Bounds on Average Treatment Effects with an Invalid Instrument, with an Application to the Oregon Health Insurance Experiment*

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Preliminary Draft: March 15, 2018

Abstract

We derive 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 IV affects the outcome only through its effect on the treatment. We allow the IV 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 (principal strata). 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 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 other channels. This enables us to use the (invalid) instrument to learn about the causal effect of the treatment on the outcome. The bounds are employed to re-analyze the effect of Medicaid insurance on health care utilization, self-reported health status, and financial strain within the Oregon Health Insurance Experiment, allowing for the possibility that the Medicaid lottery is an invalid instrument.

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

JEL classification: C13, C21, J30

*We are grateful for comments from Pietro Biroli, Martin Huber, Romuald Meango, and participants at the 2014 Midwest Econometrics Group Meetings, the 2014 Annual Meeting of the International Association of Applied Econometrics, the 2014 Asian Meeting of the Econometric Society, the 2015 Brown Bag Group at Cal Poly, the 2016 Econometric Society European Meetings, the 2016 China Meeting of the Econometric Society, the 2016 Annual Meetings of the China Labor Economists Forum, the 2016 IZA/IFAU Conference on Labor Market Policy Evaluation, the 2016 Munich Workshop on Uncovering Causal Mechanisms, Cornell University, ETH-Zurich (KOF), University of New Hampshire, the 2017 California Econometrics Conference, the Inter-American Development Bank, and Northeastern University. We thank Marisa Carlos for graciously sharing her replication of the results in Finkelstein et al. (2012). Previous versions of this paper circulated under the title "Bounds on Population Average Treatment Effects with an Invalid Instrument." All the usual disclaimers apply.

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1 Introduction

Instrumental variable (IV) methods exploit exogenous variation in an IV to address endogeneity of the treatment when evaluating the treatment effect on an outcome of interest. A widely used framework for identifying causality using 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., the 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, and thus researchers have to resort to careful argumentation of the validity of the instrument.

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 and 2010; Imai et al., 2010; Huber, 2014). 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 latent groups of individuals (principal strata) 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 obtain partial identification on the ATE and ATT by aggregation 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 obtains 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. 2018; Huber and Mellace, 2015b), while just 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 causal effects. 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 difference in Manski and Pepper (2000) and the present work 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. The two identification approaches are not nested; thus, the informativeness of the estimated bounds under each approach may differ in practice.

Our general approach is related to Hirano et al. (2000) and Mealli and Pacini (2013), 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, that is, on local intention-to-treat (ITT) effects. The focus of this paper is on average treatment effects of the treatment of interest on the outcome using an IV. A 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 can be seen as an important generalization of their results. Also, 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. This relates the present work to the recent literature proposing methods to gauge the validity of the exclusion restriction assumption under certain conditions (Hirano et al., 2000; Huber and Mellace, 2015a, 2015b; Mealli and Pacini, 2013; Mourifié and Wan, 2017).

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 portion 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 possible 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 existing experiments can be used as an invalid IV for another (non-randomized) treatment of interest. Here, the randomly assigned IV would likely violate the exclusion restriction. But in spite of the invalidity of the IV, its use can be important when it is not possible or it is too costly to randomize a treatment of interest. Recent examples of this use are in Flores and Flores-Lagunes (2013) and Amin et al. (2016).

In the second part of the paper, we illustrate our methodology with public-use data from the Oregon Health Insurance Experiment (OHIE) to re-analyzethe effect of Medicaid health insurance coverage on health care and preventive 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. In a seminal paper, Finkelstein et al. (2012) employed this lottery as an IV for Medicaid health insurance coverage to estimate the effect of the latter on a myriad of relevant outcomes. However, as noted by Finkelstein et al. (2012) and Baicker et al. (2014), it is possible that the lottery violates the exclusion restriction assumption of the IV. Thus, it is important to examine the results of OHIE without imposing the exclusion restriction. Moreover, the results in Finkelstein et al. (2012) apply to the latent subpopulation of compliers, which account for about 30% of the target population. We bound the ATE for the entire target population and ATT for the population of treated individuals covered by Medicaid. This analysis is, to our knowledge, the first to make inference about these population parameters of chief importance for policy. The bounds on ATE and ATT are informative under our two sets of weak monotonicity assumptions of average potential outcomes. We show that the $LATE$ point estimates (on compliers) that assume the exclusion restriction for several outcomes fall outside our estimated bounds for the same parameter that do not assume its validity but that assume one of our weak monotonicity assumptions. As a result, either the exclusion restriction or our weak monotonicity assumption fails. If it is the former, then the $LATE$ point estimates are upward biased. Nevertheless, such bias is not large and thus the overall qualitative findings in the original OHIE studies hold. We also document that, compared with the bounds derived by imposing the exclusion restriction in Chen et al. (2018), the exclusion restriction provides considerable identification power as it considerably shrinks the width of the bounds.

The rest of the paper is organized as follows. Section 2 presents the setup and the formal partial identification results on the ATE and ATT. All the proofs of the partial identification results are relegated to the Internet Appendix. Section 3 employs those bounds to re-analyze the effect of Medicaid health 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 $Y_i = D_i Y_{1i} + (1-D_i) Y_{0i}$. Define 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 it 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.[1]

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 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 affected in the same way 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 IA and AIR, we impose the following assumptions:

Assumption 1 (*Randomly Assigned Instrument).*

[1] Our notation implicitly imposes the stable unit treatment value assumption (SUTVA) of Rubin (1980). 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.

$\{Y(1,1), Y(0,0), Y(0,1), Y(1,0), D_0, D_1\}$ *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 D in Z).* $D_{1i} \geq D_{0i}$ *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:

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

$LATE$ refers to the average effect of D on Y for those individuals whose treatment status is affected by the instrument (i.e., the 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 IA and 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 or indirect effect) from the part that works through other channels (i.e., the net or direct effect). This provides a link to causal mediation analysis (e.g., Pearl, 2001; Sjölander, 2009; Flores and Flores-Lagunes, 2010). 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 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, as in Flores and Flores-Lagunes (2013), $Y_i(z, D_{1-z})$ 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 (Frangakis and Rubin, 2002). Following Flores and Flores-Lagunes (2010), let the mechanism average treatment effect ($MATE^z$) be given by

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

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

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

Since $Y(z) = Y(z, D_z)$, $MATE^z$ 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, which can be denoted as $E[\Delta(z)]$, for $z = 0, 1$. Following the notation above, under Assumption 1, we have $\Delta(z) \equiv E[Y_i(z, 1) - Y_i(z, 0) | Z = z] = E[Y_i(z, 1) - Y_i(z, 0)]$. Let π_k denote the proportion of the stratum k in the population, with $k = at, nt, c$. Under Assumptions 1 through 3, we can write $\Delta(z)$ as a weighed average of the "local" average effects of the strata:

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

Using equation (4), partial identification of $\Delta(z)$ will be attained from the level of the strata up. Thus, we 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

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

and

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

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 ($LMATE_c^z =$

$E[Y(z,D_1)-Y(z,D_0)|c] = E\{[D_1-D_0]\cdot[Y(z,1)-Y(z,0)]|c\} = E\left[Y(z,1)-Y(z,0)|c\right])$. In the current setting, 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 help motivate our nonparametric bounds 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 $p_{d|z} \equiv \Pr\left(D_i = d|Z_i = z\right)$ 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 $\pi_{nt}/\left(\pi_{nt} + \pi_c\right) = p_{0|1}/p_{0|0}$. Thus, $E[Y(0)|nt]$ can be bounded from above (below) by the expected value of Y for the $p_{0|1}/p_{0|0}$ fraction of *largest (smallest)* values of Y for those in the observed group $\{Z, D\} = \{0, 0\}$. Similar bounds on $E[Y(0)|c]$ can be obtained from 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 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 (to be stated below) on $LNATE_k^{1-z}$ for $k = at, nt, c$. Equation (10) takes advantage of point identification of the conditional average outcomes $\overline{Y}^{zd}$.

