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Essays on the identification of treatment effects with applications to the labor market

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Essays on the identification of treatment effects with applications to the labor market

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DEDICATION

I would like to dedicate this dissertation to my parents, without whom this would not have been possible, and to my grandfather who is always remembered. I would also like to thank my friends and classmates who made my time at Iowa State memorable.

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ABSTRACT

This dissertation contains three independent essays; each essay can be read in isolation. The first essay investigates the causal effect of criminal convictions on various labor market outcomes in young adults. The estimation method used is a nonparametric bounding approach intended to partially identify the causal effect. The data used for this essay comes from the 1997 National Longitudinal Survey of the Youth. The second essay reevaluates the causal effect of post-secondary schooling on unemployment incidence using historical data from the 1980 U.S. Census and information on cohort level Vietnam War conscription risk. Conscription risk is used as an instrument for endogenous post-secondary schooling in a specification that accounts for the discrete nature of the treatment and outcome of interest. The third essay investigates the underlying necessary assumptions needed for the monotone instrumental variable (MIV) assumption to have identifying power on both the upper and lower bounds of a treatment effect when the treatment of interest is binary. I show that if the treatment is monotonic in the instrument, as is routinely assumed in the literature on instrumental variables, then for the MIV to have identifying power on both the lower and upper bounds of the treatment effect, the conditional-on-received-treatment outcomes cannot exhibit the same monotonicity assumed by the MIV. Results are highlighted with an application investigating the effect of criminal convictions on job match quality using data from the 1997 National Longitudinal Survey of the Youth.

CHAPTER 1. The Causal Effects of Criminal Convictions on Labor Market Outcomes in Young Adults: A Nonparametric Bounds Analysis

1.1 Abstract

This paper examines the causal effects of criminal convictions on labor market outcomes in young adults using data from the National Longitudinal Survey of Youth (NLSY) 1997 cohort. Unlike previous research in this area which relies on strong assumptions to obtain point identification, this paper imposes relatively weak nonparametric assumptions that provide tight bounds on treatment effects. Within a potential outcomes framework, this identification approach is compared and contrasted with the standard Heckman Two-Step approach. The sign of the average treatment effects can be identified even in the absence of a parametric model. In particular, I estimate that criminal convictions reduce both black and white mens' earnings by at least 2.7% a year. The lower bound of the causal effect for minority women is estimated to be slightly higher at 3.8%. Upper bounds range from 11% for white men to 30% for black men. Having a conviction is estimated to lower hourly wages by at least 1% to 2%. For white males, a criminal conviction is estimated to reduce tenure length (as a measure of match quality) between 1% and 10% and estimated to increase the risk of unemployment by between 0.6% and 2.3%. Results of several sensitivity analyses provide some evidence that these estimates are robust.

1.2 Introduction

In April of 2011, the city of Philadelphia enacted a "ban the box" ordinance making it illegal for employers to inquire into applicants' criminal histories on initial job applications. Four states have similar state-wide measures: New Mexico, Connecticut, Hawaii, and Minnesota. In the same year, the US Department of Labor released nearly $12 million to 10 organizations to provide adult offenders with job market assistance. Motivating these measures is the conventional wisdom that individuals with criminal records face unique difficulties in the labor market. One statistic that might stand as evidence of the existence of these difficulties is the negative relationship between criminal convictions and average earnings. But since an association does not imply causation, and to some extent convictions may simply be a mark of individuals with poor labor market skills, the evidentiary value of this statistic is questionable.

All of the existing literature on criminal convictions and labor market outcomes is devoted to finding assumptions strong enough to point identify causal parameters.[1] In practice, however, these identifying assumptions may be implausible. This paper applies a partial identification approach to the problem that derives its power from relatively weaker assumptions than those typically imposed. Within a potential outcomes framework, this identification approach is compared and contrasted with the standard Heckman Two-Step approach. Though point identification of the causal parameters is not obtained, informative identification regions emerge. In particular, I estimate identification regions for two causal effects predicted by a search model of employment (Black 1995): the causal effect of criminal convictions on hourly wages and match quality (measured as tenure). The effect on match quality has previously been ignored in the literature.

The sheer number of people affected marks the link between convictions and employment outcomes as an area that warrants attention. In 2009, nearly 7.2 million adults, or 3.1% of the adult population, were incarcerated, on parole, or on probation (Glaze 2010). Figures are even more stark for black men. The average black man has greater than a 1 in 4 chance of spending time in state or federal prison (Bonczar and Beck 1997). These figures are significantly higher

[1] See Holzer (2007) for a survey of the existing literature on the relation between criminal convictions and labor markets.

than they were several decades ago - the correctional population has quadrupled in the last 30 years - and this trend has been overwhelmingly concentrated among young, less educated men (Western, Kling, Weiman 2001). If convictions do in fact have a stigmatizing effect on offenders that limits their future opportunities for legal work, this could lead to lower employment rates and lower average earnings. Given the concentration of convictions in young, minority men with low levels of eduction, this stigmatizing effect would work to further hinder a group already disadvantaged in the labor market.

Studies of the demand side of the labor market reveal that many employers are averse to hiring individuals with criminal records. Employer surveys indicate only about 40% would “definitely” or “probably” hire individuals with criminal records, and employer audits show applicants with criminal records can expect at least 50% fewer “call backs” (Holzer 2007). Such an aversion among employers, when set in a equilibrium search model of employment, leads to several predictions (Black 1995). Individuals with criminal records can be expected to have lower earnings and lower job match qualities. An important question then is whether the market actually produces these effects, and if so, their magnitudes.

This paper uses data on young adults from the 1997 National Longitudinal Survey of Youth (NLSY), a newer cohort of individuals than used in most of the literature. The aim of the paper is to estimate an identification region for the effect of criminal convictions on wages and match quality. I also estimate identification regions for two additional causal effects of interest in the literature: the effect of criminal convictions on yearly income and unemployment probability. In Section 2, I articulate the identification problem within the potential outcomes framework, noting the pivotal role of various assumptions in identification, and highlight the similarities and differences of an approach stemming from work by Manski (1997) and Manski and Pepper (2000) to that of the standard Heckman Two-Step approach. Section 3 discusses in detail assumptions used in this analysis. Section 4 introduces the data, estimation methods, results, and sensitivity analysis. Section 5 concludes.

1.3 Treatment Effects and Identification

Causal effects are common subjects of interest in a wide range of fields. Some recent topics include the effect of job training and education on earnings (Lee 2009, Manski and Pepper 2000) and the effect of welfare programs on employment (Grogger 2003). When the impact variable is dichotomous, as in the present setting, it is convention to refer to the causal effect as a treatment effect. The potential outcomes framework presented below provides an intuitive setting in which to analyze questions of this sort.

Following Manski (1990), I define the population as a measure space (J,Ω,P) of agents, with P denoting a probability measure over the set J of individuals in the population. Each individual j in the population J is characterized by covariates $x_j \in X$ and response function $y_j(\cdot) : T \to Y$ mapping mutually exclusive and exhaustive treatments $t \in T$ into outcomes $y_j(t) \in Y$. The treatment is dichotomous: being convicted of a crime or not. Each individual j has a realized treatment $z_j \in T$ and a realized outcome $y_j \equiv y_j(z_j)$ which are both observed. Latent outcomes $y_j(t)$, $t \neq z_j$ are not observed. Given this setup, $P[x, y(\cdot), z, y]$ characterizes the distribution of covariates, response functions, realized treatments and realized outcomes. Each individual has an unobserved latent outcome and an observed realized outcome. This duality is where the term potential outcome stems. Either outcome is potentially possible, though only one is realized.

The treatment effect is defined as $y(1)-y(0)$. This leads to what Holland (1986) termed the *Fundamental Problem of Causal Inference*. The values of $y(1)$ and $y(0)$ are not both observed for the same individual and therefore neither is the treatment effect for any individual. A researcher only observes data assumed representative of the distribution $P[x, z, y]$ of covariates, realized treatments, and realized outcomes. The goal then becomes to combine this empirical evidence with prior knowledge and assumptions to learn about the distribution of the treatment effect.

A distributional characteristic of usual interest is the average treatment effect (ATE):

$$\text{ATE} = E[y(1) - y(0)|x] = E[y(1)|x] - E[y(0)|x]. \tag{1.1}$$

The ATE is defined as the expected treatment effect if treatment were randomly assigned to the population. If interest is in the ATE, what is problematic is that neither $E[y(1)|x]$ nor $E[y(0)|x]$ is observed, but rather $E[y(1)|x, z = 1]$ and $E[y(0)|x, z = 0]$. In the present setting, these observed values might represent expected earnings under a conviction treatment for those who actually received the conviction treatment and expected earnings under a non-convicted treatment for those who actually received that treatment.

Because of this missing information, scientific experiments are held by some to be the gold standard of data generation. In particular, through randomization, they guarantee exogenous assignment. This would imply that the outcome variable under treatment t is mean independent of the actual treatment received: $E[y(t)|x, z = t] = E[y(t)|x, z = t'] = E[y(t)|x]$. If exogenous assignment holds, then a statistical solution to the unobservable causal effect issue presents itself and the ATE is identified (Holland, 1986). The ATE can then be estimated as the observed mean difference in y between the two groups. It is termed a statistical solution because the assignment assumption has identifying power due to the fact that the focus is on averages. The average effect can be identified even though the effect is not observed for any particular individual.

In some instances, exogenous assignment might hold even in the absence of true experimental randomization, and is often imposed implicitly by investigators. In the majority of social science settings where randomization is not explicit in the data generation, however, exogenous assignment is often an untenable assumption regarding the relationship between the treatment assignment mechanism and the outcome generating mechanism. Assuming exogenous assignment in the present setting would imply unobserved characteristics affecting labor market outcomes for those convicted of a crime are no different from those not convicted of a crime; if those convicted of a crime had hypothetically not been convicted, their expected outcomes would be no different from those who had actually not been convicted. Generally this assumption is not credible in social science applications such as the current study, and identifying the ATE requires further assumptions.

To see where further assumptions are necessary, we can rewrite $E[y(t)|x]$ using the law of iterated expectations:

$$E[y(t)|x] \quad = \quad E[y(t)|x, z = t]P(z = t|x) + E[y(t)|x, z = t']P(z = t'|x). \tag{1.2}$$

The data identify sample analogues of all of the right hand side quantities except the counterfactual $E[y(t)|x, z = t']$. This might represent expected income under a conviction treatment for those who actually received the non-conviction treatment. The data bring us part of the way towards identifying the ATE, but the remaining distance must be covered by credible assumptions.

By rearranging terms in Equations (2) and using the fact that $P(z = t'|x) = [1-P(z = t|x)]$, the quantities can be rewritten in the following form:

$$E[y(t)|x] \quad = \quad E[y(t)|x, z = t] \quad + \quad \Psi_t \tag{1.3}$$

where

$$\Psi_t \quad = \quad P(z = t'|x)\big\{E[y(t)|x, z = t'] - E[y(t)|x, z = t]\big\}. \tag{1.4}$$

The quantity on the left hand side of Equation (3) is of interest (expected outcomes under a specific treatment); the first quantity on the right hand side is identified by the data (expected outcomes under a treatment for those who actually received that treatment), so the problem is essentially one of identifying Ψ_t.

One well known approach to this identification problem, the Heckman Two-Step, begins with a linear framework. The selection and outcome mechanism are assumed to take the following form:

$$Y_t \quad = \quad X\beta + \epsilon_t \tag{1.5}$$

$$T^* \quad = \quad X\delta + \upsilon \tag{1.6}$$

where Y_t is the outcome, T^* is the latent treatment, ϵ_t and υ are unobserved disturbance terms, and Y_t is only observed when $T^* \geq 0$, implying treatment is received. Heckman (1979) noted that the population regression function for Equation (5) under the standard assumption $E[\epsilon_t|x] = 0$ can be written as

$$E[y(t)|x] = X\beta.$$

The regression function for the subsample for which data are available then takes the form

$$E[y(t)|x, z = t] = X\beta + E[\epsilon_t|x, T^* \geq 0].$$

This parallels Equation (3) with

$$\Psi_t = -E(\epsilon_t|x, \upsilon \geq -X\delta).$$

In general, $\Psi_t \neq 0$, so a simple regression of Equation (5) with available data would lead to biased estimates for β, and $E[y(t)|x]$ would not be correctly identified. Heckman's insight was that this can be viewed as a missing variable problem, and, if the disturbance terms ϵ_t and υ are distributed jointly normal, then Ψ_t can be characterized as the product of that missing variable and its regression parameter. Specifically:

$$\Psi_t = -E(\epsilon_t|x, \upsilon \geq -X\delta) = -\frac{\sigma_{\epsilon_t}}{\sigma_\upsilon}\frac{\phi(-X\delta/\sigma_{\epsilon_t})}{\Phi(X\delta/\sigma_{\epsilon_t})} = -\frac{\sigma_{\epsilon_t}}{\sigma_\upsilon}\lambda$$

where ϕ is the pdf of the standard normal distribution and Φ is the standard normal cdf. Including λ (commonly referred to as the inverse Mills ratio) along with X as a regressor in Equation (5), and estimating $\frac{\sigma_{\epsilon_t}}{\sigma_\upsilon}$ as a parameter, one can consistently estimate the parameters β and so correctly identify $E[y(t)|x]$.

This approach, convenient as it is, has been found to be very sensitive to distributional assumptions. Furthermore, although the model is formally identified without an exclusion restriction (a variable relevant in the selection equation but not the outcome equation), the importance of valid exclusion restrictions for this model is well documented (Li, Poirier and Tobias 2004, Altonji, Elder and Taber 2005). Unfortunately, valid candidates for exclusion restrictions appear elusive in the current setting. Moreover, imposing distributional assumptions when it is not entirely clear what the disturbance terms truly represent does not appear ideal.

This paper uses an alternative approach to identify the causal effects of criminal convictions on labor market outcomes than is typically used in the literature. The approach taken here is one from a field of growing popularity beginning with Manski and others. Rather than resting on structural assumptions to point identify the ATE, a mixture of assumptions based on economic theory are invoked to partially identify the ATE. In particular, some assumptions regarding the response function and selection mechanism are invoked to bound Ψ_t and therefore the ATE. In addition to these assumptions, a weakened form of an exclusion restriction is also adopted to tighten the bounds on the ATE. Beyond simply relying on a different set of assumptions than those typically imposed, this approach also allows the treatment effect to be heterogeneous in the population.

The focus on partial identification rather than on traditional point identification stems from what Manski has termed *The Law of Decreasing Credibility*. This law maintains that the credibility of results decreases with the strength of assumptions maintained (Manski, 2003). This is not to imply assumption routinely used in more structural settings have no place in economics. Rather, the tools in an econometricians toolkit are wide ranging and vary greatly in their strengths and weaknesses. The setting within which one analyzes the identification issue will dictate the applicable assumptions that might be brought to bear. The main results of this paper emerge from the imposition of three assumptions: mean monotone treatment response (MMTR), monotone treatment selection (MTS), and monotone instrumental variable (MIV). These assumptions are explained in full in the next section.

1.4 Assumptions and Their Identifying Power

1.4.1 Worst Case Bounds

Even if a researcher is not willing to impose any assumptions on the response function or selection mechanism, it is still possible to bound the treatment effect if the support of the outcome variable is bounded (Manski, 1989). Viewing the problem through Equations (3) and (4), Ψ_t is not observed because of the counterfactuals which define it. Though these are not observed, they can be bounded if Y has a bounded outcome space. Let $E[y(t)|x, z = (t')] \in [K_l, K_u]$.[2] Note that when Y is binary, these expectations can be viewed as probabilities which necessarily lie between 0 and 1 implying the natural values $K_l = 0$ and $K_u = 1$. When Y is continuous, the researcher may choose finite values for these parameters based, for example, on the range of the data or relevant prior knowledge. Imposing a bounded outcome space leads

[2] In general these bounds could vary with t, though in this application these values are assumed uniform across treatments.

to bounds on Ψ_t leading in turn to *worst case bounds* on the unknowns $E[y(t)|x]$:

$$LB_t \leq E[y(t)|x] \leq UB_t \tag{1.7}$$

where

$$\begin{aligned} LB_t &= E[y(t)|x, z = t] + \Psi_t^l \\ UB_t &= E[y(t)|x, z = t] + \Psi_t^u \end{aligned}$$

and

$$\begin{aligned} \Psi_t^l &= P(z = t'|x)\big\{K_l - E[y(t)|x, z = t]\big\} \\ \Psi_t^u &= P(z = t'|x)\big\{K_u - E[y(t)|x, z = t]\big\}. \end{aligned}$$

Applying these results to Equation (1) lead to worst case bounds on the average treatment effect:

$$LB_1 - UB_0 \leq ATE \leq UB_1 - LB_0. \tag{1.8}$$

These bounds are sharp in that they cannot be improved upon without additional assumptions. Worst case bounds tend to be limited in the information they convey because they necessarily include zero. More informative bounds on the ATE require further assumptions. I therefore impose Monotone Treatment Selection (MTS), Monotone Treatment Response (MTR), and Monotone Instrumental Variable (MIV) assumptions to derive more informative bounds.

1.4.2 Monotone Treatment Selection

As previously discussed, an exogenous selection assumption would not be credible in the present setting. However, a weaker Monotone Treatment Selection assumption (Manski and Pepper 2000) does seem credible:

MTS Assumption: *Let T be ordered. For each $t \in T$, each $x \in X$ and all $(u_0, u_1) \in T \times T$ such that $u_1 \geq u_0$,*

$$E[y(t)|x, z = u_1] \geq E[y(t)|x, z = u_0]. \tag{1.9}$$

MTS assumes a characteristic concerning the relationship between the selection process and the outcome process. Specifically, MTS presumes, for example, that those with a 'lower' realized treatment (criminal conviction) exhibit characteristics that would lead them to have no greater expected incomes under either potential treatment than those with a 'higher' realized treatment (not convicted) under that same potential treatment. This is precisely why standard regression methods would be considered suspect in the current setting.

Implementing MTS reduces the upper bound of Ψ_1 and raises the lower bound on Ψ_0. To impose this assumption, replace K_u in the upper bound on Ψ_1 with $E[y(1)|x, z = 1]$ and replace K_l in the lower bound on Ψ_0 with $E[y(0)|x, z = 0]$. Specifically:

Let MTS hold. Then:

$$
\begin{gathered}
P(z = 0|x)\big\{K_l - E[y(1)|x, z = 1]\big\} \\
\le \Psi_1 \le \\
\underbrace{P(z = 0|x)\big\{E[y(1)|x, z = 1] - E[y(1)|x, z = 1]\big\}}_{=0}
\end{gathered} \tag{1.10}
$$

and

$$
\begin{gathered}
\underbrace{P(z = 1|x)\big\{E[y(0)|x, z = 0] - E[y(0)|x, z = 0]\big\}}_{=0} \\
\le \Psi_0 \le \\
P(z = 1|x)\big\{K_u - E[y(0)|x, z = 0]\big\}.
\end{gathered} \tag{1.11}
$$

These bounds on Ψ_t lead to bounds on $E[y(t)|x]$ which lead to tighter bounds on the ATE by lowering the upper bound on the ATE. The lower bound is unchanged relative to the worst case bounds.

Let MTS hold. Then:

$$
\begin{gathered}
\Big[E[y(1)|x, z = 1] + P(z = 0|x)\big\{K_l - E[y(1)|x, z = 1]\big\}\Big] - \\
\Big[E[y(0)|x, z = 0] + P(z = 1|x)\big\{K_u - E[y(0)|x, z = 0]\big\}\Big] \\
\le ATE \le \\
E[y(1)|x, z = 1] - E[y(0)|x, z = 0].
\end{gathered} \tag{1.12}
$$

1.4.3 Monotone Treatment Response

The Monotone Treatment Response (Manski 1997) assumption specifies a relationship between $y(1)$ and $y(0)$. It maintains that if treatments have some natural ordering then outcomes vary monotonically with them.

MTR Assumption: *Let T be ordered. For each $j \in J$*

$$t_1 \geq t_0 \Rightarrow y_j(t_1) \geq y_j(t_0). \tag{1.13}$$

In the present study, this assumption implies, for example, that yearly income for each individual will be no greater if convicted of a crime than if not convicted. MTR also implies a weaker variant:

Mean MTR (MMTR):

$$E[y(1)|x,z] \geq E[y(0)|x,z]. \tag{1.14}$$

This follows from MTR by definition of the expectation function. In the current application, only the weaker assumption of MMTR will be implemented. Under this assumption, expected incomes would be no greater for the population under a conviction treatment than under a non-conviction treatment. This, though admittedly a strong assumption, seems reasonable in the current setting. Furthermore, as the goal is to identify a region of consensus as to the magnitude of the treatment effects, results stemming from a relaxation of this assumption are given in the sensitivity analysis section to hopefully strengthen the credibility of results.

MMTR raises the lower bound on Ψ_1 and lowers the upper bound on Ψ_0. To implement this assumption, replace K_l in the lower bound on Ψ_1 with $E[y(0)|x,z=0]$ and K_u in the upper bound on Ψ_0 with $E[y(1)|x,z=1]$. Specifically

Let MMTR hold. Then:

$$P(z=0|x)\big\{E[y(0)|x,z=0] - E[y(1)|x,z=1]\big\}$$

$$\leq \Psi_1 \leq \tag{1.15}$$

$$P(z=0|x)\big\{K_u - E[y(1)|x,z=1]\big\}$$

and

$$P(z=1|x)\{K_l - E[y(0)|x, z=0]\}$$
$$\leq \Psi_0 \leq \tag{1.16}$$
$$P(z=1|x)\{E[y(1)|x, z=1] - E[y(0)|x, z=0]\}.$$

These new bounds on Ψ_t lead to tighter bounds on $E[y(t)|x]$, which in turn lead to tighter bounds on the ATE by raising the lower bound on the ATE to zero. The upper bound is unchanged.

Let MMTR hold. Then:

$$0$$
$$\leq ATE \leq \tag{1.17}$$
$$\Big[E[y(1)|x, z=1] + P(z=0|x)\{K_u - E[y(1)|x, z=1]\}\Big] -$$
$$\Big[E[y(0)|x, z=0] + P(z=1|x)\{K_l - E[y(0)|x, z=0]\}\Big]$$

Combining MTS and MMTR lead to bounds on the ATE with very simple forms. Specifically: *Let MTS and MMTR hold. Then:*

$$0$$
$$\leq ATE \leq \tag{1.18}$$
$$E[y(1)|x, z=1] - E[y(0)|x, z=0].$$

Though the worst case bounds depend on the imposed bounded support on expected outcomes (K_l, K_u), bounds stemming from the imposition of MMTR and MTS do not.

The imposition of the joint MMTR and MTS assumptions can have significant identification power and directly relate to the response and selection process. They both help to partially identify Ψ_t as an alternative to the Heckman Two-Step approach. In what follows, monotone instrumental variables (MIV) brings to bear a different type of assumption that, when invoked along with MMTR and MTS, can further tighten the identification region.

1.4.4 Monotone Instrumental Variables

The method of instrumental variables is widely used in the evaluation of treatment effects. It is routinely employed when selection into treatment is believed to be affected by unobserved factors that also affect outcomes. In its standard form, the instrument, denoted v, is leveraged as a source of variation in the data that acts as a surrogate to randomization. Its fulcrum is variation in the treatment stemming from exogenous variation in the instrument. Though standard IV assumptions can aid greatly in identification, the credibility of the instrument is often a matter of disagreement, specifically whether the exclusion restriction is a valid assumption. This provides motivation for considering weaker, and thus more credible, assumptions to aid identification. First, consider a *mean independence* form of the standard IV condition:

IV Assumption *Covariate v is an instrumental variable if, for each $t \in T$, each value of x, and all $(u, u') \in (V \times V)$,*

$$E[y(t)|x, v = u'] = E[y(t)|x, v = u].$$

A Monotone Instrumental Variable (Manski and Pepper, 2000) assumption weakens this IV condition by replacing the equality with an inequality:

MIV Assumption *Let V be an ordered set. Covariate v is a monotone instrumental variable if, for each $t \in T$, each value of x, and all $(u, u') \in (V \times V)$ such that $u_2 \geq u_1$,*

$$E[y(t)|x, v = u_2] \geq E[y(t)|x, v = u_1].$$

To have identifying power, the pair (y, z) cannot be statistically independent of the instrument v. Thus a trivial randomized covariate lends no leverage, though the assumption would hold by construction. This is in essence the familiar rank, or covariance, condition in conventional IV (Manski, 2003). Manski and Pepper (2000) note that MTS is itself a MIV assumption, though one that takes a particular special form in which the received treatment is the instrument.

