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Partial Identification of Average Treatment Effects in Program Evaluation: Theory and Applications

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PARTIAL IDENTIFICATION OF AVERAGE TREATMENT EFFECTS IN PROGRAM EVALUATION: THEORY AND APPLICATIONS

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There has been a recent increase on research focusing on partial identification of average treatment effects in the program evaluation literature. In contrast with traditional point identification, partial identification approaches derive bounds on parameters of interest based on relatively weak assumptions. Thus, they deliver more credible results in empirical applications. This dissertation extends Instrumental Variable (IV) methods in the program evaluation literature by partially identifying treatment effects of interest when evaluating a program or intervention.

An influential approach for studying causality within the IV framework was developed by Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996). They show that, when allowing for heterogeneous effects, IV estimators point identify the local average treatment effect (*LATE*) for compliers, whose treatment status is affected by the instrument. This dissertation advances the current IV literature in two important ways. First, inspired by the common criticism that *LATE* lacks external validity, this dissertation derives sharp nonparametric bounds for population average treatment effects (*ATE*) within the *LATE* framework. Second, the dissertation extends the *LATE* framework to bound treatment effects in the presence of both sample selection and noncompliance. Even when employing randomized experiments to evaluate programs -- as is now

common in economics and other social science fields -- assessing the impact of the treatment on outcomes of interest is often made difficult by those two critical identification problems. The sample selection issue arises when outcomes of interest are only observed for a selected group. The noncompliance problem appears because some treatment group individuals do not receive the treatment while some control individuals do. The dissertation addresses both of these identification problems simultaneously and derives nonparametric bounds for average treatment effects within a principal stratification framework. More generally, these bounds can be employed in settings where two identification problems are present and there is a valid instrument to address one of them. The bounds derived in this dissertation are based on two sets of relatively weak assumptions: monotonicity assumptions on potential outcomes within specified subpopulations, and mean dominance assumptions across subpopulations.

The dissertation employs the derived bounds to evaluate the effectiveness of the Job Corps (JC) program, which is the largest federally-funded job training program for disadvantaged youth in the United States, with the focus on labor market outcomes and welfare dependence. The dissertation uses data from an experimental evaluation of JC. Individuals were randomly assigned to a treatment group (whose members were allowed to enroll in JC) or to a control group (whose members were denied access to JC for three years). However, there was noncompliance: some individuals who were assigned to participate in JC did not enroll, while some individuals assigned to the control group did. The dissertation addresses this noncompliance issue using random assignment as an IV for enrollment into JC. Concentrating on the population *ATE*, JC enrollment increases weekly earnings by at least $24.61 and employment by at least 4.3 percentage points four years after randomization, and decreases yearly dependence on public welfare benefits by at least $84.29. These bounds are significantly narrower than the ones derived in the current IV literature. The dissertation also evaluates the effect of JC on wages, which are observed only for those who are employed. Hence, the sample selection issue has to be addressed when evaluating this effect. In the presence of sample selection and noncompliance, the average treatment effect of JC enrollment on wages for the always-employed compliers, who would comply with their assigned treatment and who would be always employed regardless of their assignment statuses, is bounded between 5.7 percent and 13.9 percent four years after random assignment. The results suggest greater positive average effects of JC on wages than those found in the literature evaluating JC without adjusting for noncompliance.

The dissertation closes by pointing out that a similar analytic strategy to the one used in this dissertation can be used to address other problems, for example, to bound the *ATE* when the instrument does not satisfy the exclusion restriction, and to derive bounds on the part of the effect of a treatment on an outcome that works through a given mechanism (i.e., direct or net effects) in the presence of one identification issue (e.g., noncompliance).

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CHAPTER 1

INTRODUCTION

There has been a recent increase on research focusing on partial identification of average treatment effects in the program evaluation literature. In contrast with traditional point identification, partial identification approaches derive bounds on parameters of interest instead of estimating a single value. Their main advantage is their dependence on weaker assumptions. Thus, partial identification approaches deliver more reliable results in empirical applications. This dissertation extends instrumental variable (IV) methods in the program evaluation literature by partially identifying treatment effects of interest when evaluating a program or intervention.

This dissertation is motivated by an influential approach for studying causality in IV contexts. Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996) (hereafter IA and AIR, respectively) show that, when allowing for heterogeneous effects, IV estimators point identify the local average treatment effect ($LATE$) for compliers, whose treatment status is affected by the instrument. However, a common criticism of their approach is the focus on the effect for a subpopulation, which cannot be identified from the population of interest (e.g., Heckman, 1996; Robins and Greenland, 1996; Deaton, 2010; Heckman and Urzua, 2010). Policy makers may be more interested in the population average treatment effect (ATE). AIR (1996) and Imbens (2009) responded to the criticism that the discussion is limited to compliers because it is the only subpopulation about which the data are directly informative.

The dissertation advances the current IV literature in two import ways. First, it derives sharp nonparametric bounds for the population ATE within the $LATE$ framework.

It improves the bounds on the ATE in the current IV literature by combining two sets of assumptions that may be useful in practice. The first is monotonicity in the treatment of the average outcomes of subpopulations. This assumption infers the sign of average treatment effects for other subpopulations from that for compliers. In contrast to the existing literature (e.g., Manski and Pepper, 2000; Bhattacharya, Shaikh and Vytlacil, 2008, hereafter BSV; Shaikh and Vytlacil, 2011), monotonicity is imposed on the average outcomes within the same subpopulation rather than on the individuals' outcomes. This makes the assumption more plausible in practice by allowing some individuals to experience a treatment effect that has the opposite sign to the $LATE$. The second set of assumptions is mean dominance that compares average potential outcomes across different subpopulations. Unlike the monotonicity, which informs the unobserved outcomes in the $LATE$ framework by inferring the sign of treatment effects, the mean dominance infers the unobserved terms from the identified average outcomes across subpopulations. Different from the literature on partially identifying the ATE within an IV, some of the bounds in the present research do not require a bounded-outcome assumption once combining the monotonicity and mean dominance assumptions. Moreover, the direction of the mean dominance can be informed by comparing average baseline characteristics of strata that are likely to be highly correlated with the outcome of interest, and these average characteristics can be obtained by estimating an overidentified nonparametric GMM problem.

The second way this dissertation advances the current IV literature is to derive nonparametric bounds for treatment effects in the presence of both sample selection and noncompliance. Randomized experiments are now commonly used to evaluate programs or interventions in economics and other social science fields. Despite the fact that randomized experiments are reliable approaches to establishing causality, assessing treatment effects of interest in randomized experiments is often made complicated by two critical identification issues: sample selection and noncompliance with the assigned treatment. The sample selection issue arises when outcomes are only observed for a selected group. For example, future health status is only observed for individuals who are alive at the time of following waves of surveys. The noncompliance problem appears because individuals can still decide whether or not to take the treatment in most of randomized experiments, especially in the ones with encouragement designs. As a result, it is common that in social experiments some treatment group individuals do not take the treatment, while some control individuals do.

The dissertation extends the partial identification results in Zhang, Rubin, and Mealli (2008) (hereafter ZRM) and Lee (2009), who construct bounds in the presence of sample selection, to also account for noncompliance using the $LATE$ framework proposed by IA (1994) and AIR (1996). The $LATE$ framework is a special case of principal stratification, which partitions the population based on the joint potential values of post-treatment variables under the two treatment assignment arms. Within the principal stratification framework, this dissertation derives nonparametric bounds for the always-selected compliers, who would comply with their assigned treatment and whose outcomes would be always observable regardless of treatment assignment. Analogous to the cases analyzed in IA (1994) and AIR (1996), ZRM (2008) and Lee (2009), among others, this is the only group of individuals whose outcomes can be observed in both treatment receipt arms in settings where both sample selection and noncompliance are present.

Additional assumptions are necessary to derive bounds on treatment effects for other subpopulations.

Principal stratification has often been used to address a single post-treatment complication. While there are a few papers that employ principal stratification to point identify treatment effects in the presence of more than one complication, it is my understanding that this is one of the first studies deriving bounds for treatment effects within this framework accounting for more than one identification problem. More generally, these bounds can be employed in settings where two identification problems are present and there is a valid instrument to address one of them. Some of these complications may include sample selection, noncompliance, endogeneity of the treatment variable, missing outcomes, among others. For example, when assessing the effect of military service on future health using the Vietnam-era draft lottery as an instrument to address endogeneity (e.g., Angrist, Chen, and Frandsen, 2009), the results could be used to bound the average effect on those who enrolled in the military because of the draft lottery (compliers) and who were alive when the outcome was measured regardless of their veteran status.

This dissertation employs the derived bounds to evaluate the effectiveness of the Job Corps (JC) program, which is the largest and most comprehensive federally-funded job training program for disadvantaged youth in the United States. It provides academic, vocational, and social skills training, among many other services, at over 120 centers throughout the country. Assessing the effect of this and other programs is of great importance to policy makers (e.g., Tennessee class size project STAR, Greater Avenues for Independence (GAIN) program, Tax Deferred Account (TDA) retirement plan). This dissertation uses data from the National Job Corps Study (NJCS), which is a randomized social experiment undertaken in the mid-to-late nineties to evaluate the effectiveness of JC. Individuals were randomly assigned to a treatment group (whose members were allowed to enroll in JC) or to a control group (whose members were denied access to JC for three years). However, there was noncompliance: some individuals in the treatment group did not enroll, while some individuals in the control group did.

Previous studies evaluating the JC program (Schochet, Burghardt, and Glazerman, 2001; Schochet, Burghardt, and McConnell, 2008; Lee, 2009; Flores-Lagunes, Gonzalez, and Neumann, 2010; Flores et al., 2012; Blanco, Flores, and Flores-Lagunes, 2012) usually focus on intention-to-treat (ITT) effects. An ITT effect compares potential outcomes according to the assigned treatment and ignores possible noncompliance, and thus would dilute the effect of actual JC enrollment. Schochet, Burghardt, and Glazerman (2001) and Schochet, Burghardt, and McConnell (2008) also address noncompliance by using IV estimators, hence focusing on the $LATE$ for compliers. Using random assignment as an IV, this dissertation addresses this noncompliance issue and looks at the population ATEs of actual JC enrollment on participants' labor market outcomes and welfare. It also examines wage effects of JC enrollment by focusing on a specified subpopulation (i.e., the always-employed compliers). Thus, the dissertation also contributes to the empirical literature on the evaluation of the JC program by providing credible bounds based on relatively weak assumptions for treatment effects of interest other than $LATE$ and the ITT effect.

Focusing on the ATE on labor market outcomes and welfare, JC enrollment increases weekly earnings by at least \$24.61 and employment by at least 4.3 percentage points four years after randomization, and decreases yearly dependence on public welfare benefits by at least \$84.29. More specifically, the preferred bounds on the ATE under the monotonicity and mean dominance assumptions are $[24.61, 201.04]$ for weekly earnings, $[.042, .163]$ for employment, and $[-142.76, -84.29]$ for public benefits. These bounds are significantly narrower than the IV bounds proposed by Manski (1990), Heckman and Vytlacil (2000), Kitagawa (2009). The enhanced identification power comes from the mean dominance assumption, which is not considered in the current literature. This assumption provides new information to the bounds of the ATE by using identified average outcomes across strata. Accordingly, combining it with the monotonicity assumption sharpens the bounds. The width of the bounds is also smaller than that under both the IV and Monotone Treatment Response (MTR) assumptions of Manski and Pepper (2000), especially for public benefits. The bounds on employment are also narrower than those proposed by Balke and Pearl (1997), BSV (2008), Chesher (2010), Chiburis (2010b) and Shaikh and Vytlacil (2011) for the case of a binary outcome. The lower bounds for weekly earnings and employment are 10 percent higher than their respective ITT effects (22.19 and .038), while the upper bound for public benefits is equal to its ITT effect. The $LATE$s for compliers on the three outcomes also fall within these bounds.

The dissertation also evaluates the effect of JC enrollment on wages, which are observed only for those who are employed. Ignoring this sample selection issue would give biased results. Hence, it is necessary and important to address sample selection when evaluating wage effects. In the presence of sample selection and noncompliance, the bounds on the wage effect of JC enrollment are derived for the always-employed compliers, who would comply with their assigned treatment and who would always be employed regardless of assignment status. This is the only stratum for which wages are observed for individuals who enrolled and for individuals who did not enroll in the JC program. It is also the largest stratum in the sample, accounting for about 40 percent of the population. The wage effect of JC enrollment for the always-employed compliers is between 5.7 percent and 13.9 percent four years after random assignment, and between 7.7 and 17.5 percent for Non-Hispanics. The results suggest greater positive average effects of JC on wages than those found without adjusting for noncompliance in Lee (2009) and Blanco, Flores, and Flores-Lagunes (2012). This evidence suggests that the JC training program has positive effects not only on the employability of its participants but also on their wages, implying that JC participation is likely to increase their human capital. By mimicking the characteristics of the sample, this section also presents simulation exercises done to analyze the sensitivity of the empirical results to violations of the two main assumptions employed (monotonicity and mean dominance). The simulation results suggest that the estimated bounds are robust to small departures from these assumptions.

The remainder of the dissertation is organized as follows. Chapter 2 presents a review of the partial identification literature. Chapter 3 presents the econometric framework and the partial identification results for the parameters of interest, and then discusses estimation and inference. Chapter 4 briefly describes the JC program and the NJCS, and empirically analyzes the effects of JC enrollment on participants' labor market outcomes and welfare dependence. Chapter 5 concludes.

CHAPTER 2

LITERATURE REVIEW

2.1 Partial Identification in the IV Literature

Instrumental variable (IV) approaches have been widely used in the literature of program evaluation due to their high internal validity. IA (1994) and AIR (1996) develop an influential approach for studying causality within the IV framework. Using a randomized instrument, they partition the population into always-takers, never-takers, compliers, and defiers according to the joint potential values of treatment status. Always-takers (never-takers) are individuals who would always (never) take the treatment irrespective of instrument status. Compliers behave consistently with the assigned instrument, while defiers do the opposite of the assigned instrument. Under the monotonicity of the treatment in the instrument, defiers are ruled out. In the absence of strong homogeneity or distributional assumptions, data never reveals information on the outcome of always-takers under the inactive treatment or the outcome of never-takers under the active treatment. As a result, IV estimators point identify the local average treatment effect ($LATE$) for compliers in heterogenous treatment effect models.

A common criticism of their approach, however, is the focus on the effect for a subpopulation (e.g., Deaton, 2010; Heckman and Urzua, 2010). Heckman (1996) and Robins and Greenland (1996) state that the $LATE$ is defined for a latent subpopulation in the sense that compliers cannot be identified from the population of interest. The latter suggests that attention should be on the population average treatment effect (ATE). AIR (1996) and Imbens (2009) respond that one may also be interested in averages for the entire population from the point of view of policy makers, but they stress that compliers is the only subpopulation about which the data are directly informative, and that extension to treatment effects for other subpopulations has to be extrapolations in IV contexts.

Point identification of the population treatment effects by the IV methods usually requires parametric or structural assumptions. Heckman (2010) proposes a method to nonparametric identify the ATE, but the instrument in his approach is required that is strong enough to drive the probability of being treated from zero to one, which is hard to satisfy in practice. In contrast to traditional point identification, Manski (1990) pioneered partial identification of the population ATE under the assumption of mean independence of the instrument.

There has been a growing literature on partial identification of the ATE with IV methods since Manski (1990). One strand of this literature endeavors to improve Manski's bounds by assuming different versions of monotonicity of the outcome. Manski and Pepper (2000) introduce the assumptions of monotonicity of the treatment response (MTR) and the monotonicity of the treatment selection (MTS). Combined with the mean independence assumption, Chiburis (2010a) derives the bounds for the ATE under both MTR and MTS assumptions without specifying the direction of the monotonicity a priori. Instead of the monotonicity assumptions employed in the above papers, another strand of the literature assumes structural models on the treatment or the outcome. Under the statistical independence assumption of the instrument, Heckman and Vytlacil (2000) impose a threshold crossing model with a separable error on the treatment. Focusing on a binary outcome, Shaikh and Vytlacil (2011) impose threshold crossing models on both the treatment and the outcome, while Chiburis (2010b) considers a threshold crossing model on the outcome. Rather than assuming the threshold crossing model with separable errors, Chesher (2010) imposes a non-separable structural model on the outcome and assumes the structural function is weakly increasing in the non-separable error.

Comparison of identification power among these assumptions are also discussed in the existing literature on partial identification with IV methods. First, the monotonicity assumption on the treatment (e.g., Balke and Pearl, 1997; Huber and Mellace, 2010) and the structural model assumptions on the treatment (e.g., Heckman and Vytlacil, 2000) do not improve Manski's bounds derived under the mean independence assumption. Second, monotonicity assumptions on the outcome (e.g., Manski and Pepper, 2000) and the structural model assumptions on the outcome do improve Manski's bounds (e.g., BSV, 2008; Chiburis, 2010a; 2010b; Chesher 2010; Shaikh and Vytlacil, 2011). Third, because the bounds for the ATE involve counterfactual potential outcomes, partial identification with IV methods usually requires bounded support of the outcome. This might also be the reason why quite a few papers focus on binary outcomes (e.g., Balke and Pearl, 1997; BSV, 2008; Hahn, 2010; Chiburis, 2010b; Shaikh and Vytlacil, 2011).