Note that 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. To partially identify those objects and derive the nonparametric bounds on $\Delta(z)$ we consider different assumptions in turn. 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 $\Delta(z)$ using equation (10).

Proposition 1 *If Assumptions 1 through 4 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

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

By Assumption 4, the lower bounds in Proposition 1 are negative while the upper bounds are positive. Such bounds often provide limited information in practice. Previous work that considered bounds involving IVs under a bounded-outcome assumption include Manski (1990), Balke and Pearl (1997), Heckman and Vytlacil (2000), Kitagawa (2009), Chen et al. (2018), and Huber and Mellace (2015b).

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 into the corresponding equations and the resulting bounds are analyzed to rule out lower (upper) bounds that are always smaller (greater) than the others. The resulting bounds are our nonparametric bounds for $E[\Delta(z)]$. 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, as it is illustrated in the different outcomes re-amalyzed within the OHIE in the next section. Importantly, each set of monotonicity assumptions below could be substantiated with 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, it is implied that $ATE_{ZY} \geq 0$. Assumption 5.3 imposes non-negative average treatment effects on always takers and never takers by considering their respective counterfactual treatment status. Imposing restrictions on the sign of effects may be objectionable in certain applications, such as when theory yields ambiguous signs for those effects. In those cases, the other weak monotonicity assumption introduced below represents an alternative.

We note that Assumption 5.2 is not be strictly necessary to derive bounds on $\Delta(z)$, but it is helpful in tightening the bounds.[2] In contrast, Assumptions 5.1 and 5.3 are necessary. Assumption 5.1 allows partial identification of $Y(z, D_{1-z})$ for compliers. As for Assumption 5.3, note that $Y(z, 0)$ for always takers and $Y(z, 1)$ for never takers are entirely counterfactual outcomes (just as $Y(z, D_{1-z})$ is for compliers), and additional information is unavailable on their local mechanism and net effects because of their compliance behavior. Finally, note that since Assumption 5.3 only provides one-sided bounds for these entirely counterfactual outcomes, the bounded-support assumption (Assumption 4) will be necessary to derive bounds on these objects.

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) employed 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. In contrast to 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 in the previous subsection, 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* $z = 0, 1$,

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

[2] 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.

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 Internet 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 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 (weakly) 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 $\overline{Y}^{00}$ 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 the potential outcomes of compliers under the same potential values of the instrument and the treatment.

Three attractive features of Assumption 6 are that (1) it may be substantiated with economic theory in a given application, (2) it yields testable implications, and (3) it is possible to gather indirect evidence about its plausibility. Regarding the first feature, we may expect from theory relevant to a given setting 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. For the third feature, we propose to obtain 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 features will be illustrated and further discussed in the context of our re-analysis of OHIE.

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 Internet Appendix.

The lower and upper bounds on $\Delta(z)$ (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 by 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: $\overline{Y}^{11} \geq \overline{Y}^{00}$.[3] 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* $0 \leq \Delta(0) \leq \min\{UB^0_a, UB^0_b\}$ *and* $0 \leq \Delta(1) \leq \min\{UB^1_a, UB^1_b\}$ *are sharp. And for* $z = 0, 1$,

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

where

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

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

[3] 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 testable implication follows from combining these inequalities with equation (7) and the corresponding equation for the observed group $\{Z, D\} = \{1, 1\}$.

Proof. See Internet Appendix.

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

We close this section with a few final remarks pertaining to the fact that the specific 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 $\Delta(z)$, but they tighten the bounds. 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, as will be the case in the next section. In that case, the resulting bounds can be derived using the same procedure. Third, some specific potential outcomes in the assumptions can be changed. For instance, Assumption 6.1 could be changed to $E\left[Y(1, D_0)|c\right] \geq E\left[Y\left(0\right)|nt\right]$, which may be easier to justify in some empirical settings. Thus, the bounds presented here correspond to one set of a menu of related assumptions that can be employed to partially identify the effects of interest.

2.3 Bounds on the ATT

This subsection uses the approach outlined above to derive 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 $E[Y_{1i} - Y_{0i}|D = 1] \equiv E[E[Y_i\left(z, 1\right) - Y_i\left(z, 0\right)|Z = z, D = 1]]$. Let us denote $\Pr(Z = z) = w_z$ and $\Pr(D = 1) = r_1$. Looking at Table 1, the ATT can be written as:

$$\begin{aligned} ATT &= \frac{w_1 p_{1|1}}{r_1}(\overline{Y}^{11} - E[Y(1,0)|at,c]) + \frac{w_0 p_{1|0}}{r_1}(\overline{Y}^{01} - E[Y(0,0)|at]) && (11) \\ &= E[Y|D = 1] - \frac{w_1 p_{1|1}}{r_1}E[Y(1,0)|at,c] - \frac{w_0 p_{1|0}}{r_1}E[Y(0,0)|at] && (12) \end{aligned}$$

Equation (11) employs Bayes rule to write $\Pr(Z = z|D = 1) = \frac{w_z p_{1|z}}{r_1}$, 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 following the link we established 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 the derivation of the bounds on $\Delta(z)$, we write $\Gamma(1)$ as a function of local causal mechanism and net effects that can be either point identified or partially identified:

$$\begin{aligned}
\Gamma(1) &= \pi_{at}(E[Y(1)|at] - E[Y(1,0)|at]) + \pi_c LMATE_c^1 && (13)\\
&= \pi_{at}(E[Y(0)|at] - E[Y(1,0)|at]) + E[Y|Z=1] - E[Y|Z=0] \\
&\quad -\pi_{nt} LNATE_{nt}^0 - \pi_c LNATE_c^0 && (14)\\
&= p_{1|1}\overline{Y}^{11} - \pi_{at}E[Y(1,0)|at] - \pi_c E[Y(1,D_0)|c] && (15)
\end{aligned}$$

Each of the equations above exploits different information in the data and generates different bounds on $\Gamma(1)$ (and thus on the ATT). The rest of this subsection lists the bounds on the ATT under assumptions similar to those employed to bound the ATE. Specifically, they change in that the part of the assumptions that pertain to bounding objects for the nt stratum are not required. The corresponding propositions with the bounds for the ATT (and their proofs) are provided in the Internet Appendix.