A good example that highlights the difference in MIV and standard IV would be in the literature concerning the effect of education on earnings. There is reason to treat education as an endogenous regressor in an earnings equation. The IV solution then demands a valid instrument. It is difficult to find a variable correlated to education, $cov(v,t) \neq 0$, but conditionally mean independent of earnings, $E[y(t)|z = t, v] = E[y(t)|z = t]$. More generally, the validity of any chosen IV is bound to be questioned. But the search for a valid MIV is not nearly as difficult and the choice of instrument is likely to have a larger consensus. In their paper introducing MIV, Manski and Pepper (2000) propose using respondents' scores on the Armed Forces Qualifying Test (AFQT) as an MIV. This assumption maintains that schooling is correlated with test scores and that expected incomes weakly rise with increased test scores. Though AFQT score certainly would not hold as an IV, its credibility as an MIV seems sound.

In what follows, the instrument is discrete. The implementation of MIV is straightforward. First, the researcher separates the data according to instrument realizations. Upper and lower bounds are found on $E[y(t)|x, v = u]$ for each realization of the instrument. With slight abuse of notation, let us denote them $UB_t|u$ and $LB_t|u$. Maintaining an MIV assumption would imply that when $u' \leq u$ the lower bound given u cannot be lower than the lower bound for u'. In the test score example of Manski and Pepper, this would mean the lower bound of expected income conditional on a given test score could not be lower than the lower bound of expected income conditional on a lower test score. If the data do not maintain this monotonicity, then it is imposed by raising the lower bound for that higher test score. Similarly, when $u \leq u''$ the upper bound given u cannot exceed the upper bound for u''. If the data do not maintain this monotonicity then it is imposed by lowering the upper bound for that lower test score. Following this procedure, the bounds on $E[y(t)|x]$ when v is an MIV become:

$$\sum_{u \in V} Pr(u)\big[\max_{u' \leq u} LB_t|u'\big] \leq E[y(t)|x] \leq \sum_{u \in V} Pr(u)\big[\min_{u' \geq u} UB_t|u'\big]. \tag{1.19}$$

To combine the MIV assumption with the MMTR and MTS assumption, first conditional upper and lower bounds for $E[y(0)|x, v]$ and $E[y(1)|x, v]$ are calculated for each realization of the instrument with K_l and K_u replaced through the imposition of the MMTR and MTS assumptions. These conditional bounds are then combined as in Equation (19). Specifically:

Let MTS and MMTR hold and v be an MIV. Then:

$$\sum_{u\in V} P(u)\Big\{\max_{u'\le u}\Big[E[y(1)|x,z=1,u'] + P(z=0|x,u')\big\{E[y(0)|x,z=0,u'] - E[y(1)|x,z=1,u']\big\}\Big]\Big\}$$
$$-\sum_{u\in V} P(u)\Big\{\min_{u'\ge u}\Big[E[y(0)|x,z=0,u'] + P(z=1|x,u')\big\{E[y(1)|x,z=1,u'] - E[y(0)|x,z=0,u']\big\}\Big]\Big\}$$
$$\le ATE \le \tag{1.20}$$
$$\sum_{u\in V} P(u)\Big\{\min_{u'\ge u}\Big[E[y(1)|x,z=1,u'] + \underbrace{P(z=0|x,u')\big\{E[y(1)|x,z=1,u'] - E[y(1)|x,z=1,u']\big\}}_{=0}\Big]\Big\}$$
$$-\sum_{u\in V} P(u)\Big\{\max_{u'\le u}\Big[E[y(0)|x,z=0,u'] + \underbrace{P(z=1|x,u')\big\{E[y(0)|x,z=0,u'] - E[y(0)|x,z=0,u']\big\}}_{=0}\Big]\Big\}.$$

The NLSY has a wealth of survey questions that might serve as instruments. Focus is given to youth delinquency and test scores. The delinquency variable is a measure of the degree to which the respondent participated in delinquent activities as a youth and is construction from a list of youth delinquency questions within the questionnaire. The test score comes from the Armed Services Vocational Aptitude Battery (ASVAB) administered between the summer of 1997 and spring of 1998. In treating these variables as MIVs, it is assumed that under either treatment, those with lower instrument levels (high delinquency rate, low test score) have expected outcomes no better than those with higher instrument levels.

An important concern when estimating bounds with MIVs is that analog estimates of such bounds exhibit finite-sample bias which lead the bounds to be narrower (more optimistic) than the true bounds. By Jensen's Inequality, the estimated lower bound on $E[y(t)|x]$ is biased upwards because of the maxima operator and the estimated upper bound is biased downward because of the minima operator. To counter this bias, I implement a correction proposed by Kreider and Pepper (2007). The approach is to estimate the bias by using the bootstrap distribution and then adjust the analogue estimate in accordance with the estimated bias. For a random sample of size N, let LB_N be the analogue estimate of the lower bound in question, and let $E^b(LB_N)$ be the mean of the estimate from the bootstrap distribution (a parallel procedure is used for an upper bound). The bias is then estimated as $E^b(LB_N) - LB_N$. The bias-corrected estimate is then $LB_N - \big[E^b(LB_N) - LB_N\big] = 2LB_N - E^b(LB_N)$. While heuristic and not derived from theory, this correction seems reasonable and performs well in Monte Carlo simulations (Manski and Pepper, 2009).

1.5 Data and Estimation

1.5.1 Data

The data used in this study come from the 1997 cohort of the NLSY. The NSLY-1997 is a nationally representative sample of youths born between 1980 and 1984 with an over-sample of blacks and Hispanics. This paper's population of interest is young adults with at most a high school diploma who are not enrolled in school. Table 1 gives sample sizes for subpopulations corresponding to each outcome of interest. Table 2 gives mean outcomes and simple p-statistics for mean differences between convicted and non-convicted respondents.

Table 1.1 Sample Sizes for Initial Subpopulations, Those Observed in Years of Interest, and Those Used for Each Outcome of Interest.

	First Wave of	Present For Study		Yearly Income		Hourly Wage		Tenure Length		Probability Unemployed	
Population	Survey	*NC*	*C*	*NC*	*C*	*NC*	*C*	*NC*	*C*	*NC*	*C*
White Men	2,413	1,952	210	836	108	1078	155	892	146	1,076	152
Black Men	1,169	934	148	477	45	646	88	580	100	628	98
Hispanic Men	977	810	77	464	44	582	59	470	62	585	65
White Women	2,252	1,935	86	661	36	876	57	740	60	857	56
Minority Women	2,090	1,910	45	884	22	1250	40	1068	38	1188	38

'NC' indicates populations without a criminal record.
'C' indicates populations with a criminal record.

The conviction variable is based on criminal convictions prior to the year of the reported outcome variable not settled in juvenile court. The income variable is reported income earned from an outside employer (income from self employment is excluded) for the most recent year restricted to 2007, 2006, 2005 and 2004. Following restrictions similar to those of Ginther (2000), I further restrict the sample to those reporting working at least 35 weeks and

Table 1.2 Mean Values of Outcome Variables of Interest and P-Values Associated With Tests of Mean Differences.

Population	Yearly Income NC	Yearly Income C	Hourly Wage NC	Hourly Wage C	Tenure Length NC	Tenure Length C	Probability Unemployed NC	Probability Unemployed C
White Men	$29,621	$24,612	$14.48	$13.52	44.45	37.82	5.1%	8.2%
	(0.000)		(0.100)		(0.044)		(0.018)	
Black Men	$23,011	$14,693	$11.99	$10.85	40.89	35.48	11.5%	16.1%
	(0.000)		(0.082)		(0.162)		(0.087)	
Hispanic Men	$26,405	$29,682	$14.01	$13.49	45.98	42.99	6.3%	9.5%
	(0.103)		(0.551)		(0.558)		(0.188)	
White Women	$21,316	$16,826	$11.43	$10.20	41.49	44.18	5.3%	12.1%
	(0.019)		(0.089)		(0.561)		(0.003)	
Minority Women	$19,294	$14,791	$10.92	$9.01	40.35	37.57	9.7%	17.8%
	(0.065)		(0.008)		(0.634)		(0.027)	

'NC' indicates population without a criminal record.

'C' indicates population with a criminal record.

Values in parenthesis are p-values.

1,400 hours in the year for an outside employer. The NSLY sample procedure top codes the highest earners giving the top 2% the mean for that subpopulation. Since the population of interest here is those with at most a high school diploma, I do not use the full data set. Top coded data will skew the means if all respondents are not used; to address this, I drop respondents with reported incomes in the upper and lower percentiles resulting in a symmetrically trimmed population.[3] Each subpopulation characterized by race, gender, and conviction status is trimmed separately.

Unemployment probability is measured as the percentage of time in the labor market spent unemployed. The measure for the most recent year is used, and it is conditional on being in the market at least half of the year. The hourly wage variable is a weighted average of all wages earned from an outside employer, excluding military, for the most recent year restricted

[3]Do to large groupings around reported incomes where the trim occurs the symmetry is not exact.

to 2007, 2006, 2005 and 2004 where the weights are hours employed. Responses less than \$5 an hour or above \$50 an hour are not included. Job tenure is measured as the average length of employment for all jobs begun no earlier than 2003 or the year after first conviction, whichever is most recent. Each race/gender sub-group's ATE is estimated separately with race covering non-Hispanic blacks, non-Hispanic whites and Hispanics. Furthermore, given the small number of both black and Hispanic women with criminal convictions, these two groups are merged into one population - minority females. Custom sample weights are used in all calculations to correct for clustering and yearly participation.

I focus my analysis on subpopulations in which mean outcomes for those convicted are clearly different from those not convicted of crimes. For yearly income, unemployment probability, and hourly wages I include white males, black males, white females, and minority females. For tenure I only include white and black males. Figures 1-4 depict the cumulative distribution functions of yearly income, unemployment probability, hourly wage, and tenure for white males, black males, white females, and minority females.

1.5.2 Estimation

Estimated bounds defined in Equations 7-8, 18, and 20 are functions of expected wages, probabilities of being convicted, and probabilities of realized instrument values, all of which can easily be computed nonparametrically. For worst case bounds and bounds under MMTR/MTS, these values are calculated by sample analogs. For bounds under the delinquency and test score MIV, expectations and probabilities are

estimated via kernel estimation:

$$\hat{\mu}(z) = \frac{\sum_{i=1}^{n} y_i K(\frac{z-Z_i}{h})}{\sum_{i=1}^{n} K(\frac{z-Z_i}{h})} \tag{1.21}$$

where $K(\cdot)$ is the Gaussian kernel weighting function and h, the bandwidth, is chosen using Silverman's (1986) rule-of-thumb: $h = 1.06\sigma_z n^{-1/5}$.

Although nonparametric estimators allow researchers to estimate free of functional form, they are limited by the number of conditioning variables. The estimates in this paper condition on race, gender, education and the relevant instrument where an MIV is utilized. But this limited number of conditioning variables does not affect the consistency of the results. In a standard regression, the consistency of the results relies on an orthogonality condition surrounding the disturbance term and the regressors. In such a setting, missing regressors might cause a failure in this condition leading to inconsistent results. In the present setting however, there is no equivalent condition, and conditioning on more variables simply redefines the population. Due to data limitations such a refinement is not feasible here.

1.5.3 Inference

Statistical inference for partially identified parameters is somewhat more challenging than estimation itself and is the focus of a currently active literature. A consensus on the 'correct' type of confidence interval that should be reported is still evolving. The results of partial identification analysis are regions of identification defined by upper and lower bounds which contain the parameter of interest. When considering confidence intervals in these settings, the question arises of whether to construct intervals over the region of identification or over the actual parameter of interest. Intervals presented here cover the parameter of interest with fixed probability and stem from results by Imbens and Manski (2004).

Given estimated upper and lower bounds ($\hat{ub}$, $\hat{lb}$) and their estimated standard errors ($\hat{\sigma}$), $(1-\alpha)$-percent confidence intervals are constructed as:

$$CI_{1-\alpha} = \left(\hat{lb} - c \cdot \hat{\sigma}_{lb} \ \ , \ \ \hat{ub} + c \cdot \hat{\sigma}_{ub}\right) \tag{1.22}$$

where the parameter c is found by solving

$$\Phi\left(c + \frac{(\hat{ub} - \hat{lb})}{\max \hat{\sigma}_{lb}, \hat{\sigma}_{ub}}\right) - \Phi(-c) = 1 - \alpha. \tag{1.23}$$

1.5.4 Results

Specification tests,[4] the results of which are given in Table 3, reject the imposition of the delinquency MIV on expected income and hourly wage under the no-conviction treatment for

[4]MIV assumptions of the type imposed in this study are refutable (i.e., it is possible for the data to reject the assumption). If the MIV assumptions are invalid, they may lead to the upper and lower bounds on expected earnings to cross. The estimation technique implemented in this study follows a two step procedure. First, a specification test is performed to examine whether an assumption is consistent with the data. The procedure employed resamples the data and then estimates bounds on $E[y(t)|x]$ under the MIV assumption with the subsampled data. This is done using standard bootstrap methods. The bounds are consistent with the data if

Table 1.3 Results of Bootstrap Tests of the Consistency of MIV Assumptions.

	Monotone Instrumental Variable			
	Delinquency		Test Score	
Population	NC	C	NC	C
Outcome Variable is Yearly Income				
White Males	0	0	1	0
Black Males	7	0	9	0
White Females	75	0	1	0
Minority Females	98	0	2	0
Outcome Variable is Hourly Wage				
White Males	0	0	0	0
Black Males	1	0	12	0
White Females	49	0	0	0
Minority Females	31	0	1	0
Outcome Variable is Probability Unemployed				
White Males	0	0	0	0
Black Males	0	0	1	0
White Females	1	0	30	0
Minority Females	15	0	30	0
Outcome Variable is Tenure Length				
White Males	1	0	1	0
Black Males	0	0	60	0

'NC' indicates population without a criminal record.
'C' indicates population with a criminal record.
Values indicate the percentage of bootstrap estimates
where the assumptions are inconsistent with the data.

white women and minority women. The tests also reject imposing the delinquency MIV on probability of unemployment under the no-conviction treatment for minority females. The tests reject the imposition of the test score MIV on hourly wage and tenure length under the no-conviction treatment for black men. I also find evidence against imposing the test score

the lower bound does not exceed the upper bound. The feature of interest is the percentage of bootstrapped samples for which the assumption is consistent with the data. Assumptions that are not rejected are then imposed on the data to obtain bounds on the ATE. This procedure, though merely heuristic in nature, can be found in the literature on partial identification (Ginther 2000, Gerfin and Schellhornn 2006).

MIV on unemployment probability under the no-conviction treatment for white and minority females. In the following analysis, these MIVs are only imposed on expected outcomes under the conviction treatment for these populations.

Results are given in Tables 4 and 5. Table 4 displays worst case bounds and bounds under the joint MMTR-MTS assumptions. The main results, those coming for the addition of an MIV assumption, are given in Table 5. Initial worst case bounds on the ATE of criminal convictions on yearly income are quite large and are not very informative.[5] They confine the identification region to a range of $65,000 and necessarily contain zero. Once the MMTR and MTS assumptions are imposed the bounds shrink dramatically. For white men, white women, and minority women, they span the general range of $0 to $5,000. The range for black men is nearly twice as large and spans $0 to nearly $9,000. These ranges imply that, on average, a criminal conviction lowers black males' yearly earnings by up to $9,000, while for white males the effect is no larger than $5,000. Adding MIV assumptions further tighten the bounds on the ATE and in many cases sign the treatment effect away from zero. For white men, under the delinquency MIV, the ATE is bounded between $866 and $3,589, implying a criminal conviction leads to an expected loss of between 2.7% and 11% in yearly income.

Under the test score MIV this range is significantly higher implying a loss of at least 4% in yearly income. Bounds on the ATE for black males are also quite informative when the delinquency MIV is imposed. In this case, the ATE is bounded between $672 and $7,681, implying the expected effect of a criminal conviction on yearly earnings is a loss of between 2.7% and 31%. For minority women, the ATE is bounded between $764 and $3,980 under the delinquency MIV. This represents a loss in yearly income of between 3.8% and 20%. Under none of the MIVs can the ATE for white women be signed away from zero.

These finding align with previous studies that find a substantial causal effect and stand in contrast with studies that find a smaller and diminishing effect. Grogger (1995), using data from the early 1980's, finds arrests to have a negative effect on young mens' earnings in the range of $42 to $128 a quarter. After adjusting for inflation this amounts to about a $320 to $1,000 decline in yearly earnings, which does fit within the lower end of the findings here.

[5]To obtain these bounds K_l is set to $5,000 and K_u is set to $70,000.

Table 1.4 Worst Case Bounds of the ATE of Criminal Convictions and Bounds Under Joint MMTR MTS assumptions.

	Worst Case Bounds				MMTR/MTS			
Population	L-	L	U	U+	L-	L	U	U+
	Outcome Variable is Yearly Income							
White Males	-38,694	-37,854	27,146	27,986	0	0	5,288	7,238
Black Males	-44,161	-42,733	22,267	23,695	0	0	8,942	11,709
White Females	-47,554	-46,652	18,348	19,250	0	0	4,897	7,480
Minority Females	-50,383	-49,592	15,408	16,199	0	0	4,696	7,636
	Outcome Variable is Hourly Wage							
White Males	-32.59	-32.06	12.94	13.47	0	0	0.99	1.82
Black Males	-34.60	-33.78	11.22	12.10	0	0	1.35	2.29
White Females	-36.99	-36.46	8.54	9.07	0	0	1.19	2.28
Minority Females	-38.23	-37.88	7.12	7.47	0	0	2.38	3.08
	Outcome Variable is Probability Unemployed							
White Males	-0.18	-0.16	0.84	0.86	0	0	0.035	0.065
Black Males	-0.25	-0.22	0.78	0.81	0	0	0.052	0.106
White Females	-0.11	-0.10	0.90	0.91	0	0	0.069	0.110
Minority Females	-0.13	-0.12	0.88	0.89	0	0	0.053	0.135
	Outcome Variable is Tenure Length							
White Males	-54.71	-52.60	47.40	49.51	0	0	6.93	11.57
Black Males	-57.05	-54.50	45.50	48.05	0	0	7.96	13.03

L and U are lower and upper estimated bounds.

L- and U+ are lower and upper bounds on 95% confidence regions.

But, in contrast to the findings here, an important aspect of his findings is that this loss is dissipated within six quarters. The average time between last conviction and reported income for all populations in this study lie between five and six years with standard deviations in the range of about two. Part of this discrepancy might be that Grogger focuses on arrests while I focus on convictions.

Freeman (1991), also using data from the early 1980's, finds the causal effect of criminal convictions on income to be in the range of 3% to 9%, corresponding nearly identically to the results found here for white males. Allgood, Mustard and Warren (2003), using the 1979 NLSY

Table 1.5 Bounds of the ATE of Criminal Convictions Under Joint MMTR, MTS and MIV Assumptions.

Population	Delinquency				Test Score			
	L-	L	U	U+	L-	L	U	U+
Outcome Variable is Yearly Income								
White Men	0	866	3,589	6,641	24	1,230	6,289	8620
Black Men	0	672	7,681	10,528	0	0	9,784	14,034
White Women	0	0*	4,795*	7,632	0	0	3,074	5,503
Minority Women	126	764*	3,980*	7,105	0	0	4,735	8,158
Outcome Variable is Hourly Wage								
White Men	0	0	1.09	2.49	0	0.28	1.23	2.69
Black Men	0	0.15	1.41	2.71	0	0.04	0.52	2.63
White Women	0	0.10*	1.16*	2.86	0	0	0.86	2.22
Minority Women	0	0.16*	2.98*	4.95	0	0	2.51	3.29
Outcome Variable is Probability Unemployed								
White Men	0	0.002	0.032	0.035	0	0.006	0.023	0.052
Black Men	0	0	0.046	0.099	0	0	0.034	0.092
White Women	0	0	0.056	0.095	0	0*	0.065*	0.113
Minority Women	0	0*	0.066*	0.144	0	0*	0.023*	0.117
Outcome Variable is Tenure Length								
White Men	0.34	0.40	3.99	4.35	0.44	0.56	3.64	4.56
Black Men	0	0	7.21	7.51	0	0*	6.73*	7.68

L and U are lower and upper estimated bounds.

L- and U+ are lower and upper bounds on 95% confidence regions.

Estimated bounds presented have been corrected for sample bias.

*: Bounds where MIV is only imposed on expected earnings under conviction treatment.

cohort, find a criminal conviction causes a reduction in earnings of 12% and lasts up to ten years. This is in the high range of bounds for white males but fits well within those found for black males. Waldfogel (1994), finds a conviction to reduce income by upwards of 30%, though this high effect falls on those who in some way committed a breach of trust and actually spent time in prison.

Initial worst case bounds on the ATE on hourly wages span a range of $45 and necessarily contain zero.[6] Bounds under the joint MMTR-MTS assumptions significantly reduce this range. The upper bound for white males is \$0.99 and is slightly higher for black males and white females at \$1.35 and \$1.19. But for minority females the upper bound is much higher at \$2.38. In all subpopulations the ATE can be signed away from zero. For white males the lower bound is signed away from zero under the test score MIV and is estimated to be \$0.28. This implies that a criminal conviction, on average, lowers hourly wages by at least about a quarter of a dollar. The estimated lower bounds are somewhat smaller for the other populations at \$0.15 for black males, \$0.10 for white females, and \$0.16 for minority females. These positive findings align well qualitatively with the prediction of Black's (1995) search model concerning earnings. For all four of the subpopulations I find a negative causal effect of criminal convictions on hourly wages and yearly income.

Worst case bounds on probability of unemployment span a range of 1 and by definition include zero. The joint MMTR-MTS assumptions reduce this range between 93% and 97%. The upper bound on the treatment effect is lowest for white males at 3.5% and highest for white females at nearly 7%. The treatment can only be signed for white males. Under the test score MIV, the treatment effect on unemployment probability is bounded between 0.6% and 2.3%. This implies that a criminal conviction increases white mens' probability of unemployment by at least about 10% and perhaps as high as nearly 50%. These findings cover similar ranges as results found in previous studies that use survey data - primarily the 1979 NLSY (Holzer 2007). Though the treatment cannot be signed for populations other than white men, the upper bounds on the effect are reduced considerably for black males and minority females by the inclusion of a MIV. For black males, the test score MIV brings the upper bound on the effect down to 3.4% from 5.2%. For minority females the reduction is even more substantial; the imposition of the test score MIV brings the upper bound on the effect down to just 2.3% from 5.3%.

Worst case bounds on job tenure span a range of 100 weeks and by construction include zero.[7] The joint MMTR-MTS assumptions reduce this range by over 90%. For white males

[6] To obtain these bounds K_l is set to \$5 and K_u is set to \$50.

[7] To obtain these bounds K_l is set to 0 and K_u is set to 100.

the ATE is bound between zero and about seven weeks, for black males the range is between zeros and about eight weeks. Though for black males the ATE cannot be signed, for white males the addition of both the delinquency and test score MIV sign the treatment effect and reduce the size of the identification region considerably to a range of about 0.4 to just under four weeks. This implies a criminal conviction shortens job tenure by at least half of a week and as much as about a month for white males. These results lend some qualitative support to the prediction of Black's model concerning match quality. Though match quality is not observed, tenure is routinely used as an indicator of match quality (Centeno 2004) with lower tenure being a signal of lower match quality. The fact that a causal effect of convictions on tenure does not show up in women is not all that unexpected. Centeno (2004), when investigating the effect of unemployment insurance on match quality, uses only the male subsample, citing a higher probability of female employment spells terminating for reasons other than poor match quality.