It's worth noting that for a binary dependent variable, the monotonicity assumptions and the structural model assumptions are equivalent. Vytlacil (2002) shows the equivalence between the monotonicity assumption and the threshold crossing model on the treatment. Machado et al. (2011) notice the equivalence between the MTR assumption and the threshold crossing model on the outcome. In the absence of covariates, Chiburis (2010b) observe the equivalence between the threshold crossing model with a separable error and the non-separable structural function being weakly increasing in the non-separable error. BSV (2008) show that in the absence of covariates, the bounds for a binary outcome under MTR and the mean independence assumptions are equal to the ones derived from the threshold crossing models on both the treatment and the outcome. Chiburis (2010b) noticed that his bounds obtained by imposing the threshold crossing model on the outcome are equal to the ones under MTR and the mean independence assumptions.

This dissertation improves Manski's nonparametric bounds on the population ATE by extending the work of IA (1994) and AIR (1996). The setup of a binary treatment and a binary instrument is used in most of the program evaluation literature (e.g., Imbens and Wooldridge, 2009) and common in empirical applications (e.g., Angrist, 1990; Oreopoulous, 2006). And the $LATE$ framework allows nonparametric identification of the ATE within the context of heterogeneous treatment effects. This dissertation adds to the literature by considering two different sets of assumptions. The first is monotonicity in the treatment of the average outcomes of subpopulations defined by the joint potential values of the treatment status under each value of the instrument. As in BSV (2008) and Shaikh and Vytlacil (2011), prior knowledge about the direction of the monotonicity is not required. This assumption infers the sign of average treatment effects for other subpopulations from that for compliers. In contrast to the existing literature (e.g., Manski and Pepper, 2000; BSV, 2008; Shaikh and Vytlacil, 2011), monotonicity is imposed on the average outcomes of the strata rather than on the individuals' outcomes. This makes the assumption more plausible in practice by allowing some individuals to experience a treatment effect that has the opposite sign to the ATE. The second set of assumptions is mean dominance that compares average potential outcomes across different subpopulations. Assumptions similar to the mean dominance have been shown to have significant identifying power in other settings (e.g., ZRM, 2008; Flores and Flores-Lagunes, 2010). Different from the monotonicity, which informs the unobserved outcomes by inferring the sign of treatment effects, the mean dominance infers the unobserved terms from the identified average outcomes across subpopulations. Once combining the two sets of assumptions, the sharp bounds of the ATE in the present study are significantly narrower than the ones obtained in the current IV literature, and some of the bounds do not require a bounded-outcome assumption. Moreover, the directions of the mean dominance can be informed by comparing average baseline characteristics of strata that are likely to be highly correlated with the outcome of interest. These average characteristics are estimated by solving an overidentified nonparametric GMM problem.

A recent paper by Huber and Mellace (2010) also derives bounds on the ATE within the IV framework. The main difference between this research and theirs is that the monotonicity assumption is imposed on the average outcomes of the strata, which results in narrower bounds and can be justified by economic theory in many applications. Also, a priori direction of the monotonicity is avoided, while its direction can be inferred from data. In addition, the mean dominance assumptions not only differ from theirs, but the direction of the mean dominance can be informed by comparing the average baseline characteristics across strata, which are estimated by solving an overidentified nonparametric GMM problem.

2.2 Partial Identification Addressing Sample Selection

One of the leading examples of sample selection in the program evaluation literature is to evaluate the effect of a training program on participants' wages. The sample selection issue arises from the fact that wages are only observed for those who are employed, with the employment decision itself being potentially affected by the program. Assessing the effect of training programs on wages is of great importance to policy makers. Most of the econometric evaluations of training programs, however, focus on the impact on total earnings, which are the product of the hourly wage and the hours worked. As discussed by Lee (2009), focusing only on total earnings fails to answer the relevant question of whether the programs lead to an increase in participants' wages (e.g., through human capital accumulation), or to an increase in the probability of being employed (e.g., through counseling and job search assistance services) without any increase in wages.

Standard approaches for point identification of treatment effects in the presence of sample selection require strong parametric assumptions or the availability of a valid instrument (e.g., Heckman, 1979). In settings where an instrument is unavailable, an alternative strategy is to partially identify the effects under relatively mild assumptions (Zhang and Rubin, 2003; ZRM, 2008; Imai, 2008; Lee, 2009; Lechner and Melly, 2010; Huber and Mellace, 2010). Part of this literature uses principal stratification (Frangakis and Rubin, 2002), which provides a framework for studying causal effects when controlling for a variable that has been affected by the treatment (in this example, the employment decision). The basic idea behind principal stratification is to compare individuals within common principal strata (subpopulations of individuals who share the same potential values of the employment variable under both treatment arms). Since membership to a particular principal stratum is not affected by treatment assignment, sample selection is not an issue within the principal strata, and the estimated effects are causal effects.

ZRM (2008) and Lee (2009) derive bounds for the average effect of a training program on wages for a particular stratum: the "always-employed" (those individuals who would be employed whether or not they were assigned to enroll in the training program). They focus on this stratum because it is the only one for which the individuals' outcomes are observed under both treatment assignments. Following Zhang and Rubin (2003), ZRM (2008) consider two assumptions and derive bounds for this effect under each assumption and when both are imposed. The first assumption is a monotonicity assumption on the effect of the treatment (training program) on the selection (employment), and the second is a stochastic dominance assumption comparing the potential outcomes of the always-employed to those of other strata. Lee (2009) uses an alternative approach to that in ZRM (2008) to derive bounds under the monotonicity assumption. Importantly, the bounds derived in these papers do not impose the assumption that the support of the outcome is bounded. Lee (2009) uses his bounds to evaluate the wage effects of JC. More recently, Blanco, Flores, and Flores-Lagunes (2012) employ the bounds used by ZRM (2008) and Lee (2009), and their extension to quantile treatment effects by Imai (2008) to study the wage effects of JC for different demographic groups without adjusting for noncompliance.

Huber and Mellace (2010) and Lechner and Melly (2010) derive bounds for subpopulations other than the always-employed. Huber and Mellace (2010) use a principal stratification approach to construct bounds on the effects for two other strata (those who would be employed only if assigned to the treatment group, and those who would be employed only if assigned to the control group), as well as for the "selected" subpopulation (those whose wages are observed and are a mixture of different strata). While their assumptions are similar to those in ZRM (2008) and Lee (2009), additional assumptions are required (e.g., bounded support of the outcome), since bounds are constructed for strata and subpopulations for which the outcome is never observed under one of the treatment states. Lechner and Melly (2010) derive bounds for mean and quantile treatment effects for the "treated and selected" subpopulation (employed individuals who received training and are also a mixture of different strata). Contrary to the previously described literature, they do not follow a principal stratification approach to derive their results. The assumptions they consider involve monotonicity assumptions on the training program's effect on employment (conditional on covariates), as well as stochastic dominance assumptions involving observed subpopulations (e.g., employed versus unemployed) rather than involving different strata. Similar to Huber and Mellace (2010), they require an outcome with bounded support to partially identify the mean effects.

The previously discussed literature, with the exception of Lechner and Melly (2010), focuses on the intention-to-treat (ITT) effect of being offered to participate in the training program. The ITT effect compares potential outcomes according to the assigned treatment and ignores possible noncompliance. The popularity of the ITT effect in the program evaluation literature is partly stimulated by the fact that randomized and natural experiments are now commonly used in economics and other social science fields to deliver causal effects. However, though individuals are randomly assigned to the treatment group and the control group, it is usually the case that they can still choose whether or or not to actually take the treatment. In the case of training programs, the noncompliance problem appears when some treatment group individuals did not enroll in the program, while some of the control individuals did enroll. For example, in the data set of the NJCS employed in the dissertation, only 73 percent of the individuals assigned to the treatment group enrolled in JC, while 4 percent of the individuals assigned to the control group enrolled in JC in the four years after random assignment. The noncompliance issue dilutes the effect of actual participation in the program.

This dissertation derives nonparametric bounds for treatment effects in settings where both sample selection and noncompliance are present. It extends the partial identification results in ZRM (2008) and Lee (2009) to account for noncompliance. Thus, this part bounds the wage effect of actual *enrollment* in the program, rather than the effect of *being allowed* to enroll in the program. The approach to account for noncompliance is based on the works by IA (1994) and AIR (1996), who use the IV approach to address noncompliance in the absence of sample selection. Their approach is also a special case of principal stratification. Based on the individuals' potential compliance behavior under the two treatment assignments, they stratify the population into four strata: the so-called always-takers, never-takers, compliers, and defiers. The dissertation employs principal stratification to address the sample selection and noncompliance problems simultaneously, and derive bounds for the average effect of participating in a training program on wages for the stratum of always-employed compliers. This stratum consists of those who comply with their treatment assignment and would be employed whether or not they enrolled in the training program. Analogous to the cases analyzed in IA (1994), AIR (1996), ZRM (2008), and Lee (2009), among others, this is the only stratum for which wages are observed for individuals who enrolled and did not enroll in the training program. In the context of analyzing the effects of JC on wages, this is also the largest stratum (about 40 percent of the population).

Principal stratification has often been used to address a single post-treatment complication. While there are a few papers that employ principal stratification to point identify treatment effects in the presence of more than one complication, it is my understanding that this is one of the first papers deriving bounds for treatment effects within this framework accounting for more than one identification problem. A particularly relevant paper in a similar setting is the one by Frumento et al. (2012), who analyze the effects of JC on employment and wages using data from the NJCS. They perform a likelihood-based analysis to simultaneously address three problems: sample selection, noncompliance and missing outcomes due to non-response. They stratify the population based on the potential values of the compliance behavior and employment status to address the noncompliance and sample selection issues, and they employ a "missing at random" assumption (Rubin, 1976) to address the missing-outcome problem.[1] Under some parametric assumptions, Frumento et al. (2012) point identify the effect of JC on wages for the always-employed compliers.[2] The thesis complements the work by Frumento et al. (2012) by constructing nonparametric bounds for the effect of JC on wages based on an alternative set of assumptions. In the empirical part, it also presents results

[1]The missing at random assumption states that the probability that the outcome is missing for a given individual is random conditional on a set of observable characteristics.

[2]Another assumption in Frumento et al. (2012) is that the individuals in the control group never enroll in JC, which rules out the existence of "always-takers". This assumption may not be plausible in applications in the dissertation, especially when looking at outcomes four years after random assignment.

that account for missing values due to non-response by using weights constructed by the NJCS using non-public use data that account for sample design and non-response.

CHAPTER 3

THEORY: BOUNDS ON THE PARAMETERS OF INTEREST

3.1 The $LATE$ Framework

Consider a random sample of size n from a population. Let $D_i \in \{0,1\}$ indicate whether unit i is treated ($D_i = 1$) or not ($D_i = 0$), and let $Z_i \in \{0,1\}$ be an instrument for treatment. Let $D_i(1)$ and $D_i(0)$ denote the treatment individual i would receive if $Z_i = 1$ or $Z_i = 0$, respectively. The outcome of interest is Y. Denote by $Y_i(1)$ and $Y_i(0)$ the potential outcomes as a function of D, i.e., the outcomes individual i would experience if she received the treatment or not, respectively. Finally, let $Y_i(z,d)$ be the potential outcome as a function of the instrument and the treatment. For each unit, econometricians observe $\{Z_i, D_i(Z_i), Y_i(Z_i, D_i(Z_i))\}$. The notation implicitly imposes the stable unit treatment value assumption (SUTVA) (Rubin 1978, 1980, 1990), which is common in the literature and implies that the potential outcomes for each unit are unrelated to the treatment assignment and treatment receipt of the other individuals. For the sake of simplicity, the subscript i is omitted unless deemed necessary for clarity. This setting has received considerable attention in the literature (e.g., AIR, 1996; BSV, 2008).

AIR (1996) partition the population into four strata based on the joint potential values of $\{D_i(0), D_i(1)\}$: $\{1,1\}$, $\{0,0\}$, $\{0,1\}$ and $\{1,0\}$. AIR (1996) and the subsequent literature refer to these strata as always-takers (a), never-takers (n), compliers (c), and defiers (d), respectively. AIR (1996) impose the following assumptions:

Assumption 1.1 *(Randomized Instrument). $\{Y(z,d), D(z)\}$ is independent of Z for all $z, d \in \{0,1\}$.*

Assumption 1.2 *(Exclusion Restriction).* $Y_i(0,d) = Y_i(1,d) = Y_i(d), d \in \{0,1\}$ *for all i.*

Assumption 1.3 *(Nonzero First Stage).* $E[D(1) - D(0)] \neq 0$.

Assumption 1.4 *(Individual-Level Monotonicity of D in Z). Either* $D_i(1) \geq D_i(0)$ *for all i or* $D_i(1) \leq D_i(0)$ *for all i.*

Assumptions 1.1 through 1.4 are standard assumptions in the IV literature (e.g., IA, 1994; AIR, 1996; Huber and Mellace, 2010; Blanco, Flores, and Flores-Lagunes, 2012). Assumption 1.4 rules out the existence of defiers (compliers) when the monotonicity is non-decreasing (non-increasing). The direction of the monotonicity can be inferred from the data given the independence of Z. Following BSV (2008), Z is ordered so that $E[D|Z=1] \geq E[D|Z=0]$ to simplify notation.

Let $LATE_k = E[Y(1) - Y(0)|k]$ and π_k denote, respectively, the local average treatment effect and the stratum proportion in the population, for stratum k, with $k = a$, n, c. Let $\overline{Y}^{zd} = E[Y|Z=z, D=d]$ and $p_{d|z} = \Pr(D=d|Z=z)$. Under Assumptions 1.1 through 1.4, the following quantities are point identified: $\pi_a = p_{1|0}$, $\pi_n = p_{0|1}$, $\pi_c = p_{1|1} - p_{1|0}$, $E[Y(1)|a] = \overline{Y}^{01}$, $E[Y(0)|n] = \overline{Y}^{10}$ and $LATE_c = (E[Y|Z=1] - E[Y|Z=0])/(p_{1|1} - p_{1|0})$. As shown in IA (1994) and AIR (1996), $LATE_c$ is point identified for compliers whose treatment status is affected by the instrument, and equals the conventional IV estimand in the absence of covariates.

3.2 Bounds on the Population ATE

The parameter of interest in this section is the population average treatment effect, $ATE = E[Y_i(1) - Y_i(0)]$. To derive the bounds, ATE is decomposed as a weighted average of the $LATE$s for always-takers, never-takers, and compliers:

$$ATE = LATE_a\pi_a + LATE_n\pi_n + LATE_c\pi_c \quad (3.1)$$

$$= \overline{Y}^{11}p_{1|1} - \overline{Y}^{00}p_{0|0} + E[Y(1)|n]p_{0|1} - E[Y(0)|a]p_{1|0}, \quad (3.2)$$

where $E[Y|Z=z] = E[E[Y|Z=z,D=d]|Z=z]$ is used in the second equality. By equation (3.2), since $Y(1)$ for never-takers and $Y(0)$ for always-takers are never observed in the data, additional assumptions are needed to bound ATE. The most basic assumption considered in the previous literature is the bounded support of the outcome.

Assumption 2.1 *(Bounded Outcome).* $Y(0),Y(1) \in [y^l, y^u]$.

This assumption states that the potential outcomes under the two treatment arms have a bounded support. Replacing $E[Y(1)|n]$ and $E[Y(0)|a]$ in equation (3.2) with y^l and y^h, I obtain sharp bounds on the ATE under Assumptions 1.1 through 1.4, and 2.1.

Proposition 2.1 *Under Assumptions 1.1 through 1.4, and 2.1, the bounds $LB \leq ATE \leq UB$ are sharp, where*

$$LB = \overline{Y}^{11}p_{1|1} - \overline{Y}^{00}p_{0|0} + y^l p_{0|1} - y^u p_{1|0}$$

$$UB = \overline{Y}^{11}p_{1|1} - \overline{Y}^{00}p_{0|0} + y^u p_{0|1} - y^l p_{1|0}.$$

The bounds in Proposition 2.1, which is presented for reference, coincide with the IV bounds in Manski (1990), Heckman and Vytlacil (2000) and Kitagawa (2009), and with those in Huber and Mellace (2010). When the outcome is binary, these bounds also coincide with those in Balke and Pearl (1997).

3.2.1 Bounds under Monotonicity

In this subsection, the monotonicity assumption is introduced to improve the identification power of the bounds in Proposition 2.1.

Assumption 2.2 *(Monotonicity in D of Average Outcomes of Strata).(i) Either* $E[Y(1)|k] \geq E[Y(0)|k]$ *for all* $k = a,n,c$*; or* $E[Y(1)|k] \leq E[Y(0)|k]$ *for all* $k = a,n,c$*. (ii)* $E[Y(1) - Y(0)|c] \neq 0$.