Under assumptions similar to those in Proposition 1, the bounds $lb \leq ATT \leq ub$ are sharp, where

$$\begin{aligned}
lb &= E[Y|D=1] - y^u \\
ub &= E[Y|D=1] - y^l.
\end{aligned}$$

Under assumptions similar to those in Proposition 2, the bounds $0 \leq ATT \leq \min\{ub_a, ub_b\}$ are sharp, where

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

Under assumptions similar to those in Proposition 3, 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 assumptions similar to those in Proposition 4, 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

All of the bounding functions in Propositions 1, 2, and 3, and the corresponding bounds on ATT can be estimated using plug-in estimators. However, some of our bounds 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, e.g., Tamer, 2010, and the references therein).

We employ the methodology proposed by Chernozhukov, Lee and Rosen (2013) (hereafter CLR) to obtain confidence regions for the true parameter value, as well as half-median unbiased estimators for the bounds on ATE and ATT that contain min and max operators. 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). For bounds without min or max operators, we construct confidence intervals for the true parameter value based on the procedure by Imbens and Manski (2004).[4]

3 Application to the Oregon Health Insurance Experiment

We present an application of the bounds developed in the previous section to the public-use data from the Oregon Health Insurance Experiment (OHIE) to re-analyze the effects of Medicaid coverage on health care and preventive 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. The OHIE is one of the rare cases in which a random assignment through a lottery is used to estimate the effects of Medicaid health insurance coverage on a myriad of outcomes. For this reason, the OHIE has been an influential series of ongoing studies (e.g., Finkelstein et al., 2012; Baicker et al., 2014).[5]

[4] The Imbens and Manski (2004) confidence intervals we employ are valid for the situation where the length of the bounds on the parameter of interest is bounded away from zero (Stoye, 2009).

[5] See also the experiment's website at http://www.nber.org/oregon/.

However, since not everyone who won the lottery obtained Medicaid health insurance, most of the focus of the original studies is on ITT effects (the effect of the lottery itself) or on the $LATE$ for compliers, who represent about 30% of the target population.[6] In addition, as discussed by Finkelstein et al. (2012) and Baicker et al. (2014), it is possible that the lottery violates the exclusion restriction of the IV. One relevant reason the lottery may violate the exclusion restriction is that, if the Medicaid application was made in person, caseworkers in Oregon were instructed to offer assistance to interested applicants in applying for other public programs (Finkelstein et al. 2012, and Baicker et al., 2014).[7] As a result, the lottery may have an effect on outcomes independently of Medicaid health insurance coverage, violating the exclusion restriction. In this section, we present nonparametric bounds on the ATE and ATT of the target population while allowing the lottery IV for Medicaid health insurance coverage to have a direct effect on the outcomes of interest.

3.1 Data from the Oregon Health Insurance Experiment

In January 2008, Oregon initiated a Medicaid expansion program for low-income adults, the Oregon Health Plan (OHP) Standard. Eligible adults for OHP were aged 19-64, Oregon residents, U.S. citizens or legal immigrants, without health insurance for at least six months, and not otherwise eligible for public insurance. Their income was below the federal poverty level ($10,400 for an individual and $21,200 for a family of four in 2008), and assets below $2,000. 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 depending on income). The state conducted eight random lottery drawings 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 to be used as IV is random conditional on the number of household members on the waiting list. Selected individuals who completed the application process and met the eligibility requirements were enrolled in Medicaid (Finkelstein et al., 2012; Taubman et al., 2014).

We employ public-use data from OHIE to re-analyze the effects of Medicaid health insurance coverage. More specifically, we use data from pre-treatment demographic variables obtained at the time of signing up for the lottery, an initial survey that was conducted between June 2008 and November 2008 (shortly after lottery randomization), and a 12-month survey that contains the outcomes we focus on. The 12-month survey was mailed out in seven waves over July and August 2009, and the average survey response occurs roughly one year after insurance

[6] The reason for this noncompliance is that about 60% of those who won the lottery filled out the required paperwork to receive Medicaid health insurance coverage, and among this group only about half actually met the eligibility requirements for the coverage (Finkelstein et al., 2012).

[7] The main programs interested applicants obtained help applying for are the Temporary Assistance for Needy Families (TANF) and the Supplemental Nutrition Assistance Program (SNAP); see Baicker et al. (2014).

approval (mean = 13.1 months; std. dev. = 2.9 months). The three sets of outcomes we consider include health care and preventive care utilization, self-reported health status, and financial strain. Following Finkelstein et al. (2012), the IV is an indicator for whether the household was selected by the lottery, and the treatment is defined by whether the individual of that household was ever on Medicaid during the study period. Due to the sampling scheme, the probability of winning the lottery varies by survey waves and within household size. To account for this feature of the data, we construct a set of weights to be employed throughout the analysis, which predicts the probability of winning the lottery conditional on household size, survey waves, their interaction terms, and the 12-month sampling weight provided in the original survey data. Additionally, because winning the lottery occurs at the household level, we calculate standard errors accounting for this clustering.

Table 2 presents summary statistics of demographic variables in the OHIE public-use data. The upper panel shows the means of pre-treatment variables by treatment and control groups defined by the lottery (and their differences), as well as the proportions of missing values in our sample that contains 23,741 observations. These pre-treatment variables were gathered at the time individuals signed up for the lottery. Approximately 60% of individuals are female, 68% are 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% have ever participated in TANF. Except for English as a preferred language, which shows an imbalance between treatment and control groups significant at the 5 percent level, the sample shows balanced characteristics on these pre-treatment variables. The lower panel in Table 2 shows the statistics for other demographic variables from the initial OHIE survey. Over 80% of individuals are white, over one-half have a high school diploma or GED, over one-half have household income above 150% of the federal poverty line, and 52% of them do not have jobs at the time. Most of these demographic variables are balanced between treatment and control groups defined by the lottery, although the treatment group has significantly (at the 5 percent level) higher average household income and less of them have income below 50% of the federal poverty line.

The upper panel of Table 3 shows estimated effects of the lottery (ITT effects) on relevant variables from the initial survey conducted shortly after the lottery randomization. Treatment-group individuals had a significantly higher enrollment rate in Medicaid (OHP/Medicaid) shortly after randomization of 8.5 percentage points relative to the control group. As documented in Finkelstein et al.(2012), there are no significant effects on health care utilization (at the 5 percent level), and the significant effects on the indicators of self-reported health status and financial strain are small and economically insignificant. The effects on the proportion of individuals who have ever been diagnosed with chronic diseases are also negligible and some of them are not significant. We note that we have transformed the outcomes on financial strain as one minus each corresponding binary measure of financial strain in Finkelstein et al. (2012). Thus, our financial strain outcomes are indicators of "alleviation of financial strain". We do this to facilitate the application of the weak monotonicity assumptions to these outcomes in the estimation of the bounds. Given that we do not have access to a rich set of pre-treatment outcomes, we will regard the outcomes in the initial survey as baseline characteristics and estimate their average for different strata to inform the ranking of the weak monotonicity assumption of mean potential outcomes across strata (Assumption 6). We note that, as Finkelstein et al. (2012, p. 1099) argue, it seems unlikely that the impact of the lottery on the initial survey outcomes in the top panel of Table 3 reflect changes in objective physical health.