1.5.5 Sensitivity Analysis

One potential concern with these results is their sensitivity to the MMTR assumption. Assuming MMTR imposes ex ante a non-negative ATE. (It does not, however, impose a non-negative effect for each individual; the analysis in this paper allows for heterogeneity in treatment effect) In some cases then, when the MIV assumption is imposed along with the MMTR assumption, the treatment effect is signed. It is interesting then to see how these bounds respond to a relaxation of the MMTR assumption, in particular, whether the ATE remains signed when the MMTR assumption is relaxed. I weaken the MMTR assumption as follows:

$$(1+\alpha)\cdot E[y(1)|x,z] \geq E[y(0)|x,z] \tag{1.24}$$

and allow α to vary from zero and up. Setting $\alpha = 0.05$ implies, for example, expected earnings under a conviction treatment are at most 5% greater than expected earnings under a non-conviction treatment. This analysis is applied only to the bounds that sign the ATE. Results that are bounded from below by zero rest solely on the MMTR assumption, and so weakening it yields trivial results. Figure 5 and 8 depict how the bounds on the ATE on yearly income and hourly wage respond to the weakening of MMTR for populations of interest. The top left graphs in Figures 11 and 12 depict how the bounds on the effect on tenure and unemployment probability respond to the weakening of MMTR for white males.

The results respond in varying degrees to the weakening of the MMTR assumption. For both black males and white males, under the delinquency MIV, the ATE of criminal convictions on yearly income remains signed with α as high as about 0.03. In other words, merely imposing that a criminal conviction increases expected income by no more than 3%, when combined with a MIV assumption, still leads to a signed ATE. For minority females under the delinquency MIV the ATE remains signed with α as high as about 0.04, and for white males under the test score MIV the ATE remains signed with α as high as about 0.045. The bounds for hourly wages are somewhat more sensitive. For white females, the signed nature of the ATE disappears with α of less than 0.01, though for white males the ATE remains signed with α as high as about 0.02. This means merely assuming convictions increase hourly wages by no more than 2% still signs the ATE. Similar findings emerge from the analysis on tenure and unemployment. Imposing the weakened assumption that convictions increase tenure by no more than 1.5% still leads to bounds that sign the ATE. For unemployment probability, α must be nearly 0.13 before the treatment effect is no longer signed. This means merely assuming a criminal conviction decreases the probability of unemployment by at most 13% still leads to a signed treatment effect.

A second potential concern is the effect of attrition within the survey. Not all of the respondents that were present in the initial survey year were interviewed in the time frame considered in this analysis. There is a varying degree of attrition in the NLSY: 10% for white males, 7% for black males, 10% for white females and 6% for minority females. The use of custom sample weights might ease concerns about the representative nature of the observed population, and simple tests on specific variables between the present and missing groups might lend support to the assumption that the two groups are similar. In regards to this latter point, F-tests are performed to test for mean differences in several characteristics between these groups. Tested characteristics include the presence of a father figure in the home, mothers age at time of respondents birth, household poverty ratio, delinquency level, substance abuse level, and parental education. Results are given in Table 6. For the majority of tests, results fail to reject the null hypothesis that the two groups' means are the same. These tests provide some evidence, based on observables, that attrition should not be an overwhelming concern.

But an important aspect of this study is that unobservables are an important part of the labor outcome/conviction story. To address this, I test the sensitive of my results to assumptions about the missing respondents. I focus on two specific hypothetical cases: one in which all of the absent respondents are individuals with convictions and 'good' outcomes (high earnings and hourly wages, long job tenures, and low unemployment probability), and the other in which all of the absent respondents are individuals without convictions and with 'bad' outcomes . These are the two types of individuals that might overturn the findings if they were to be present in a significant enough degree with extreme enough outcomes.[8] The goal of this analysis is to see how extreme the missing data must be in order to overturn the main results.

Figures 6 and 9 are graphical depictions of how the bounds on the ATE on yearly income and hourly wage respond to assumptions regarding the mean earnings of missing respondents *when they are assumed to be individuals with convictions* for relevant populations. The top right hand graphs in Figures 11 and 12 depict how the bounds on the effect on tenure and unemployment probability respond for white males.

For none of the populations are the lower bounds on the ATE of criminal convictions on yearly income very sensitive to even the inclusion of very high earning individuals.[9] The upper bounds, on the other hand, are sensitive. For white men, the incomes of missing respondents,

[8]There is a further caveat. Simply assuming the missing respondents are of one of these two types will affect the upper bounds on the ATE. The upper bounds under MTS are defined as $E[y(1)|x, z = 1] - E[y(0)|x, z = 0]$, thus increasing the mean incomes of those with convictions or reducing the mean incomes of those without convictions will necessarily lower the upper bound. However, the lower bound on the ATE, where it differs from zero, owes its bounding power to the MIV assumption. The identifying power of the MIV stems from divergence in the conditional bounds from the monotonicity assumed. So to assume, for example, high earning individuals with convictions were evenly distributed across their delinquency rates would not do much to contradict the signed nature of the lower bound. So I assume the missing individuals are high earning individuals with convictions and low delinquency rates (or high test scores). Similarly, I alternatively assume the missing individuals are low earning individuals without convictions that exhibit high delinquency rates (or low test scores). This is done to yield the most conservative findings.

[9]This is due to the MIV and the fact that the conviction population is relatively small compared to the non-convicted population. The fluctuations in conditional bounds across instrument levels, from where the MIV gains identification power, is due to both populations. So even after smoothing out the fluctuations in the mean incomes of the convicted populations, the variations in the non convicted population's incomes still give the MIV power, leading to lower bounds on the ATE to remain above zero.

Table 1.6 Sample Means of Select Characteristics of Present and Absent Respondents and P-Values Associated With Tests of Mean Differences.

Population	Father Figure In Household		Mother's Age		Poverty Ratio		Delinquency Index		Substance Abuse		Father's Education		Mother's Education	
	P	NP	P	NP	P	NP	P	NP	P	NP	P	NP	P	NP
White Male	0.83	0.86	26.3	26.4	3.62	3.90	1.76	1.51	1.14	1.10	13.7	13.6	13.5	13.2
	(0.22)		(0.82)		(0.25)		(0.07)		(0.63)		(0.60)		(0.17)	
Black Male	0.54	0.44	24.2	23.9	1.91	1.82	1.72	1.89	0.90	0.92	12.4	12.4	12.5	12.2
	(0.09)		(0.60)		(0.73)		(0.48)		(0.86)		(0.94)		(0.36)	
White Female	0.80	0.83	26.0	26.8	3.60	3.52	0.97	0.63	1.12	1.12	13.6	13.5	13.4	12.9
	(0.19)		(0.03)		(0.76)		(0.01)		(0.97)		(0.88)		(0.01)	
Minority Female	0.59	0.66	24.7	24.7	1.79	2.36	0.95	0.67	0.84	0.71	11.62	11.92	11.46	11.48
	(0.12)		(0.99)		(0.01)		(0.04)		(0.19)		(0.46)		(0.96)	

Numbers in parenthesis are p-values.

'P' indicates those present for the study. 'NP' indicates those not present for the study.

if they were all assumed to have criminal records, would have to be quite high to invalidate the bounds. Depending on the MIV, their mean incomes would have to be $34,000 to $38,000, well above the $31,511 actually observed for those even without criminal convictions. Similarly, for black men, in order to invalidate the bounds the missing respondents would have to have incomes of about $34,000 which is well above the $24,322 never convicted men are observed to earn. Yet for minority women, the incomes of missing individuals would only have to be about $21,000, not much out of line with earnings for women without convictions in order to overturn the results.

Similar results emerge from this analysis on hourly wages, tenure, and unemployment probability. The lower bounds are not very sensitive while the upper bounds are. Yet again, the missing individuals would have to have wages, tenure lengths, and unemployment durations well outside the range of the averages observed to invalidate the bounds. Missing respondents would need to earn about $2 or more than the average of those without criminal convictions to cause the bounds to cross. Their tenure lengths would need to be about 10 weeks longer than the average of those without criminal convictions to overturn the results. For unemployment probability, even assuming the missing respondent had a zero probability of unemployment still leads to valid bounds.

Figures 7 and 10 are graphical depictions of how the bounds on the ATE on yearly income and hourly wage respond to assumptions regarding the mean earnings of missing respondents *when they are assumed to be never convicted individuals* for relevant populations. The bottom graphs in Figures 11 and 12 depict how the bounds on the effect on tenure and unemployment probability respond for white males.

Here, some of the lower bounds are sensitive to the outcomes of the missing respondents. For white men, the missing respondents need only have mean yearly incomes of about $28,000 to leave the MIV with no identifying power. For black men, incomes would need to average about $21,000 to nullify the MIV's identifying power. Similarly, for the bounds on hourly wages for black men, if the missing respondents were to have mean hourly wages of about $10.75 the MIV loses identifying power. All three of these are well within the observed ranges. But in contrast to the previous analysis, the upper bounds here are not very sensitive to the yearly incomes or hourly wages of the missing respondents. For job tenure and unemployment probability, the missing respondents would need mean outcomes well outside the range of what is observed to invalidate the bounds. This is because the non-convicted population is such a substantial portion of the overall, and the attrition rate is relatively small. For example, for minority women, assuming the missing individuals have zero income still leaves the upper bound on yearly income at about $3,000. Similarly, for unemployment probability, the missing respondents would need to exhibit a 30% chance of unemployment, well beyond the range of the observed data, to overturn the bounds.

1.6 Conclusion

This paper investigates the causal effects of criminal convictions on yearly income, hourly wages, match quality, and unemployment probability. A potential outcomes framework is presented in a fashion that links two approaches to the identification of treatment effects in the presence of endogenous treatment selection: the standard Heckman Two-Step and a partial identification approach developed by Manski (1989,1990,1997) and others. Unlike previous research in this area which relies on assumptions strong enough to yield point identification, this paper focuses on weaker assumptions that yield tight bounds on the ATE. In general, the findings here lend support to the predictions of a search model of employment when a portion of the firms are averse to hiring a subpopulation of workers (Black 1995). For white and black males, the prediction of lower wages and worse match quality for those with criminal records seem supported by the results. This wage effect is also observed for women, though the match quality effect is not apparent. The main findings are summarized below.

- Imposing two relatively innocuous restrictions stemming from economic theory regarding the response and selection mechanism are sufficient to provide informative identification regions of the average treatment effects of criminal convictions on labor market outcomes.

- Plausible monotone instrumental variables, when available, further narrow the bounds on the average treatment effect. In many cases, the inclusion of an MIV assumptions signs the ATE away from zero giving evidence of a causal effect.

- The treatment effect on yearly income for white males, black males, and minority females is bounded from below by \$866, \$672, and \$764. These represent a yearly loss of income of about 2.7% for black and white males and a 3.8% for minority females. The treatment effect is bounded from above by 11%, 30% and 20% for these populations. For white women, the effect cannot be signed away from zero but is bounded from above by \$4,795, or about 22%.

- The treatment effect on hourly wages for white males, black males, white females, and minority females is bounded from below by \$0.28, \$0.15, \$0.10, and \$0.16. These imply, at a minimum, a criminal conviction causes between about a 1% and 2% loss in hourly wages. The effect is bounded from above by about a 10% loss for white males, black males, and white females, and 30% for minority females.

- For white males, criminal convictions are estimated to reduce job tenure between about one half and four weeks. This range corresponds to an effect of between 1% and 10%. This is interpreted as a causal effect of criminal convictions on match quality. For black men, the effect cannot be signed away from zero, but is bounded from above at about a 15% decrease in tenure length.

- Criminal convictions appear to affect unemployment probability in all populations of interest, but this effect can only be signed for white men, in which case the effect is bounded away from zero and found to add at least a 0.6% risk of unemployment and as much as 2.3%.

- Bounds that sign treatment effects away from zero appear fairly robust to the weakening of a key identifying assumption - MMTR. Results also hold up fairly well in the face of various assumptions concerning attrition rates in the study.

When estimating the treatment effects of criminal convictions on labor market outcomes, endogenous selection requires the researcher to make explicit assumptions regarding data generation. This paper has studied the identifying power of various assumptions. Assumptions directly related to the selection and response functions have substantial identifying power. The inclusion of a variant of the traditional instrumental variable assumption yields informative bounds on the ATEs, allowing me to lend support to the predictions of a search model of employment, but still fall short of being able to point identify the average treatment effects. Stronger conclusions about treatment effects require stronger statistical or structural assumptions.

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Figure 1.1 CDF of Yearly Income For Populations of Interest. Solid Line Indicates Convicted Individuals.

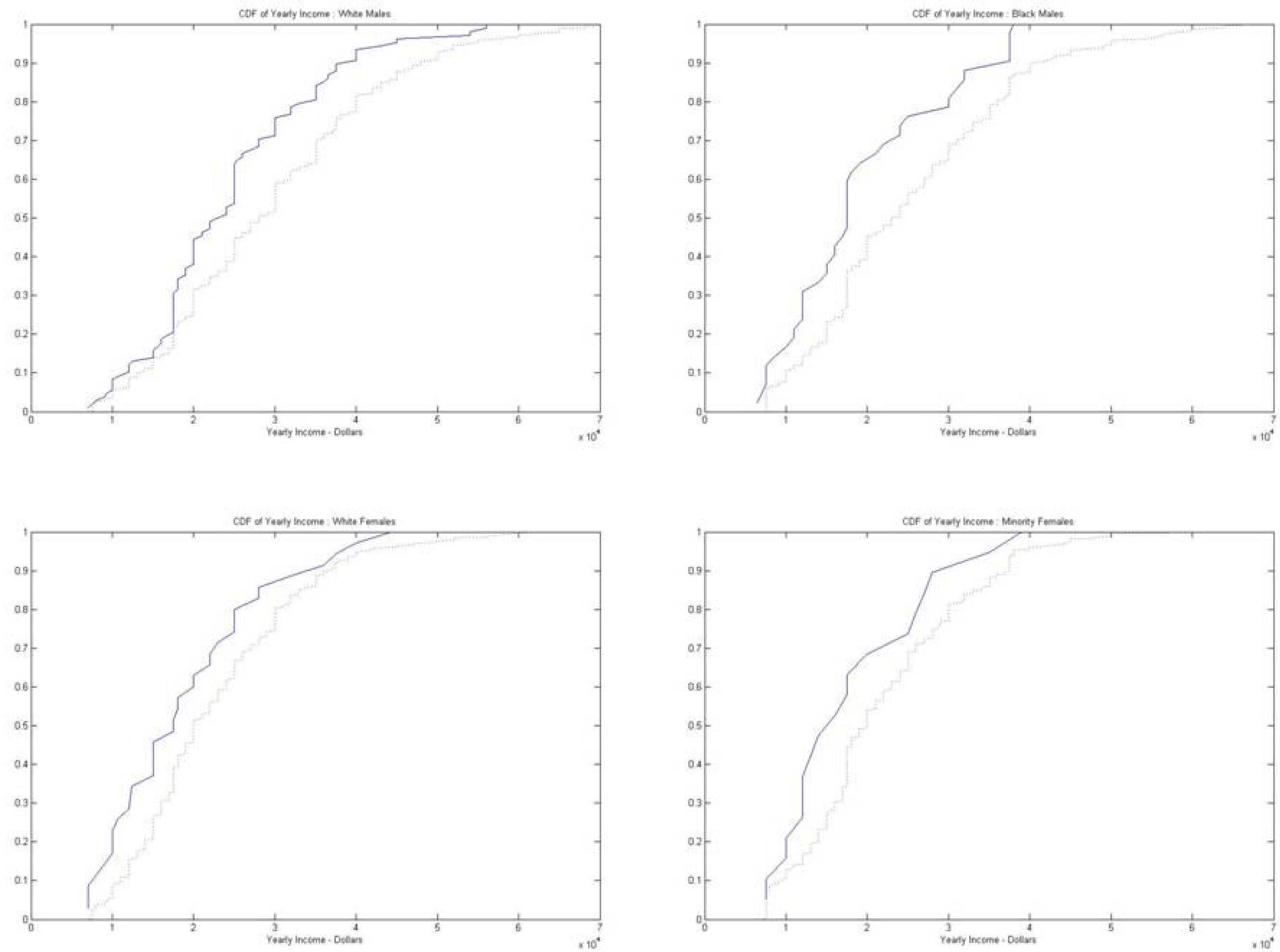


Figure 1.2 CDF of Unemployment Probability For Populations of Interest. Solid Line Indicates Convicted Individuals.

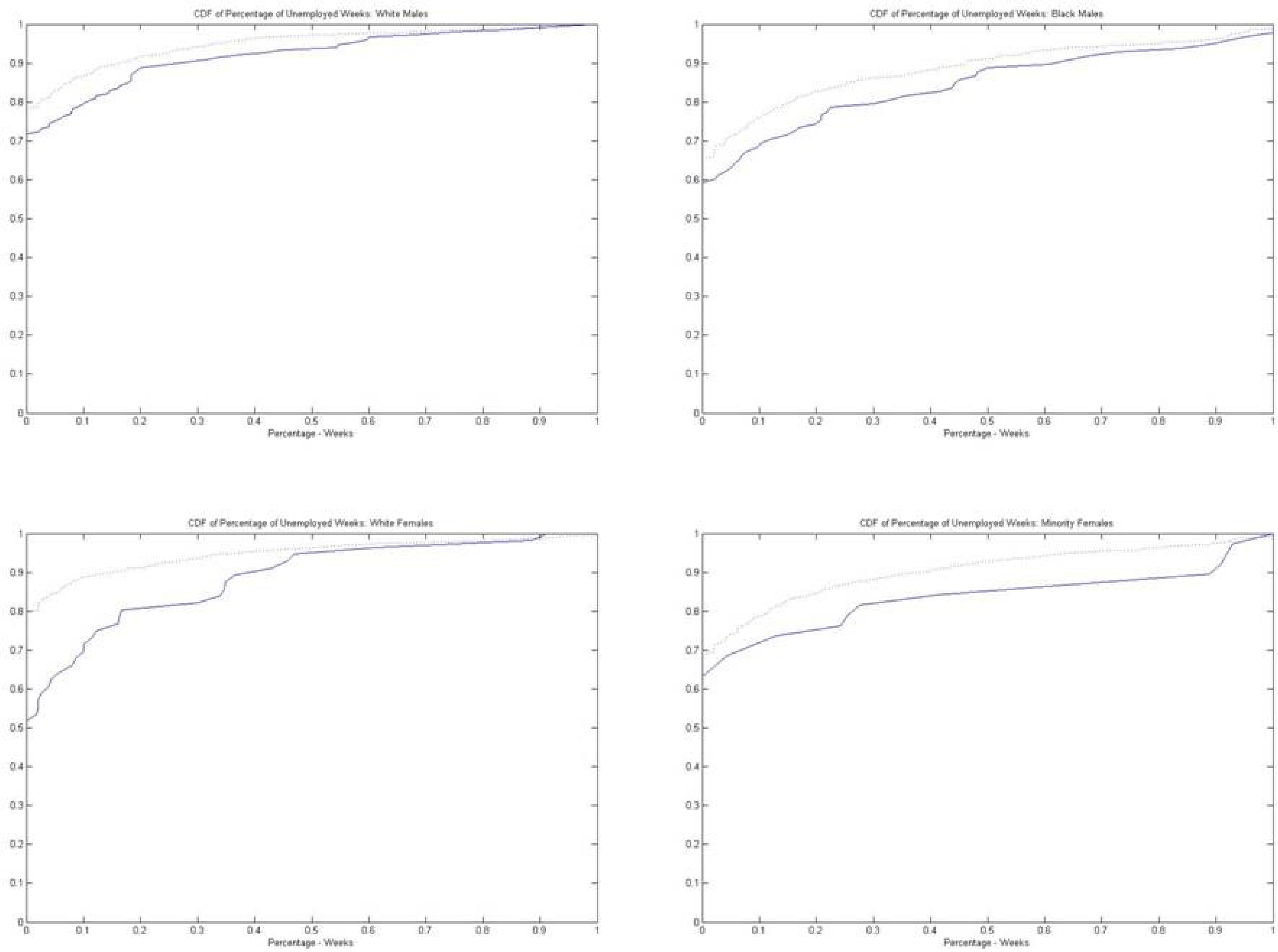


Figure 1.3 CDF of Hourly Wage For Populations of Interest. Solid Line Indicates Convicted Individuals.

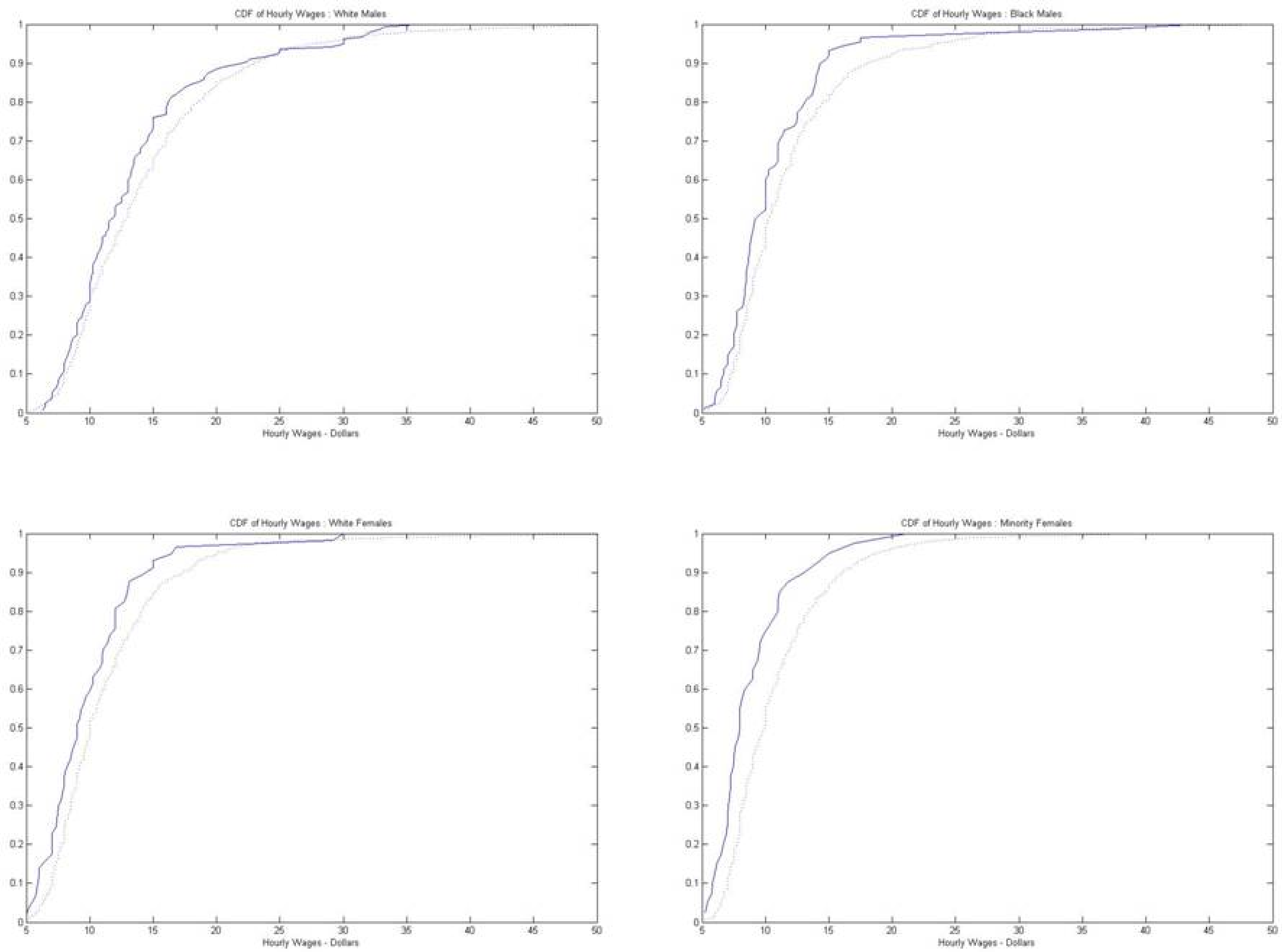


Figure 1.4 CDF of Tenure Length For Populations of Interest. Solid Line Indicates Convicted Individuals.