Assumption 2.2 requires that the $LATE$s of the three existing strata are all either non-negative or non-positive. This assumption is similar to that in BSV (2008), with the important distinction that the monotonicity is imposed on the $LATE$s rather than on the individual effects, which makes it more plausible in practice by allowing some individuals to have a treatment effect of the sign different from that of the ATE. Since Z is ordered so that $E[D|Z=1] \geq E[D|Z=0]$, the direction of the monotonicity is identified from the sign of the IV estimand ($LATE_c$) under the current assumptions. The following proposition presents sharp bounds on the ATE under the additional Assumption 2.2.

Proposition 2.2 *Under Assumptions 1.1 through 1.4, 2.1 and 2.2, the bounds* $LB \leq ATE \leq UB$ *are sharp, where, if* $E[Y|Z=1] - E[Y|Z=0] > 0$,

$$\begin{aligned} LB &= E[Y|Z=1] - E[Y|Z=0] \\ UB &= \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + y^u p_{0|1} - y^l p_{1|0}; \end{aligned}$$

and if $E[Y|Z=1] - E[Y|Z=0] < 0$,

$$\begin{aligned} LB &= \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + y^l p_{0|1} - y^u p_{1|0} \\ UB &= E[Y|Z=1] - E[Y|Z=0]. \end{aligned}$$

Depending on the sign of $LATE_c$, either the lower or the upper bound in Proposition 2.2 improves upon the corresponding bound in Proposition 2.1. When $LATE_c > 0$, the lower bounds on $LATE_a$ and $LATE_n$ become zero; otherwise, their upper bounds become zero. Consequently, either the lower or upper bound on the ATE equals the ITT effect, depending on the sign of $LATE_c$. When the outcome is binary, the bounds in Proposition 2.2 coincide with those in BSV (2008) and Chiburis (2010b), which both equal the bounds in Shaikh and Vytlacil (2011) and Chesher (2010) when there are no exogenous covariates other than the binary instrument. Moreover, if $LATE_c$ is positive (negative) and Assumptions 1.1 through 1.4, 2.1 and 2.2, hold, then the bounds in Proposition 2.2 equal the bounds obtained by imposing the mean independence assumption of the instrument and increasing (decreasing) MTR assumptions in Manski and Pepper (2000). MTR imposes monotonicity of the outcome in the treatment at the individual level, and it requires one to know the direction of the effect a priori. Depending on the sign of the individual effect, BSV (2008) shows the equivalence of their bounds to those under the IV and MTR assumptions for the case of a binary outcome. Thus, in this setting along with the relaxed version of the monotonicity assumption, these results can be seen as an extension of those in BSV (2008) to the case of a non-binary outcome.[3]

3.2.2 Bounds under Mean Dominance

In practice, some strata are likely to have more favorable characteristics and thus better mean potential outcomes than others. The three alternative assumptions below formal-

[3]For a discussion of the trade-off between the MTR assumption and assuming monotonicity of the treatment in the instrument, see BSV (2008).

ize the notion that under the same treatment status, never-takers tend to have the best average potential outcome among the three strata, while always-takers tend to have the worst one.

Assumption 2.3a $E[Y(d)|a] \leq E[Y(d)|n]$ *for* $d = 0,1$.

Assumption 2.3b $E[Y(0)|a] \leq E[Y|Z=0, D=0]$ *and* $E[Y(1)|n] \geq E[Y|Z=1, D=1]$.

Assumption 2.3c $E[Y(0)|a] \leq E[Y(0)|c]$ *and* $E[Y(1)|n] \geq E[Y(1)|c]$.

The direction of these assumptions can be inverted depending on the application in question. The always-takers and never-takers are likely to be the most “extreme" groups in many applications, so Assumption 2.3a may be viewed as the weakest of the three. Assumption 2.3b compares the mean $Y(0)$ and $Y(1)$ of the always-takers and never-takers, respectively, to those of a weighted average of the other two strata, while Assumption 2.3c compares them to those of the compliers. Although none of these assumptions is directly testable, it is possible to obtain indirect evidence about their plausibility by comparing relevant average pre-treatment characteristics of the different strata that are highly related to the outcome. These average characteristics of each stratum can be estimated from an overidentified nonparametric GMM problem. For details on the GMM procedure see Appendix C. For Assumption 2.3c, the direction may also be inferred by comparing point identified quantities, $E[Y(1)|a]$ to $E[Y(1)|c]$ and $E[Y(0)|n]$ to $E[Y(0)|c]$, if these inequalities also hold under the alternative treatment status.

The following bounds are presented under Assumptions 1.1 through 1.4, 2.1, and each of the three versions of Assumption 2.3. In each case, the lower bound is higher than that in Proposition 2.1.

Proposition 2.3 *Let* $UB = \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + y^u p_{0|1} - y^l p_{1|0}$.

(a) Under Assumptions 1.1 through 1.4, 2.1 and 2.3a the bounds $LB \leq ATE \leq UB$ *are sharp, where*

$$LB = \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + \overline{Y}^{01} p_{0|1} - \overline{Y}^{10} p_{1|0}.$$

(b) Under Assumptions 1.1 through 1.4, 2.1 and 2.3b the bounds $LB \leq ATE \leq UB$ *are sharp, where*

$$LB = \overline{Y}^{11} - \overline{Y}^{00}.$$

(c) Under Assumptions 1.1 through 1.4, 2.1 and 2.3c the bounds $LB \leq ATE \leq UB$ *are sharp, where*

$$LB = \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + \frac{\overline{Y}^{11} p_{1|1} - \overline{Y}^{01} p_{1|0}}{p_{1|1} - p_{1|0}} p_{0|1} - \frac{\overline{Y}^{00} p_{0|0} - \overline{Y}^{10} p_{0|1}}{p_{1|1} - p_{1|0}} p_{1|0}.$$

Assumptions 2.3a through 2.3c have testable implications when combined with Assumption 2.2, if $LATE_c < 0$. The following inequalities are expected to hold: $\overline{Y}^{01} \leq \overline{Y}^{10}$ (2.3a); $\overline{Y}^{01} \leq \overline{Y}^{00}$ and $\overline{Y}^{11} \leq \overline{Y}^{10}$ (2.3b); and, $\overline{Y}^{01} \leq E[Y(0)|c]$ and $E[Y(1)|c] \leq \overline{Y}^{10}$ (2.3c). If some (or all) of these inequalities are not rejected in applications, then their corresponding assumptions are expected to hold. The following three propositions provide bounds when Assumptions 2.2 and 2.3 are combined.

Proposition 2.4 *Under Assumptions 1.1 through 1.4, 2.1, 2.2 and 2.3a the bounds* $LB \leq ATE \leq UB$ *are sharp, where, if* $E[Y|Z=1] - E[Y|Z=0] > 0$,

$$\begin{aligned} LB &= \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + \max\{\overline{Y}^{10}, \overline{Y}^{01}\} p_{0|1} - \min\{\overline{Y}^{10}, \overline{Y}^{01}\} p_{1|0} \\ UB &= \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + y^u p_{0|1} - y^l p_{1|0}; \end{aligned}$$

and if $E[Y|Z=1]-E[Y|Z=0]<0$,

$$\begin{aligned} LB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+\overline{Y}^{01}p_{0|1}-\overline{Y}^{10}p_{1|0} \\ UB &= E[Y|Z=1]-E[Y|Z=0]. \end{aligned}$$

Proposition 2.5 *Under Assumptions 1.1 through 1.4, 2.1, 2.2 and 2.3b the bounds* $LB \leq ATE \leq UB$ *are sharp, where, if* $E[Y|Z=1]-E[Y|Z=0]>0$,

$$\begin{aligned} LB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+\max\{\overline{Y}^{10},\overline{Y}^{11}\}p_{0|1}-\min\{\overline{Y}^{01},\overline{Y}^{00}\}p_{1|0} \\ UB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+y^{u}p_{0|1}-y^{l}p_{1|0}; \end{aligned}$$

and if $E[Y|Z=1]-E[Y|Z=0]<0$,

$$\begin{aligned} LB &= \overline{Y}^{11}-\overline{Y}^{00} \\ UB &= E[Y|Z=1]-E[Y|Z=0]. \end{aligned}$$

Proposition 2.6 *Under Assumptions 1.1 through 1.4, 2.1, 2.2 and 2.3c the bounds* $LB \leq ATE \leq UB$ *are sharp, where, if* $E[Y|Z=1]-E[Y|Z=0]>0$,

$$\begin{aligned} LB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+\max\{\overline{Y}^{10},\frac{\overline{Y}^{11}p_{1|1}-\overline{Y}^{01}p_{1|0}}{p_{1|1}-p_{1|0}}\}p_{0|1} \\ &\quad -\min\{\overline{Y}^{01},\frac{\overline{Y}^{00}p_{0|0}-\overline{Y}^{10}p_{0|1}}{p_{1|1}-p_{1|0}}\}p_{1|0} \\ UB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+y^{u}p_{0|1}-y^{l}p_{1|0}; \end{aligned}$$

and if $E[Y|Z=1]-E[Y|Z=0]<0$,

$$\begin{aligned} LB &= \overline{Y}^{11}p_{1|1}-\overline{Y}^{00}p_{0|0}+\frac{\overline{Y}^{11}p_{1|1}-\overline{Y}^{01}p_{1|0}}{p_{1|1}-p_{1|0}}p_{0|1}-\frac{\overline{Y}^{00}p_{0|0}-\overline{Y}^{10}p_{0|1}}{p_{1|1}-p_{1|0}}p_{1|0} \\ UB &= E[Y|Z=1]-E[Y|Z=0]. \end{aligned}$$

Note that, if $LATE_c < 0$, the bounds in Propositions 2.4 through 2.6 do not require boundedness of the outcome, because Assumption 2.2 improves upon the upper bound in Proposition 2.1, while Assumption 2.3 improves upon the lower bound. In contrast, if $LATE_c > 0$, Assumptions 2.2 and 2.3 each improve only upon the lower bound in Proposition 2.1. The bounds in Propositions 2.4 through 2.6 are narrower compared with the bounds in Proposition 2.2 and the corresponding bounds in Proposition 2.3. This is because under the combined assumptions, the monotonicity assumption improves upon further either the lower or upper bound in Proposition 2.3, depending on the sign of $LATE_c$, while the mean dominance assumptions further improve upon the lower bound in Proposition 2.2.

Proposition 2.5 overlaps with the bounds recently derived by Chiburis (2010a) under the MTR assumption without specifying a priori direction and the decreasing MTS assumption, as well as the mean independence assumption of the instrument. This is because Assumption 2.3b coincides with the decreasing MTS assumptions imposed on the counterfactual average outcomes for always-takers and never-takers (i.e., $E[Y(0)|a]$ and $E[Y(1)|n]$). The form of Chiburis' bounds, however, cannot simplify to Proposition 2.6, in that his monotonicity assumptions also involve the counterfactual average outcome for the mixture of never-takers and compliers and that for the mixture of always-takers and compliers (i.e., $E[Y(1)|Z=0,D=0]$ and $E[Y(0)|Z=1,D=1]$), which are not involved in the current setting.

It is important to note that the bounds in Proposition 2.6 are also sharp for ATE if Assumption 2.3c is replaced with the assumption, $E[Y(d)|a] \leq E[Y(d)|c] \leq E[Y(d)|n]$ for $d=0,1$. However, since $E[Y(d)|c]$ may suffer from the potential issue of a weak

IV (i.e., $p_{1|1} - p_{1|0}$ is close to zero), and thus it may be more difficult to estimate than $E[Y|Z = z, D = d]$.

3.3 Bounds Addressing Sample Selection and Noncompliance

3.3.1 Setup, Principal Strata, and Parameter of Interest

To setup the framework in the presence of sample selection and noncompliance, again assume a random sample of size n from a large population is available. For each unit i in the sample, let $Z_i = z \in \{0,1\}$ indicate whether the unit was randomly assigned to the treatment group ($Z_i = 1$) or to the control group ($Z_i = 0$). Let $D_i = d \in \{0,1\}$ indicate whether individual i actually received the treatment ($D_i = 1$) or not ($D_i = 0$). Let me further introduce the sample selection indication and the latent outcome. Let $S_i = s \in \{0,1\}$ be a post-treatment sample selection variable indicating whether the latent outcome variable Y_i^* is observed ($S_i = 1$) or not ($S_i = 0$). In the setting of evaluating wage effects, S_i specifies whether individual i is employed or not, and Y_i^* is the offered market wage. The observed outcome variable is $Y_i = Y_i^*$ if $S_i = 1$, and Y_i is missing if $S_i = 0$.

As in IA (1994) and AIR (1996), $D(z)$ denotes the potential compliance behavior as a function of the treatment assignment. In addition, let $S(z,d)$ and $Y^*(z,d)$ denote the potential values of the selection indicator and the potential latent outcome, respectively, as a function of the treatment assignment (z) and the treatment received (d). In the new setting, we observe $\{Z_i, D_i(Z_i), S_i(Z_i, D_i(Z_i))\}$ for all units, and $Y_i^*(Z_i, D_i(Z_i))$ for those with $S_i = 1$.

The following assumptions are imposed in the new setting to address sample selection and noncompliance simultaneously.

Assumption 3.1 (*Randomly Assigned Instrument*). $\{Y^*(z,d), S(z,d), D(z)\}$ *is independent of* Z *for all* $z,d \in \{0,1\}$.

Assumption 3.2 *(Exclusion Restriction of Z).* $Y^*(z,d) = Y^*(z',d) = Y^*(d)$ *and* $S(z,d) = S(z',d) = S(d)$ *for all* $z,d \in \{0,1\}$.

Assumption 3.3 *(Nonzero Average Effect of Z on D).* $E[D(1) - D(0)] \neq 0$.

Assumption 3.2 states that any effect of the instrument Z on the potential outcomes Y^* and on the potential sample selection indicator S must be via an effect of Z on the treatment D. In other words, this assumption prevents the instrument from having a direct effect on Y^* and S. In the context of the empirical application, Assumption 3.2 requires that randomization affects potential wages and employment only through its effect on JC enrollment. Assumption 3.2 allows me to write the potential variables $Y^*(z,d)$ and $S(z,d)$ as a function of the treatment d only.

As in IA (1994) and AIR (1996), a valid instrument in this context should satisfy Assumptions 3.1, 3.2, and 3.3 simultaneously. An important difference with respect to the assumptions in those two papers is that here Z is required to be a valid instrument for both Y^* and S.

To derive bounds for wage effects accounting for sample selection and noncompliance, a principal stratification framework (Frangakis and Rubin, 2002) is employed. This framework, which generalizes the approach in AIR (1996), is useful for studying causal effects when controlling for post-treatment or intermediate variables (i.e., variables that have been affected by the treatment). The basic principal stratification with respect to a given post-treatment variable is a partition of the population into groups such that, within each group, all individuals share the same potential values of the post-treatment variable under each treatment arm. A principal effect is then defined as a comparison of potential outcomes within a given stratum. Since membership to a particular stratum is not affected by treatment assignment, individuals within a group are comparable and thus principal effects are causal effects.

The intermediate variables to control for are the compliance behavior (D) and the sample-selection (S) indicator. Thus, in this setting, the principal strata are defined by the joint potential values of $\{D(z=0), D(z=1)\} \times \{S(z=0), S(z=1)\}$. Four strata are defined based on the potential compliance behavior: always-takers (a), never-takers (n), compliers (c), and defiers (d) in AIR (1996). Following ZRM (2008) and Frumento et al. (2012), the following subpopulations are defined based on potential employment status:

- $EE = \{i : S_i(0) = S_i(1) = 1\}$, the "always-employed", those who would be employed regardless of treatment assignment;
- $NN = \{i : S_i(0) = S_i(1) = 0\}$, the "never-employed", those who would be unemployed regardless of treatment assignment for them;
- $NE = \{i : S_i(0) = 0, S_i(1) = 1\}$, those who would be employed only if assigned to the treatment group;
- $EN = \{i : S_i(0) = 1, S_i(1) = 0\}$, those who would be employed only if assigned to the control group.

In total, sixteen strata: $\{a,n,c,d\} \times \{EE,NN,NE,EN\}$ are defined. These strata are the same as those in Frumento et al. (2012), and they result from combining the strata employed in AIR (1996) to account for noncompliance with those used in ZRM (2008) to address sample selection.

An important characteristic of principal strata is that they are latent subpopulations, meaning that, in general, econometricians cannot observe to which stratum each individual belongs. Thus, additional assumptions are usually imposed to point or partially identify effects of interest by reducing the number of strata that exist in the population. Note that Assumption 3.2 implies that the following four strata do not exist: aNE, aEN, nNE, and nEN. The reason is that for the individuals in these four strata there exists an effect of the treatment assignment (Z) on employment (S) that is not through their JC enrollment status (since $D_i(1) = D_i(0)$), which contradicts the exclusion restriction of Z.

The following assumption, which was also employed by AIR (1996) is introduced to further reduce the number of existing strata.