Table 4 shows the estimated proportions of the three strata under Assumptions 1 to 3 in our analysis samples. In the main sample, consisting of the target population, never takers make up 57.6% of the target population while compliers account for 28.9% and always takers for 13.5%. The estimated probability of winning the lottery ($Pr(Z = 1)$) is .05 while that of being covered by Medicaid during the 12-month survey ($Pr(D = 1)$) is .28. In addition to the main sample, we focus on samples of women above 40 years old to examine Medicaid effect on mammograms, and on women of any age to examine the effects on pap tests. The estimated stratum proportions and probabilities in these two subsamples show similar patterns to the ones in the main sample. Overall, the results presented in Tables 2, 3, and 4 mirror those in Finkelstein et al. (2012).

3.2 Assessment of Assumptions

We take each of the assumptions from the previous section in turn and discuss their plausibility within the present empirical setting. We pay particular attention to Assumption 5 and Assumption 6 since they are central to constructing our bounds and are not as common as the other assumptions.

The random assignment of the IV (Assumption 1) holds by design in the OHIE. The non-zero average effect of the lottery on Medicaid health insurance coverage (Assumption 2) also holds in the OHIE. As shown in Table 4, the estimated average effect of the lottery on Medicaid coverage (i.e., π_c), is a highly statistically significant 0.289. The individual-level monotonicity of Medicaid coverage in the lottery indicator (Assumption 3) requires that no individuals would obtain Medicaid coverage if they lost the lottery but would not obtain coverage if they won the lottery. This requirement of Assumption 3 seems plausible in this context where an application had to be submitted to have a chance of obtaining Medicaid coverage. For the bounded-outcome support assumption (Assumption 4), outcomes that are binary indicators are naturally bounded; for other outcomes we follow the common practice of using the in-sample minimum and maximum values as their bounds.

3.2.1 Assumption 5

Assumption 5 imposes weak monotonicity of mean potential outcomes within strata. Assumption 5.1 states that the effect of the lottery that works through actual Medicaid health insurance coverage (the mechanism or indirect effect) is non-negative for compliers. It implies that, for compliers, Medicaid health insurance coverage has a non-negative average effect on health care and preventive care utilization, self-reported health, and the alleviation of financial strain. Theoretically, Medicaid health insurance coverage decreases the price of medical care and thus is expected to increase the quantity demanded for health care and preventive care. In turn, the increased quantity demanded of health care is expected to help improve health status, including self-reported health measures.[8] Moreover, from the point of view of risk-spreading, Medicaid health insurance coverage is expected to reduce the probability of suffering financial strain due to unexpected medical expenses. Thus, we expect positive local mechanism effects on each of those outcomes.

Assumption 5.2 requires that, for each stratum, winning the lottery has a non-negative average effect on the outcomes of interest through channels other than Medicaid coverage (the net or direct effect). As pointed out by Finkelstein et al. (2012) and Baicker et al. (2014), individuals who applied for Medicaid health insurance in person were also encouraged to apply for other public programs, such as SNAP and TANF. These other transfer programs are expected to improve individuals' nutritional intake directly (SNAP) or through an increase in income availability (TANF), likely improving their health status. The income effect induced by these public transfer programs may also increase individuals' consumption of health care services and reduce the probability of suffering financial strain due to unforeseen medical expenses. The bottom panel of Table 3 shows estimated effects of the lottery on health insurance coverage and public assistance programs measured at the 12-month survey. Consistent with the findings in those papers, winning the lottery increased the probability of ever receiving SNAP (food stamps) by a statistically significant 2.7 percentage points, and increased total SNAP benefits by a statistically significant \$96.99.

Assumption 5.3 relates the average of the entirely counterfactuals for always takers and never takers ($Y(z,0)$ and $Y(z,1)$, respectively) to their average potential outcomes under exposure to the instrument ($Y(z)$). Particularly, the weak inequalities in Assumption 5.3 require that Medicaid health insurance coverage has a non-negative average effect for always takers and never takers. Intuitively, If we could force always takers to be absent of Medicaid health insurance coverage, then Assumption 5.3 implies that they would be no better off in terms of their health care and preventive care utilization, self-reported health status, and financial

[8]Although self-reported health measures might be less accurate than physical health measures, several types of diagnosis, such as mental health diagnosis (e.g., depression), rely on such self-reports (Finkelstein et al., 2012). Thus, as mentioned by Finkelstein et al. (2012), a positive effect on self-reported health status may also reflect a general sense of improved wellbeing due to Medicaid health insurance coverage.

strain. Conversely, if we could force never takers to be covered by Medicaid health insurance, Assumption 5.3 implies that they would be no worse off in terms of the same outcomes. Thus, to justify Assumption 5.3, we can appeal to the same arguments that were employed in the context of justifying Assumption 5.1.

Since the ITT effect can be decomposed into the sum of local mechanism and net effects for each stratum, the combination of Assumption 5.1 and Assumption 5.2 imply non-negative ITT effects. Table 5 presents relevant point estimates, including the estimated ITT effects of the Medicaid lottery on health care utilization and preventive care (fourth row). The estimated ITT effects on currently taking prescription drugs and having had outpatient visits in the last six months (both extensive and intensive margin measures) are all positive and statistically significant. The estimated ITT effects on preventive care (ever had blood cholesterol checked and blood tested for high blood sugar/diabetes, mammogram and pap test within the last 12 months) are also positive and statistically significant. Correspondingly, Table 6 (fourth row) shows the estimated ITT effects on seven measures of self-reported health status, all of which are also positive and statistically significant.[9] Table 7 (fourth row) also shows positive and statistically significant estimated ITT effects on the alleviation of financial strain.[10] Tables 5 to 7 (fifth rows) also show, for reference, point estimates of the $LATE_c$ under the assumption that the Medicaid lottery is a valid IV. These $LATE_c$ point estimates are all positive, statistically significant, and are very close to those in Finkelstein et al. (2012).

3.2.2 Assumption 6

Assumption 6 imposes weak monotonicity assumptions of mean potential outcomes across strata. The basic notion behind this assumption in the current empirical setting is that always takers have characteristics that make them likely to be in poor health relative to compliers and never takers. The reason for this notion is that 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 definition, always takers are individuals who become covered by Medicaid regardless of lottery selection, while never takers are individuals who never become covered by Medicaid irrespective of lottery selection. Thus, based on the notion of adverse selection, it is reasonable to presume that always takers are in the poorest health among the three strata, while never takers are the healthiest group. According to this notion, the relative health ranking across strata will result in one of two weak rankings of the

[9] The measures of self-reported health status include currently not fair/poor and not poor, health about the same or gotten better over last six months, did not screen positive for depression in the last two weeks, numbers of days in good physical health and in good mental health, and poor physical or mental health did not impair usual activity in the past 30 days.