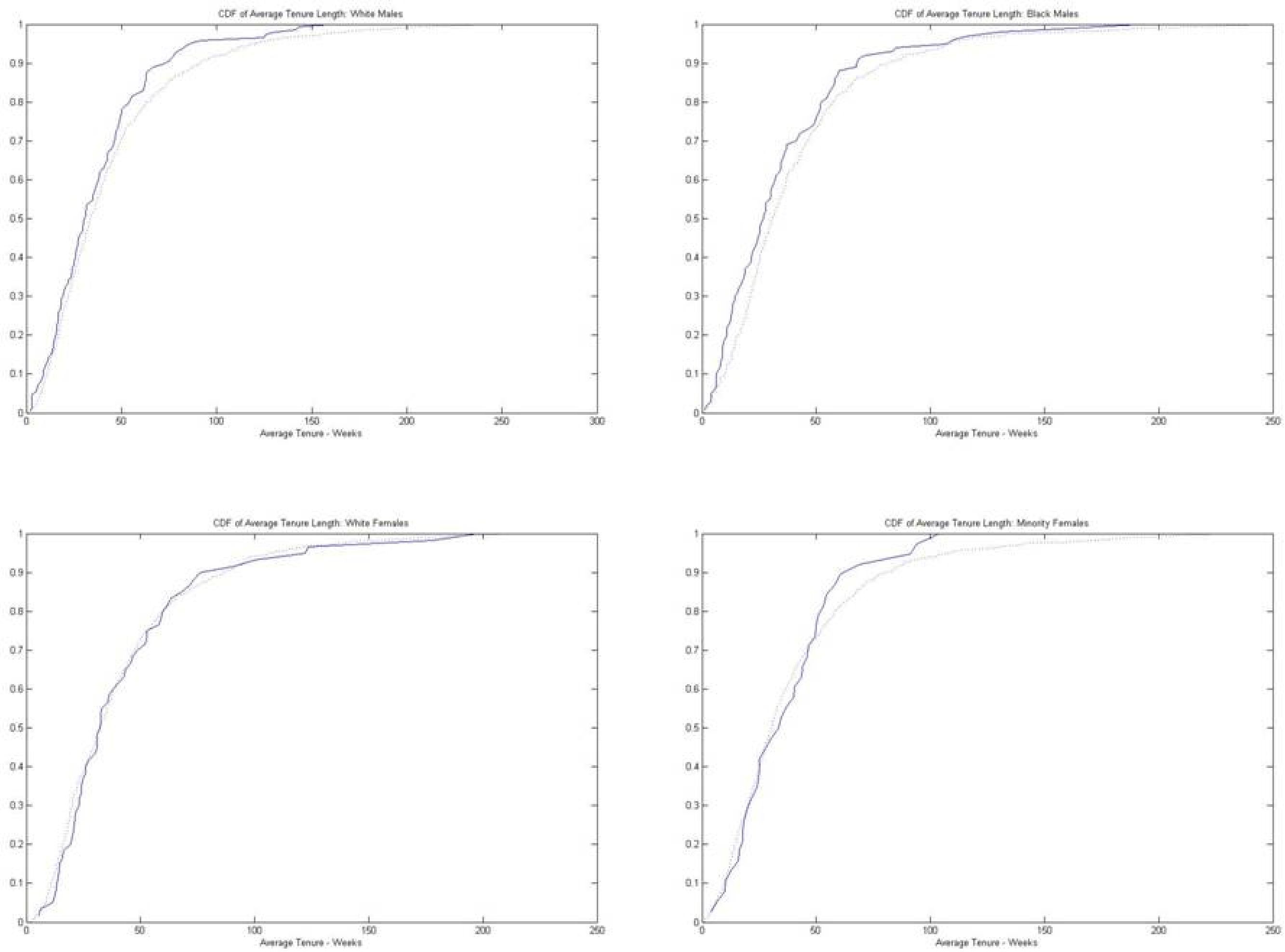
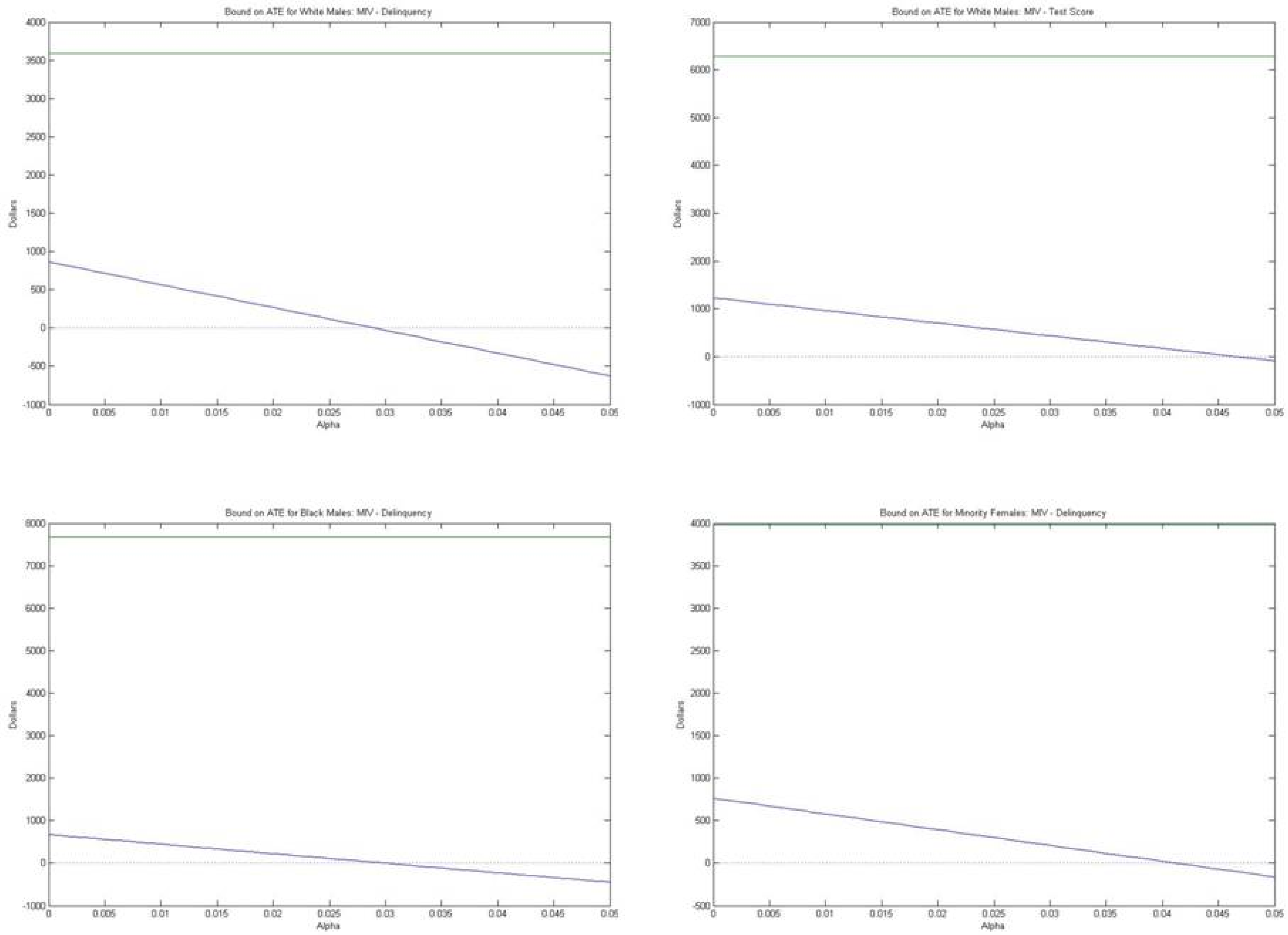
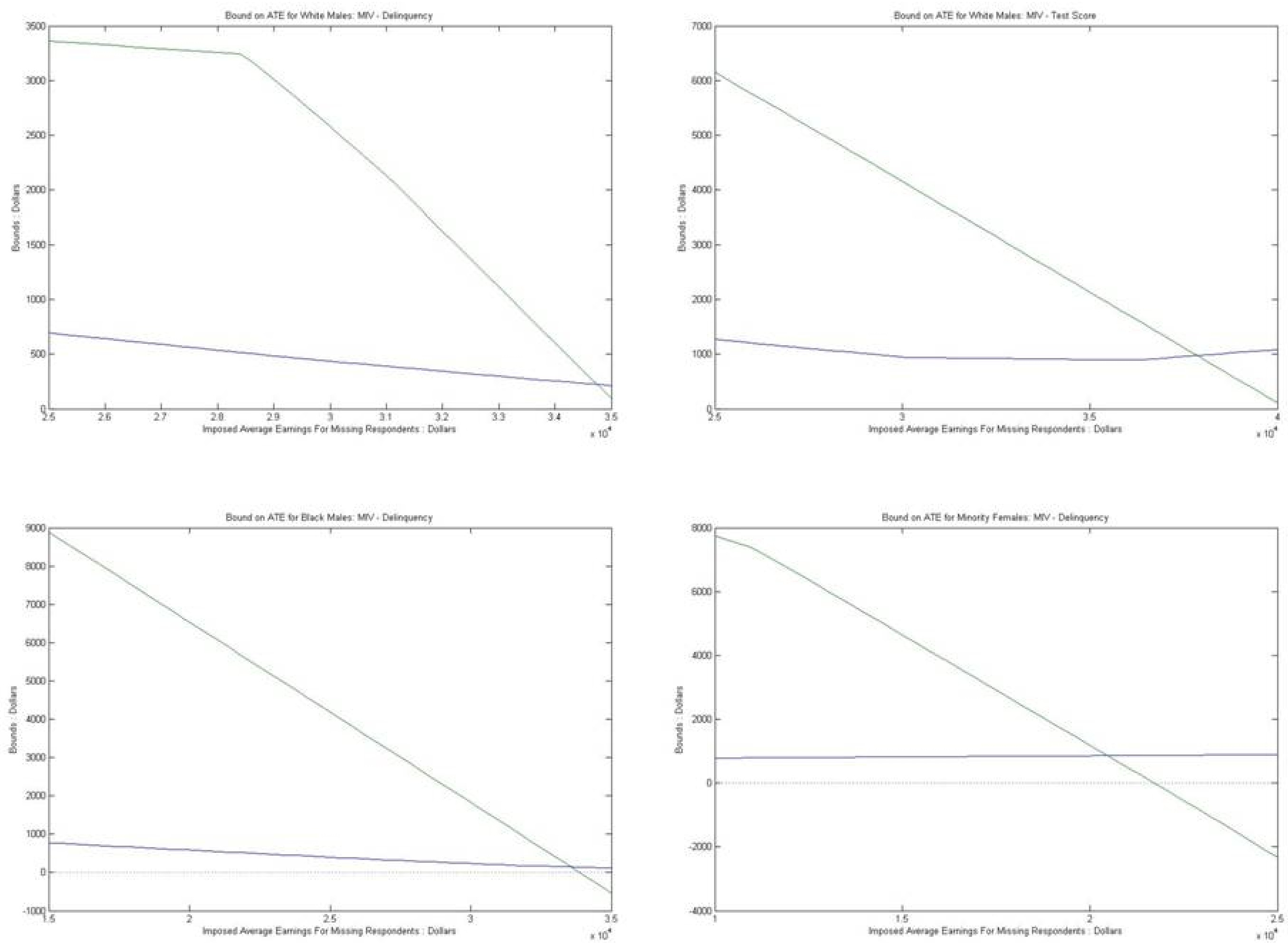


Figure 1.5 Various Bounds on ATE on Yearly Income Under MMTR/MTS/MIV and their sensitivity to the MMTR assumption.



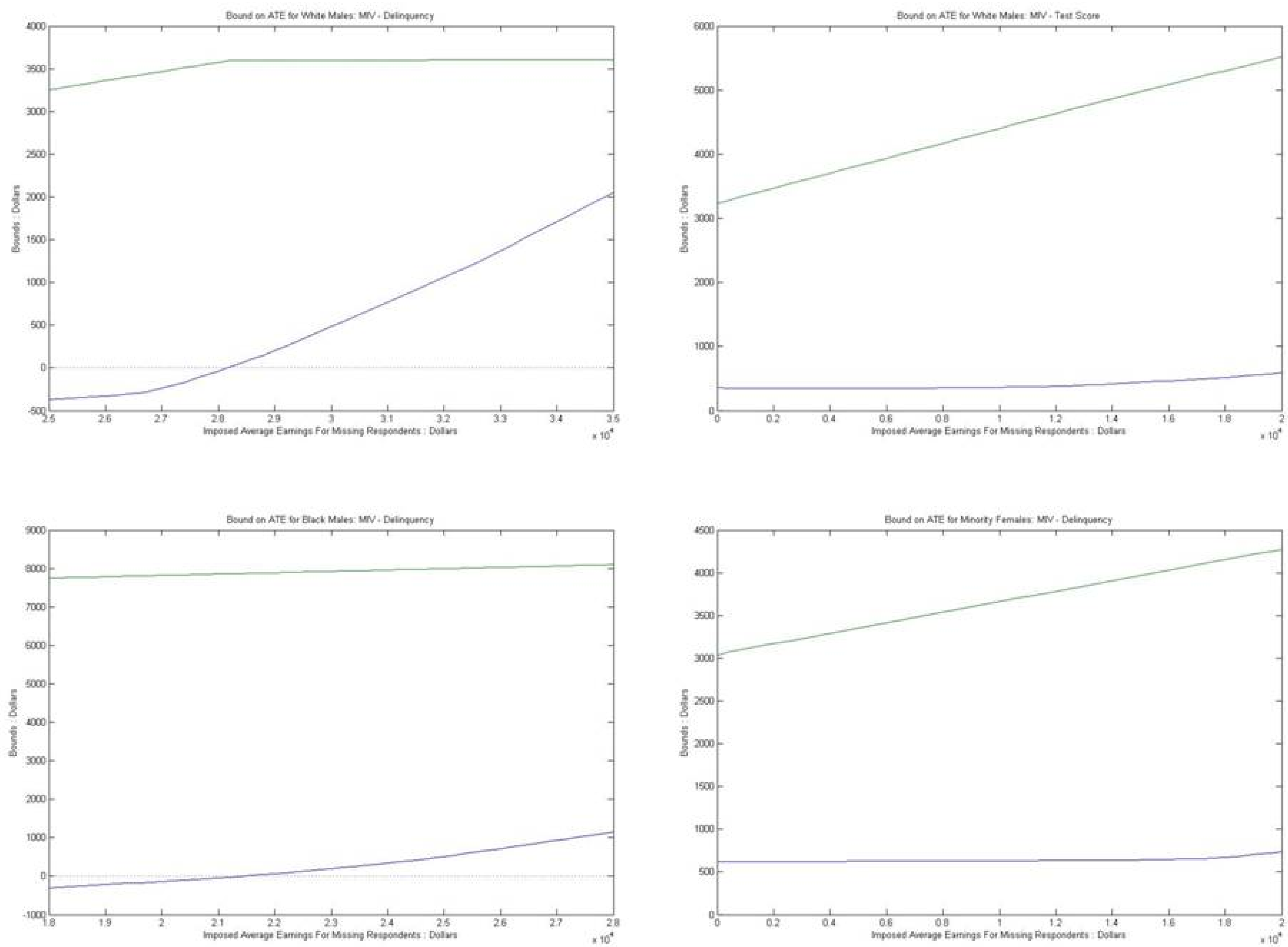
As α increases from left to right the MMTR assumption is weakened.

Figure 1.6 Various Bounds on Yearly Income Under MMTR/MTS/MIV and their sensitivity to Attrition I.



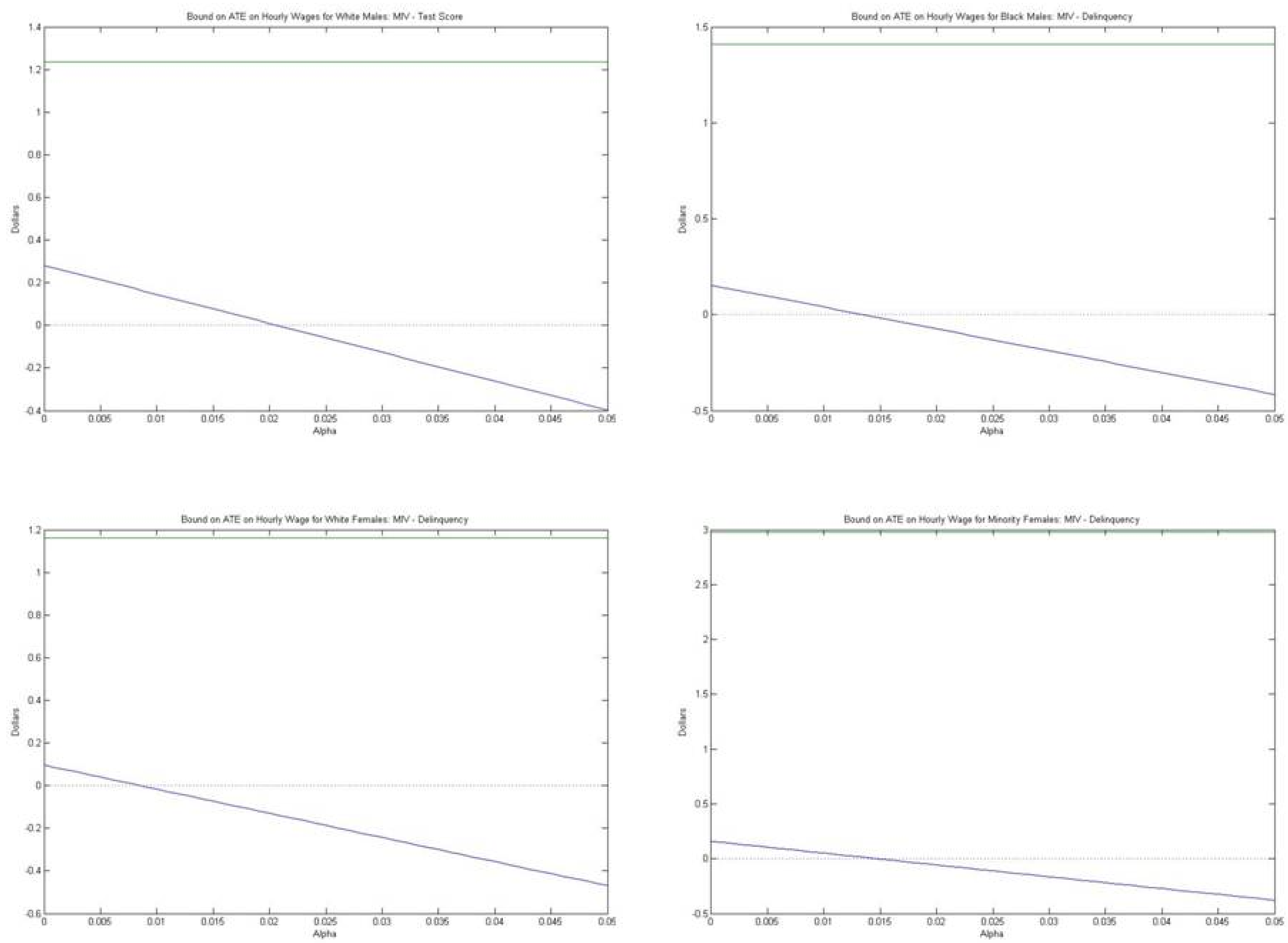
Attrition I is when missing respondents are assumed having a criminal record. Graph shows how bounds respond to possible mean incomes of this group.

Figure 1.7 Various Bounds on Yearly Income Under MMTR/MTS/MIV and their sensitivity to Attrition II.



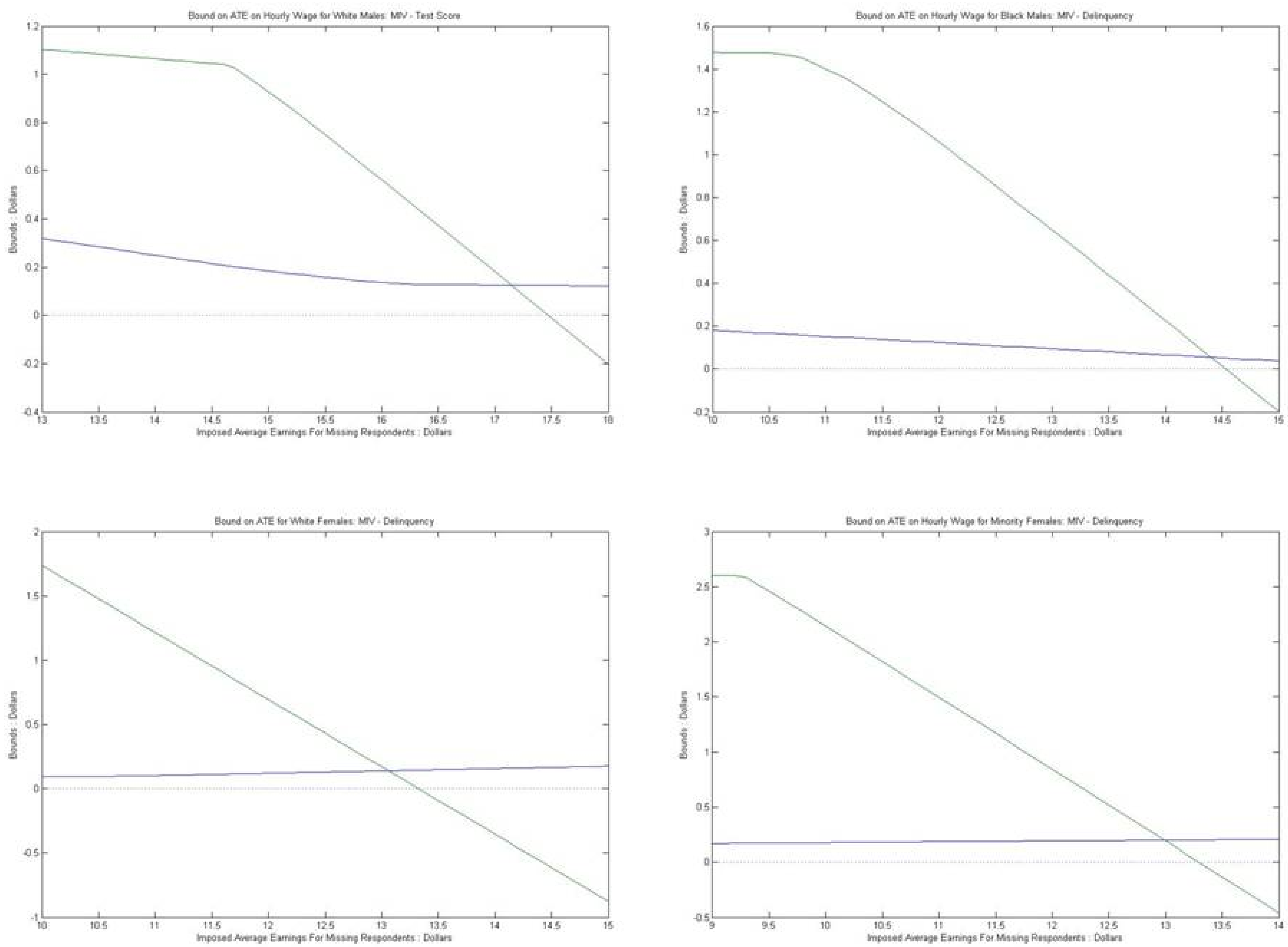
Attrition II is when missing respondents are assumed not having a criminal record. Graph shows how bounds respond to possible mean incomes of this group.

