 |
| Monotonicity (A6) | (.123, 8.119) | | (.269, 9.012) | | (.061, 7.094) | |
| Proposition 3c | -.156 | 7.832 | .099 | 8.706 | -.703 | 6.820 |
| Mean Dominance (A7c) | (-.691, 8.122) | | (-.471, 9.015) | | (-1.198, 7.096) | |
| Proposition 6 | .347 | 7.832 | .594 | 8.706 | .268 | 6.820 |
| A5 & A6 &A7c | (.070, 8.119) | | (.212, 9.012) | | (.012, 7.094) | |
| *Bounds on ATT* | | | | | | |
| Proposition 1 | -4.689 | 9.780 | -4.848 | 9.621 | -4.441 | 10.03 |
| Bounded Outcome (A5) | (-5.257, 10.42) | | (-5.463, 10.30) | | (-4.964, 10.67) | |
| Proposition 2 | .745 | 9.780 | 1.053 | 9.621 | .595 | 10.03 |
| Monotonicity (A6) | (.214, 10.43) | | (.472, 10.31) | | (.104, 10.68) | |
| Proposition 3c | .532 | 9.780 | 1.425 | 9.621 | .086 | 10.03 |
| Mean Dominance (A7c) | (-.418, 10.45) | | (.380, 10.33) | | (-.775, 10.69) | |
| Proposition 6 | .685 | 9.780 | 1.317 | 9.621 | .536 | 10.03 |
| A5 & A6 &A7c | (.169, 10.43) | | (.408, 10.33) | | (.060, 10.68) | |
| *Bounds on $LATE_{nt}$* | | | | | | |
| Proposition 2 | 0 | 8.494 | 0 | 9.941 | 0 | 6.690 |
| Monotonicity (A6) | (0, 8.757) | | (0, 10.23) | | (0, 6.937) | |
| Proposition 3c | -.890 | 8.494 | -1.030 | 9.941 | -1.549 | 6.690 |
| Mean Dominance (A7c) | (-1.507, 8.760) | | (-1.657, 10.23) | | (-2.119, 6.940) | |
| Proposition 6 | -.000 | 8.494 | -.000 | 9.941 | -.000 | 6.690 |
| A5 & A6 &A7c | (-.000, 8.757) | | (-.000, 10.23) | | (-.000, 6.937) | |
| *Bounds on $LATE_{at}$* | | | | | | |
| Proposition 2 | 0 | 18.73 | 0 | 17.77 | 0 | 19.56 |
| Monotonicity (A6) | (0, 19.28) | | (0, 18.34) | | (0, 20.11) | |
| Proposition 3c | -.442 | 18.73 | .772 | 17.77 | -1.054 | 19.56 |
| Mean Dominance (A7c) | (-1.493, 19.28) | | (-.341, 18.34) | | (-2.030, 20.11) | |
| Proposition 6 | -.000 | 18.73 | .767 | 17.77 | -.000 | 19.56 |
| A5 & A6 &A7c | (-.000, 19.28) | | (-.000, 18.34) | | (-.000, 20.11) | |

Table A5: Bounds of Average Effects on Less Financial Strain with a Valid IV

| | No out of pocket medical expenses | | Not owe for medical expenses currently | | Not borrow money to pay medical bills | | Not be refused treatment due to medical debt | |
|---|---|---|---|---|---|---|---|---|
| *Bounds on ATE* | | | | | | | | |
| Proposition 1 | -.260 | .450 | -.284 | .426 | -.359 | .351 | -.534 | .176 |
| Bounded Outcome (A5) | (-.273, .461) | | (-.296, .437) | | (-.372, .362) | | (-.544, .184) | |
| Proposition 2 | .056 | .450 | .052 | .426 | .045 | .351 | .010 | .176 |
| Monotonicity (A6) | (.044, .461) | | (.040, .437) | | (.033, .362) | | (.003, .184) | |
| Proposition 3c' | -.260 | .187 | -.284 | .104 | -.359 | .137 | -.534 | .019 |
| Mean Dominance (A7c') | (-.273, .210) | | (-.296, .126) | | (-.372, .157) | | (-.544, .030) | |
| Proposition 4' | .056 | .187 | .052 | .104 | .045 | .137 | .010 | .019 |
| A5 & A6 &A7c' | (.043, .211) | | (.038, .129) | | (.032, .159) | | (.002, .032) | |
| *Bounds on ATT* | | | | | | | | |
| Proposition 1 | -.139 | .343 | -.196 | .286 | -.063 | .419 | -.018 | .464 |
| Bounded Outcome (A5) | (-.165, .366) | | (-.222, .310) | | (-.086, .443) | | (-.031, .485) | |
| Proposition 2 | .101 | .343 | .093 | .286 | .081 | .419 | .018 | .464 |
| Monotonicity (A6) | (.077, .367) | | (.070, .311) | | (.059, .444) | | (.006, .485) | |
| Proposition 3' | -.139 | .123 | -.196 | .122 | -.063 | .123 | -.018 | .028 |
| Mean Dominance (A7c') | (-.167, .166) | | (-.223, .163) | | (-.088, .165) | | (-.032, .052) | |
| Proposition 4' | .101 | .123 | .093 | .122 | .081 | .123 | .018 | .028 |
| A5 & A5 &A7c' | (.074, .170) | | (.067, .168) | | (.056, .167) | | (.004, .053) | |
| *Bounds on $LATE_{nt}$* | | | | | | | | |
| Proposition 2 | 0 | .567 | 0 | .556 | 0 | .367 | 0 | .073 |
| Monotonicity (A6) | (0, .579) | | (0, .568) | | (0, .378) | | (0, .079) | |
| Proposition 3c' | -.443 | .217 | -.444 | .076 | -.633 | .138 | -.927 | .010 |
| Mean Dominance (A7c') | (-.446, .243) | | (-.456, .103) | | (-.645, .162) | | (-.934, .024) | |
| Proposition 6' | 0 | .217 | 0 | .076 | 0 | .138 | 0 | .010 |
| A5 & A6 &A7c' | (0, .245) | | (0, .106) | | (0, .164) | | (0, .026) | |
| *Bounds on $LATE_{at}$* | | | | | | | | |
| Proposition 2 | 0 | .502 | 0 | .401 | 0 | .701 | 0 | .925 |
| Monotonicity (A6) | (0, .527) | | (0, .426) | | (0, .723) | | (0, .938) | |
| Proposition 3c' | -.498 | .046 | -.599 | .060 | -.299 | .086 | -.075 | .022 |
| Mean Dominance (A7c') | (-.524, .093) | | (-.624, 107) | | (-.322, .132) | | (-.088, .049) | |
| Proposition 6' | 0 | .046 | 0 | .060 | 0 | .086 | 0 | .022 |
| A5 & A6 &A7c' | (0, .100) | | (0, .113) | | (0, .136) | | (0, .050) | |