[10] The measures of alleviation of financial strain include not having out of pocket medical expenses, not currently owing for medical expenses, not borrowing to pay medical bills in the last six months, and has not been refused treatment due to medical debt in the last six months.

strata in terms of mean potential outcomes that depend on the nature of each outcome. The first weak ranking of the strata is the one represented in Assumption 6, where always takers have no lower mean potential outcomes than compliers, whom in turn have no lower mean potential outcomes than never takers. This weak ranking applies to the outcomes related to health and preventive care utilization and the alleviation of financial strain. The second weak ranking applies to self-reported health outcomes and corresponds to the exact opposite of the weak ranking in Assumption 6 (we refer to it as Assumption 6' below). We will discuss below the implications of these two weak strata rankings for the nonparametric bounds. These two versions of Assumption 6 illustrate how it can be adapted to a particular empirical setting.

The bottom panels of Tables 5, 6, and 7 show point estimates of average outcomes, which indirectly suggest the weak ranking of mean potential outcomes across strata. For health care and preventive care utilization (Table 5), we have $\overline{Y}^{11} > \overline{Y}^{10}$ and $\overline{Y}^{01} > \overline{Y}^{00}$. Also note that $\overline{Y}^{11} > \overline{Y}^{00}$, which is a testable implication under the combination of Assumption 5 and Assumption 6. These inequalities indirectly support the weak ranking in mean potential outcomes in Assumption 6 since $\overline{Y}^{11}$ is the mean outcome of a mixture of at and c, and $\overline{Y}^{00}$ is the mean outcome of a mixture of nt and c. Thus, in the context of health and preventive care utilization, these point estimates are consistent with the notion that always takers generally have the poorest health status (and thus use more health care services).

The bottom panel of Table 6 shows estimated average outcomes related to self-reported health status. Based on the presumption that never takers have the best health status among the three strata and always takers have the poorest, the ranking of the strata in Assumption 6 will be reversed—referred to as Assumption 6'—for the self-reported health status outcomes. As a consequence, Assumption 6' will lead to different bounds to those in Propositions 3 and 4, which are referred to as Propositions 3' and 4', and are given in the Internet Appendix. The estimated average outcomes for self-reported health in Table 6 are consistent with the weak ranking of mean potential outcomes across strata in Assumption 6', while the testable implications under Assumption 6' and its combination with Assumption 5 hold for the seven measures of self-reported health status.[11]

The bottom panel of Table 7 shows estimated average outcomes related to the alleviation of financial strain. For reasons explained below in the context of the analysis of average baseline characteristics by strata, the weak monotonicity assumption across strata that we use for this set of outcomes is as in Assumption 6. That is, strata are ranked in terms of their mean potential outcomes as always takers, then compliers, and last never-takers. For no out of pocket medical expenses and not borrowing money to pay medical bills, we see that $\overline{Y}^{11} > \overline{Y}^{00}$, $\overline{Y}^{11} > \overline{Y}^{10}$, and $\overline{Y}^{01} > \overline{Y}^{00}$. These inequalities are consistent with Assumption 6. For not owing for medical

[11]Under Assumption 6' the testable implications are $\overline{Y}^{11} \leq \overline{Y}^{10}$ and $\overline{Y}^{01} \leq \overline{Y}^{00}$, while that when combined with Assumption 5 is $\overline{Y}^{11} \leq \overline{Y}^{00}$.

expenses and for not being refused treatment due to medical debt, we also have $\overline{Y}^{11} > \overline{Y}^{00}$ and $\overline{Y}^{11} > \overline{Y}^{10}$, while $\overline{Y}^{01} - \overline{Y}^{00}$ is not statistically different from zero (and thus still consistent with the weak ranking). Overall, then, these inequalities are consistent with the testable implications when adopting Assumption 6.

To further inform the weak ranking of mean potential outcomes across strata (in Assumption 6 or Assumption 6'), we can estimate average baseline characteristics by strata. This can be done using a non-parametric GMM approach as in Chen et al. (2018).[12] Table 8 shows average baseline characteristics for the main analysis sample (from the 12-month survey), using the pre-treatment variables available and also some of the variables collected during the initial survey.[13] As shown in Table 8, always takers are more likely to be female, younger, and more of them live in a metropolitan area. They are more likely to ever had enrolled in SNAP 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 the never takers enrolled in private insurance, while more of the compliers and always takers enrolled in Medicaid. Therefore, never takers appear to have the most favorable economic situation among the three strata, while always takers have the worst. Given their better economic situation, one may conjecture that never takers may also demand a larger quantity of health care services relative to always takers, which would seem to contradict the earlier notion based on the prediction from adverse selection. However, in line with the adverse selection prediction, the differences between the two groups on health care utilization at the initial survey in Table 8 show that always takers actually demand the highest quantity of health care services among the three strata, lending indirect support to the weak ranking of strata in Assumption 6. Table 8 also shows that always takers are more likely to ever have been diagnosed with chronic diseases, and never takers are the healthiest group, consistent again with the prediction of adverse selection. For the alleviation of financial strain, the evidence from estimated average baseline characteristics is somewhat mixed. The last few rows in Table 8 indicate that 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. In general, perhaps with the exception of the alleviation of financial strain outcomes, the analysis of average baseline characteristics offer additional support to our weak ranking of strata in our

[12]The details on the GMM estimation approach are provided in the Internet Appendix.

[13]Recall that the initial survey was conducted on average 2.6 months after randomization, and about 1 month after coverage approval. Thus, those variables from the initial survey are not baseline variables, strictly speaking. However, in line with Finkelstein et al. (2012), taking into account the short time span and the results presented in Table 3, we argue that Medicaid generally had no meaningful effects on those variables.

assumptions for the different outcomes considered.

3.3 Estimated Bounds on Average Effects of Medicaid Coverage in the OHIE

3.3.1 Health and Preventive Care Utilization Outcomes

Table 9 presents the estimated bounds on average effects for the health care utilization outcomes. The bounds on ATE and ATT (top two panels) under the bounded support assumption (Assumption 4) are usually wide and include zero. Under the monotonicity assumption of mean potential outcomes within strata (Assumption 5), the estimated bounds are restricted to non-negative regions by assumption. Under the weak monotonicity assumption of mean potential outcomes across strata where always takers have no less average health care utilization among the three strata (Assumption 6), the bounds are narrower than those under Assumption 4, especially the upper bounds. These estimated bounds are able to rule out large effects. The estimated bounds under the combination of Assumption 5 and Assumption 6 are narrower than those under Assumption 5 only, although by construction they do not rule out a zero effect.