Figure 1.8 Various Bounds on ATE on Hourly Wage Under MMTR/MTS/MIV and their sensitivity to the MMTR assumption.



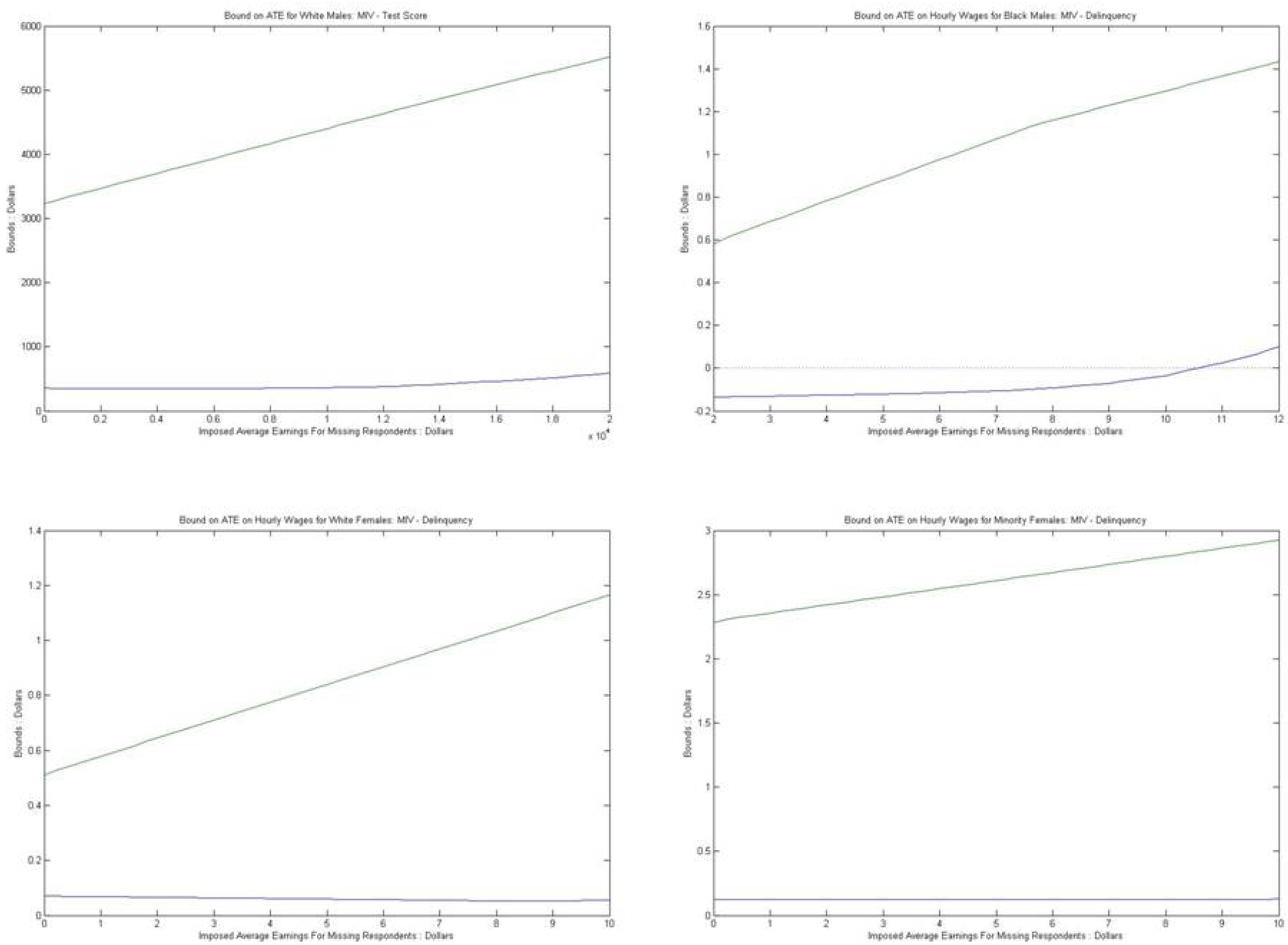
As α increases from left to right the MMTR assumption is weakened.

Figure 1.9 Various Bounds on Hourly Wage Under MMTR/MTS/MIV and their sensitivity to Attrition I.



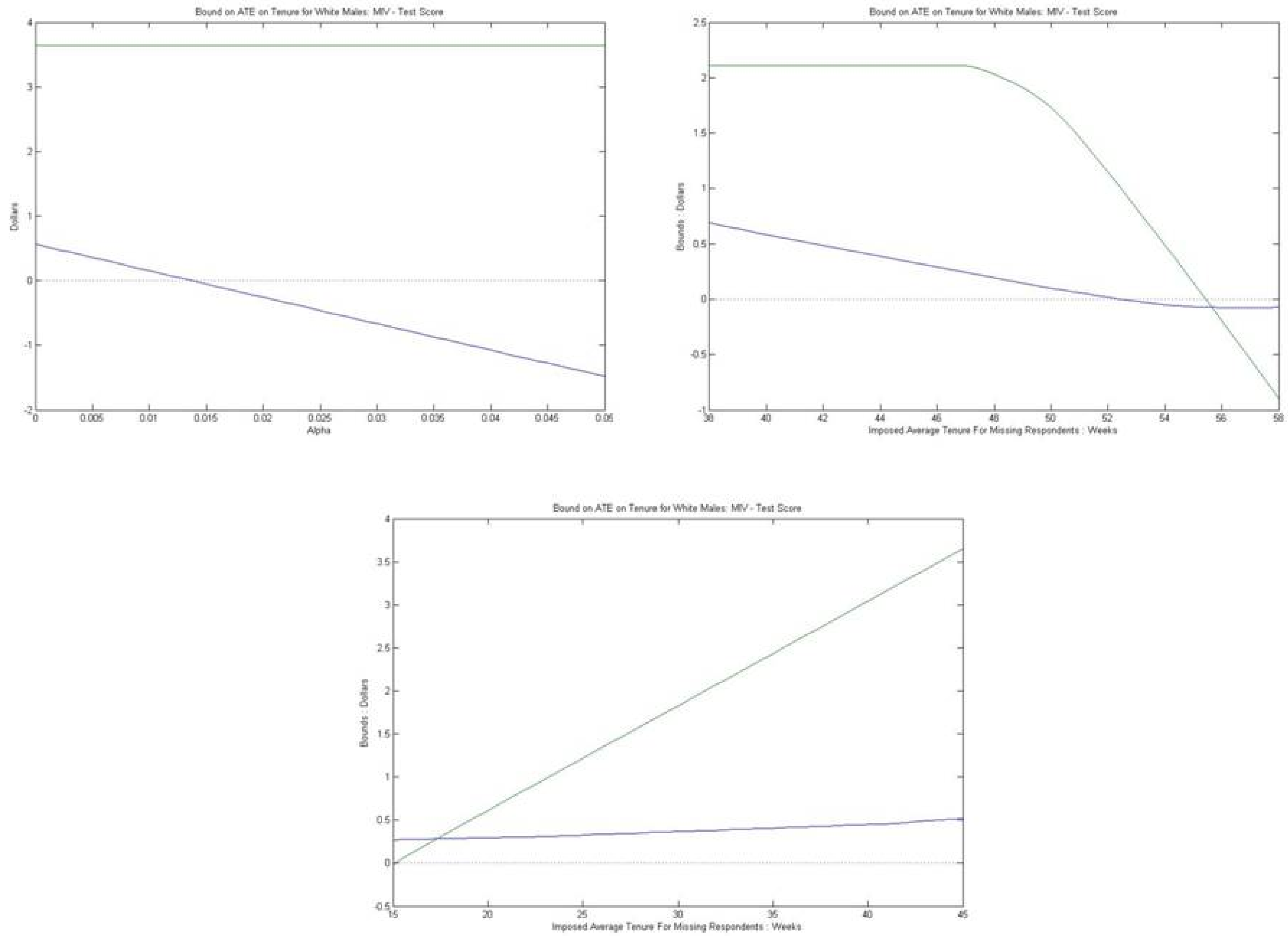
Attrition I is when missing respondents are assumed having a criminal record. Graph shows how bounds respond to possible mean wages of this group.

Figure 1.10 Various Bounds on Hourly Wage Under MMTR/MTS/MIV and their sensitivity to Attrition II.



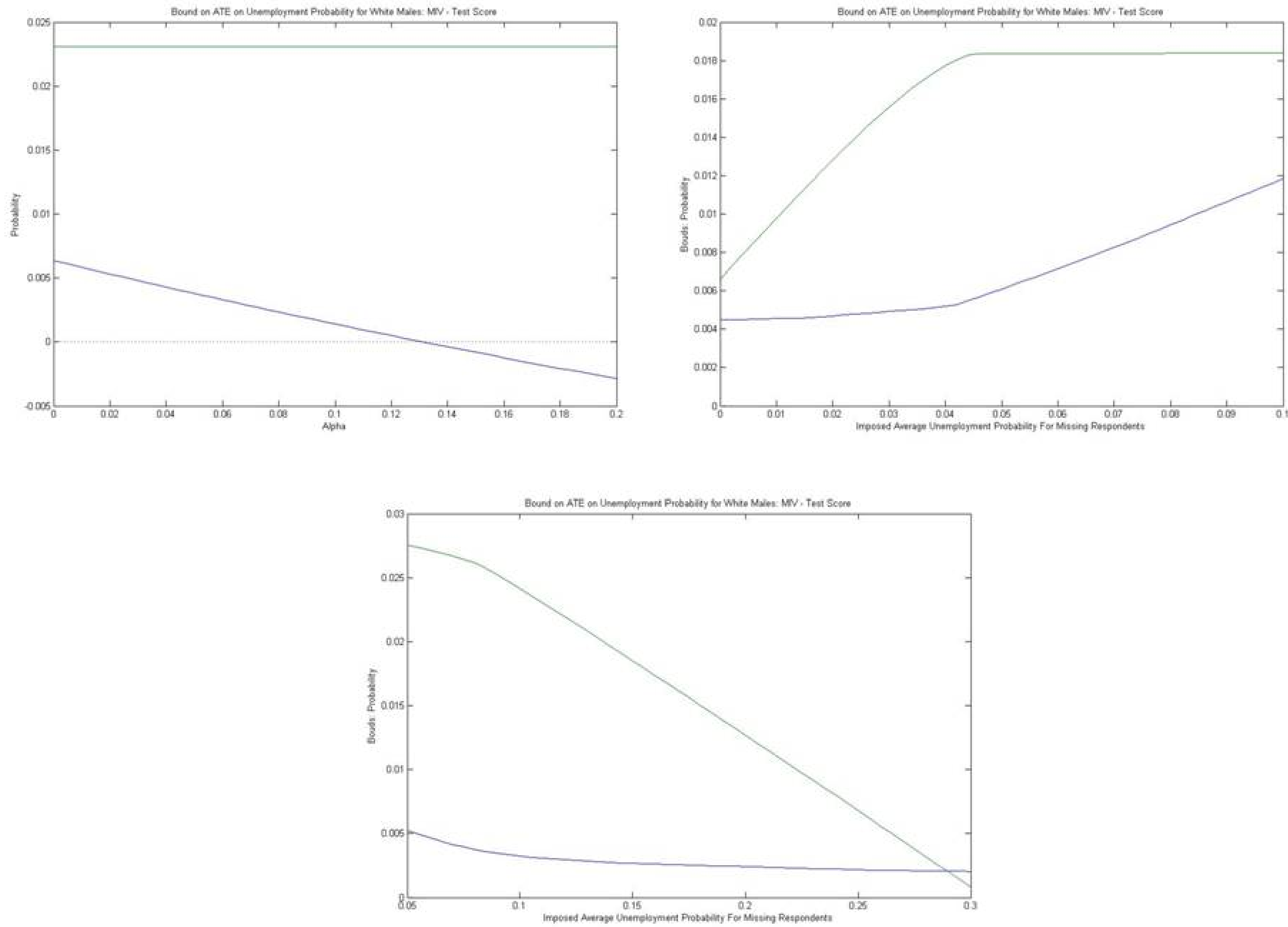
Attrition II is when missing respondents are assumed not having a criminal record. Graph shows how bounds respond to possible mean wages of this group.

Figure 1.11 Bounds on the ATE on Tenure Under MMTR/MTS/MIV and their sensitivity to the MMTR Assumption and Attrition.



Top left is bounds response to weakening of MMTR assumption. Top right is response to attrition I, bottom is response to attrition II. Attrition I is when missing respondents are assumed having a criminal record. Attrition II is when missing respondents are assumed not having a criminal record. Graph shows how bounds respond to possible mean tenure lengths of this group.

Figure 1.12 Bounds on the ATE on Unemployment Probability Under MMTR/MTS/MIV and their sensitivity to the MMTR Assumption and Attrition.



Top left is bounds response to weakening of MMTR assumption. Top right is response to attrition I, bottom is response to attrition II. Attrition I is when missing respondents are assumed having a criminal record. Attrition II is when missing respondents are assumed not having a criminal record. Graph shows how bounds respond to possible mean unemployment probabilities of this group.

CHAPTER 2. The Causal Effect of Post Secondary Education on Unemployment Incidence: An Instrumental Variable Analysis

2.1 Abstract

This paper reevaluates the causal effect of post-secondary schooling on unemployment incidence using historical data from the 1980 U.S. Census and information on cohort level Vietnam War conscription risk. Conscription risk is used as an instrument for endogenous post-secondary schooling in a specification that accounts for the discrete nature of the treatment and outcome of interest. Common econometric approaches such as two-stage least squares do not provide consistent estimates due to the nonlinear nature of the model. Instead, this paper utilizes a semiparametric instrumental variable estimator developed by Abadie (2003) to identify local average treatment effects (LATE) along with bounding methods developed by Manski (1990) and Chiburis (2010) to partially identify average treatment effects (ATE). For white men "compliers" in the private sector, college attendance reduces the risk of unemployment incidence by 1.8 - 2.1% while college graduation reduces the risk by 2.1-2.3% depending on the definition of the compliers. The effects on minority men are larger and range from 2.5% for college attendance to 4.1% for college graduation. Though the ATEs of post-secondary education on unemployment incidence are only partially identified, for some populations estimated bounds are quite informative. In particular, a threshold crossing model with an IV rules out a region around zero. The size of these null regions are evidence of a substantial causal effect of post-secondary schooling on unemployment incidence. This region ranges in size from 3.1% for white men in any sector to 7.6% for minority men in the private sector.

2.2 Introduction

The link between education and labor market outcomes is a central area of study within many fields. In particular, much attention has been paid towards investigating the impact of education on expected earnings. Considerably less attention has been paid towards investigating the effect of education on unemployment incidence. The sum of this literature seems confined to a handful of papers (Mincer 1991, Nickell 1979, Ashenfelter and Ham 1979, Kiefer 1985, Parsons 1972), all of which find a negative relationship between education and unemployment incidence. But as noted more recently by Riddell and Song (2011), the results of these papers may not quantify the actual causal effect as they do not account for the endogenous aspect of education. Riddell and Song's analysis, which does attempt to control for this endogeneity, seems to be the first to establish a causal link. These authors reaffirm some of the previous findings. Notably, they find evidence that college graduation lowers the risk of unemployment incidence. Unfortunately, their estimation strategy - two-staged least squares - is inappropriate given the nonlinear nature of the problem.

Verifying the causal link between college graduation and unemployment incidence is confounded by two issues. First, since individuals self-select into education there is a fundamental selection problem. Second, both college graduation and unemployment status are inherently binary in nature, and any estimation strategy should incorporate this fact. Endogenous regressors are frequently encountered in empirical work and are typically handled with some instrumental variable (IV) approach when a viable instrument is available. The most common of these approaches is two-staged least squares (2SLS). However, if the model of interest is nonlinear, 2SLS is theoretically inappropriate. If the endogenous regressor is continuous, a "control-function" approach offers an alternative. Binary endogenous regressors pose added difficulties to estimation. Until recently, binary endogenous regressors in nonlinear models were handled with approaches that required the specification of joint error distributions (bivariate probit models). More recently, advances in econometrics have given researchers tools that free them from this necessity.

This paper makes use of some of these advances to estimate the causal effect of post-secondary schooling on unemployment incidence. The approaches taken here incorporate the binary nature of the treatment and outcome while also accounting for the endogenous aspect of education. I both point-identify the Local Average Treatment Effect (LATE) and partially identify the Average Treatment Effect (ATE). In order to estimate these effects I make use of data from the 1980 U.S. Census. Law required the 1980 Census to be conducted on or about April 1, 1980. The first two quarters of 1980 were recessionary periods with real GDP declining 2% in the first quarter and an additional 0.2% in the second quarter. Thus the 1980 Census is not only a large data set, but one that provides a window into individuals' labor market experiences at a particular tumultuous time. Furthermore, the 1980 Census provides information on the current labor market status, as well as labor market activities of the previous year, for men of the "Vietnam War Generation" - males born between 1935 and 1959. As will be discussed in more detail in the following section, this is leveraged to provide an instrument for post-secondary education.

Theory suggests there should be a negative relationship between education and the probability of becoming unemployed. Education leads directly to increased human capital which might increase the rate of return to future training (Nickell 1979, Mincer 1991). Alternatively, higher education might simply work as a signalling mechanism indicating greater innate ability. Under either scenario, workers with more education will tend to have higher levels of firm-specific human capital giving firms an incentive to maintain the match (Nickell 1979, Keifer 1985). But as yet, the empirical evidence of this causal link has not been firmly established; this paper seeks to fill this gap in the literature and is organized as follows. Section 2 discusses the basic empirical framework, the data, and motivation behind the choice of instrument. Section 3 discusses and estimates two estimators of the LATE. Section 4 discusses successive estimators of the ATE which are linked and embedded in a potential outcome framework. Section 5 concludes.

2.3 Basic Framework and Data

Consider a model of potential outcomes. For each individual, we observe a binary outcome Y (unemployment status), an endogenous binary treatment T (educational attainment), a set of exogenous covariates X, and an instrument Z. The relationship between post secondary education and unemployment incidence can be modeled in a very general framework as:

$$\begin{aligned} Y(1) &= f_1(X, \epsilon_1) \\ Y(0) &= f_0(X, \epsilon_0) \\ T &= g(X_t, Z, \upsilon) \end{aligned}$$

where ϵ and υ are random disturbance terms, $Y(1)$ is an individual's potential employment status had he graduated from college (received treatment $T = 1$), and $Y(0)$ is his potential employment outcome had he not graduated from college ($T = 0$). Only one of $Y(1)$ and $Y(0)$ is actually observed. The goal is to estimate some measure of the treatment effect of post-secondary education on unemployment incidence. One common parameter of interest is the average treatment effect:

$$ATE = E[Y(1)|X] - E[Y(0)|X]. \tag{2.1}$$

The ATE is defined as the expected treatment effect for an individual with characteristics X selected at random from the population. The endogeneity issue arises in this setting from fear that $E[Y(T)|X] \neq E[Y(T)|T, X]$, i.e. that expected outcomes under a specific treatment differ between the entire population and the subpopulation who are observed to have received that treatment. This poses a challenge to the researcher to devise a strategy that will overcome this issue and identify the actual causal effect.

The population used for estimation in this paper is men born between 1935 and 1959 who worked the entire 1979 calender year. Two post-secondary schooling treatments are considered: college graduation and college attendance. The outcome of interest is whether the respondent is employed at the time of the 1980 Census. The analysis will investigate both the full working population as well as just those employed in the private sector. Table 1 provides some basic descriptive statistics for the populations of interest.

In order to identify the causal effect of post-secondary education on unemployment incidence, Vietnam War cohort conscription risk is used as an instrument for education. More than two and a half million men served in uniform within the borders of South Vietnam during the Vietnam War; about one third of these service men were drafted. Nearly 60,000 men were killed in Vietnam and another 75,000 severely disabled. Throughout the war, men could obtain

Table 2.1 Numbers of Observations for Men With Various Characteristics.

	All Sectors		Private Sector	
	White Men	Minority Men	White Men	Minority Men
Observations	1,004,182	117,477	724,033	83,941
	(2.02%)	(4.25%)	(2.42%)	(4.98%)
Completed College	272,933	14,235	167,207	7,760
	(0.75%)	(1.41%)	(0.96%)	(1.87%)
Did Not Complete College	731,249	103,242	556,826	76,181
	(2.49%)	(4.64%)	(2.86%)	(5.30%)
Attended College	477,812	36,319	316,788	22,454
	(1.14%)	(2.59%)	(1.42%)	(3.27%)
Did Not Attend College	526,370	81,158	407,245	61,487
	(2.82%)	(4.99%)	(3.20%)	(5.60%)

Numbers in parenthesis are the percentage unemployed at time of Census.

draft deferments by being enrolled in college. Card and Lemieux (2001) explore in detail the relationship between conscription risk and college attendance and note "these deferments provided a strong incentive to remain in school for men who wanted to avoid the draft." They find that conscription risk varied significantly across cohorts and that these differences are strongly related to college attendance for men of the Vietnam War era; college entry rates of men rose from 54% at the beginning of the war in 1963 to 62% at the peak of the draft in 1968 before slowly declining as the draft was being phased out. Cohort level conscription risk is measured as the average number of inductions during the years a cohort was aged 19 to 22 divided by cohort size (Riddell and Song 2011).[1]

[1]This is the same measurement used by Riddell and Song (2011) when they analyze the effect of college education on labor market transitions and Malamud and Wozniak (2008) when they investigate the effect of college education on geographic mobility.

2.4 Local Average Treatment Effects

2.4.1 LPM

Perhaps the simplest estimation technique at hand involves punting on explicitly modeling the binary nature of unemployment status, as well as the endogenous nature of education, and simply running ordinary least squares (OLS) to estimate a linear probability model (LPM):

$$Y = \beta X + \delta T + \epsilon. \tag{2.2}$$

In this model, δ is the average treatment effect parameter, β are regression parameters for all other covariates, and ϵ is a random disturbance term. Covariates include age, marital status, and state unemployment rate at the end of the second quarter of 1980. Estimates of the ATE stemming from OLS are given in the second column of Table 2.

This model predicts post-secondary education to decrease the risk of unemployment incidence by about 1.5% for white men and between 2.3% and 3.2% for minority men depending on the definition of treatment and sector. These effects tend to be more pronounced in men in the private sector. Also, the effect of college graduation tends to be larger than the effect of simply having attended college, and this difference is most pronounced in minority men in the private sector. These men see a 2.4% reduction of unemployment incidence for attending college and a 3.2% reduction for graduating from college. This model predicts minority men to have a larger response than white men for all treatment in all sectors. In fact, the treatment effect of college graduation is estimated to be double the size for black men than for white men in the private sector. In general, all estimates indicate a considerable reduction in the risk of job loss.

Though the linear assumption is unrealistic, and predicted outcomes can be larger than 1 or less than 0, the LPM still finds its way into the literature. Furthermore, education is surely endogenous. A natural next step is to address this endogeneity. If one has an instrumental variable at hand that reflects exogenous variation in the treatment, the most common approach is two-staged least squares.

2.4.2 2SLS

The 2SLS model is founded on a two equation framework with a structural equation (identical to that of the OLS model) modeling the outcome and a reduced form equation that models the endogenous variable as a function of the IV and all exogenous covariates:

$$Y = \beta_y X + \delta T + \epsilon_1 \tag{2.3}$$

$$T = \beta_t X + \gamma Z + \epsilon_0. \tag{2.4}$$

The key identifying assumptions of the 2SLS model are the rank condition and exclusion restriction:

- **Rank Condition:** $\gamma \neq 0$
- **Exclusion Restriction:** $E[Z'\epsilon_1] = 0$.