Assumption 3.4 *(Individual-Level Monotonicity of D in Z).* $D_i(1) \geq D_i(0)$ *for all i.*

Assumption 3.4 rules out the existence of defiers, thus eliminating the strata dEE, dNN, dEN, and dNE in this setting. In the context of the application, it eliminates the existence of individuals who would enroll in JC only if assigned to the control group. A necessary condition for this assumption to hold is that Z has a non-negative average effect on D, which can be falsified by the data. As further discussed in Subsection 3.3.4, it is possible to relax Assumption 3.4 by letting the direction of the monotonicity be unknown, just as Assumption 1.4 imposes monotonicity without a priori direction.

Under Assumptions 3.1 through 3.4, IV estimators point identify the average treatment effect of D on Y^* (and S) for the compliers in the absence of sample selection. If sample selection is present, however, those assumptions are not enough to point identify the average effect of D on Y^*.

In this section, the parameter of interest is the average treatment effect of D on wages for the "always-employed compliers" (i.e., the cEE stratum):[4]

$$\Delta = E[Y^*(1) - Y^*(0)|cEE] = E[Y(1) - Y(0)|cEE]. \tag{3.3}$$

As can be seen from the definition of the different subpopulations above, this stratum is the only one for which wages are observed for individuals who enrolled and did not enroll into JC after imposing Assumption 3.4. The parameter in (3.3) is also considered in Frumento et al. (2012). It is the average effect for the intersection of the subpopulation IA (1994) and AIR (1996) focus on when accounting for noncompliance with what Lee (2009) and ZRM (2008) focus on when addressing sample selection. In the application, this stratum is the largest one in the population, accounting for about 40 percent. The following subsections construct bounds for (3.3) by considering two more assumptions.

3.3.2 Bounds under Monotonicity

This subsection derives the bounds for Δ in equation (3.3) by extending the trimming procedure bounds in Zhang and Rubin (2003), ZRM (2008), and Lee (2009) to allow

[4]Note that since for compliers $Z = D$, the stratum cEE can also be interpreted as those compliers who would be always employed regardless of treatment *receipt*.

for noncompliance. The following assumption is imposed to further reduce the number of principal strata.

Assumption 3.5 *(Individual-Level Monotonicity of S in D). $S_i(1) \geq S_i(0)$ for all i.*[5]

Assumption 3.5 states that there is a non-negative effect of D on S for every unit. In the application, it assumes that there is a non-negative effect of JC on employment for every individual. While this assumption is similar to the monotonicity assumption employed in ZRM (2008) and Lee (2009), it differs in that the monotonicity of S is imposed in the actual treatment received (D), rather than in the treatment assigned (Z). This type of monotonicity assumption has been employed in the partial identification literature to address problems other than sample selection (e.g., AIR, 1996; Manski and Pepper, 2000; Flores and Flores-Lagunes, 2010a). A testable implication of Assumption 3.5 is that the average effect of D on S for compliers, which is point identified under Assumptions 3.1 through 3.4, is non-negative. Similar to Assumption 3.4, and as further discussed in Subsection 3.3.4, it is possible to relax Assumption 3.5 by letting the direction of the monotonicity be unknown.

Assumption 3.5 rules out strata where the selection indicator S is negatively affected by D. From the strata remaining after imposing Assumptions 3.1 through 3.4, Assumption 3.5 rules out the existence of the cEN stratum. Therefore, under Assumptions 3.1 through 3.5 there are seven strata in the population: aEE, aNN, nEE, nNN, cEE, cNN and cNE. The relationship between these seven strata and the observed groups defined by the values of $\{Z, D(Z), S(Z, D(Z))\}$ is given in Table 3.1.

[5] Under Assumptions 3.1 through 3.4, Assumption 3.5 can be relaxed as "$S_i(1) \geq S_i(0)$ for all compliers" in deriving the bounds for Δ.

Table 3.1: Observed Groups and Principal Strata

		$Z=0$				$Z=1$	
		D				D	
		0	1			0	1
S	0	cNE, cNN, nNN	aNN	S	0	nNN	cNN, aNN
	1	cEE, nEE	aEE		1	nEE	cNE, cEE, aEE

Thus, while some observed groups are composed of only one stratum, some of them are mixtures of two or more strata. Under Assumptions 3.1 through 3.5, the proportion of each stratum in the population can be identified. Let π_k denote the proportion of stratum k in the population, and let $p_{ds|z} \equiv \Pr(D=d, S=s|Z=z)$ and $q_{s|z} \equiv \Pr(S=s|Z=z)$. Then:

$$\pi_{aNN} = p_{10|0}; \pi_{aEE} = p_{11|0}; \pi_{nNN} = p_{00|1}; \pi_{nEE} = p_{01|1} \tag{3.4}$$

$$\pi_{cEE} = p_{01|0} - p_{01|1}; \pi_{cNN} = p_{10|1} - p_{10|0}; \pi_{cNE} = q_{1|1} - q_{1|0}.$$

In addition, the mean outcomes for those observed cells with $S=1$ can be written as a function of mean potential outcomes for different strata. Letting $\overline{Y}^{zds} \equiv E[Y|Z=z, D=d, S=s]$, then:

$$\overline{Y}^{011} = E[Y(1)|aEE] \tag{3.5}$$

$$\overline{Y}^{101} = E[Y(0)|nEE] \tag{3.6}$$

$$\overline{Y}^{001} = E[Y(0)|cEE]\frac{\pi_{cEE}}{p_{01|0}} + E[Y(0)|nEE]\frac{\pi_{nEE}}{p_{01|0}} \tag{3.7}$$

$$\overline{Y}^{111} = E[Y(1)|cEE]\frac{\pi_{cEE}}{p_{11|1}} + E[Y(1)|cNE]\frac{\pi_{cNE}}{p_{11|1}} + E[Y(1)|aEE]\frac{\pi_{aEE}}{p_{11|1}} \tag{3.8}$$

The average potential outcomes under treatment and control are point identified for the nEE and aEE strata, respectively. Moreover, it is possible to point identify $E[Y(0)|cEE]$ by combining equations (3.4), (3.6), and (3.7) to obtain:

$$E[Y(0)|cEE] = \frac{p_{01|0}}{p_{01|0} - p_{01|1}}\overline{Y}^{001} - \frac{p_{01|1}}{p_{01|0} - p_{01|1}}\overline{Y}^{101}. \tag{3.9}$$

Thus, one of the terms of Δ in (3.3) is point identified. But the term $E[Y(1)|cEE]$ is not point identified because two of the conditional means in (3.8) are not point identified. Next, bounds are constructed for $E[Y(1)|cEE]$ and Δ.

In a setting without noncompliance, Zhang and Rubin (2003), ZRM (2008), and Lee (2009) construct bounds for the non-point identified expectation of the potential outcome in the definition of their average effect based on a cell containing only two strata. To illustrate the main idea behind their "worst-case" bounds, suppose that there were no individuals in the aEE stratum, so that $\pi_{aEE} = 0$ and the cell $\{Z = 1, D = 1, S = 1\}$ contained only two strata, cEE and cNE. Then, $E[Y(1)|cEE]$ would be bounded from above (below) by the mean of Y for the fraction $\pi_{cEE}/(\pi_{cEE} + \pi_{cNE})$ of the largest (smallest) values of Y for those individuals in that cell. A key difference between the bounds derived in those studies and this one is that in the current setting the bounds for $E[Y(1)|cEE]$ are derived from a cell containing three strata.

By equations (3.5) and (3.8), although the observed mean $\overline{Y}^{111}$ is a weighted average of the mean potential outcome of $Y(1)$ for three strata, the mean $E[Y(1)|aEE]$ is point identified. Thus, the bounds are constructed by considering "worst-case" scenarios that exploit the information that $\overline{Y}^{011} = E[Y(1)|aEE]$. To motivate the way to construct the bounds, the problem can be thought of as finding "worst-case" scenarios for $E[Y(1)|cEE]$ subject to the constraint that $\overline{Y}^{011} = E[Y(1)|aEE]$. The strategy to derive bounds for $E[Y(1)|cEE]$ is to solve the unconstrained problem first, and then check whether the value of $E[Y(1)|aEE]$ implied by this solution can satisfy the constraint that $\overline{Y}^{011} = E[Y(1)|aEE]$. If the constraint can be satisfied, then the unconstrained solution is just the solution to the constrained problem. Otherwise, impose the constraint first and then obtain the solution to the constrained problem.

Additional notations are introduced to describe the bounds for Δ. Let y_r^{111} be the r-th quantile of Y in the cell $\{Z=1, D=1, S=1\}$, and let

$$\overline{Y}(y_{r'}^{111} \leq Y \leq y_r^{111}) \equiv E[Y|Z=1, D=1, S=1, y_{r'}^{111} \leq Y \leq y_r^{111}]. \tag{3.10}$$

Hence, $\overline{Y}(y_{r'}^{111} \leq Y \leq y_r^{111})$ gives the mean outcome in the cell $\{Z=1, D=1, S=1\}$ for those outcomes between the r'-th and r-th quantiles of Y in that cell. Suppose that I want to derive the lower bound for $E[Y(1)|cEE]$. To begin, I consider the problem without the constraint and ignore the information about aEE. In this case, I can directly apply the existing trimming procedure in ZRM (2008) and Lee (2009) and bound $E[Y(1)|cEE]$ from below by the expected value of Y for the $\pi_{cEE}/p_{11|1}$ fraction of the smallest values of Y in the cell $\{Z=1, D=1, S=1\}$, or, $\overline{Y}(Y \leq y_{\pi_{cEE}/p_{11|1}}^{111})$, where $p_{11|1} = \pi_{cEE} + \pi_{cNE} + \pi_{aEE}$. Next, check whether this solution is consistent with the constraint that $\overline{Y}^{011} = E[Y(1)|aEE]$. To do this, I construct the "worst-case" scenario lower bound for $E[Y(1)|aEE]$, call it $LY_{1,aEE}$, implied by the unconstrained solution by assuming that all the observations that belong to the aEE stratum are at the bottom of the remaining observations in the cell $\{Z=1, D=1, S=1\}$. This yields $LY_{1,aEE} = \overline{Y}(y_{\pi_{cEE}/p_{11|1}}^{111} \leq Y \leq y_{1-(\pi_{cNE}/p_{11|1})}^{111})$. If $LY_{1,aEE} \leq \overline{Y}^{011}$, the unconstrained solution is consistent with the constraint and the lower bound for $E[Y(1)|cEE]$ is $\overline{Y}(Y \leq y^{111}_{\pi_{cEE}/p_{11|1}})$. If $LY_{1,aEE} > \overline{Y}^{011}$, then the unconstrained solution is inconsistent with $\overline{Y}^{011} = E[Y(1)|aEE]$. Intuitively, having $LY_{1,aEE} > \overline{Y}^{011}$ implies that some observations from the aEE stratum must be at the bottom $\pi_{cEE}/p_{11|1}$ fraction of the smallest values of Y in the cell $\{Z=1, D=1, S=1\}$ and thus $\overline{Y}(Y \leq y^{111}_{\pi_{cEE}/p_{11|1}})$ is not a sharp lower bound for $E[Y(1)|cEE]$. In this case, the "worst-case" scenario lower bound for $E[Y(1)|cEE]$ is constructed by placing all the observations in the aEE and cEE strata at the bottom of the distribution of Y in the cell $\{Z=1, D=1, S=1\}$. Thus, if $LY_{1,aEE} > \overline{Y}^{011}$, the lower bound for $E[Y(1)|cEE]$, call it $LY_{1,cEE}$, is derived from the equation:

$$\overline{Y}(Y \leq y^{111}_{1-(\pi_{cNE}/p_{11|1})}) = \frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} LY_{1,cEE} + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} \overline{Y}^{011}, \tag{3.11}$$

where $\overline{Y}(Y \leq y^{111}_{1-(\pi_{cNE}/p_{11|1})})$ is the mean of Y for the $1-(\pi_{cNE}/p_{11|1})$ fraction of the smallest values of Y in the cell $\{Z=1, D=1, S=1\}$.

Note that the lower bound $LY_{1,cEE}$ derived from equation (3.11) does not yield a sharp bound for $E[Y(1)|cEE]$ if $LY_{1,aEE} \leq \overline{Y}^{011}$. For example, if $\overline{Y}^{011} = E[Y(1)|aEE]$ is large, so that it is impossible that all individuals from the aEE stratum are at the bottom $1-(\pi_{cNE}/p_{11|1})$ fraction of the smallest values of Y in the cell $\{Z=1, D=1, S=1\}$, then the lower bound derived from (3.11) will be much lower than $\overline{Y}(Y \leq y^{111}_{\pi_{cEE}/p_{11|1}})$, the lower bound derived without using the information on $E[Y(1)|aEE]$. Intuitively, in this case the value of $\overline{Y}^{011} = E[Y(1)|aEE]$ is so large that it provides little information about the "worst-case" lower bound scenario for $E[Y(1)|cEE]$ (but it will provide valuable information for the upper bound of $E[Y(1)|cEE]$).

The upper bound for $E[Y(1)|cEE]$ is derived in a similar way as the lower bound, but now by placing the observations in the corresponding strata in the upper part of the distribution of Y in the cell $\{Z=1, D=1, S=1\}$. Once bounds for $E[Y(1)|cEE]$ are obtained, they can be combined with the point identification of $E[Y(0)|cEE]$ in (3.9) to construct bounds for the average effect of the always-selected compliers, Δ in (3.3). The following proposition presents bounds for Δ under Assumptions 3.1 through 3.5.

Proposition 3.1 *If Assumptions 3.1 through 3.5 hold, then $L_{cEE} \leq \Delta \leq U_{cEE}$. L_{cEE} and U_{cEE} are lower and upper bounds for Δ given by:*

$$L_{cEE} = LY_{1,cEE} - \overline{Y}^{001} \frac{p_{01|0}}{p_{01|0} - p_{01|1}} + \overline{Y}^{101} \frac{p_{01|1}}{p_{01|0} - p_{01|1}}$$

$$U_{cEE} = UY_{1,cEE} - \overline{Y}^{001} \frac{p_{01|0}}{p_{01|0} - p_{01|1}} + \overline{Y}^{101} \frac{p_{01|1}}{p_{01|0} - p_{01|1}},$$

where

$$LY_{1,cEE} = \begin{cases} \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), \ \text{if} \ \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) \leq \overline{Y}^{011} \\ \overline{Y}(Y \leq y^{111}_{1-\alpha_{cNE}}) \frac{q_{1|0} - p_{01|1}}{p_{01|0} - p_{01|1}} - \overline{Y}^{011} \frac{p_{11|0}}{p_{01|0} - p_{01|1}}, \ \text{otherwise} \end{cases}$$

$$\begin{aligned} UY_{1,cEE} &= \begin{cases} \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}}), \ \text{if} \ \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}}) \geq \overline{Y}^{011} \\ \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}}) \frac{q_{1|0} - p_{01|1}}{p_{01|0} - p_{01|1}} - \overline{Y}^{011} \frac{p_{11|0}}{p_{01|0} - p_{01|1}}, \quad \text{otherwise} \end{cases} \\ \alpha_{cEE} &= \frac{\pi_{cEE}}{p_{11|1}} = \frac{p_{01|0} - p_{01|1}}{p_{11|1}}, \text{ and} \\ \alpha_{cNE} &= \frac{\pi_{cNE}}{p_{11|1}} = \frac{q_{1|1} - q_{1|0}}{p_{11|1}}. \end{aligned}$$

Proof. See Appendix A.

3.3.3 Bounds under Mean Dominance

This subsection considers a mean dominance assumption that narrows the bounds in Proposition 3.1.

Assumption 3.6 *(Mean Dominance).* $E[Y(1)|cEE] \geq E[Y(1)|cNE]$.

The intuition behind Assumption 3.6 is the same as the one behind Assumption 2.3: some strata are expected to have more favorable characteristics and thus better potential outcomes than others. In the application, this assumption states that the mean potential outcome under treatment of the always-employed compliers is greater than or equal to that of those who would be employed only if they enrolled in JC. Assumption 3.6 implies a positive correlation between employment and wages, which is supported by standard economic models of labor supply. Zhang and Rubin (2003), ZRM (2008) and Huber and Mellace (2010) consider stochastic-dominance versions of Assumption 3.6. For example, in the current setting, such an assumption would state that the potential outcome under treatment of the cEE stratum at any rank of the outcome distribution is weakly less than that of the cNE stratum. For the purposes of the dissertation, stochastic dominance is much stronger than needed.

As Assumption 2.3 in the Section 3.2, even though Assumption 3.6 is not directly testable, it is possible to get indirect evidence about its plausibility by comparing the average baseline characteristics of the cEE and cNE strata that are closely related to the outcome of interest (e.g., values of the outcome prior to randomization). Assumption 3.6 is less likely to hold if these comparisons suggest that the cNE stratum has better characteristics at the baseline than does the cEE stratum. Under Assumptions 3.1 through 3.5 it is possible to point identify the average characteristics for all seven strata at the baseline. This can be seen by noting that the observed average characteristics at the baseline for each of the observed groups $\{Z, D, S\}$ in Table 3.1 is a weighted average of the average characteristics for the different strata (see, for reference, equations (3.7) and (3.8)), with the weights being point identified from (3.4). Based on these equations, the average characteristics at the baseline for all seven strata can be estimated by solving an overidentified GMM problem. This tool is implemented in the application and further details are provided in the Appendix C.