The lower panels of Table 9 shows the estimated bounds on the local net effects of never takers and always takers, as well as on the local average treatment effect of compliers. This last effect is the same as that (point) estimated by Finkelstein et al. (2012) assuming the validity of the exclusion restriction. The bounds on this parameter do not assume the validity of the exclusion restriction (Flores and Flores-Lagunes, 2013), and are estimated as a weighted average of the local mechanism effects for compliers at $z = 1$ and $z = 0$.

The estimated bounds of $LNATE_{nt}$ on any outpatient visits are in the positive region under Assumption 6, which do not assume the sign of the local effects (Assumption 5). They imply that winning the lottery increases the probability of any outpatient visits for never takers by between 0.23 and 0.256 percentage points, and the bounds' 95% confidence interval exclude zero. Winning the lottery also increases the number of outpatient visits by between .086 and 1.377, with the bounds' 90% confidence interval ($[0.008, 1.445]$) ruling out a zero effect. This evidence could be interpreted as the lottery IV violating the exclusion restriction when estimating effects on outpatient visits. However, that interpretation maintains Assumption 6 when constructing the bounds. Thus, a correct interpretation is that either the exclusion restriction fails or Assumption 6 fails. Given the evidence presented before and the fact that Assumption 6 is imposed on mean outcomes and not at the individual level, we tend to trust more Assumption 6. Under the combination of Assumption 5 and Assumption 6, the bounds of $LNATE_{nt}$ and $LNATE_{at}$ on prescription drugs stay in non-negative regions. However, in this case we hesitate to imply that this is evidence of a violation of the exclusion restriction since Assumption 5 imposes the sign of the effects. Interestingly, the point estimate of $LATE_c$ on the probability of any outpatient visits under the exclusion restriction in Table 5 falls outside the estimated bounds of $LATE_c$ under the combined assumptions. To formally assess this, we employ CLR's method to calculate the half-median-unbiased estimators and corresponding confidence intervals for the difference between the bounds on $LATE_c$ and the point estimate of $LATE_c$ under the exclusion restriction. Although the confidence interval from this procedure contains zero, the difference on the probability of any outpatient visits is bounded between $-.026$ and $-.215$. This could be interpreted as a sign of upward bias in the point estimates due to a violation of the exclusion restriction, provided that the two weak monotonicity assumptions (Assumption 5 and Assumption 6) hold.

Table 10 shows the estimated bounds on average effects for the preventive care utilization outcomes. Similar to the case of the health care utilization outcomes, the bounds on ATE and ATT are wide under Assumption 4. Under Assumption 5, the estimated bounds are restricted to non-negative regions. Under the same weak monotonicity assumption across strata as the one for health care utilization (Assumption 6), the estimated bounds are narrower than those under Assumption 4, especially the upper bounds. Also, the estimated bounds under the combination of Assumption 5 and Assumption 6 are narrower than those under Assumption 5 alone. In particular, the estimated upper bounds on the probability of having blood cholesterol checked and blood tested for high blood sugar or diabetes are informative. For example, the estimated upper bounds on the ATE and ATT of Medicaid health coverage on the probability of having blood cholesterol checked are no larger than 0.42 and 0.53 percentage points, respectively.

The three lower panels in Table 10 show the estimated bounds on the local effects for different strata. The estimated bounds on $LNATE_{nt}$ are non-negative under Assumption 6 for all four measures of preventive care utilization, with the 90% confidence interval for $LNATE_{nt}$ for having a mammogram test and the 95% confidence interval for the same parameter for having a pap test excluding zero.[14] In addition, the estimated bounds on $LNATE_{at}$ for the probability of having blood cholesterol checked indicate that this direct effect is bounded between 0.54 and 0.364 percentage points, with a 95% confidence interval that excludes zero. The estimated bounds on $LATE_c$ under the combination of Assumption 5 and Assumption 6 lie in non-negative regions. All of the point estimates of $LATE_c$ under the exclusion restriction in Table 5 fall outside these bounds of $LATE_c$. Applying the CLR procedure to the difference in point estimate and bounds, the estimated bounds on the difference exclude zero (are in the negative region), with the corresponding 95% confidence intervals also ruling out zero. Furthermore, the estimated bounds on the difference between the point estimate and the bounds on $LATE_c$ on having blood cholesterol checked under Assumption 6 only also exclude zero. Therefore, as before, these results could suggests that the lottery IV violates the exclusion restriction and results in upward biases for some of the preventive care utilization outcomes, provided that Assumption 5 and/or Assumption 6 holds (depending which bounds are considered).

[14] The 90% confidence interval for $LNATE_{nt}$ for having a mammogram test, not reported in Table 10, is $[.004, .309]$.

3.3.2 Self-Reported Health Status Outcomes

Table 11 shows the estimated bounds on average effects for self-reported health status outcomes that are binary. As before, the estimated bounds on ATE and ATT are wide under Assumption 4, while adding Assumption 5 restricts the estimated bounds to non-negative regions. The weak mean monotonicity assumption across strata for the self-reported health status states that never takers are, on average, the group in best health among the three strata (Assumption 6'). The estimated lower bounds under Assumption 6' are substantially narrower than those under Assumption 4. The estimated bounds are non-negative under the combination of Assumption 5 and Assumption 6'. Looking at the third and fourth panels in Table 11, the estimated bounds on the local net effects for never takers and always takers do not rule out a zero effect. At the same time, however, with the exception of whether the individual has been screened positive for depression, the point estimates of $LATE_c$ under the exclusion restriction in Table 6 fall outside the estimated bounds of $LATE_c$ under the combination of Assumption 5 and Assumption 6'. The estimated bounds on the difference between the point estimate and the bounds on $LATE_c$ exclude zero and are in the negative region, while their 95% confidence intervals (for health is not fair or poor and for health is the same or gotten better) and the 90% confidence intervals (for health is not poor) exclude a zero difference. Interpreting this evidence is slightly more complicated given that none of the bounds under Assumption 6' on $LNATE_{nt}$ and $LNATE_{at}$ exclude zero, which would hint at the possible violation of the exclusion restriction. Meanwhile, the fact that the point estimates of $LATE_c$ that assume the validity of the lottery IV are outside the corresponding estimated bounds that use Assumption 5 and Assumption 6' may be due to the invalidity of the IV or to the failure of any of the two weak monotonicity of mean outcomes assumptions.

Table 12 shows the estimated bounds on average effects for self-reported health status outcomes that are measured in number of days in good health out of the last 30 days. The estimated bounds on ATE and ATT are again essentially uninformative under Assumption 4. The estimated bounds under Assumption 5 yield a lower bound equal to zero (by assumption) and narrow the upper bound. Under the weak mean monotonicity assumption across strata in which never takers are the healthiest group (Assumption 6'), the majority of the estimated bounds for these outcomes are contained in the positive region. And, while the estimated bounds under the combination of Assumption 5 and Assumption 6 are considerably narrower than those under Assumption 4, the estimated lower bound remains at zero, only ruling out large effects. In turn, the estimated bounds on the local net effects for never takers and always takers do not rule out a zero effect. Lastly, for these self-reported health status outcomes, all the point estimates of $LATE_c$ under the exclusion restriction fall within the bounds of $LATE_c$ under Assumption 5 and Assumption 6'. Thus, for these self-reported outcomes, it is plausible that the exclusion restriction of the lottery IV and the weak monotonicity of mean outcomes assumptions are all simultaneously valid.