The former asserts that the instrument is a relevant regressor in the reduced form model of the treatment, and the latter asserts that the instrument should not be in the structural model of the outcome. Since conscription risk is measured at the cohort level, female cohort level college graduation rates are included as a regressor to help ensure the exclusion restriction is valid.

Results stemming from the 2SLS model are given in the third column of Table 2. These results are substantially larger in magnitude than the OLS results, though the overall trends surrounding sector, definition of treatment, and racial status are similar. This model predicts that white men in the private sector can reduce their risk of job loss by a full 6.7% by having a college degree and by 5.3% for merely attending college. These are substantial gains in job security. Results for minority men much larger. This model predicts post-secondary education to reduce their risk of unemployment incidence anywhere from 8.3% to an unbelievable 74% depending on sector and definition of the treatment. These results for minority men are not significant however, and the extremely large estimates and standard errors are likely due to a weak instruments problem.

The 2SLS estimator essentially side steps explicitly modeling the binary nature of both the treatment and the outcome, and thus is inherently inappropriate given the nonlinear nature of the true model. Some, notably Angrist (2001), have argued that the "difficulties with endogenous variables in nonlinear limited dependent variables models are usually more apparent than real," and that this downfall of 2SLS should be overlooked. He points out that 2SLS seems to provide good estimates in many applications. Yet a study by Bhattacharya, Goldman and McCaffrey (2006) shows that 2SLS tends to perform rather poorly when the the average probability of the dependent variable is close to 0 or 1. Given that unemployment incidence is a rather low probability event, this seems evidence against 2SLS in the current setting.

Furthermore, the presence of a valid instrument itself is not enough to identify any treatment effect (Imbens and Angrist 1994).[2] One common additional assumption is that the treatment effect is homogenous in the population. In the simple linear constant effect model, 2SLS will identify the ATE in the population. However, if the treatment effect is heterogeneous in the population, alternative assumptions are needed. One identifying assumption then is "monotonicity" (Imbens and Angrist 1994).

Monotonicity Assumption: *Define $T(Z)$ as potential treatment under a given instrument realization. For all Z_j, Z_l, either $T_i(Z_j) \geq T_i(Z_l)$ for all i, or $T_i(Z_j) \leq T_i(Z_l)$ for all i.*

Monotonicity assumes that the instrument affects participation in the treatment in the same direction for all individuals. Under this assumption, 2SLS identifies the treatment effect for the "compliers" (those induced into the treatment by the instrument), known as the local average treatment effect (LATE) (Imbens and Angrist 1994).

For most populations, results from the 2SLS approach predict causal effects in the range of 3-5 times as large as those predicted by OLS . These large differences might be due to the two estimates measuring effects for different populations. The compliers are those who only attended college due to the draft deferment. These are individuals who were induced into the treatment by the instrument, and who otherwise would not have attended college. Under a standard model of human capital formation, we might hypothesize these compliers are individuals with lower innate ability. And in such a scenario, these results would imply that lower skilled individuals have greater returns to education - an unexpected result. On the other

[2]The presence of a valid instrument does allow one to sharpen bounds on identification regions. This was shown by Manski (1990) and is explored below.

hand, the large differences in results might plausibly be assumed to stem, at least in part, from the inappropriateness of 2SLS in the current nonlinear model. Either one of these issues might lead one to search for alternative estimators of the treatment effect.

2.4.3 Abadie's Estimator

Alternative estimators are available that can give consistent estimates of the LATE while accounting for the binary nature of the outcome and treatment. Assume for a moment conscription risk is also binary ($Z \in \{0, 1\}$),ie. having high or low risk. Let $T(1)$ be an individual's potential treatment (college degree status) had he 'received' the high risk instrument ($Z = 1$), and $T(0)$ be his potential treatment had he received the low risk instrument ($Z = 0$). Furthermore, let $Y(T(Z))$ be an individual's potential outcome had he received treatment T and instrument Z. Given the binary nature of the treatment and instrument, there are four types of individuals defined by their treatment response to the instrument: $\tau \in \{n, c, d, a\}$ where

- Never Takers: $\tau = n \Leftrightarrow T(1) = 0$ and $T(0) = 0$
- Compliers: $\tau = c \Leftrightarrow T(1) = 1$ and $T(0) = 0$
- Defiers: $\tau = d \Leftrightarrow T(1) = 0$ and $T(0) = 1$
- Always Takers: $\tau = n \Leftrightarrow T(1) = 1$ and $T(0) = 1$.

In this setting, the monotonicity assumption of Imbens and Angrist rules out the possibility of there being "defiers". Assume the following:

- **Independence of the Instrument:** Conditional on X, the random vector $\{Y(1(1)), Y(1(0)), Y(0(1)), Y(0(0)), T(0), T(1)\}$ is independent of Z.
- **Exclusion of the Instrument:** $P[Y(T(1)) = Y(T(0))|X] = 1$ for $T = 0, 1$.
- **First Stage:** $0 < P(Z = 1|X) < 1$ and $P(T(1) = 1|X) > P(T(0) = 1|X)$.
- **Monotonicity:** $P(T(1) \geq T(0)|X) = 1$.

If these conditions hold, then the ATE for the subpopulation of compliers, the LATE, is identified and can be nonparametrically estimated by the standard Wald estimator as:

$$LATE = E[Y(1) - Y(0)|\tau = c] = \frac{E[Y = 1|Z = 1] - E[Y = 1|Z = 0]}{E[T = 1|Z = 1] - E[D = 1|Z = 0]}.$$

Though a simple solution at first glance, problems quickly begin to arise with this estimator. In many cases, the mean independence of the instrument may only hold conditional on some subset of covariates. In the current setting, as discussed above, it seems necessary to control for female graduation rates. Furthermore, the incorporation of additional covariates can reduce the variability of the dependent variable leading to estimates with greater precision (Angrist and Pischke 2009). In theory, the Wald estimator can be modified to handle additional covariates. One could simply estimate the LATE for each X and then compile a weighted average of these conditional LATEs where the weights represent the distribution of X among the compliers. But this simple plug-in estimator can quickly become untenable due to the curse of dimensionality. Also, the distribution of X for the compliers is unknown as the subpopulations of the compliers in unidentifiable.

Here is where Abadie's (2003) estimator becomes attractive. Not only can it incorporate the binary nature of the outcome and treatment while controlling for the endogenous nature of the treatment, but it can also easily allow for the incorporation of additional covariates. In what follows, define the compliers as $T(1) > T(0)$. It can be shown that comparisons by treatment status for compliers has a direct causal interpretation. To see this, note the following from Abadie (2003):

$$E[Y|X, T = 0, T(1) > T(0)] = E[Y(0)|X, Z = 0, T(1) > T(0)] = E[Y(0)|X, T(1) > T(0)].$$

Similarly,

$$E[Y|X, T = 1, T(1) > T(0)] = E[Y(1)|X, Z = 1, T(1) > T(0)] = E[Y(1)|X, T(1) > T(0)].$$

In the first line, the first equality follows in part from the definition of $Y(0)$. Also, since we are conditioning on the population of compliers, $T = 0$ implies $Z = 0$. The second equality follows from the independence of the instrument. The equalities in the second line hold for parallel reasons. Combining these results we have:

$$E[Y|X,T=1,T(1)>T(0)]-E[Y|X,T=0,T(1)>T(0)]=E[Y(1)-Y(0)|X,T(1)>T(0)].$$

What this implies is that, if we could identify the compliers, then comparing their outcomes across treatment statuses would give us their ATE. But the compliers are not identified since we have no way to distinguish them from other subpopulations ("always takers" and "never takers," "defiers" are ruled out by the monotonicity assumption). Abadie's (2003) Theorem 3.1 shows that a function of interest for the compliers can be written as a function of the whole population. This allows the researcher to identify the treatment effect for the compliers (LATE) by using data on the entire population. Specifically, given a function of interest, $g(Y,T,X)$, Abadie shows:

$$E[g(Y,T,X)|T(1)>T(0)]=\frac{1}{P(T(1)>T(0))}E[\kappa\cdot g(Y,T,X)]$$

where

$$\kappa=1-\frac{T\cdot(1-Z)}{P(Z=0|X)}-\frac{(1-T)\cdot Z}{P(Z=1|X)}.$$

Abadie's estimator is designed for a binary instrument. In the current setting proceed by defining the instrument as "low risk" verses "high risk" of conscription.[3] Abadie's procedure follows a three step process:

1. Estimate $\tau(X)=P(Z=1|X)$ and calculate $\hat{\tau}(X_i)$ for each individual.

2. Determine $\kappa_i=1-\frac{T_i(1-Z_i)}{1-\hat{\tau}(X_i)}-\frac{(1-T_i)Z_i}{\hat{\tau}(X_i)}$ for each individual.

3. Solve $(\hat{\alpha},\hat{\beta})=\arg\min_{(\alpha,\beta)\in\Theta}\frac{1}{n}\Sigma_{i=1}^{n}\kappa_i(Y_i-\Phi(\alpha T_i+X_i'\beta))^2$.

Results for this estimator are provided in the last three columns of Table 2. Each column represents a different cut-off point between "low" and "high" conscription risk with the cut-off point increasing from estimator I to estimator III. The estimates resulting from low cut-off points tend to be very similar to those of OLS. But, when the cut-off point is increased, estimates of the causal effect tend to grow substantially higher in magnitude than OLS estimates. For

[3]The results from Abadie's estimator are consistent across various divisions of "high" and "low" risk, all of which are not presented.

example, for minority men in the private sector, the effect of a college degree is estimated at -3.2% by OLS, but at -4.1% by Abadie's estimator with a relatively high cut-off point. This is a large reduction in unemployment risk. That the estimates increase as the cut-off point increases seems to suggest that lower skilled individuals have larger returns to post-secondary schooling, though these differences are not significantly different. The higher cut-off points identify treatment effects for populations that required greater risk of conscription before they would be "pushed" into attending college. And as discussed above, one might reason these are individuals with lower innate ability. While these estimates are slightly larger than the OLS estimates, they are considerably smaller than the 2SLS estimates and are much more plausible.

Another key result to note is that the differences in magnitudes of the effects between white and minority men still shows up here. In fact, when using a high cut-off point and focusing on the effect of college graduation for men in the private sector, the effect for minority men is still nearly double the size as that for white men (-2.3% compared to -4.1%). These results provide evidence that, at least for the complier subpopulation, post-secondary education provides a significant reduction in unemployment incidence; a 4% reduction from risk of unemployment is a hefty gain for obtaining a college degree. Even the 2% reduction white men receive for merely attending college is substantial.

2.5 Average Treatment Effects

The estimators designed to handle the endogeneity of education discussed above only identified the LATE. Though the LATE might in its own right be a parameter of interest, the ATE is a parameter with greater policy implications. Tools for point-identifying the ATE when the outcome and the endogenous treatment are binary are limited. Yet the toolbox expands when we consider a weaker form of identification. In particular, if we open up the door to "partial identification," we have much at our disposal.

2.5.1 Worst Case Bounds

First recall that the average treatment effect (ATE) is defined as

$$\text{ATE} = E[Y(1) - Y(0)|X] = E[Y(1)|X] - E[Y(0)|X]. \tag{2.5}$$

Further, note that we can rewrite $E[Y(T)|X]$ using the law of iterated expectations, where S represents received treatment, as:

$$E[Y(T)|X] \quad = \quad E[Y(T)|X, S = T]P(S = T|X) + E[Y(T)|X, S = T']P(S = T'|X). \tag{2.6}$$

The data identify sample analogues of all of the right hand side quantities except the counterfactual $E[Y(T)|X, S = T']$. This is the expected outcome under treatment T for those in the population who received treatment T'. Though unobserved, since our outcome variable is the employment status of the individual at the time of the Census, this counterfactual is naturally bounded: $E[Y(T)|X, S = T'] \in \{0, 1\}$. This directly leads to upper and lower bounds on $E[Y(1)|X]$ and $E[Y(0)|X]$. Bounds on the ATE are then given by the difference in the bounds of $E[Y(1)|X]$ and $E[Y(0)|X]$:

$$ATE_{ub} = E[Y(1)|X]_{ub} - E[Y(0)|X]_{lb} \tag{2.7}$$

$$ATE_{lb} = E[Y(1)|X]_{lb} - E[Y(0)|X]_{ub}. \tag{2.8}$$

Applying this natural bounded outcome space yields the worst case bounds on the ATE:

$$ATE_{ub} = \{\underbrace{E[Y(1)|X, S = 1]P(S = 1|X) + P(S = 0|X)}_{E[Y(1)|X]_{ub}}\} - \underbrace{E[Y(0)|X, S = 0]P(S = 0|X)}_{E[Y(0)|X]_{lb}} \tag{2.9}$$

$$ATE_{lb} = \underbrace{E[Y(1)|X, S = 1]P(S = 1|X)}_{E[Y(1)|X]_{lb}} - \{\underbrace{E[Y(0)|X, S = 0]P(S = 0|X) + P(S = 1|X)}_{E[Y(0)|X]_{ub}}\}. \tag{2.10}$$

Results for the worst case bounds are provided in Table 3. These bounds cover a span of one (the size of the outcome space) and necessarily include zero. While these bounds are fairly wide, and in general not very informative, they cut in half the possible range of values for the ATE; ex ante the ATE could lie in the range of $[-1, 1]$. For white men, the results imply post-secondary education reduces unemployment risk by no more that 25-50% for white men and 15-30% for minority men depending on the sector of employment and whether the treatment is defined as college graduation or only attendance.

2.5.2 Bounds with IV

Within the bounding framework, an instrumental variable can be called to tighten the worst case bounds. Consider a *mean independence* form of the standard IV condition:

IV Assumption *Covariate Z is an instrumental variable if, for each T, each value of X, and all $(V, V') \in (Z \times Z)$,*

$$E[Y(T)|X, Z = V'] = E[Y(T)|X, Z = V].$$

Imposing the IV assumption will tighten the bounds on $E[Y(1)|X]$ and $E[Y(0)|X]$. In what follows, the instrument is discrete. The implementation of the IV assumption in this framework is straightforward. First, the researcher separates the data according to instrument realizations. Then, upper and lower bounds are found for each $E[Y(T)|X, Z = V]$ as defined by the worst case bounds. With slight abuse of notation, let us denote these $UB_T|V$ and $LB_T|V$. Imposing the IV assumption then yields bounds on the treatment effect which are derived as follows:

$$ATE_{ub} = \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_1|V'\big] - \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_0|V'\big] \tag{2.11}$$

$$ATE_{lb} = \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_1|V'\big] - \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_0|V'\big]. \tag{2.12}$$

There is a caveat in this setting regarding conditioning the on covariates and the assumed independence of the instrument. It was mentioned above that cohort level female graduation rates were controlled for to help ensure the exclusion restriction held. Though this variable is controlled for in this current partial identification framework, it cannot be "completely" controlled for due to the nature of the nonparametric approach. If we were to separate populations into separate "bins" according to female graduation rates, there would not be any variation within the bins with regards to the instrument. This is because both move together as they are measured at the cohort level. The previous models exploited their linear framework to deal with this, but in the nonparametric framework here, this is not possible. To overcome this, I simply condition on "low," "medium," and "high" female graduation rates.

Results stemming from the imposition of the IV are given in Table 3. Imposing the IV on the worst case bounds reduces the span of the identification region by 10-20% depending on the population of interest. For some subpopulations, notably minority men in the private sector, this gain is quite informative. For this population, college graduation is found to reduce the risk of unemployment by at most 11%. Furthermore, these lower bounds rule out the results from the 2SLS model for the effect of college graduation in minority men.

2.5.3 Threshold Crossing Model For Outcome

Adding the assumption that the outcome Y is determined by a latent threshold-crossing process adds structure to Manski's general framework (Chiburis 2010). Assume unemployment incidence can be modeled as follows:

$$Y(1) = I[f_1(X) - \upsilon > 0]$$
$$Y(0) = I[f_0(X) - \upsilon > 0].$$

Here $f_1(X)$ and $f_0(X)$ are functions that map X into the real line, υ is a random disturbance term, and $I[\ ]$ is the indicator function. This added assumption restricts the joint bounds on $E[Y(1)|X]$ and $E[Y(0)|X]$. Under the threshold crossing assumption, $E[Y(1)|X]$ and $E[Y(0)|X]$ are confined to the union of two subsets of the Manki worst case bounds. These are represented graphically in Figure 1 and are given by:

$$A \left\{ \begin{array}{c} E[Y|X,S=1]P(S=1)+E[Y|X,S=0]P(z=0) \\ \leq E[Y(1)|X] \leq \\ E[Y|X,S=1]P(S=1)+P(S=0) \\ \\ E[Y|X,S=0]P(S=0) \\ \leq E[Y(0)|X] \leq \\ E[Y|X,S=1]P(S=1)+E[Y|X,S=0]P(S=0) \end{array} \right\}$$

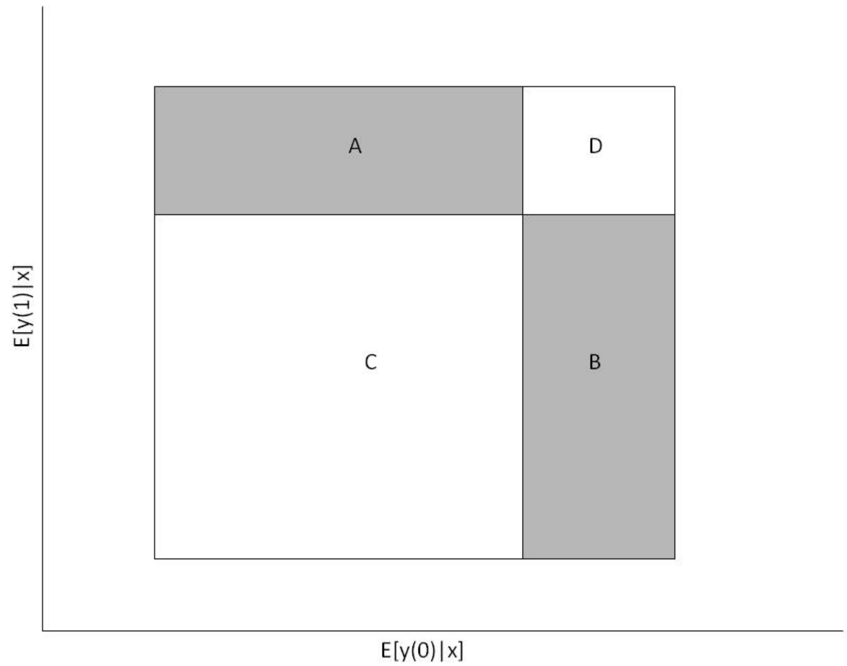
or

$$B \left\{ \begin{array}{c} E[Y|X,S=1]P(S=1) \\ \leq E[Y(1)|X] \leq \\ E[Y|X,S=1]P(S=1)+E[Y|X,S=0]P(S=0) \\ \\ E[Y|X,S=1]P(S=1)+E[Y|X,S=0]P(S=0) \\ \leq E[Y(0)|X] \leq \\ E[Y|X,S=0]P(S=0)+P(S=1) \end{array} \right\}$$

All quantities in the above bounds are identified by the data. The upper bound on $E[Y(1)|X]$ given in A and its lower bound given in B are identical to Manski's worst case bounds. Similarly, the upper bound on $E[Y(0)|X]$ given in B and its lower bound given in A are identical to Manski's worst case bounds. The two regions A and B meet at a single point where $E[Y(1)|X] = E[Y(0)|X] = E[Y(1)|X,S=1]P(S=1)+E[Y(0)|X,S=0]P(S=0) = E[Y|X]$. The threshold crossing assumption rules out regions C and D. To see this, note that, in region C, $E[Y(0)|X] < E[Y|X]$. This implies $E[Y(0)|X,S=1] < E[Y(1)|X,S=1]$ which in turn implies $f_0(X) < f_1(X)$;[4] but this then rules out the possibility of $E[Y(1)|X] < E[Y|X]$. A parallel argument holds for why region D is ruled out. This added assumption alone does not tighten the bounds on the ATE over Manski's worst case bounds. As can be seen in the figure, the upper left corner and lower right corner represent the worst case bounds and are contained

[4] The first inequality can be seen by decomposing $E[Y(0)|X]$ and $E[Y|X]$ by the law of iterated expectations and noting the only differences are the terms $E[Y(0)|X,S=1]$ and $E[Y(1)|X,S=1]$. The second inequality then follows from the first by the separability of the disturbance term in the threshold crossing assumption.

Figure 2.1 Comparison of bounds under Threshold Crossing and Manski Worst Case



in the shaded area (for any pair of $E[Y(1)|X]$ and $E[Y(0)|X]$ in C or D an identical ATE can be found by tracing a 45° line into a shaded region).

However, once an IV assumption is imposed, this threshold crossing assumption does restrict the bounds of the ATE when compared with Manski's IV bounds. In particular, the bounds will in general be a disjoint set with a null region around zero thus ruling out $ATE_X = 0$ (Chiburis 2010). This can be seen graphically in Figure 2. The outer rectangle is still identical to the IV bounds of Manski, yet now the threshold crossing assumption restricts the bounds on the ATE through the disjoint nature of A and B. The bounds under the IV assumption, coupled with the threshold crossing assumption, are formed in a parallel fashion as the Manski IV bounds. Again, first separate the data according to instrument realizations. Then, each $E[Y(T)|X, Z = V]$ is restricted to two regions identified by upper and lower bounds (regions A and B in Figure 1). Denote them as $\{UB_{TA}|V, LB_{TA}|V\}$ and $\{UB_{TB}|V, LB_{TB}|V\}$. Finally, the IV assumption is imposed and bounds on the ATE are formed yielding two disjoint identification regions:

$$ATE \in \left\{ \begin{array}{c} \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_{1A}|V'\big] - \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_{0A}|V'\big] \\ \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_{1A}|V'\big] - \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_{0A}|V'\big] \end{array} \right\}$$

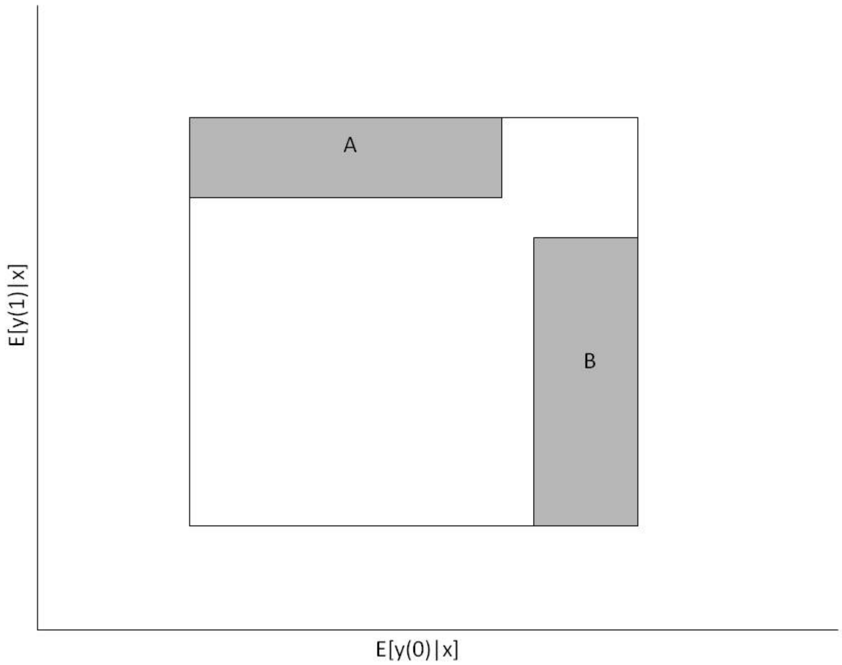
or

$$ATE \in \left\{ \begin{array}{c} \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_{1B}|V'\big] - \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_{0B}|V'\big] \\ \sum_{V \in Z} Pr(V)\big[\max_{V'} LB_{1B}|V'\big] - \sum_{V \in Z} Pr(V)\big[\min_{V'} UB_{0B}|V'\big] \end{array} \right\}$$

A concern when estimating bounds with IVs is that analog estimates of such bounds exhibit finite-sample bias which lead the bounds to be narrower (more optimistic) than the true bounds. By Jensen's Inequality, the estimated lower bound on $E[Y(0)|X, T = 1]$ is biased upwards because of the maxima operator, and the estimated upper bound is biased downward because of the minima operator. To counter this bias, I implement a correction proposed by Kreider and Pepper (2007). The approach is to estimate the bias by using the bootstrap distribution and then adjust the analogue estimate in accordance with the estimated bias. For a random sample of size N, let LB_N be the analogue estimate of the lower bound in question, and let $E^b(LB_N)$ be the mean of the estimate from the bootstrap distribution (a parallel procedure is used for an upper bound). The bias is then estimated as $E^b(LB_N) - LB_N$. The bias-corrected estimate is then $LB_N - \big[E^b(LB_N) - LB_N\big] = 2LB_N - E^b(LB_N)$. While heuristic and not derived from theory, this correction seems reasonable and performs well in Monte Carlo simulations (Manski and Pepper, 2009). Furthermore, given the large sample size in the current study this bias is rather small.