The mean dominance assumption above tightens the bounds in Proposition 3.1 by increasing the lower bound on $E[Y(1)|cEE]$. To get the new lower bound, note that, similar to equation (3.8), I can write

$$\overline{Y}^{111} = E[Y(1)|cEE, cNE]\frac{\pi_{cEE} + \pi_{cNE}}{p_{11|1}} + E[Y(1)|aEE]\frac{\pi_{aEE}}{p_{11|1}}, \tag{3.12}$$

where the stratum proportions and $E[Y(1)|aEE]$ are point identified. With Assumption 3.6, $E[Y(1)|cEE] \geq E[Y(1)|cEE, cNE]$, which provides a lower bound for $E[Y(1)|cEE]$ that is greater than or equal to the one obtained in Proposition 3.1. The following proposition presents bounds for Δ under Assumptions 3.1 through 3.6.

Proposition 3.2 *If Assumptions 3.1 through 3.6 hold, then $L_{cEE} \leq \Delta \leq U_{cEE}$. L_{cEE} and U_{cEE} are lower and upper bounds for Δ, where U_{cEE} is equal to the upper bound for Δ given in Proposition 3.1 and L_{cEE} equals:*

$$L_{cEE} = LY_{1,cEE} - \overline{Y}^{001}\frac{p_{01|0}}{p_{01|0} - p_{01|1}} + \overline{Y}^{101}\frac{p_{01|1}}{p_{01|0} - p_{01|1}},$$

with

$$LY_{1,cEE} = \frac{p_{11|1}\overline{Y}^{111} - p_{11|0}\overline{Y}^{011}}{p_{11|1} - p_{11|0}}.$$

Proof. See Appendix A.

3.3.4 Remarks on Assumptions

The following remarks discuss how to relax the assumptions used in Section 3.3.

Remark 1. As Assumption 1.4 in Section 3.1, it is possible to relax the individual-level monotonicity assumptions (Assumptions 3.4 and 3.5) by not requiring prior knowledge about their direction. This is closely related to the work by Shaikh and Vytlacil (2011), who derive bounds for average treatment effects on binary outcomes with a valid instrument by imposing monotonicity (or threshold crossing models) assumptions similar to those in Assumptions 3.4 and 3.5 without specifying the direction of the monotonicity (see also BSV, 2008). In this setting, Assumptions 3.4 and 3.5 can be replaced with the following assumptions.

Assumption 3.4' *(Individual-Level Monotonicity of D in Z, unknown direction). Either* $D_i(1) \geq D_i(0)$ *for all i or* $D_i(1) \leq D_i(0)$ *for all i.*

Assumption 3.5' *(Individual-Level Monotonicity of S in D, unknown direction). Either* $S_i(1) \geq S_i(0)$ *for all i or* $S_i(1) \leq S_i(0)$ *for all i.*

Assumption 3.7 $E[S|Z=1] - E[S|Z=0] \neq 0$.

Under Assumptions 3.1, 3.2, 3.3, 3.4', 3.5', and 3.7, it is possible to infer the directions of the monotonicity assumptions above from the observed data, and hence, derive bounds for the average effect of D on Y for either the cEE or dEE stratum by the procedure described in Subsection 3.3.2.

The direction of the monotonicity of D in Z can be inferred directly from $E[D|Z=1] - E[D|Z=0]$. In addition, note that under Assumption 3.7 the instrumental variable estimator of the effect of D on S, $(E[S|Z=1] - E[S|Z=0])/(E[D|Z=1] - E[D|Z=0])$, point identifies the effect of D on S for a subpopulation (either the compliers or the defiers, depending of the direction of Assumption 3.4'). Since all the individuals in the population share the same direction of the monotonicity, the direction of the monotonicity of S in D can be inferred from the sign of the instrumental variable estimator.[6]

Depending on the direction of the monotonicity in Assumption 3.4', the parameter of interest would be the average effect of D on Y either for the always-employed compliers or the always-employed defiers. In the context of constructing bounds for this effect, the three assumptions above imply the existence of only one stratum out of cNE, cEN, dNE and dEN.[7] Thus, similar to the case considered in Subsection 3.3.2, one of the mean potential outcomes in the parameter of interest would be point identified (e.g., $E[Y(0)|cEE]$), while the other is partially identified (e.g., $E[Y(1)|cEE]$). The bounds for the non-point identified term can be constructed following the same procedure described in Subsection 3.3.2. Moreover, an appropriate mean dominance assumption similar to Assumption 3.6 could be used to narrow the bounds, as in Subsection 3.3.3.

Remark 2. It is possible to construct bounds on Δ without Assumption 3.5, in which case the stratum cEN is not ruled out and appears in the observed cells $\{Z = 0, D = 0, S = 1\}$ and $\{Z = 1, D = 1, S = 0\}$ in Table 3.1. Although the proportions of the strata aEE, aNN, nEE, and nNN are still point identified, neither the proportions of the strata cEE, cNN, cNE, and cEN nor the term $E[Y(0)|cEE]$ is now point identified. To construct bounds for Δ in this case, the approach in Subsection 3.3.3 can be combined with that followed by Zhang and Rubin (2003), ZRM (2008), Imai (2008), and Huber and Mellace (2010) in a setting with sample selection but without the noncompliance

[6]Note that, under the current assumptions, if $E[S|Z=1]-E[S|Z=0]=0$, the number of strata reduces to six: aEE, aNN, nEE, nNN, plus either cEE and cNN or dEE and dNN. In this case, the average treatment effect of D on Y is point identified for either the cEE or the dEE stratum.

[7]To see this, note that by Assumptions 3.1 and 3.2, $E[D|Z=1]-E[D|Z=0]=\pi_c-\pi_d$ and $E[S|Z=1]-E[S|Z=0]=[\Pr(cNE|c)-\Pr(cEN|c)]\pi_c+[\Pr(dNE|d)-\Pr(dEN|d)]\pi_d$.

issue. The main idea for constructing their bounds is to consider "worst-case" scenarios for their effect of interest that are consistent with the possible values that π_{EN} (and hence π_{EE}) can take based on the data.[8] In the current setting, the "worst-case" scenarios for Δ occur when π_{cEE} is at its minimum value that is consistent with the data, which equals $p_{01|0} - p_{01|1} - p_{10|1} + p_{10|0}$.[9] Given this lower bound for π_{cEE}, the same approach as in Subsection 3.3.2 to derive bounds for Δ by constructing bounds for $E[Y(1)|cEE]$ and $E[Y(0)|cEE]$. However, the bounds in this case will be wider than those presented in Proposition 3.1, and may result in uninformative bounds (see e.g., Blanco, Flores, and Flores-Lagunes, 2012).

Remark 3. In the absence of Assumptions 3.5 and 3.6, the lower bound for Δ in Proposition 3.2 provides information for another parameter of interest, $ATE_{cEE,cNE}$, which is defined as the weighted average of Δ and the ATE for cNE. L_{cEE} in Proposition 3.2 can be viewed as the lower bound for $ATE_{cEE,cNE}$, under Assumptions 3.1 through 3.4 and the following assumption.

Assumption 3.5" $E[Y(0)|cEE,cEN] \geqslant E[Y(0)|cEE,cNE]$, where $E[Y(0)|k_1,k_2]$ is the weighted average of $Y(0)$ between two strata k_1 and k_2.

This assumption states that the mean value of $Y(0)$ (i.e., the potential wage if not attending JC) for compliers who would be employed if they did not attend JC (cEE and

[8]Zhang and Rubin (2003) and ZRM write the bounds for their parameter as functions of π_{EN}, and then obtain the upper or lower bound by minimizing or maximizing the bounds over all possible values of π_{EN} that are consistent with the data. Huber and Mellace (2010) show that the numerical optimization is not necessary, and the bounds are obtained at the maximal value of π_{EN}.

[9]The range of possible values of π_{cEE} is calculated from the eight cells in Table 3.1, which yields $\pi_{cEE} \in [\max(0, p_{01|0} - p_{01|1} - p_{10|1} + p_{10|0}), \min(p_{01|0} - p_{01|1}, p_{11|1-}p_{11|0})]$. As noticed by Lee (2009), the bounds of Δ are well defined only if $\pi_{cEE} > 0$, which implies π_{cEE} is minimized at $p_{01|0} - p_{01|1} - p_{10|1} + p_{10|0}$.

cEN) is greater than or equal to that for compliers who would be employed if they did attend JC (cEE and cNE). This assumption exploits the positive correlation between employment and wages implied by standard models of labor supply, as the cEN are employed under the control treatment but the cNE are not. Indirect evidence regarding the plausibility of Assumption 3.5" can be obtained by comparing the weighted average baseline characteristics, $E[X|cEE, cEN]$ and $E[X|cEE, cNE]$, derived from the cells $\{Z = 0, D = 0, S = 1\}$ and $\{Z = 1, D = 1, S = 1\}$, respectively. In applications where Assumption 3.5 or 3.6 are difficult to justify, Assumption 3.5" may become attractive. Furthermore, the mixture of the strata cEE and cNE seems to be an interesting target group, since those are individuals who would comply with the treatment assignment and would be employed if they attended JC.

Remark 4. This section focuses on the average treatment effect of the cEE stratum. It is possible to combine the methods in the previous subsections with those in Huber and Mellace (2010) to construct bounds for the average effects of other subpopulations. For instance, consider the average effect of the treated and selected individuals (those with $D = 1$ and $S = 1$), or of the treated and selected compliers. As can be seen from Table 3.1, these other subpopulations are mixtures of different strata for which, with the exception of cEE, wages are unobserved under one of the treatment assignments. For example, wages are never observed under the control treatment for those who would be unemployed if they did not enroll in JC (the cNE group), or for those who would be always employed regardless of treatment assignment (the always-takers). Thus, additional assumptions (e.g., a bounded outcome) are needed to partially identify the effects for other strata or subpopulations.

3.4 Estimation and Inference

A first consideration when performing statistical inference in partially identified models is whether one wants to construct confidence regions for the identified set (e.g., $[LB, UB]$, $[L_{cEE}, U_{cEE}]$) or for the true value of the parameter (e.g., ATE, Δ). The section focuses on confidence regions for the partially identified parameter.

Imbens and Manski (2004) and Stoye (2009) provide confidence intervals that asymptotically cover the true value of a parameter θ_0 with a fixed probability when the bounds are of the form $\theta_0^l \leq \theta_0 \leq \theta_0^u$ and there are estimators of θ_0^l and θ_0^u that behave asymptotically like sample means. Their analysis, however, does not apply to all the bounds in the dissertation because the bounds in Propositions 2.4, 2.5, 2.6, and 3.1, 3.2 involve minimum (min) and maximum (max) operators. For example, the upper bound $UY_{1,cEE}$ for $E[Y(1)|cEE]$ in Proposition 3.1 can be written as $UY_{1,cEE} = \min\{\overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}}), \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}})\frac{q_{1|0}-p_{01|1}}{p_{01|0}-p_{01|1}} - \overline{Y}^{011}\frac{p_{11|0}}{p_{01|0}-p_{01|1}}\}$. When the two quantities in the min operator are close to each other, the distribution of the estimator for the upper bound is not well approximated by a normal distribution. Those two operators create complications for estimation and inference. First, sample analog estimators of the bounds can be severely biased in small samples. Because of the concavity (convexity) of the min (max) function, sample analog estimates of the bounds tend to be much narrower than the true bounds. Second, closed-form characterization of the asymptotic distribution of estimators for parameters involving min or max functions is very difficult to derive and, thus, usually unavailable. Moreover, Hirano and Porter (2012) show that there exist no locally asymptotically unbiased estimators and no regular estimators for parameters that are nonsmooth functionals of the underlying data distribution, such as those involving min or max operators.[10] Hence, the uniform asymptotic normality condition in Imbens and Manski (2004) and Stoye (2009) does not hold when the two operators are present. This has generated a growing literature on inference methods for partially identified models of this type (see Tamer, 2010, and the references therein).

To address those issues, the dissertation employs the methodology proposed by Chernozhukov, Lee, and Rosen (2011) that are applicable to bounds of the form $[\theta_0^l, \theta_0^u]$, where $\theta_0^l = \sup_{v \in \mathscr{V}} \theta^l(v)$, $\theta_0^u = \inf_{v \in \mathscr{V}} \theta^u(v)$, $\theta^l(v)$ and $\theta^u(v)$ are bounding functions, and $\mathscr{V}$ is the set over which the infimum and supremum are taken. They employ precision-corrected estimates of the bounding functions before applying the infimum and supremum operators. The precision adjustment consists of adding to each estimated bounding function its pointwise standard error times an appropriate critical value. Hence, estimates with higher standard errors require larger adjustments. Depending on the choice of the critical value, it is possible to obtain confidence regions for either the identified set or the true parameter value, as well as half-median unbiased estimators for the lower and upper bounds. The half-median-unbiasedness property means that the upper (lower) bound estimator exceeds (falls below) the true value of the upper (lower) bound with the probability of at least one-half asymptotically. This property is important because achieving local asymptotic unbiasedness is impossible (Hirano and Porter, 2012). For details on the procedure of applying their method to the bounds in these five propositions see Appendix B. For the bounds without min or max operators, sample

[10] As documented by Hirano and Porter (2011), nonexistence of local asymptotic unbiased estimators implies that bias correction procedures cannot completely get rid of local bias and that reducing bias too much will eventually lead to arbitrarily large variance.

analog estimators and the confidence regions for the true parameter value proposed by Imbens and Manski (2004) are used.

CHAPTER 4

APPLICATION: EVALUATION OF JOB CORPS

4.1 Job Corps and the National Job Corps Study

Since 1964, Job Corps (JC) has been a central part of the federal government efforts to provide job training and employment assistance to disadvantaged youth. Services such as academic education, vocational training, residential living, health care and health education, counseling and job placement assistance are delivered at more than 120 centers nationwide. To be eligible for the program, an individual must be a legal resident of the United States, be between 16 and 24 years old and come from a low-income household. According to the US Department of Labor (2005), a typical JC student lives at a local JC center for eight months and receives about 1100 hours of academic and vocational instruction, which is equivalent to approximately one year in high school.

This dissertation employs data from the National Job Corps Study (NJCS), a randomized experiment funded by the US Department of Labor to evaluate the effectiveness of JC. The study examined the impacts of JC on labor market outcomes, welfare dependence and several other outcomes to help assess whether the program achieved its goals of helping students become more responsible and productive citizens. Eligible individuals who applied to JC for the first time between November 1994 and December 1995 (80,833 individuals) were randomly assigned to a program, control, or program non-research group. Individuals in the control group (5,977) were embargoed from the program for a period of three years, while those in the program (treatment) group (9,409) were allowed to enroll in JC. The research sample was interviewed at the time of random assignment and at 12, 30, and 48 months after random assignment. Due to both design and programmatic reasons, some subpopulations were randomized in the NJCS with different (but known) probabilities (Schochet, Burghardt, and Glazerman, 2001). Hence, design weights are employed throughout the analysis.

Taking advantage of randomization, most of previous literature evaluating the JC program studies ITT effects or $LATE$s for compliers (e.g., Burghardt et al., 2001; Schochet, Burghardt, and Glazerman, 2001; Schochet, Burghardt, and McConnell, 2008; Lee, 2009; Blanco, Flores, and Flores-Lagunes, 2012). The noncompliance behavior, however, tends to dilute the impacts of JC. Across the samples in the dissertation, 73% of individuals of the treatment group actually enrolled in JC, while 4% of individuals of the control group also enrolled. Burghardt et al. (2001), Schochet, Burghardt, and Glazerman (2001) and Schochet, Burghardt, and McConnell (2008) also adjust to noncompliance by examining the $LATE$, which is representative for the subpopulation (i.e., compliers) accounting for 69% in the population. Different from the previous literature, using the random assignment as an IV, the present research addresses this noncompliance issue and analyzes the population ATEs of actual JC enrollment on participants' labor market outcomes and welfare. It also examines wage effects of JC enrollment by focusing on a specified subpopulation, i.e., the always-employed compliers. Thus, the dissertation also contributes to the previous literature on the evaluation of the JC program by providing credible partial identification results for treatment effects other than $LATE$ and the ITT effect.

4.2 The Population ATE of JC Enrollment

4.2.1 Specific Sample

Given the objectives and services provided by JC (e.g., academic and vocational training, job search assistance), making inference about the population average treatment effects of JC enrollment is of great public interest. This section uses data on individuals who responded to the 48-month interview to examine the ATEs on weekly earnings and employment at Week 208 (i.e., four years) and public assistance benefits received during the fourth year after randomization[11]. On one hand, JC tends to have positive effects on participants' labor market outcomes given its objectives and services. On the other hand, participants may experience a reduction in welfare receipt while they enroll in JC, because the program provides shelter (except to nonresidential students), food, and a stipend. After they leave JC, participants may receive less public income support because of higher earnings. Schochet, Burghardt, and Glazerman (2001) report that the reductions in benefit receipt persisted throughout four years after randomization.