3.3.3 Alleviation of Financial Strain Outcomes

Table 13 presents the estimated bounds on average effects for the outcomes related to the alleviation of financial strain. As before, the estimated bounds on ATE and ATT using only Assumption 4 are wide and largely uninformative. The monotonicity assumption within strata (Assumption 5) substantially shrinks the width of the estimated bounds relative to those under Assumption 4, restricting the estimated bounds to non-negative identification regions. Under the weak mean monotonicity assumption across strata in which never takers are on average more likely to suffer financial strain due to medical expenses (Assumption 6), the estimated bounds are narrower compared with those under Assumption 4, but still wide. The combination of Assumption 5 and Assumption 6 produces informative estimated bounds that lie in the non-negative region. For example, according to the estimated bounds, Medicaid health insurance coverage increases the probability of not currently owing for medical expenses for the target population (ATE) by no more than 0.019 percentage points, and for the treated individuals in the target population (ATT) by no more than 0.032 percentage points.

Regarding the estimated bounds on $LNATE_{nt}$ in Table 13, with the exception of no out of pocket medical expenses, all other estimated bounds under Assumption 6 lie in positive regions. For instance, according to these estimated bounds, the lottery increases the probability of not currently owing for medical expenses for never takers by between 0.035 and 0.331 percentage points, with a 95% confidence interval that excludes zero. Also, the lottery increases the probability of not being refused treatment due to medical debt by between 0.008 and 0.048 percentage points, with a 90% confidence interval that excludes zero ($[.0004, .058]$). In addition, looking at the estimated bounds on $LNATE_{at}$ under Assumption 6, the lottery increases the probability of all measures of financial strain alleviation for always takers, with their 95% confidence intervals excluding zero except for not being refused treatment due to medical debt. Under the validity of Assumption 6, this evidence based on estimated bounds for the net effects for never takers and always takers would indicate that the lottery may violate the exclusion restriction for outcomes related to the alleviation of financial strain. Finally, the point estimates of $LATE_c$ under the exclusion restriction for the four measures of alleviation of financial strain fall outside the estimated bounds of $LATE_c$, both under Assumption 6 and under the combination of Assumption 5 and Assumption 6. Estimated bounds on the difference between the point estimate and the bounds on $LATE_c$ and their corresponding 95% confidence intervals are entirely in the negative region, with the exception of the confidence interval for not being refused treatment due to medical debt.[15] As before, if one is willing to

[15]However, the 90% confidence interval for the difference between the point estimate and the bounds on $LATE_c$ for this last outcome excludes zero.

maintain Assumption 6 or Assumption 5 and Assumption 6, this evidence can be interpreted as indicating that the point estimates of $LATE_c$ that assume the validity of the lottery IV might be upward-biased for these outcomes.

3.3.4 A Comparison with Bounds that Assume the Validity of the Exclusion Restriction

To end this section, we compare the previous results with estimated bounds on average effects when the lottery IV is assumed to satisfy the exclusion restriction. Recall that under the validity of the exclusion restriction, the $LATE_c$ is point identified, but other average effects such as ATE and ATT have to be bounded.[16] We offer this comparison as a way of illustrating the (partial) identification power that the exclusion restriction has when it may be valid. In addition, if the exclusion restriction is satisfied in the OHIE, the estimated bounds on ATE and ATT under its validity represent new evidence on the effect of Medicaid on the target population. The estimated bounds are presented in Tables A1 to A5 in the Internet Appendix.

Table A1 shows estimated bounds on health care utilization outcomes. Compared with the bounds in Table 9, the width of the estimated bounds on ATE and ATT shrinks substantially under the exclusion restriction and most of the estimated bounds in Table A1 are contained inside the corresponding estimated bounds in Table 9. Furthermore, most of the estimated bounds on ATE and ATT under the exclusion restriction identify positive effects under the corresponding combined assumptions. Table A2 presents estimated bounds on preventive care outcomes. Generally, the width of the estimated bounds shrinks substantially under the exclusion restriction, and the majority of their estimated identification regions overlap with the ones in Table 10 (with the exception of the outcome blood cholesterol checked). Table A3 and Table A4 show that the estimated bounds on ATE and ATT on self-reported health status are similar to those without the exclusion restriction in Table 11 and Table 12, respectively, but the former set of estimated bounds identifies positive effects without assuming their signs. Table A5 presents the estimated bounds that assume the validity of the exclusion restriction on the alleviation of financial strain. The estimated bounds on ATE and ATT overlap with the ones in Table 13 for the outcomes no out of pocket medical expenses and not borrowing money to pay medical bills, while the estimated bounds for not currently owing for medical expenses and not being refused treatment due to medical debt are to the right of those in Table 13. Overall, the comparison of estimated bounds on ATE and ATT in OHIE that impose and do not impose the validity of the exclusion restriction underscore the identification power brought about

[16]We follow the methodology in Chen et al. (2018) to calculate the bounds on ATE and ATT, Assuming the exclusion restriction, Assumption 5 herein would reduce to the Assumption 6 in that paper, and Assumption 6 herein would reduce to Assumption 7c in that paper (with the direction reversed). As a result, the bounds here reduce to the bounds under the validity of the exclusion restriction because the equations to derive our bounds simplify to those under the validity of the exclusion restriction.

by that assumption. For the most part, the estimated bounds under the exclusion restriction assumption are substantially narrower relative to those not imposing it.

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 instrumental variable (IV) that may violate the exclusion restriction. The construction of our bounds rests on 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 latent subpopulations 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, a non-zero net effect implies the violation of the exclusion restriction. We propose the use of two weak monotonicity assumptions relating mean potential outcomes within and across different principal strata to partially identify the ATE and ATT. These weak monotonicity assumptions are employed in addition to the basic "$LATE$" assumptions within the framework of Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996). The direction of the two weak monotonicity assumptions can be modified according to the specific empirical setting at hand and the available (economic) theory behind it. Some of these assumptions also provide testable inequalities on pointed identified average outcomes, which can be used to falsify the assumptions. Furthermore, our bounds on the local net effects for non-compliers whose treatment status are not affected by the instrument (i.e., never takers and always takers) provide a straightforward test for the exclusion restriction assumption. In principle, the methods developed herein can be extended to the context where the potentially invalid IV is random conditional on covariates, which is often necessary in observational studies. This extension is at the top of our research agenda.