Results for the bounds on the ATE under the threshold crossing and IV assumption are given in Table 4 and 5. Adding this threshold crossing assumption does not alter the absolute upper and lower bound on the ATE, but does open up a symmetric null region around zero. This region ranges in size from 3.1% for white men in any sector to 7.6% for minority men in the private sector. For example, when looking at the effect of college graduation on men in all sectors, the bounds derived under the threshold crossing model form a null region around zero large enough to rule out the OLS estimates for these population. For minority men in the private sector, the bounds derived here rule out the region $[-0.033, 0.033]$ for the causal effect

Figure 2.2 Comparison of bounds under Threshold Crossing and Manski's Bounds with an IV



of college graduation on unemployment incidence. This implies there is a substantial effect for the population in general, not just the complier subpopulation.

The trends regarding treatment definition and racial status found in previous results show up in these bounds as well. The null region around zero tends to be larger for minority men. When looking at the effect of college graduation, the region is $[-0.033, 0.033]$ for minority men in the private sector compared to $[-0.017, 0.017]$ for white men. Similarly the null region tends to be larger when looking at the effect of college graduation than when investigating the effect of college attendance. For minority men in the private sector, this difference is about 33%: $[-0.033, 0.033]$ compared to $[-0.024, 0.024]$. Across all definitions of sector, treatment, and race, the results here imply substantial treatment effects of post-secondary education on unemployment incidence.

2.5.4 Biprobit and Probit Models

Until recently, binary endogenous regressors in nonlinear models were generally handled with bivariate probit models. These stem from extending the threshold crossing assumption to the treatment selection mechanism and assuming a linear parameterization. This yields the following linear latent index model:

$$\begin{aligned}
Y &= I(Y^* \geq 0) \\
Y^* &= \beta_y X + \delta T + \epsilon_y \\
T &= I(T^* \geq 0) \\
T^* &= \gamma Z + \epsilon_t.
\end{aligned}$$

It is convention to assume the error terms are distributed jointly normal and then estimate the model with maximum likelihood. After fitting this "biprobit" model, estimates of the ATE can be obtained through simulations. Though this model yields consistent estimates of the ATE if the distribution of the error terms is correctly specified, it is well documented that results are sensitive misspecification of the actual data generating process. Thus, if the true underlying distribution of the error terms is not bivarite normal, results may be unreliable. One might be inclined to ask if there is a two step process similar to 2SLS that can allow one to get away from this strong distributional assumption. Blundell and Powell (2004) do introduce such a procedure, a semiparametric 'control function' approach, which allows the researcher to explicitly model the binary nature of the outcome while relaxing the joint distributional assumption. Unfortunately, this approach is only valid if the endogenous variable is continuous, and so is not available in the current setting.

Estimates of the treatment effect under the biprobit model are provided in Table 4 and 5. Results predict a treatment effect of anywhere from -2.8% for the effect of college graduation on all men in the private sector to -17% when looking at the effect of college attendance in minority men in the private sector. In general these results tend to be rather large when compared to the OLS results and parallel those found under the 2SLS model. These results would imply minority men in the private sector can reduce their unemployment risk by 17% by merely attending college. Similarly, these results would lead one to believe white men in the private sector could reduce their unemployment risk by 4.3% by merely attending college. These imply huge gains when one considers the relatively low probability of unemployment incidence.

If the researcher is willing to add one more assumption, that the correlation between the two error terms in the above model is zero, then we again have a model that assumes exogeneity of the treatment effect. This is a standard linear latent index model of unemployment

$$Y = I(Y^* \geq 0) \tag{2.13}$$

$$Y^* = \beta X + \delta T + \epsilon \tag{2.14}$$

which incorporates the binary nature of unemployment, though not the endogeneity of education, and can be estimated with a Logit or Probit model. Results for the probit model are given in the last column of Table 4 and 5 and tend to be nearly identical to those coming from the LPM. This model predicts post-secondary education to decrease the risk of unemployment incidence by about 1.5% for white men and between 2.2% and 3.1% for minority men depending on definition of treatment and sector. In general, the trends regarding treatment definition, sector, and racial status found in previous results again show up in these results.

2.6 Conclusion

Post-secondary education has pronounced correlations with multiple labor market outcomes including unemployment incidence. However, due to the fact that individuals self-select into education, even when conditioning on a large set of observed characteristics, these correlations cannot be taken at face value as actual causal effects. Previous studies of the effect of education on unemployment incidence have either ignored this endogeneity issue or used identification strategies that are inappropriate given the binary nature of employment status. This paper readdresses this issue with recent advances in econometrics that point-identify the LATE and partially identify the ATE. Using data from the 1980 U.S. Census, which overlaps a recessionary period in the U.S., I find evidence for a causal effect of post-secondary education on unemployment incidence.

For white men "compliers" in the private sector, college graduation reduces the risk of unemployment incidence by 2.2% while college attendance reduces the risk by 1.8%. The effect on minority men is larger at 2.7% and 2.5%. Furthermore, if unobserved ability of the "complier" subpopulation is inversely correlated with the degree of conscription risk needed to induce college attendance, then the results found here imply the impact of education on unemployment might be diminishing in skill level with largest beneficial impacts accruing to the lowest ability workers. Also, the causal effect of both college graduation and attendance in minority men tend to be larger than the effect in white men.

Results for the ATE of post-secondary education on unemployment incidence are not as strong as this effect is only partially identified, though for some populations the estimated bounds are quite informative. In particular, the threshold crossing model with an IV rules out a region around zero, thus giving evidence of a causal effect in the general population. This region ranges in size from 3.1% for white men in any sector to 7.6% for minority men in the private sector. Furthermore, when looking at the effect of college graduation on men in all sectors, the bounds derived under the threshold crossing model rule out the OLS estimates for these population. The size of these null regions are evidence of a substantial causal effect of post-secondary schooling on unemployment incidence.

Table 2.2 Various Estimates of the Effect of Education on Unemployment Incidence.

Population	1st Stage	OLS	2SLS	Abadie I	Abadie II	Abadie III
		Effect of College Attendance: all sectors				
All Men	0.449	-0.015	-0.047	-0.015	-0.016	-0.020
	(0.0199)	(0.0003)	(0.0134)	(0.0009)	(0.0009)	(0.0020)
Minority Men	0.416	-0.023	-0.083	-0.019	-0.024	-0.026
	(0.0583)	(0.0012)	(0.0618)	(0.0037)	(0.0039)	(0.0099)
White Men	0.447	-0.015	-0.046	-0.016	-0.016	-0.020
	(0.0211)	(0.0003)	(0.0135)	(0.0009)	(0.0009)	(0.0018)
		Effect of College Attendance: private sector only				
All Men	0.329	-0.016	-0.058	-0.018	-0.018	-0.020
	(0.0234)	(0.0004)	(0.0238)	(0.0011)	(0.0011)	(0.0026)
Minority Men	0.331	-0.024	-0.119	-0.025	-0.026	-0.025
	(0.0661)	(0.0017)	(0.1002)	(0.0047)	(0.0049)	(0.012)
White Men	0.325	-0.016	-0.053	-0.018	-0.018	-0.021
	(0.0249)	(0.0003)	(0.0242)	(0.0011)	(0.0011)	(0.0023)
		Effect of College Graduation: all sectors				
All Men	0.429	-0.015	-0.050	-0.017	-0.018	-0.023
	(0.0174)	(0.0003)	(0.0141)	(0.0013)	(0.0012)	(0.016)
Minority Men	0.152	-0.028	-0.227	-0.022	-0.036	-0.039
	(0.0413)	(0.0018)	(0.1766)	(0.0084)	(0.0053)	(0.0061)
White Men	0.454	-0.014	-0.045	-0.017	-0.016	-0.021
	(0.0188)	(0.0003)	(0.0133)	(0.0011)	(0.0012)	(0.0016)
		Effect of College Graduation: private sector only				
All Men	0.239	-0.017	-0.079	-0.022	-0.021	-0.024
	(0.0195)	(0.0004)	(0.0329)	(0.0014)	(0.0014)	(0.021)
Minority Men	0.0535	-0.032	-0.741	-0.027	-0.038	-0.041
	(0.0434)	(0.0026)	(0.8376)	(0.0112)	(0.0067)	(0.0078)
White Men	0.256	-0.016	-0.067	-0.022	-0.021	-0.023
	(0.0212)	(0.0004)	(0.0307)	(0.0013)	(0.0013)	(0.0020)

Numbers in parenthesis are standard errors.

All regressions control for age, marital status, and state unemployment rate.

Table 2.3 Multiple Estimates of Bounds on the Effect of Education on Unemployment Incidence.

	Worst Case Bound				Bound Under IV			
Population	LB-	LB	UB	UB+	LB-	LB	UB	UB+
				Effect of College Attendance: all sectors				
All Men	-0.470	-0.469	0.531	0.532	-0.389	-0.387	0.470	0.474
Minority Men	-0.337	-0.336	0.664	0.665	-0.304	-0.294	0.620	0.630
White Men	-0.486	-0.485	0.515	0.516	-0.400	-0.398	0.452	0.456
				Effect of College Attendance: private sector only				
All Men	-0.435	-0.434	0.566	0.567	-0.385	-0.380	0.515	0.520
Minority Men	-0.300	-0.299	0.701	0.702	-0.273	-0.257	0.667	0.677
White Men	-0.450	-0.449	0.551	0.552	-0.396	-0.390	0.496	0.521
				Effect of College Graduation: all sectors				
All Men	-0.276	-0.275	0.725	0.726	-0.197	-0.195	0.662	0.663
Minority Men	-0.161	-0.160	0.840	0.841	-0.147	-0.140	0.797	0.806
White Men	-0.229	-0.288	0.712	0.713	-0.204	-0.202	0.647	0.650
				Effect of College Graduation: private sector only				
All Men	-0.240	-0.239	0.761	0.762	-0.191	-0.189	0.709	0.712
Minority Men	-0.140	-0.139	0.861	0.862	-0.124	-0.116	0.825	0.835
White Men	-0.252	-0.251	0.749	0.750	-0.209	-0.207	0.686	0.688

LB and UB are estimated bounds and LB- and UB+ are confidence intervals for those bounds.

Table 2.4 Estimates of Bounds on the Effect of College Attendance on Unemployment Incidence.

	Region I				Region II				Biprobit	Probit
Population	LB-	LB	UB	UB+	LB-	LB	UB	UB+	Pt Est	Pt Est
	Effect of College Attendance: all sectors									
All Men	-0.389	-0.387	-0.015	-0.014	0.014	0.015	0.470	0.474	-0.038	-0.015
									(0.0059)	(0.0003)
Minority Men	-0.304	-0.294	-0.025	-0.022	0.022	0.025	0.620	0.630	-0.116	-0.022
									(0.0319)	(0.0011)
White Men	-0.400	-0.398	-0.014	-0.013	0.013	0.014	0.452	0.456	-0.037	-0.014
									(0.0061)	(0.0003)
	Effect of College Attendance: private sector only									
All Men	-0.385	-0.380	-0.015	-0.014	0.014	0.015	0.515	0.520	-0.040	-0.016
									(0.0085)	(0.0003)
Minority Men	-0.273	-0.257	-0.024	-0.020	0.020	0.024	0.667	0.677	-0.170	-0.022
									(0.024)	(0.0014)
White Men	-0.396	-0.390	-0.014	-0.013	0.013	0.014	0.496	0.521	-0.043	-0.015
									(0.0104)	(0.0003)

Numbers in parenthesis are standard errors for point estimates under the bipobit and probit models.

LB and UB are estimated bounds and LB- and UB+ are confidence intervals for those bounds.

Table 2.5 Estimates of Bounds on the Effect of College Graduation on Unemployment Incidence.

	Region I				Region II				Biprobit	Probit
Population	LB-	LB	UB	UB+	LB-	LB	UB	UB+	Pt Est	Pt Est
Effect of College Graduation: all sectors										
All Men	-0.197	-0.195	-0.022	-0.020	0.020	0.022	0.662	0.663	-0.029	-0.016
									(0.0035)	(0.0002)
Minority Men	-0.147	-0.140	-0.034	-0.030	0.030	0.034	0.797	0.806	-0.058	-0.028
									(0.0094)	(0.0012)
White Men	-0.204	-0.202	-0.020	-0.019	0.019	0.020	0.647	0.650	-0.049	-0.014
									(0.0069)	(0.0002)
Effect of College Graduation: private sector only										
All Men	-0.191	-0.189	-0.021	-0.020	0.020	0.021	0.709	0.712	-0.028	-0.017
									(0.0034)	(0.0003)
Minority Men	-0.124	-0.116	-0.033	-0.028	0.028	0.033	0.825	0.835	-0.095	-0.031
									(0.0166)	(0.0017)
White Men	-0.209	-0.207	-0.017	-0.016	0.016	0.017	0.686	0.688	-0.032	-0.016
									(0.0033)	(0.0003)

Numbers in parenthesis are standard errors for point estimates under the bipobit and probit models.

LB and UB are estimated bounds and LB- and UB+ are confidence intervals for those bounds.

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CHAPTER 3. Monotone Instrumental Variables and Binary Treatments

3.1 Abstract

This paper investigates monotone instrumental variables (MIV) and their ability to aide in identifying treatment effects when treatment is binary. I show that if the treatment is monotonic in the instrument, as is routinely assumed in the literature on instrumental variables, then for the MIV to have identifying power on both the lower and upper bounds of the treatment effect, the conditional-on-received-treatment outcomes cannot have the same monotonicity assumed in the MIV assumption. This clutters the cleanliness of the economic theory surrounding many MIVs and places potentially untenable restrictions on the unobserved counterfactuals that should be investigated. I prove this proposition within a potential outcomes framework, investigate the inclusion of other assumptions, and explore the implications for empirical work. Results are highlighted with an application investigating the effect of criminal convictions on job tenure using data from the 1997 National Longitudinal Survey of the Youth (NLSY). Though the main results are shown to hold only for the binary treatment case, they are shown to have important implications even for the multi-valued treatment case.

3.2 Introduction

Endogenous variables are often encountered in empirical work. In order to identify these variables' causal effects on outcomes, econometricians often rely on some form of instrumental variable (IV) assumption. In this vein, Manski and Pepper (2000) introduced a Monotone Instrumental Variable (MIV) assumption. The main appeal of their MIV assumption is its gain in credibility over a traditional IV assumption. The necessary exclusion restriction in a standard IV approach is commonly an area of contention that cannot be verified with data but must be argued from economic theory. Relaxing this restriction to one that merely postulates that an instrument affects the outcome in a monotone fashion opens up a large array of viable candidates, and the disagreement over its legitimacy is bound to be reduced. Moreover, in many cases, even this weakened assumption can have significant identifying power and has become an increasingly popular identifying assumption (Gonzalez 2005, Gerfin and Schellhorn 2006, Haan 2011, Kang 2011).

A key distinction between statistics and econometrics is the latter's focus on causation; it is out of this focus that the IV literature grew. Inherent in this literature is the reliance on economic theory to support underlying assumptions. Thus, when a researcher proposes an MIV, much like an IV, she is proposing a story regarding the relationship between the instrument, the treatment, and the outcome. In this sense, an MIV's appeal over a traditional IV stems from hope its story will be more believable. I show in this paper, that for binary treatments, an MIV cannot have identifying power on both the upper and lower bounds of the treatment effect if the treatment is monotonic in the instrument and the conditional-on-received-treatment outcomes have the same monotonicity assumed by the MIV. If the MIV does lend identifying power on both upper and lower bounds, then one of these conditions must not hold. Not only would this seem to muddle the underlying story of the MIV in many cases, but it also puts constraints on the relationship between unobserved counterfactuals that must themselves be credible.

This paper proves the above proposition within a general potential outcomes framework, investigates the inclusion of other assumptions, and explores the implications for empirical work. These results are then highlighted with an application investigating the effect of criminal convictions on job match quality. Though the main results are shown to hold only for the binary treatment case, they are shown to have important implications even for the multi-valued treatment case.

3.3 Set Up and Worst Case Bounds

Begin by assuming treatment is binary, say whether or not one has obtained a college degree, and the outcome of interest is earnings. Define t as a potential treatment and z as the realized treatment. Suppose the goal is to estimate the average treatment effect (ATE), defined as:

$$ATE = E[y(1)] - E[y(0)] \tag{3.1}$$

where y(1) indicates earnings under a college degree treatment and y(0) indicates earnings under a non-college treatment.[1] There is reason to believe education is endogenous in an earnings equation, implying $E[y(t)|z = t] \neq E[y(t)]$, and the ATE is not identified by the data alone because what is observed is $E[y(t)|z = t]$ and not $E[y(t)]$. A researcher might aim to bound the treatment effect. Bounds on the ATE can be achieved by bounding the unknowns $E[y(1)]$ and $E[y(0)]$. The following focuses on one value, $E[y(1)]$, as similar arguments will hold for the other.

By iterated expectations

$$E[y(1)] = E[y(1)|z = 1] \cdot P(z = 1) + E[y(1)|z = 0] \cdot P(z = 0). \tag{3.2}$$

The data identify all of the right hand side quantities except the counterfactual $E[y(1)|z = 0]$. But, if this counterfactual has a naturally bounded outcome space, defined by K_L and K_U, we can define Manski's (1990) worst case bounds on our unknown:

$$\begin{aligned} &E[y(1)|z = 1] \cdot P(z = 1) + K_L \cdot P(z = 0) \\ &\leq E[y(1)] \leq \\ &E[y(1)|z = 1] \cdot P(z = 1) + K_U \cdot P(z = 0). \end{aligned} \tag{3.3}$$

[1] Conditioning on additional covariates is left out to simplify notation. But the inclusion does not alter the results of the paper.

Note all of terms defining these bounds are identified by the data. The worst case bounds in Equation 3 lead directly to worst case bounds on the ATE:

$$ATE_{ub} = E[y(1)|x]_{ub} - E[y(0)|x]_{lb}$$

$$ATE_{lb} = E[y(1)|x]_{lb} - E[y(0)|x]_{ub}.$$

3.4 Assumptions and Identification

3.4.1 The MIV Assumption and Proposition 1

Manski and Pepper (2000) introduce the concept of an MIV, a weakened form of an IV assumption, that can aid in identification by tightening the bounds in Equation (3).

Assumption I (M-P Eq.2) MIV: *Let V be an ordered set. Covariate v is a monotone instrumental variable if, for each $t \in T$ and all $(u, u') \in (V \times V)$ such that $u_2 \geq u_1$,*

$$E[y(t)|v = u_2] \geq E[y(t)|v = u_1].$$

If the instrument v is test scores, this assumption implies that individuals with higher test scores have weakly higher mean wage functions.

Following Manski and Pepper, $E[y(1)]$ can be bounded under the MIV assumption by (M-P Eq. 7)

$$\sum_{u \in V} P(u) \Big\{ \sup_{u' \leq u} \left[E[y(1)|v = u', z = 1] \cdot P(z = 1|v = u') + K_L \cdot P(z = 0|v = u')\right\} \\ \leq E[y(1)] \leq \qquad (3.4) \\ \sum_{u \in V} P(u) \Big\{ \inf_{u'' \geq u} \left[E[y(1)|v = u'', z = 1] \cdot P(z = 1|v = u'') + K_U \cdot P(z = 0|v = u'')\right\}.$$

If the bounds defined in Equation 4 exhibit the same monotonicity in the instrument as assumed by the MIV assumption, then the MIV has no identifying power. For the MIV to have any 'bite,' there must exist a region of the instrument in which the bounds run counter to the monotonicity of the MIV assumption. Define Treatment Monotonicity as follows:

Condition TM: Treatment Monotonicity *Let V be an ordered set. For all $(u, u') \in (V \times V)$ such that $u_2 \geq u_1$,*

$$P(t = 1|v = u_2) \geq P(t = 1|v = u_1).$$

TM would imply, in the current example, that higher test scores weakly increase the probability of graduating from college. Though some form of this assumption is required in the standard IV literature (the rank condition), this is not explicitly needed in the general framework set up by Manski and Pepper. The only requirement for the MIV to have identifying power is that the pair (y, z) not be independent of the instrument (Manski 2003). Yet, in many instances, this TM condition is embedded in the story surrounding the MIV proposed by the econometrician, either implicitly or explicitly.[2] Define conditional MIV as follows:

Condition CM: Conditional MIV *Let V be an ordered set. Covariate v is a conditional monotone instrumental variable if, letting z be received treatment, for each $t \in T$ and all $(u, u') \in (V \times V)$ such that $u_2 \geq u_1$,*

$$E[y(t)|z = t, v = u_2] \geq E[y(t)|z = t, v = u_1].$$

This assumption differs from Manski and Pepper's MIV assumption by proposing observed outcomes conditional on received treatment are monotonic in the instrument. This should not be confused with Manski and Pepper's Monotone Treatment Response (MTS) assumption, which while also conditions on received treatment, uses received treatment as the instrument.

The main finding of this paper is the following proposition.

Proposition 1 *An MIV assumption cannot provide identifying power on both sides of the unknown, $E[y(t)]$, when the treatment is binary if Conditions TM and CM hold.*

The implications of Proposition 1 directly affect the relationship between the underlying economic motivation of many MIVs and their source of identification. It was noted above that Condition TM is implicitly assumed in many MIVs. For example, Haan (2011) investigates the causal effect of parents' education on child schooling. In her analysis she uses grandparents' education as an MIV for parents' education. In her analysis, she also assumes parents' education will not lower the amount of schooling a child attains. The combination of these two assump-

[2] Note however, that this monotonicity is not the same monotonicity assumption of Imbens and Angrist (1994). The monotonicity of Imbens and Angrist assumes every individual responds in the same way to exposure of the instrument; there does not exist a person who goes to school with test score u_1 but does not go to school with test score u_2 when $u_2 > u_1$. Their's is a non-verifiable identifying assumption, the TM assumption here can be verified with data.

tions implicitly assumes Condition TM. In another recent paper, Kang (2011) investigates the effect of family size on educational investments in children. In his analysis he makes use of the sex of the first born child as an MIV for family size. Explicit in his story is son preference; he assumes families with first born daughters are more likely to have larger families, i.e. condition TM. So one can see that, although TM is not explicitly a necessary condition in the general framework, it is commonly assumed within the underlying economic motivation of many MIVs.