The treatment variable indicates whether or not the individual ever enrolled in JC during the 208 weeks after random assignment. The random assignment indicator serves as an instrument for JC enrollment. Two samples are obtained by dropping individuals with missing relevant variables from the survey.[12] The sample for weekly earnings and employment involves 10,520 individuals (4,187 and 6,333 in the control and treatment

[11]Benefits include Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF), food stamps, Supplemental Security Income (SSI) or Social Security Retirement, Disability, or Survivor (SSA), and General Assistance.

[12]Two samples are derived because individuals with missing labor market outcomes and with missing public benefits are different.

groups, respectively), and for public benefits 10,976 individuals (4,387 and 6,589 in the control and treatment groups, respectively).

Table 4.1 reports the average baseline characteristics of both samples by treatment assignment status along with the percentage of missing values for each of those variables. The pre-treatment variables include demographic characteristics, education and background variables, employment, earnings and public benefits dependency at baseline, as well as labor market outcomes in the year prior to randomization. As one would expect, the average pre-treatment characteristics of the treatment and control groups are similar in both the samples due to randomization, with the difference in means being statistically different from zero at the five percent level for only one variable (personal income: 3,000-6,000). Thus, both samples maintain the balance of baseline variables between the control and treatment groups.

4.2.2 Assessment of Assumptions

Assumptions 1.1 through 1.4 are commonly used in the literature to address noncompliance in experimental settings. This subsection concentrates on the discussion of assessment of Assumptions 2.2 and 2.3.

Table 4.2 shows some relevant point identified averages for both samples. The noncompliance behavior is similar between the two samples. As already mentioned, 73% of individuals of the treatment group actually enrolled in JC, while 4% of individuals of the control group also enrolled during the 208 weeks after randomization. The ITT effects on weekly earnings, employment and public benefits are 22.19 , .038 and -84.29, respectively. These effects are all statistically significant, with their signs as expected.

Table 4.1: Summary Statistics of Baseline Variables (Population ATE of JC)

	Sample for Labor Market Outcomes				Sample for Public Assistance Benefits			
	Missing Prop.	Z=1	Z=0	Diff. (Std.Err.)	Missing Prop.	Z=1	Z=0	Diff. (Std.Err.)
Female	0	.417	.407	.009 (.010)	0	.415	.406	.009 (.010)
Age at Baseline	0	18.42	18.38	.035 (.042)	0	18.41	18.38	.031 (.041)
White, Non-hispanic	0	.273	.266	.007 (.009)	0	.274	.269	.005 (.009)
Black, Non-Hispanic	0	.483	.478	.005 (.010)	0	.477	.474	.003 (.010)
Hispanic	0	.171	.179	-.008 (.008)	0	.172	.180	-.008 (.007)
Other Race/Ethnicity	0	.073	.078	-.005 (.005)	0	.076	.076	.000 (.005)
Never Married	.017	.916	.915	.001 (.006)	.020	.914	.915	-.001 (.005)
Married	.017	.020	.022	-.002 (.003)	.020	.020	.022	-.001 (.003)
Living Together	.017	.040	.041	-.001 (.004)	.020	.040	.041	-.001 (.004)
Separated	.017	.024	.022	.002 (.003)	.020	.025	.022	.003 (.003)
Has Child	.007	.181	.184	-.003 (.008)	.008	.181	.183	-.002 (.008)
Number of Children	.011	.253	.248	.005 (.012)	.012	.251	.247	.004 (.012)
Personal Education	.018	10.08	10.09	-.008 (.031)	.021	10.08	10.10	-.019 (.030)
Mother's Education	.194	11.50	11.51	-.011 (.058)	.197	11.49	11.53	-.042 (.057)
Father's Education	.391	11.43	11.54	-.110 (.073)	.394	11.45	11.57	-.127* (.072)
Ever Arrested	.017	.258	.263	-.005 (.009)	.019	.259	.266	-.007 (.009)
Household Inc.: <3000	.368	.252	.258	-.006 (.011)	.371	.250	.255	-.005 (.011)
3000-6000	.368	.201	.204	-.004 (.010)	.371	.198	.208	-.010 (.010)
6000-9000	.368	.116	.111	.006 (.008)	.371	.117	.109	.008 (.008)
9000-18000	.368	.245	.243	.001 (.011)	.371	.246	.241	.005 (.011)
>18000	.368	.187	.183	.003 (.010)	.371	.189	.187	.002 (.010)
Personal Inc.: <3000	.083	.786	.790	-.004 (.008)	.086	.783	.788	-.006 (.008)
3000-6000	.083	.129	.129	.000 (.007)	.086	.130	.131	-.000 (.007)
6000-9000	.083	.055	.046	.009** (.005)	.086	.056	.046	.010** (.004)
>9000	.083	.031	.036	-.005 (.004)	.086	.031	.035	-.004 (.004)
Have Job	.031	.216	.209	.007 (.008)	.034	.219	.211	.009 (.008)
Weekly Hours Worked	0	21.69	21.13	.563 (.417)	0	21.71	21.14	.576 (.407)
Weekly Earnings	0	110.35	104.29	6.059 (4.482)	0	110.66	104.53	6.136 (4.328)
Had Job, Prev. Yr.	.016	.651	.643	.008 (.010)	.019	.653	.646	.007 (.009)
Months Employed,Prev.Yr.	0	3.575	3.516	.058 (.085)	0	3.582	3.518	.064 (.083)
Earnings, Prev.Yr.	.081	2991.8	2873.1	118.65 (109.10)	.084	3020.7	2893.8	126.84 (107.01)
Received Public Benefits	.115	.590	.595	-.005 (.010)	.118	.582	.590	-.008 (.010)
Months Received Benefits	.127	6.554	6.542	.012 (.125)	.129	6.469	6.493	-.024 (.122)
Numbers of Observations	10520	6333	4187		10976	6589	4387	

Note: Z denotes whether the individual was randomly assigned to participate ($Z = 1$) or not ($Z = 0$) in the program. Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance. Numbers in parentheses are standard errors. ** and * denote that difference is statistically different from 0 at 5% and 10% level, respectively. Computations use the weights that account for sample and interview design and interview non-response.

The $LATE_c$ estimates for compliers on earnings, employment and public benefits are 32.29, .055 and -122.28, respectively, 45 percent higher than their corresponding ITT estimates. By Assumption 2.2, the sign of $LATE_c$ identifies the sign of the $LATE$s for the other two strata. Thus, the estimates of $LATE_c$ indicate positive population average treatment effects on weekly earnings and employment and a negative population effect on public benefits.

Table 4.2 also shows the proportion of each stratum in the samples. In both of them, the proportion of compliers is the largest, .69, followed by never-takers, .27, and always-takers, .04. And by Assumption 2.2, there are no defiers in the samples. The end part of Table 4.2 reports the point identified averages cited in Assumptions 2.2 and 2.3.[13] These estimates are all statistically significant and follow a certain pattern in both samples: under the treated status, the average outcome for always-takers is the smallest, followed by the average for the mixture of always-takers and compliers, and the average for compliers, while under the untreated status, the average outcome for compliers is the smallest, followed by the average for the mixture of never-takers and compliers, and the average for never-takers. Always-takers seem to be the least favorable group despite their strong initiative of participation while never-takers seem to do fine even without participation.

As mentioned previously, differences across these point identified averages may provide a preliminary hint for Assumption 2.3. To begin, the direction of Assump-

[13] As in Lee (2009), a *transformed measure* is used to estimate the sample averages of weekly earnings and public benefits to minimize the effect of outliers. Specifically, the entire observed outcome distribution (for either weekly earnings or public benefits) is split into 20 percentile groups ($5^{th}, 10^{th}, \ldots, 95^{th}, 100^{th}$), and then the mean outcome within each of the 20 groups is assigned to each individuals.

Table 4.2: Point Identified Average Outcomes after Random Assignment

	Labor-Market-Outcome Sample			Public-Benefit Sample	
Variables:	Enrollment	Earnings	Employment	Enrollment	Public benefits
Averages for $Z = 1$	.730**	228.78**	.608**	.732**	747.21**
	(.006)	(3.004)	(.006)	(.005)	(23.40)
Averages for $Z = 0$	.043**	206.60**	.570**	.043**	831.50**
	(.003)	(3.552)	(.008)	(.003)	(30.28)
$ITT\ Effects$	.687**	22.19**	.038**	.689**	-84.29**
	(.006)	(4.652)	(.010)	(.006)	(38.27)
$LATE_c$		32.29**	.055**		-122.28**
		(7.007)	(.015)		(56.78)
Proportions of Strata under Assumptions 1.1 through 1.4					
π_n	.270**			.268**	
	(.006)			(.006)	
π_c	.687**			.689**	
	(.007)			(.006)	
π_a	.043**			.043**	
	(.003)			(.003)	
Other Point Identified Average Outcomes under Assumptions 1.1 through 1.4					
$E[Y(1)\|a]$		132.10**	.393**		545.45**
		(14.94)	(.037)		(110.12)
$E[Y(0)\|n]$		223.79**	.600**		880.67**
		(5.967)	(.012)		(47.98)
$E[Y(1)\|c]$		236.82**	.624**		707.81**
		(4.022)	(.008)		(28.26)
$E[Y(0)\|c]$		204.53**	.569**		830.09**
		(5.655)	(.012)		(49.69)
$E[Y\|Z = 1, D = 1]$		230.63**	.611**		698.35**
		(3.614)	(.007)		(25.87)
$E[Y\|Z = 0, D = 0]$		209.96**	.578**		844.25**
		(3.709)	(.008)		(33.18)

Note: Z denotes whether the individual was randomly assigned to participate ($Z = 1$) or not ($Z = 0$) in the program. D denotes whether the individual was ever enrolled in the program ($D = 1$) or not ($D = 0$) during the 4 years (208 weeks) after randomization. Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance. Numbers in parentheses are standard errors. ** denotes estimate is statistically different from 0 at 5% level. Computations use the weights that account for sample and interview design and interview non-response. The standard errors of $LATE$s, proportions of strata and other identified average outcomes are calculated by 5000-repetition bootstrap.

tion 2.3c may be inferred by comparing the identified averages of always-takers and never-takers to those of compliers under the same treatment status. The hypotheses that $E[Y(1)|at] \leq E[Y(1)|c]$ and $E[Y(0)|c] \leq E[Y(0)|nt]$ are not rejected for all of the three outcomes. Thus, if the same relationship with compliers also hold under the alternative treatment status, Assumption 2.3c is expected to hold. Furthermore, since the ITT effect on public benefits is negative, testable implications are available when Assumption 2.3c is combined with Assumption 2.2, as discussed in Subsection 3.2.3. These testable implications are not rejected in the application.

More importantly, indirect evidence of Assumption 2.3 is obtained by comparing the average baseline characteristics across strata. These average characteristics of each stratum are estimated from a nonparametric GMM problem. For each baseline variable, 5 moment functions (4 derived from the conditional expectations defined by $\{Z, D\}$ plus 1 from the the entire sample) are used to identify three (stratum) means. The procedure for estimating this overidentified nonparametric GMM problem is provided in Appendix C. Tables 4.3 and 4.4 show these estimates and their differences across strata for the samples. The average characteristics across strata are similar between the two samples. Among the three strata, never-takers are more likely to be female, older, married, have children, a higher level of education, personal income above $9,000 (less likely to have personal income below $3,000), higher weekly earnings at baseline, and to have better labor market outcomes the year before randomization. By contrast, always-takers tend to be male, younger, have a lower level of education at baseline, and have lower earnings in the previous year. The higher earnings of never-takers may be explained by their higher level of education and around one more year of working experience compared with those of always-takers. Related to the identified outcomes of the two strata in Table 4.2, this may explain why never-takers lacked an initiative to participate in the JC. Doing household work and taking care of children may also prevent them from spending extra time training. The statistically significant difference between always-takers and never-takers indirectly supports Assumption 2.3a, while the differences obtained by comparing to compliers (i.e., columns 4 and 5) tend to support Assumption 2.3c (except those in proportion of individuals who have household income above $18,000 or personal income below $3,000). When the differences across the three strata are all statistically significant, Assumption 2.3b are more likely to hold. Note that the differences across the strata in the public benefit dependency at the baseline are not statistically significant.[14] Thus, it is concluded from these results that the data do not provide indirect evidence against Assumption 2.3, and that the point estimates of the differences suggest that this assumption is plausible.

4.2.3 Empirical Results

Table 4.5 shows the bounds on the population ATEs on the labor market outcomes and the public dependency under Proposition 2.1 through Proposition 2.6. Under each pair of the estimated bounds, a 95% level confidence interval for the true parameter is reported. Since the bounds for weekly earnings and employment in Propositions 2.4 through 2.6 involve max or min operators, this subsection reports the half-median unbiased estimators and the corresponding confidence intervals proposed by Chernozhukov,

[14] Unfortunately, information about the amount of public benefits in dollars is unavailable at the baseline.

Table 4.3: Average Baseline Characteristics in the Sample for Labor Market Outcomes

Variable	nt	c	at	$nt-c$	$c-at$	$nt-at$
Female	.467**	.397**	.324**	.070**	.073**	.143**
	(.011)	(.007)	(.035)	(.015)	(.037)	(.036)
Age at Baseline	18.74**	18.32**	17.64**	.428**	.674**	1.102**
	(.052)	(.029)	(.133)	(.063)	(.137)	(.143)
White, Non-hispanic	.284**	.263**	.296**	.021*	-.033	-.012
	(.011)	(.006)	(.034)	(.013)	(.036)	(.036)
Black, Non-Hispanic	.472**	.484**	.488**	-.012	-.004	-.016
	(.012)	(.007)	(.037)	(.015)	(.039)	(.039)
Married	.035**	.016**	.005	.019**	.011**	.030**
	(.004)	(.002)	(.005)	(.005)	(.005)	(.006)
Has Child	.237**	.162**	.148**	.075**	.015	.089**
	(.010)	(.005)	(.028)	(.012)	(.030)	(.029)
Personal Education	10.27**	10.05**	9.637**	.224**	.408**	.632**
	(.035)	(.020)	(.095)	(.044)	(.101)	(.100)
Household Inc.:<3000	.267**	.255**	.187**	.012	.068**	.080**
	(.008)	(.005)	(.021)	(.010)	(.022)	(.022)
>18000	.181**	.181**	.233**	.000	-.052*	-.052*
	(.007)	(.004)	(.027)	(.009)	(.028)	(.027)
Personal Inc.: <3000	.750**	.799**	.843**	-.049**	-.044	-.093**
	(.010)	(.005)	(.026)	(.012)	(.027)	(.027)
>9000	.042**	.030**	.015*	.012*	.015*	.027**
	(.005)	(.002)	(.008)	(.006)	(.009)	(.009)
Have Job	.224**	.208**	.216**	.015	-.008	.008
	(.010)	(.006)	(.031)	(.012)	(.033)	(.032)
Weekly Hrs.Worked	22.07**	21.29**	20.44**	.775	.853	1.629
	(.488)	(.272)	(1.652)	(.585)	(1.734)	(1.700)
Weekly Earnings	113.79**	102.76**	92.63**	11.03**	10.13	21.15**
	(2.989)	(2.041)	(7.986)	(3.989)	(8.328)	(8.562)
During the Year Prior to Random Assignment						
Had Job	.667**	.640**	.651**	.027**	-.010	.016
	(.010)	(.006)	(.035)	(.013)	(.036)	(.035)
Mths.Worked	3.684**	3.527**	3.120**	.157	.407	.563*
	(.102)	(.057)	(.310)	(.125)	(.324)	(.325)
Earnings	3246.8**	2831.5**	2302.9**	415.30**	528.64**	943.94**
	(101.80)	(63.58)	(251.57)	(127.99)	(263.42)	(273.94)
Received Benefits	.607**	.588**	.596**	.020	-.009	.011
	(.011)	(.006)	(.037)	(.013)	(.038)	(.037)
Mths. Received	6.744**	6.503**	6.518**	.240	-.014	.226
	(.122)	(.073)	(.414)	(.153)	(.437)	(.424)

Note: Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance. Numbers in parentheses are standard errors. ** and * denote that estimate is statistically different from 0 at 5% and 10% level, respectively. Computations use the weights that account for sample and interview design and interview non-response. Missing values for each of the baseline variables were imputed with the mean of the variable.