In a substantive application of our nonparametric bounds, we employ the public-use data to re-analyze results from the Oregon Health Insurance Experiment (OHIE). The OHIE sparked an influential series of studies with the goal of estimating the effects of Medicaid health insurance coverage on a myriad of outcomes (e.g., Finkelstein, 2012; Baicker et al, 2014). The cornerstone of OHIE is that Medicaid health insurance coverage was randomly assigned through a lottery. However, two features complicate estimation of the effects of interest. First, there is noncompliance: about 30% of those offered Medicaid health insurance through winning the lottery took it up. For this reason, studies within the OHIE employ the lottery as an IV for actual Medicaid coverage, thereby estimating the local average treatment effect on compliers ($LATE_c$) as opposed to the ATE or ATT on the target population. Second, it is possible that the lottery IV has an effect on the outcomes that is independent from the effect that works through actual Medicaid health coverage (the treatment). One reason for this is the documented feature that caseworkers in Oregon aided lottery applicants to sign up for other public programs (e.g., SNAP and TANF) which presumably have an impact on their own on the outcomes (e.g., Finkelstein, 2012; Baicker et al, 2014). This second feature could render the exclusion restriction assumption invalid, potentially casting doubt on the results.

We contribute to the literature on OHIE by employing our nonparametric bounds to provide inference on the ATE and ATT (and other local effects) of Medicaid health insurance coverage on health care and preventive care utilization, self-reported health status, and the alleviation of financial strain. Our main findings are as follows. First, Our estimated bounds on ATE and ATT are informative under the two sets of weak monotonicity assumptions we use, although they do not generally exclude zero. Second, we find several outcomes for which the estimated bounds on the direct (or net) effect of the lottery IV on the outcome for never-takers or always-takers exclude zero, as well as the corresponding confidence intervals. Given that these bounds employ one of the weak monotonicity assumptions of mean outcomes that do not impose the sign of the effects, the interpretation is that, for these outcomes, either the exclusion restriction or the weak monotonicity assumption fails. Third, we also find that for several outcomes the point estimated $LATE_c$ imposing the exclusion restriction assumption in the original OHIE often falls outside the estimated bounds on $LATE_c$ that do not impose that assumption, but that impose the two weak monotonicity assumptions of mean outcomes. As before, this implies that either the exclusion restriction or the weak monotonicity assumptions fail. If the weak monotonicity assumptions hold—which is supported by the indirect evidence provided in this paper—the implication is that the exclusion restriction fails and the point estimates are upward biased (since they fall above the estimated upper bound). Lastly, we document that the exclusion restriction contains considerable identification power, as estimated bounds on ATE and ATT derived by imposing the exclusion restriction are substantially narrower relative to our bounds that do not impose that assumption. To close, it is important to point out that, despite our re-analysis of the OHIE for the outcomes available in the public-use data potentially implying upward biases in the original point estimates on $LATE_c$ in Finkelstein et al. (2012), those biases would not seem to reverse the main qualitative conclusions for most of the outcomes.

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Table 2: 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 SNAP	0	.547	.550	-.003 (.008)
SNAP benefits	0	1,136.3	1,123.6	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	7,968.1	7,560.0	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	23,741	11,808	11,933	

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 3: Estimated Effects of the Lottery (*ITT* Effects) on 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)**
Alleviation of 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 SNAP	0	.027 (.005)**
Other types of insurance	.016	-.007 (.005)	SNAP benefits	0	96.99 (46.48)**
Number of months on insurance	.012	1.140 (.043)**			
Observations	23,741			23,741	

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 4: Estimates of Stratum Proportions

	Main Sample	Women≥40	Women
π_{nt}	.576**	.587**	.559**
	(.006)	(.008)	(.007)
π_c	.289**	.296**	.281**
	(.007)	(.010)	(.008)
π_{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	23,741	8,274	14,086

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 5: Point Estimates of Average Health Care and Preventive Care Utilization

	Health Care Utilization				Preventive Care			
	Prescription Drugs		Outpatient Visits		Blood Cholesterol Checked	Blood Tested for High Blood Sugar/Diabetes	Mammogram (Women $\geq$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								
$E[Y(1)\|at]=\overline{Y}^{01}$	.775**	3.208**	.794**	3.392**	.636**	.695**	.523**	.599**
	(.013)	(.101)	(.012)	(.130)	(.015)	(.014)	(.027)	(.018)
$E[Y(0)\|nt]=\overline{Y}^{10}$	.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)
$\overline{Y}^{11}-E[Y(0)\|nt]$	.123**	.821**	.174**	1.090**	.060**	.074**	.144**	.139**
	(.011)	(.074)	(.010)	(.084)	(.010)	(.010)	(.018)	(.014)
$E[Y(1)\|at]-\overline{Y}^{00}$	.172**	1.098**	.257**	1.734**	.015	.105**	.251**	.231**
	(.015)	(.107)	(.013)	(.134)	(.016)	(.015)	(.028)	(.019)

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 6: Point Estimates of Average Self-Reported Health

	Binary Outcomes				# of Days (out of last 30 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							
$E[Y(1)\|at]=\overline{Y}^{01}$	.524** (.015)	.827** (.012)	.708** (.013)	.626** (.014)	18.73** (.331)	17.77** (.345)	19.56** (.334)
$E[Y(0)\|nt]=\overline{Y}^{10}$	.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)
$\overline{Y}^{11}-E[Y(0)\|nt]$	-.018 (.011)	-.017* (.007)	-.022* (.009)	-.067** (.010)	-1.489** (.248)	-1.431** (.264)	-2.249** (.231)
$E[Y(1)\|at]-\overline{Y}^{00}$	-.033* (.017)	-.038** (.012)	-.009 (.014)	-.056** (.015)	-1.992** (.358)	-1.266** (.375)	-2.848** (.357)

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 7: Point Estimates on the Alleviation of 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				
$E[Y(1)\|at]=\overline{Y}^{01}$	.502**	.401**	.701**	.925**
	(.015)	(.015)	(.014)	(.007)
$E[Y(1)\|nt]=\overline{Y}^{10}$	.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)
$\overline{Y}^{11}-E[Y(0)\|nt]$	.170**	.038**	.116**	.006
	(.011)	(.011)	(.010)	(.006)
$E[Y(1)\|at]-\overline{Y}^{00}$	.061**	-.009	.074**	.006
	(.016)	(.016)	(.015)	(.008)

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 8: Average Baseline Characteristics by Strata (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 SNAP	.431**	.695**	.746**	-.264**	-.051*	-.315**
	(.006)	(.011)	(.013)	(.015)	(.020)	(.014)
Total benefits from SNAP	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 8 (Con't): Average Baseline Characteristics by Strata (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)

Note: * and ** denote statistical significance at the 10% and 5% level, respectively.

Table 9: Estimated Bounds on 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)	

Note: In parentheses are 95 percent confidence intervals.

Table 10: Estimated Bounds on Average Effects on Preventive Care

	Blood Cholesterol Checked		Blood Tested for High Blood Sugar/Diabetes		Mammogram (Women $\geq$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 11: Estimated Bounds on 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 12: Estimated Bounds on 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 13: Estimated Bounds on Average Effects on the Alleviation of 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)	