If Condition TM does in fact hold, Proposition 1 implies that an MIV can have identifying power on the lower bound of $E[y(1)]$ only if the conditional-on-received-treatment outcomes are not monotonic in the same direction as the MIV assumption. That is, there must exist a range of the instrument for which $u_2 > u_1$ implies $E[y(1)|z = 1, v = u_1] > E[y(1)|z = 1, v = u_2]$. What does this imply in practice? In Haan's (2011) analysis, she investigates the effect of a child's mother having a college degree (the binary treatment) on a child's expected years of schooling (the outcome). The MIV assumption is that, though a child's level of schooling may not be mean independent of grandparents' education level (the MIV), it is related in a monotonic fashion (grandparents' education will not lower a child's schooling outcome). So if the TM condition holds (grandparents' education does not lower the probability that a child's mother will attain a college degree), then for the MIV to have any bounding power on the lower bound of a child's expected years of schooling if their mother has a college degree ($E[y(1)]$), it must be that, in some range, greater grandparents' education ($u_2 > u_1$) leads to lower expected years of schooling for children whose mother did receive a college degree ($E[y(1)|z = 1]$). This seems an odd, and in many ways undesirable, way of achieving identification.

But Condition CM need not hold for the MIV assumption to be a valid assumption. This, in its most famous form, is Simpson's Paradox. But this is simply a statistical fact. An econometrician must harmonize this 'paradox' with the story underlying the MIV. So what would this mean for the treatment effect investigated by Haan (2011)? It would mean, for example, that children whose mother has a college degree do not exhibit the assumed monotonicity in grandparents' education: children with grandparents with high levels of education actually achieve less years of schooling than children whose grandparents have lower levels of education. Then, for the MIV assumption to be valid, it must be that those children whose mother did not receive a college degree, had she, would exhibit the monotonicity of the MIV to a great enough degree to overcome this observation. Before we explore the implications of this further, and its meaning for empirical work, I prove Proposition 1.

3.4.2 Proof of Proposition 1

In what follows I prove Proposition 1 for $E[y(1)]$, a parallel argument holds for $E[y(0)]$. For $E[y(1)]$ it is the lower bound where MIV has no identifying power, and for $E[y(0)]$ it is the upper bound.

Proof Part A: Upper Bound

Assume Assumptions I (MIV) and Conditions TM and CM hold and treatment is binary.

For the upper bounds to decrease implies that there exists a pair $u_2, u_1 \in V$ such that $u_2 > u_1$ and

$$\begin{gathered} E[y(1)|v = u_1, t = 1] \cdot P(t = 1|v = u_1) + K_u \cdot P(t = 0|v = u_1) \\ > \\ E[y(1)|v = u_2, t = 1] \cdot P(t = 1|v = u_2) + K_u \cdot P(t = 0|v = u_2) \end{gathered} \tag{3.5}$$

Note that

$$K_u \geq P[y(1)|v = u_2, t = 1] \geq P[y(1)|v = u_1, t = 1].$$

The first inequality holds by the definition of the upper bound (K_u) and the second holds by Condition CM. For the inequality in Equation (5) to hold, this implies it must be that $P(t = 1|v = u_1) < P(t = 1|v = u_2)$, which is consistent with Condition TM.[3] So no issue arises here; however, an issue does arise when we examine the lower bound.

[3]Since $P[y(1)|v = u_2, t = 1] \geq P[y(1)|v = u_1, t = 1]$, for (5) to hold $P(t = 0|v = u_1)$ must be greater than $P(t = 0|v = u_2)$ thus putting more weight on the upper bound K_u. This implies, due to the dichotomy of the treatment, that $P(t = 1|v = u_1) < P(t = 1|v = u_2)$.

Proof Part B: Lower Bound

For the lower bounds to decrease implies that there exists a pair $u_2, u_1 \in V$ st $u_2 > u_1$ and

$$E[y(1)|v = u_1, t = 1] \cdot P(t = 1|v = u_1) + K_l \cdot P(t = 0|v = u_1) \\ > \\ E[y(1)|v = u_2, t = 1] \cdot P(t = 1|v = u_2) + K_l \cdot P(t = 0|v = u_2). \tag{3.6}$$

Note that

$$P[y(1)|v = u_2, t = 1] \geq P[y(1)|v = u_1, t = 1] \geq K_l.$$

The first inequality holds by Condition CM and the second holds by the definition of the lower bound (K_l). For the inequality in Equation (6) to hold, this implies it must be that $P(t = 1|v = u_1) > P(t = 1|v = u_2)$.[4] But this contradicts Condition TM. ■

3.4.3 Implications of Proposition 1

As noted above, Condition CM need not hold for the MIV assumption to be valid. If Condition CM does not hold, for the MIV to be a valid assumption requires constraints on the relationship between the unobserved counterfactuals $E[y(1)|v = u_1, z = 0]$ and $E[y(1)|v = u_2, z = 0]$. Of course *any* observed quantities in the decomposition of $E[y(1)|v]$ put constraints on the relationship between the counterfactuals when the MIV assumption is imposed; it is that if CM does not hold, those restrictions are likely to be stronger. To see this, decompose the MIV assumption. An MIV imposes:

$$\underbrace{E[y(1)|v = u_1, t = 1]}_{A} \cdot \underbrace{P(t = 1|v = u_1)}_{B} + \underbrace{E[y(1)|v = u_1, t = 0]}_{X} \cdot \underbrace{P(t = 0|v = u_1)}_{C} \\ \leq \\ \underbrace{E[y(1)|v = u_2, t = 1]}_{D} \cdot \underbrace{P(t = 1|v = u_2)}_{E} + \underbrace{E[y(1)|v = u_2, t = 0]}_{Y} \cdot \underbrace{P(t = 0|v = u_2)}_{F}. \tag{3.7}$$

[4] Since $P[y(1)|v = u_2, t = 1] \geq P[y(1)|v = u_1, t = 1]$, for (6) to hold $P(t = 0|v = u_1)$ must be *less* than $P(t = 0|v = u_2)$ in order to put less weight on the lower bound K_l. This implies, due to the dichotomy of the treatment, that $P(t = 1|v = u_1) > P(t = 1|v = u_2)$.

All quantities are observed except X and Y. Though these counterfactuals are not observed, the MIV assumption implies a relationship between them. In particular, it implies

$$X \leq \frac{DE - AB}{C} + (\frac{F}{C} - 1) \cdot Y + Y. \tag{3.8}$$

So we can define a minimum "gap" between the counterfactuals as the maximum amount X can be greater than Y as

$$\Gamma = \frac{DE - AB}{C} + (\frac{F}{C} - 1) \cdot Y. \tag{3.9}$$

If Γ is positive then X is at most that much larger than Y. If Γ is negative then X is at least that much smaller than Y. If TM holds, then $\frac{F}{C} \leq 1$ thereby implying $(\frac{F}{C} - 1) \leq 0$; in this case, as the unobserved counterfactual Y increases, the gap between it and the other unobserved counterfactual X also increases in a linear fashion. Crucial here then is the intercept $\frac{DE-AB}{C}$ for the plausibility of the MIV assumption. If the intercept is sufficiently large, then for plausible values of Y, the restrictions on X will in general by innocuous. When conditions TM and CM hold, the intercept will be unambiguously positive; but in this case, MIV can have bounding power on only one side of the treatment effect. When condition CM does not hold, in which case MIV can have bounding power on both sides of the treatment effect, the intercept may become negative and for plausible values of Y we may find unrealistic restrictions on X. To see this more clearly, consider a hypothetical set of values.

Consider the effect of college education on earnings, and suppose we use test scores as an MIV; in particular, assume we are looking at hourly wages. Suppose $B = 0.8$ and thus $C = 0.2$, so the probability of going to college with test score u_1 is 80%. Also suppose $E = 0.82$ and thus $F = 0.18$, so those with a higher test score have a slightly higher probability of going to college. Assume we observe $A = \$20$: individuals who went to college with test score u_1 earn \$20 an hour. In the scenario in which CM holds, perhaps $D = \$22$, so individuals who went to college and had higher test scores earn \$22 an hour. In this case, the gap becomes $\Gamma = 10.2 - 0.1Y$, and until $Y > \$102$, the MIV assumption does not even impose $X < Y$ (lower test scores lead to lower earnings for the counterfactuals). However, in the case where CM does not hold, and the MIV can have bounding power on both sides of the treatment effect, the picture begins to change. Assuming $D = \$18$ implies a gap of $\Gamma = -6.2 - 0.1Y$. Now, for a plausible value of Y

of \$20, X must be less than \$11.80. This implies having a slightly lower text score, which only lowers probability of college graduation by 2%, lowers expected wages by over 40%. Perhaps this is believable; but here we can see gaining identifying power at the cost of Condition CM can impose considerable implications for the unobserved counterfactuals.

With this in mind, when treatment is binary, if an MIV assists a researcher in bounding both sides of the treatment effect, it seems the researcher should at a minimum explore the implications imposed on the unobserved counterfactuals. There is no way to test the validity the MIV assumption, just as there is no way to test the exclusion restrictions of standard IV, but its implications can be investigated.

3.5 MIV with MTR and MTS

3.5.1 Additional Assumptions and Proposition 1

MIVs are rarely invoked in isolation as the MIV assumption alone generally leads to bounds too wide to be very informative. Rather, it is routinely invoked along with other assumptions. Two common assumptions are Monotone Treatment Response (MTR) and Monotone Treatment Selection (MTS). The MTR assumption tends to aide in signing the treatment effect.

MTR Assumption: *Let T be ordered. For each $j \in J$*

$$t_1 \geq t_0 \Rightarrow y_j(t_1) \geq y_j(t_0). \tag{3.10}$$

The Monotone Treatment Response (Manski 1997) assumption specifies a relationship between $y(1)$ and $y(0)$. For treatments that have some natural ordering, it maintains that outcomes vary monotonically with them. The MTR assumption aides in bounding the lower bound of the treatment effect and in fact results in a lower bound of zero. The MTS assumption (Manski and Pepper 2000) assumes the direction of the selection mechanism. The MTS assumption aides in bounding the upper bound of the treatment effect.

MTS Assumption: *Let T be ordered. For each $t \in T$, each $x \in X$ and all $(u_0, u_1) \in T \times T$ such that $u_1 \geq u_0$,*

$$E[y(t)|x, z = u_1] \geq E[y(t)|x, z = u_0]. \tag{3.11}$$

Previous results regarding the underlying conditions necessary for an MIV to aide in identification when treatment is binary were shown to hold when an MIV was invoked in isolation. Similar results hold when it is assumed jointly with MTR and MTS. In particular:

Proposition 1.2 *An MIV cannot provide identifying power beyond MTS on the upper bound of the treatment effect if condition CM holds, regardless of whether condition TM holds.*

and

Proposition 1.3 *An MIV cannot provide identifying power beyond MTR on the lower bound of the treatment effect if conditions CM and TM hold if 'weak monotonicity' -* $E[y(1)|z = 1, v] \geq E[y(0)|z = 0, v]$ *- also holds.*

Proofs of these extensions are simple variations of the proof for Proposition 1.

3.5.2 An Application: The Effect of Criminal Convictions on Match Quality

A researcher might be interested in the causal effect of criminal convictions on job match quality (measured as job tenure). Studies of the demand side of the labor market reveal that many employers are averse to hiring individuals with criminal records (Holzer 2007). Such an aversion among employers, when set in a equilibrium search model of employment, leads to several predictions. One of these predictions is that individuals with criminal records can be expected to have lower match qualities (Black 1995). But a criminal conviction is likely to be endogenous due to unobserved characteristics correlated with both convictions and job tenure. A researcher might aim to bound the treatment effect, and perhaps use delinquency rates as an MIV. Delinquency rates surely affect the "first stage" (convictions), yet there is a good chance they also affect the "second stage" (tenure) beyond their effect through convictions. Thus, though it is not a viable IV, it seems a prime candidate for an MIV as the effect of delinquency rates are likely correlated with job tenure in a monotone fashion: it would seem plausible to assume that individuals with higher delinquency rates would not have higher mean job tenure functions.

The data used in this application come from white male respondents in the 1997 NLSY with at most a high school diploma. This yields a population of 892 individuals without criminal convictions and 146 individuals with criminal convictions. The conviction variable is

Table 3.1 Bounds on the ATE of Criminal Convictions on Job Tenure.

	MMTR/MTS				MIV			
Population	L-	L	U	U+	L-	L	U	U+
White Men	0	0	6.93	11.57	0.34	0.40	3.99	4.35

L and U are lower and upper estimated bounds.

L- and U+ are lower and upper 95% confidence regions.

based on criminal convictions not settled in juvenile court prior to the year of the reported outcome variable. Tenure length is measured as the average length in weeks of employment for all jobs begun no earlier than 2003 or year after first conviction, whichever is most recent. The delinquency variable is a measure of the degree to which the respondent participated in delinquent activities as a youth and is construction from a list of youth delinquency questions within the NLSY questionnaire.

Initial bounds on the treatment effect are found by combining the MTR and MTS assumptions jointly with the data. These first set of results are given in Table 1. The MTR/MTS bounds imply that a criminal conviction decreases one's expected tenure length by at most 6.93 weeks and will not increase tenure. Using delinquency rates as an MIV tightens these bounds. When paired with MTR we find a lower bound on the effect on tenure to shorten tenure by at least 0.4 weeks - significantly higher than lower bounds found under MTR. Similarly, when combining the MIV with the MTS assumption, we find the upper bound to be at most 3.99 weeks - significantly smaller than upper bounds found under MTS. The delinquency MIV has aided in identification by tightening both the lower and upper bounds beyond the MTS/MTR bounds.

Figure 1 plots the probability of being convicted of a crime as a function of delinquency rates. The graph seems to indicate that, in general, condition TM holds for this instrument. This implies the identifying power of the MIV comes, for the most part, from the CM condition not holding for men who have not been convicted of a crime.

Figure 2 plots convicted and non-convicted mens' job tenure as a function of their delin-

Figure 3.1 Probability of Conviction as a Function of Delinquency Rates.

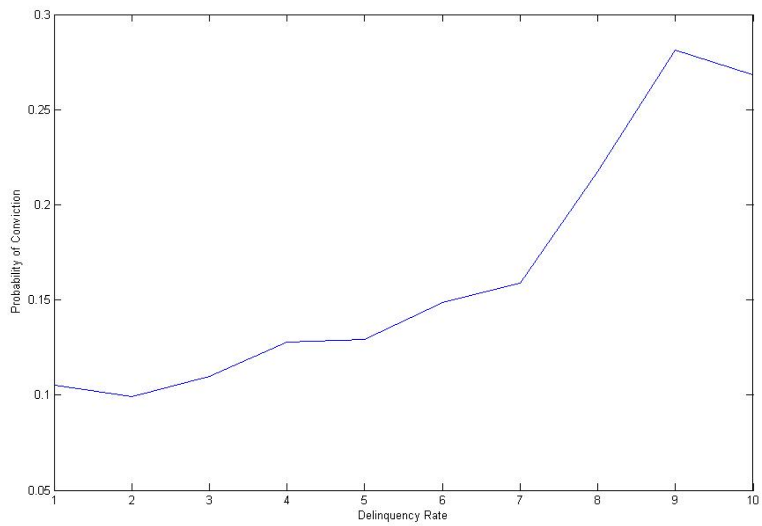


Figure 3.2 Convicted (bottom) and Non-Convicted (top) Mens' Job Tenure as a Function of Delinquency Rates.

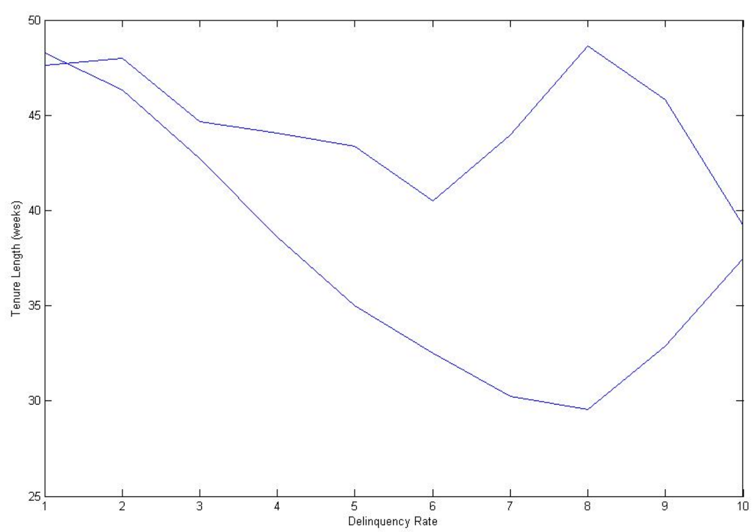
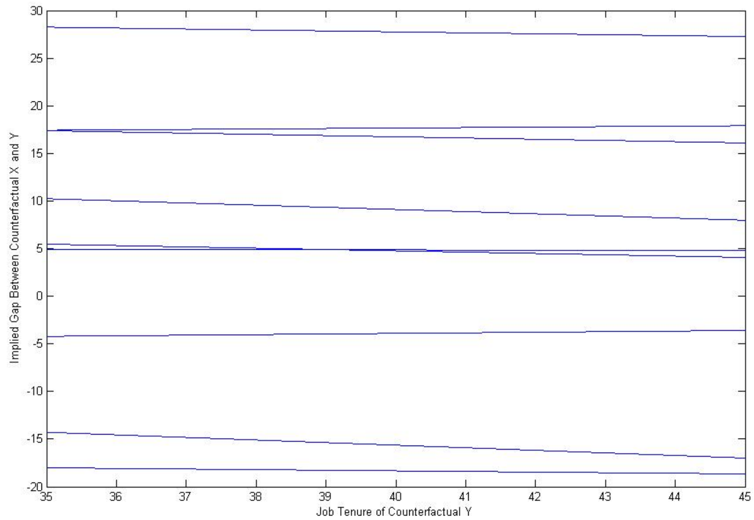


Figure 3.3 Implied Maximum Gaps Between Counterfactuals.



quency rates. It is clear that in general "weak monotonicity" holds, and, more importantly, that condition CM does not hold. This might lead one to question whether the MIV assumption holds under reasonable assumptions regarding the relationship between the unobserved counterfactuals.

Figure 3 plots constraints on the gap between the unobserved counterfactuals for various instrument realizations ($E[y(nocon)|t = con, v]$) over plausible tenure lengths. Each line represents a gap between two counterfactuals (X, Y) as defined by Equation (9).

The majority of the constraints merely impose that having a worse delinquency rate should not increase job tenure by too much for the counterfactuals, and there is a single constraint imposing worse delinquency rates should lower ones expected tenure by about a month. Yet there are two outlying constraints implying worse delinquency rates should shorten expected tenure by 3-4 months. How to interpret these results is debatable. One might reasonably argue that these latter differences are not believable. Further investigation finds these constraints correspond to the region of delinquency rates 6-8 which, for non-convicted individuals, do not exhibit the monotonicity implied by the MIV assumption; furthermore, it is precisely this region that yields identifying power. And this is the main point of this paper. For binary treatments, if treatment is monotone in the instrument, identifying power must come solely from areas of the instrument where the CM condition does not hold. And for an MIV to maintain validity, it may be that untenable assumptions must be made about the unobserved counterfactuals.

3.6 Multi-Valued Treatment

A natural next question is: Is this a special case where the treatment is binary? That is, say the treatment is more generally schooling, which is not dichotomous (and is the example given by Manski and Pepper 2000), do these findings still hold? The answer is no, i.e. this non-monotonicity of the conditional-on-received-treatment outcome is a special requirement for binary treatments.

To see this, suppose the treatment takes on three values: high-school diploma, 2 year technical degree, and 4 year college degree. Similarly, suppose the instrument also takes on three values: low, medium, and high test scores. Assume the relationship between test scores and probability of education level take the values given in Table 2.

Table 3.2 Hypothetical Probabilities of Education Levels for Multi-Valued Treatment Example.

V	HS	2yr	4yr
u_1	60	40	0
u_2	25	50	25
u_3	0	40	60

First note that treatment is monotonic in the instrument in the sense that $u'' \geq u'$ implies $E[ed|u''] \geq E[ed|u']$ (ie. Condition TM holds by construction). In such a setting, bounding the expected earnings under a "2 year degree" treatment with an MIV (u) yields the following:

$$\sum_{u \in V} P(u) \left\{ \max_{u' \leq u} \left[E[y(2yr)|v = u', t = 2yr] \cdot P(t = 2yr|v = u') + K_l \cdot P(t \neq 2yr|v = u') \right\} \right.$$

$$\leq E[y(2yr)] \leq \tag{3.12}$$

$$\sum_{u \in V} P(u) \left\{ \min_{u' \geq u} \left[E[y(2yr)|v = u', t = 2yr] \cdot P(t = 2yr|v = u') + K_U \cdot P(t \neq 2yr|v = u') \right\} \right.$$

For the lower bound to decrease over a region of the instrument, and thus for the MIV to have any bounding power, requires that there exists a pair, $u_3, u_2 \in V$, such that $u_3 > u_2$ and

$$\begin{gathered} E[y(2yr)|v = u_2, t = 2yr] \cdot P(t = 2yr|v = u_2) + K_l \cdot P(t \neq 2yr|v = u_2) \\ > \\ E[y(2yr)|v = u_3, t = 2yr] \cdot P(t = 2yr|v = u_3) + K_l \cdot P(t \neq 2yr|v = u_3). \end{gathered} \tag{3.13}$$

Suppose Condition CM holds along with Condition TM. Note then, by Condition CM and definition of the lower bound, that:

$$P[y(2yr)|v = u_3, t = 2yr] \geq P[y(2yr)|v = u_2, t = 2yr] \geq K_l.$$

Then, in order for inequality (13) to hold, it must be that $P(t = 2yr|v = u_2) > P(t = 2yr|v = u_3)$. But this holds by construction as seen in Table 2. So, in the case of a multi-valued treatment, this friction between Condition TM and CM is not a *necessary* issue for the MIV to have bounding power on both sides of $E[y(t)]$ (though one could construct an example where the issue still does arise).

However, even in such an ideal example as presented here, this friction does appear when we consider the bounds on the ATE between the "least" and "greatest" treatments (HS and 4yr in this example). That is, if treatment can be ordered ($t = 1, 2, 3...T$), then an MIV cannot bound both sides of the expected outcomes $E[y(t)]$ for $t = 1$ and $t = T$, and thus neither the ATE between the two. This is for reasons parallel to the binary case; note that when $t = 1$, treatment can be seen as binary (either $= 1$ or > 1). For example, Gonzalez (2004), using a bounding approach as discussed here, investigates the causal effect of a multi-valued treatment. His Table VI (middle column) presents the bounds on treatment effects under joint MTR/MIV assumptions. All of the lower bounds are zero except the bound on the treatment effect of $T - 1$ which is found to be greater than zero. So where does this identification comes from? If TM does hold, as the economic underpinnings of his MIV assume, then identification must come at the cost of Condition CM. And in this case, what restrictions must be imposed on the counterfactuals again becomes a relevant concern.

3.7 Conclusions

This paper has highlighted underlying conditions necessary for an MIV to have identifying power on the upper and lower bounds of binary treatment effects. If the treatment is monotonic in the instrument, then for the MIV to have identifying power on both the lower and upper bounds of the treatment effect, the conditional-on-received-treatment outcomes cannot exhibit the same monotonicity assumed in by MIV. This clutters the cleanliness of the economic theory surrounding many MIVs and places potentially untenable restrictions on the unobserved counterfactuals that should be investigated.

These findings are highlighted with an application that studies the causal effect of criminal convictions on job tenure and successfully uses youth delinquency rates as an MIV to tighten both the upper and lower bounds of the treatment effect. In this application, the majority of restrictions on the counterfactuals implied by the MIV are innocuous. But there are two implied restrictions that are highly unlikely. Further investigation finds these two constraints correspond to the regions of delinquency rates for which the conditional-on-received-treatment outcomes do not exhibit the monotonicity implied by the MIV assumption, and it is precisely these regions that yield identifying power. These results exemplify the main findings of this paper. For binary treatments, if treatment is monotone in the instrument, identification power must come solely at the cost of Condition CM and may imply considerable implications for the unobserved counterfactuals.

Furthermore, though the main results of this paper are shown to hold only for the binary treatment case, they are shown to have important implications even for the multi-valued treatment case. Researchers should be aware of these underlying conditions in their applications and consider their implications.

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