Table 4.4: Average Baseline Characteristics in the Sample for Public Assistance Benefits

Variable	nt	c	at	$nt-c$	$c-at$	$nt-at$
Female	.464** (.011)	.396** (.006)	.330** (.035)	.069** (.014)	.066* (.037)	.134** (.036)
Age at Baseline	18.75** (.049)	18.31** (.027)	17.68** (.126)	.435** (.061)	.635** (.135)	1.070** (.133)
White, Non-hispanic	.289** (.011)	.265** (.006)	.289** (.035)	.024* (.014)	-.024 (.037)	-.000 (.036)
Black, Non-Hispanic	.461** (.012)	.480** (.007)	.503** (.037)	-.019 (.015)	-.023 (.039)	-.042 (.039)
Married	.036** (.004)	.016** (.002)	.006 (.005)	.020** (.005)	.010** (.005)	.030** (.006)
Has Child	.234** (.009)	.163** (.005)	.164** (.029)	.072** (.012)	-.001 (.031)	.071** (.030)
Personal Education	10.27** (.034)	10.05** (.020)	9.663** (.091)	.225** (.043)	.382** (.096)	.607** (.094)
Household Inc.:<3000	.262** (.008)	.253** (.004)	.198** (.020)	.009 (.010)	.055** (.022)	.064** (.021)
>18000	.184** (.007)	.184** (.004)	.233** (.028)	.000 (.009)	-.050* (.029)	-.049* (.028)
Personal Inc.: <3000	.746** (.010)	.797** (.005)	.840** (.024)	-.051** (.012)	-.043* (.026)	-.094** (.025)
>9000	.042** (.005)	.030** (.002)	.015** (.007)	.012** (.006)	.015* (.008)	.027** (.009)
Have Job at Baseline	.227** (.010)	.211** (.005)	.213** (.028)	.016 (.012)	-.002 (.030)	.014 (.029)
Weekly Hrs. Worked	21.80** (.460)	21.41** (.291)	20.63** (1.426)	.392 (.594)	.774 (1.548)	1.165 (1.494)
Weekly Earnings	112.60** (2.890)	103.55** (2.180)	94.21** (7.394)	9.025** (4.094)	9.342 (7.954)	18.37** (7.804)
During the Year Prior to Random Assignment						
Had Job	.667** (.011)	.642** (.006)	.668** (.031)	.025* (.013)	-.026 (.033)	-.001 (.032)
Mths. Employed	3.644** (.103)	3.553** (.057)	3.060** (.282)	.091 (.130)	.492 (.302)	.584* (.299)
Earnings	3241.9** (99.19)	2863.6** (65.20)	2390.4** (233.19)	378.31** (130.21)	473.14* (250.73)	851.45** (249.72)
Received Benefits	.601** (.010)	.581** (.006)	.583** (.033)	.020 (.013)	-.001 (.035)	.019 (.034)
Mths. Received	6.684** (.122)	6.433** (.076)	6.395** (.378)	.251 (.158)	.038 (.408)	.289 (.385)

Note: Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance. Numbers in parentheses are standard errors. ** and * denote that estimate is statistically different from 0 at 5% and 10% level, respectively. Computations use the weights that account for sample and interview design and interview non-response. Missing values for each of the baseline variables were imputed with the mean of the variable.

Table 4.5: Bounds on the Population Average Treatment Effects

	Earnings		Employment		Public Benefits	
	LB	*UB*	*LB*	*UB*	*LB*	*UB*
Bounds under Each Assumption						
Proposition 2.1	-69.86	201.02	-.150	.163	-632.86	1812.4
Bounded Outcome (A2.1)	(-78.34, 210.61)		(-.167, .179)		(-702.21, 1901.6)	
Proposition 2.2	22.19	201.02	.038	.163	-632.86	-84.29
Monotonicity (A2.2)	(14.18, 210.61)		(.021, .179)		(-702.21, -22.13)	
Proposition 2.3a	-6.507	201.02	-.027	.163	-188.43	1812.4
Mean Dominance (A2.3a)	(-16.65, 210.61)		(-.050, .179)		(-265.90, 1901.6)	
Proposition 2.3b	20.67	201.02	.033	.163	-145.90	1812.4
Mean Dominance (A2.3b)	(11.97, 210.61)		(.015, .179)		(-212.69, 1901.6)	
Proposition 2.3c	22.57	201.02	.037	.163	-142.76	1812.4
Mean Dominance (A2.3c)	(13.72, 210.61)		(.019, .179)		(-210.62, 1901.6)	
Bounds under Combined Assumptions						
Proposition 2.4	20.43	201.02	.034	.163	-188.43	-84.29
(A2.1, A2.2, A2.3a)	(13.01, 210.58)		(.018, .180)		(-265.95, -22.09)	
Proposition 2.5	22.97	201.01	.039	.163	-145.90	-84.29
(A2.1, A2.2, A2.3b)	(14.53, 210.56)		(.020, .180)		(-213.01, -21.83)	
Proposition 2.6	24.61	201.04	.042	.163	-142.76	-84.29
(A2.1, A2.2, A2.3c)	(16.01, 210.59)		(.023, .180)		(-210.62, -22.13)	

Note: Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance. Numbers in parentheses are 95% level confidence intervals for true parameters of interest. The confidence intervals of the bounds under each assumption are calculated by the method of Imbens and Manski (2004). For earnings and employment, the confidence intervals of the bounds under combined assumptions are calculated by the method of Chernozhukov, Lee and Rosen (2011), while the bounds under combined assumptions are estimated by their half-median unbiased estimators. For public benefits, the confidence intervals are calculated by the method of Imbens and Manski (2004). Computations use the weights that account for sample and interview design and interview non-response. Standard errors are calculated by 5000-repetition bootstrap.

Lee, and Rosen (2011). The bounds without max or min operators are estimated with sample analogs and their confidence intervals are obtained by the method of Imbens and Manski (2004).[15]

[15] Specifically, the Imbens and Manski (2004) confidence interval at 95% level is calculated from $(\widehat{\Delta}_{LB} - \overline{C}_n * \widehat{\sigma}_{LB}/\sqrt{N}, \widehat{\Delta}_{UB} + \overline{C}_n * \widehat{\sigma}_{UB}/\sqrt{N})$, where $\widehat{\sigma}_{LB} = \sqrt{\widehat{V(\widehat{\Delta}_{LB})}}$, $\widehat{\sigma}_{UB} = \sqrt{\widehat{V(\widehat{\Delta}_{UB})}}$, and $\overline{C}_n$ satisfies $\Phi(\overline{C}_n + \sqrt{N}(\widehat{\Delta}_{UB} - \widehat{\Delta}_{LB})/\max(\widehat{\sigma}_{LB}, \widehat{\sigma}_{UB})) - \Phi(-\overline{C}_n) = .95$.

Let me begin with the ATE on weekly earnings in the first two columns. Proposition 2.1 provides the bounds in the AIR setting under the bounded-outcome assumption (A2.1). The estimated bounds are rather wide and fail to identify the sign of the ATE. Note that these bounds are also the IV bounds proposed by Manski (1990), Heckman and Vytlacil (2000), Kitagawa (2009) and Huber and Mellace (2010). The ATE in Proposition 2.2 under the monotonicity assumption (A2.2) is bounded between [22.19, 201.02], obtained by identifying positive $LATE$s for always-takers and never-takers. Note that they are also the ones under the IV and MTR assumptions proposed by Manski and Pepper (2000). The mean dominance assumptions (A2.3) improve upon the lower bound in Proposition 2.1, and negative effects are ruled out in Propositions 2.3b and 2.3c in the absence of inferring the sign of the $LATE_a$ and $LATE_n$ from that of the $LATE_c$. When Assumptions 1.1 through 1.4, and 2.1 through 2.3 are all imposed together, all of the bounds and their corresponding confidence intervals lie in the positive region. In particular, the bounds on the ATE on the weekly earnings in Proposition 2.6 are the narrowest, [24.61, 201.04], with the lower bound 10 percent higher than the ITT effect (22.19), while the $LATE_c$ for compliers (32.29) falls within the bounds. It turns out that inferring the unobserved terms (i.e., $E[Y(0)|a]$ and $E[Y(1)|n]$) from the point identified outcomes of compliers (A2.3) provides a sharper lower bound on the ATE on the weekly earnings than that obtained by identifying the sign of $LATE_a$ and $LATE_n$ under the monotonicity assumption (A2.2).

The next two columns show the bounds on the ATE on employment, whose value is between 0 and 1. A similar pattern to the bounds for the weekly earnings is also found in the bounds for employment. Note that the identified set in Proposition 2.1, [−.015,

.163], is unable to identify the sign of the ATE, and that it also coincides with those in Balke and Pearl (1997) for the case of a binary outcome. The bounds in Proposition 2.2 indicate a positive ATE, which varies between [.038, .163], and also equal the ones proposed by BSV (2008), Chesher (2010), Chiburis (2010b) and Shaikh and Vytlacil (2011), all of which analyze a binary outcome. Again, Proposition 2.6 provides the narrowest bounds on the ATE on employment under Assumptions 2.1, 2.2 and 2.3c, [.042, .163], with the lower bound 10 percent higher than the ITT effect (.038), while the $LATE_c$ for compliers (.055) falls within the bounds in Proposition 2.6.

The final two columns report the bounds on the ATE on public benefits. Different from the labor market outcomes, since the ITT effect on public benefits is negative, imposing only the monotonicity assumption improves upon its upper bound in Proposition 2.1, while imposing only the mean dominance assumptions improves upon its lower bound. The bounds in Proposition 2.1 are extremely wide and uninformative. The monotonicity assumption (A2.2) has strong identification power compared with the mean dominance assumptions (A2.3) in the case of the public benefits, in that the former identifies the negative sign of the ATE on the public benefits, though the latter greatly sharpens the lower bound in Proposition 2.1 by at least 70 percent. However, once the two types of assumptions are taken into account together, the bounded outcome assumption is no longer necessary and the width of the bounds shrinks dramatically. Under the combined assumptions, the estimated bounds and their corresponding confidence intervals lie in the negative region. Proposition 2.6 provides the narrowest bounds on the ATE on public benefits, $[-142.76, -84.29]$, with the upper bound equal to the ITT effect, while the $LATE_c$ on compliers (-122.28) falls within the bounds.

I draw the following conclusions from the empirical results of the population average treatment effects of JC enrollment. Focusing on the labor market outcomes and welfare dependence, the monotonicity assumption (A2.2) and the mean dominance assumption (A2.3) together provide the narrowest bounds on the ATEs. In particular, when the former is combined with Assumption 2.3c, these bounds are [24.61, 201.04] for weekly earnings, [.042, .163] for employment, and [-142.76, -84.29] for public benefits. These bounds are significantly narrower than the IV bounds proposed by Manski (1990), Heckman and Vytlacil (2000), Kitagawa (2009) and Huber and Mellace (2010). The width of these bounds is also smaller than that under the IV and MTR assumptions of Manski and Pepper(2000), especially for public benefits. The bounds on employment are also narrower than the ones proposed by Balke and Pearl (1997), BSV (2008), Chesher (2010), Chiburis (2010b) and Shaikh and Vytlacil (2011) for the case of a binary outcome. The lower bounds for weekly earnings and employment are 10 percent higher than their respective intention-to-treat (ITT) effects (22.19 and .038), while the upper bound for public benefits is equal to its ITT effect. The $LATE$s for compliers on the three outcomes also fall within these narrowest bounds. In sum, these empirical results suggest that enrolling into the Job Corps program increases weekly earnings by at least \$24.61 and employment by at least 4.3 percentage points four years after randomization, and decreases yearly dependence on public welfare benefits by at least \$84.29.

4.3 The Wage Effect of JC Enrollment

4.3.1 Specific Sample

Assessing the effect of training programs on wages is of great importance to policy makers. Further analysis of the wage effects of the Job Corps program answers an important question of whether the program increases participants' human capital accumulation and thus leads to an increase in their wages. To compare with the wage effects of JC in the absence of addressing the noncompliance issue, this section employs the same sample from the NJCS used by Lee (2009). This sample involves only individuals with non-missing values for weekly earnings and weekly hours worked for every week after random assignment (9,145 individuals). I construct the data set by adding enrollment information at Week 208 (i.e., 48 months) after random assignment. Again, this binary variable indicates whether or not the individual was ever enrolled in JC during the 208 weeks after random assignment. 55 observations are dropped from Lee's sample due to the missing enrollment variable, resulting in the final sample of 9,090 individuals (3,599 and 5,491 individuals in the control and treatment groups, respectively). Wages at Week 208 are obtained by dividing weekly earnings by weekly hours worked at that week. An individual is regarded as unemployed when the wage is missing, and as employed otherwise.

The first four columns of Table 4.6 report the average baseline characteristics of the entire sample by treatment assignment status, along with the percentage of missing values for each of those variables. The pre-treatment variables include demographic characteristics, education and background variables, employment and earnings information at the baseline, and labor market outcomes in the year prior to randomization. The average pre-treatment characteristics of the treatment and control groups are similar as expected, with the difference in means being statistically different from zero at the five percent level only for weekly hours worked at baseline. Thus, this sample maintains the balance of baseline variables between the treatment and control groups.

The first three columns of Table 4.7 show the averages of some relevant post-treatment variables based on treatment assignment status, along with their differences, at Week 208 after randomization. The first row shows information on the JC enrollment variable. By Week 208, 73.8 percent of those assigned to the treatment group and 4.4 percent of the control group had ever enrolled in JC.[16] The difference in these two numbers, which equals the proportion of compliers in the population, is 69.4 percent. The rest of the rows in Table 4.7 present the ITT effect and $LATE$ for JC compliers on various labor market outcomes at Week 208. All the ITT effects of JC on weekly hours worked, weekly earnings, and employment are positive and statistically significant. The $LATE$ estimates for those three variables are also positive and statistically significant, and they are larger than the ITT estimates by about 44, 44 and 50 percent, respectively. The estimated average effects of JC on earnings and employment for compliers is 39.9 dollars and 6 percentage points, respectively. These results are consistent with the findings in the NJCS (Burghardt et al., 2001).

For reference, Table 4.7 also shows the estimated ITT and $LATE$ effects of JC on ln(wage) for employed individuals. The $LATE$ estimate implies an average effect of JC on ln(wage) of about 5.4 percent for compliers. However, these estimates are biased

[16] From these controls, 3.2 percent enrolled after the end of the embargo period, while 1.2 percent of them enrolled in the program despite the three-year embargo imposed on them.

Table 4.6: Summary Statistics of Baseline Variables (Wage Effect of JC)

	Entire Sample				Non-Hispanics			
	Missing Prop.	Z=1	Z=0	Diff.(Std.Err.)	Missing Prop.	Z=1	Z=0	Diff.(Std.Err.)
Female	0	.454	.458	-.004 (.011)	0	.454	.454	-.010 (.012)
Age at Baseline	0	18.44	18.35	.087* (.046)	0	18.44	18.34	.096* (.050)
White, Non-hispanic	0	.265	.263	.002 (.009)	0	.319	.318	.001 (.011)
Black, Non-Hispanic	0	.494	.491	.003 (.011)	0	.595	.593	.002 (.012)
Hispanic	0	.169	.172	-.003 (.008)	—	—	—	—
Other Race/Ethnicity	0	.072	.074	-.002 (.006)	0	.087	.089	-.003 (.007)
Never married	.017	.917	.916	.002 (.006)	.018	.926	.924	.002 (.006)
married	.017	.020	.023	-.003 (.003)	.018	.015	.018	-.003 (.003)
Living together	.017	.039	.040	-.002 (.004)	.018	.035	.037	-.002 (.004)
Separated	.017	.024	.021	.003 (.003)	.018	.023	.020	.003 (.003)
Has Child	.007	.189	.193	-.004 (.008)	.006	.187	.190	-.004 (.009)
Number of children	.010	.270	.268	.002 (.014)	.180	.269	.271	-.003 (.015)
Personal Education	.018	10.12	10.11	.013 (.033)	.018	10.14	10.12	.022 (.036)
Mother's Education	.188	11.49	11.46	.030 (.061)	.182	11.81	11.83	-.021 (.055)
Father's Education	.383	11.40	11.54	-.145* (.077)	.379	11.72	11.86	-.147** (.072)
Ever Arrested	.017	.248	.249	-.001 (.009)	.017	.255	.257	-.002 (.010)
Household Inc.: <3000	.358	.253	.251	.001 (.012)	.357	.248	.244	.004 (.013)
3000-6000	.358	.205	.208	-.003 (.011)	.357	.202	.213	-.012 (.012)
6000-9000	.358	.117	.114	.003 (.009)	.357	.119	.105	.015 (.009)
9000-18000	.358	.246	.245	.001 (.011)	.357	.244	.248	-.003 (.013)
>18000	.358	.180	.182	-.002 (.010)	.357	.187	.191	-.003 (.012)
Personal Inc.: <3000	.079	.788	.789	-.001 (.009)	.077	.787	.788	-.001 (.010)
3000-6000	.079	.128	.131	-.003 (.008)	.077	.129	.136	-.007 (.008)
6000-9000	.079	.053	.046	.007 (.005)	.077	.052	.043	.009* (.005)
>9000	.079	.031	.034	-.003 (.004)	.077	.031	.033	-.001 (.004)
At Baseline:								
Have job	.021	.198	.192	.007 (.009)	.021	.204	.187	.017* (.009)
Weekly hours worked	0	21.83	20.91	.922** (.447)	0	21.97	20.75	1.216** (.491)
Weekly earnings	0	111.08	102.89	8.183 (5.134)	0	107.79	102.28	5.516** (2.804)
Had job, Prev. Yr.	.017	.635	.627	.008 (.010)	.017	.642	.627	.015 (.011)
Months employed,Prev.Yr.	0	3.603	3.530	.074 (.091)	0	3.654	3.512	.143 (.100)
Earnings, Prev.Yr.	.062	2911.0	2810.5	100.56 (117.58)	.064	2900.3	2794.7	105.57 (106.34)
Numbers of observations	9090	5491	3599		7529	4551	2978	

Note: Z denotes whether the individual was randomly assigned to participate ($Z = 1$) or not ($Z = 0$) in the program. Numbers in parentheses are standard errors. ** and * denote that difference is statistically different from 0 at 5% and 10% level, respectively. Computations use design weights.

Table 4.7: Summary Statistics of Post-treatment Variables by Random Assignment

	Entire Sample			Non-Hispanics		
	$Z=1$	$Z=0$	Diff. (Std.Err.)	$Z=1$	$Z=0$	Diff. (Std.Err.)
Enrollment Variable						
Ever enrolled in JC	.738	.044	.694** (.007)	.737	.047	.689** (.008)
Intention-to-Treat (ITT) Effects						
Hours per week	27.80	25.83	1.967** (.559)	28.05	25.53	2.523** (.617)
Earnings per week	228.19	200.50	27.69** (5.121)	230.22	194.66	35.57** (5.555)
Employed	.607	.566	.041** (.011)	.609	.559	.050** (.012)
ln(wage)	2.029	1.991	.038** (.011)	2.028	1.977	.050** (.013)
Local ATE for Compliers, $LATE$ (IV estimates)						
Hours per week			2.834** (.782)			3.661** (.867)
Earnings per week			39.90** (6.457)			51.62** (6.960)
Employed			.060** (.015)			.072** (.017)
ln(wage)			.054** (.016)			.073** (.017)

Note: Z denotes whether the individual was randomly assigned to participate ($Z=1$) or not ($Z=0$) in the program. Numbers in parentheses are standard errors. ** denotes that difference is statistically different from 0 at 5% level. Computations use design weights. The standard error of the effect for employed compliers is calculated by an ML estimator, where the endogenous dummy variable is the treatment receipt indicator. The treatment assignment indicator is used as the exclusion restriction and all baseline characteristics (where mean values were imputed for missing values) are included in both the selection and outcome equations.

because of sample selection.

4.3.2 Assessment of Assumptions

Since Assumptions 3.1 through 3.4 have been previously used in the NJCS to estimate the effect of JC on labor market outcomes that are not affected by sample selection

(Burghardt et al., 2001; Schochet, 2001; Schochet, Burghardt, and Glazerman, 2001), here the discussion is concentrated on the plausibility of Assumptions 3.5 and 3.6.

Assumption 3.5 states that there is a non-negative effect of JC enrollment on employment for every individual at Week 208. This assumption seems plausible in the application given the objectives and services provided by JC (e.g., academic and vocational training, job search assistance). A testable implication of this assumption is that the $LATE$ for JC compliers on employment is non-negative. As discussed above, this effect is positive and highly statistically significant.

There are two potential threats to the validity of Assumption 3.5. First, individuals who enroll in JC may be less likely to be employed while undergoing training than those who do not enroll, which is usually referred to as the "lock-in" effect (van Ours, 2004). Second, trained individuals may raise their reservation wages because of the JC training, which may lead them to reject some job offers that they would otherwise accept if they had not received training. Both potential threats, however, are likely to become less relevant in the long run, as trained individuals are no longer "locked-in" away from employment, and individuals who chose to remain unemployed in the short run because of raising their reservation wages find jobs in the long run. Consistent with this view, Schochet, Burghardt, and Glazerman (2001), and Lee (2009) find negative effects of JC on employment in the short run, and positive effects in the long run. Thus, the analysis focuses on wages at Week 208 after random assignment, which is the latest wage measure available in the NJCS.

Based on a likelihood-based analysis, Frumento et al. (2012) provide evidence that there may be a positive proportion of compliers in the population for whom JC has a negative effect on employment at Week 208, even though this proportion falls over time after randomization. Therefore, to further increase the plausibility of Assumption 3.5 in the application, a sample that excludes Hispanics is also considered. Hispanics were the only demographic group in the NJCS for which negative (although statistically insignificant) effects of JC on both employment and earnings were found (Schochet, Burghardt, and Glazerman, 2001; Flores-Lagunes, Gonzalez, and Neumann, 2010). Flores-Lagunes, Gonzalez and Neumann (2010) investigate this issue and find that the different outcomes of white, blacks and Hispanics are strongly related to the different local labor market conditions they face. In particular, they find evidence that during that period Hispanics faced worst local unemployment rates than blacks and whites. Therefore, Assumption 3.5 may not be appropriate for Hispanics. The last set of columns in Tables 4.6 and 4.7 present summary statistics of pre- and post-treatment variables for the Non-Hispanics sample (7,529 individuals). As expected, the estimated ITT and $LATE$ effects of JC on labor outcomes for Non-Hispanics are stronger than those for the entire sample, with a statistically significant average effect on employment for compliers of 7.2 percentage points.

Assumption 3.6 states that the mean potential outcome under treatment of the always-employed compliers (cEE stratum) is greater than or equal to that of individuals who would be employed only if they enrolled in JC (cNE stratum). As discussed before, it is possible to indirectly shed some light on the plausibility of this assumption by comparing average baseline characteristics of the cEE and cNE strata that are likely to be highly correlated to wages at Week 208. Appendix Table C.1 presents the average characteristics of two strata for the entire and Non-Hispanics samples, obtained by estimating the overidentified GMM procedure described in Appendix C. Focusing on the Non-Hispanics sample, relative to individuals in the cNE stratum, individuals in the cEE stratum are more likely to be male and white, to have never been arrested at the baseline, and to have better labor market outcomes the year before randomization. These differences, however, are not statistically different from zero. Thus, it is concluded that the data do not provide indirect evidence against Assumption 3.6, and that the point estimates of the differences suggest that the assumption is plausible.

Related to their plausibility, I also check the sensitivity of the bounds in Propositions 3.1 and 3.2 to these assumptions. Subsection 3.3.4 provides simulation exercises to examine how the bounds behave when Assumptions 3.5 and 3.6 fail.

4.3.3 Empirical Results

This analysis begins with bounding the average effect of being allowed to enroll in JC on wages (ITT effect) for the individuals who would always be employed regardless of treatment *assignment*, and then bound the average effect of JC enrollment on wages for those compliers who would always be employed regardless of the treatment *receipt* (Δ in (3.3)). The first parameter is the one considered in Lee (2009) and ZRM (2008), which ignores the noncompliance issue. In their setting, the principal strata are EE, NN, NE, and EN, where the last stratum is ruled out by assuming the monotonicity of S in Z.

Table 4.8 presents bounds on the average ITT effect of JC on ln(wage) for always-employed individuals (EE stratum). The first column of Table 4.8 presents results for the entire sample. The proportion of the always-employed individuals (EE) in the population equals 56.6 percent, and the proportion of those in the NE stratum (which equals the ITT effect of JC on employment) equals 4.1 percent. Under the monotonicity of S in Z assumption, the estimated lower and upper bounds for the ITT effect of JC on ln(wage) for the EE stratum are $-.022$ and .100, respectively. These results are very similar to those obtained by Lee (2009).[17] As noted by Lee (2009), although the bounds include zero, they rule out plausible negative effects. Also as noted by Lee (2009), these particular lower bounds are based on the extreme and unintuitive assumption that wages are perfectly negatively correlated with the probability of being employed.[18] This is contradicted by standard models of labor supply, in which individuals with higher wages are also more likely to be employed.

The last set of rows in Table 4.8 present the bounds on the ITT effect of JC on ln(wage) for the EE stratum under the mean dominance assumption that the average potential wage under $z = 1$ of the EE stratum is greater than that of the NE stratum. This assumption can be seen as a way to rule out the implausible extreme case mentioned above by implying a positive correlation between wages and employment. In this case, the estimated lower bound for the ITT effect for the EE stratum is .038. Thus, under this additional assumption, the bounds are able to rule out the negative ITT effect of JC on wages, which illustrates the identifying power of this assumption. Table 4.8 also presents 95 percent confidence intervals, which are calculated based on the results from

[17] The results are not numerically the same as those in Lee (2009) who uses a transformed wage variable to calculate bounds to minimize the effect of outliers by splitting the entire observed wage distribution into 20 percentile groups ($5^{th}, 10^{th}, \ldots, 95^{th}, 100^{th}$), and then assigning the mean wage within each of the 20 groups to each individual. Here the original wage variable is used, and 55 observations are dropped from Lee's sample because of missing enrollment information. For reference, the corresponding lower and upper bounds in Lee (2009) are $-.019$ and .093, respectively.

[18] To obtain the lower bound all the EE individuals are placed at the bottom of the wage distribution in the cell $\{Z = 1, S = 1\}$, which is a mixture of the EE and NE strata.

Table 4.8: Bounds for the ITT Effects on ln(wage) at Week 208

	Entire Sample	Non-Hispanics
Proportions of strata:		
Always-employed(EE)	.566** (.009)	.559** (.009)
Never-employed(NN)	.393** (.007)	.391** (.007)
Employed only if assigned to program(NE)	.041** (.011)	.050** (.012)
$E[Y(Z=0)\|EE]$	1.991** (.009)	1.977** (.010)
Proportion of EE in cell$\{Z=1,S=1\}$	.932** (.017)	.918** (.019)
Bounds with Monotonicity		
Lower bound for the ITT effect for EE stratum	-.022 (.016)	-.018 (.017)
Upper bound for the ITT effect for EE stratum	.100** (.014)	.119** (.015)
Imbens and Manski 95% confidence interval	[-.048, .123]	[-.047, .144]
Bounds with Monotonicity and Mean Dominance		
Lower bound for the effect for EE stratum	.038** (.012)	.050** (.013)
Upper bound for the ITT effect for EE stratum	.100** (.014)	.119** (.015)
Imbens and Manski 95% confidence interval	[.019, .123]	[.029, .144]

Note: Numbers in parentheses are standard errors. ** denotes that estimate is statistically different from 0 at 5% level. Computations use design weights. The standard error is calculated by a 5,000-repetition bootstrap. Imbens and Manski 95% confidence interval is calculated as $(\widehat{\Delta}_{LB}-1.645*\widetilde{\sigma}_{LB},\widehat{\Delta}_{UB}+1.645\widetilde{\sigma}_{UB})$, where $\overline{C}_n = 1.645$ and $\widetilde{\sigma}_{LB}$ and $\widetilde{\sigma}_{UB}$ are calculated by bootstrap.

Imbens and Manski (2004) and asymptotically cover the true value of the parameter with .95 probability.[19] As above, while the 95 percent confidence intervals do not rule out a zero effect under the monotonicity assumption, they rule out negative effects once the mean dominance assumption is employed.

The second column of Table 4.8 presents results for Non-Hispanics, for whom the monotonicity assumption of S in Z is more plausible. In general, the lower and upper bounds under the two sets of assumptions are larger for Non-Hispanics than for the entire

[19]Imbens and Manski (2004) confidence intervals are valid for the ITT effect of JC on wages because the bounding functions do not involve min or max operators.

population. Under the monotonicity and mean dominance assumptions, the estimated lower and upper bounds on the ITT effect of JC on ln(wage) for the EE stratum are .050 and .119, respectively, and the 95 percent confidence interval is [.029, .144].

Table 4.9 shows the estimation results for the parameter of interest, the average effect of JC enrollment on ln(wage) for always-employed compliers. This table shows the estimated proportions, relevant quantities used in estimating the bounds, and the unbiased half-median estimators for the bounds and the confidence intervals for true parameters proposed by Chernozhukov, Lee, and Rosen (2011) (hereafter, CLR). As above, the first column presents the results for the entire sample, and the second shows the results for Non-Hispanics. For both samples, the largest stratum is the cEE stratum, with an estimated proportion of almost 40 percent, while the stratum of always-employed always-takers (aEE) is the smallest stratum, with an estimated proportion of about 1.6 and 1.8 percent for the entire and Non-Hispanics samples, respectively. The estimated proportion of always-takers ($\pi_{aEE} + \pi_{aNN}$) is 4.4 percent for the entire sample, while the proportion of never-takers is 26.2 percent. These proportions are slightly higher for the Non-Hispanics sample. All the estimated stratum proportions in Table 4.9 are statistically different from zero.

For the entire population, the estimated lower and upper bounds on the average effect of JC on ln(wage) for the cEE stratum under Assumptions 3.1 through 3.5 are $-.022$ and .130, respectively, while the corresponding numbers for Non-Hispanics are $-.014$ and .161. Given the weak effects of JC on labor market outcomes for Hispanics, it is not surprising that the bounds for Non-Hispanics cover a larger positive region than those for the entire population. For both samples, the estimated lower and upper bounds are larger

Table 4.9: Bounds for the Effects of JC on ln(wage) for the cEE Stratum

	Entire Sample	Non-Hispanics
Number of observations	9090	7529
π_{aEE}	.016** (.002)	.018** (.002)
π_{nEE}	.158** (.005)	.160** (.005)
π_{cEE}	.391** (.010)	.381** (.011)
π_{cNE}	.041** (.011)	.050** (.012)
π_{aNN}	.028** (.003)	.030** (.003)
π_{nNN}	.104** (.004)	.104** (.005)
π_{cNN}	.261** (.007)	.258** (.008)
α_{cEE}	.872** (.023)	.849** (.025)
$E[Y(1)\|aEE]$	2.033** (.059)	2.016** (.061)
$E[Y(0)\|nEE]$	2.033** (.016)	2.033** (.017)
$E[Y(0)\|cEE]$	1.972** (.015)	1.952** (.016)
$\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$	2.429** (.066)	2.376** (.057)
$\overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$	1.676** (.034)	1.703** (.035)
Bounds with Monotonicity (Proposition 3.1)		
$[LY_{1,cEE}, UY_{1,cEE}]$	[1.951, 2.102]	[1.938, 2.113]
CLR 95% level confidence interval	(1.921, 2.128)	(1.907, 2.140)
$[L_{cEE}, U_{cEE}]$	[-.022, .130]	[-.014, .161]
CLR 95% level confidence interval	(-.061, .168)	(-.057, .201)
Bounds with Monotonicity and Mean Dominance (Proposition 3.2)		
$[LY_{1,cEE}, UY_{1,cEE}]$	[2.027, 2.102]	[2.026, 2.113]
CLR 95% level confidence interval	(2.011, 2.129)	(2.008, 2.141)
$[L_{cEE}, U_{cEE}]$	[.055, .130]	[.074, .161]
CLR 95% level confidence interval	(.023, .170)	(.039, .202)

Note: ** denotes estimate is statistically different from 0 at 5% level and numbers in parentheses are standard errors. Computations use design weights. The standard error is calculated by a 5,000-repetition bootstrap. Numbers in square brackets are half-median unbiased estimators for the bounds, while numbers below them are CLR 95% level confidence intervals.

than the corresponding bounds for the ITT effect presented in Table 4.8, especially the upper bound (e.g., for Non-Hispanics, .119 versus .161, or a 35.3 percent increase). From Tables 4.8 and 4.9, the positive region covered by the bounds on the effect of JC enrollment on wages for the cEE stratum is larger than the positive region covered by the bounds on the ITT effect of JC on wages for the EE stratum. This suggests that the effects of JC on wages obtained by Lee (2009) were conservative, as the effect was weakened by noncompliance to the assigned treatment.

As in Lee (2009), the bounds in Proposition 3.1 are unable to rule out zero effects of JC on wages at Week 208 employing only the monotonicity assumption on the effect of JC on employment. However, as before, the lower bounds are constructed under the implausible "worst-case" scenario of a perfect negative correlation between employment and wages, which is contradicted by standard economic models. The mean dominance assumption rules out this implausible extreme case and helps to increase the lower bound. The last set of rows in Table 4.9 show results when the monotonicity and mean dominance assumptions are both used. Under Assumptions 3.1 through 3.6, the estimated lower bound on the average effect of JC on ln(wage) for the cEE stratum is .055 for the entire population, and it is .074 for Non-Hispanics. Therefore, under all six assumptions, the results imply positive average effects of JC on wages for the cEE stratum in both the entire and Non-Hispanics samples. These results also reinforce the notion that the ITT effects of JC on wages are likely to be lower than the effect of JC enrollment on wages. Also note that, as already mentioned in Remark 3, the lower bound for Δ in Proposition 3.2 can be interpreted as the lower bound for $ATE_{cEE,cNE}$ under Assumptions 3.1 through 3.4 and 3.5".

The following conclusions are derived from the empirical analysis of the effects of JC on wages. First, the results in the subsection strongly suggest a positive average effect of participating in JC on wages four years after random assignment for the always employed compliers. For Non-Hispanics, for whom the monotonicity assumption on the effect of JC on employment is more likely to hold, the estimated bounds under Assumptions 3.1 through 3.6 imply that, on average, JC enrollment increases the average wage (as oppose to ln(wage)) of the always-employed compliers who participate in JC by at least 7.7 and at most 17.5 percent. Therefore, this evidence suggests that JC has an effect on participants' earnings not only by increasing their probability of being employed but also by increasing their wages, which is most likely a consequence of their human capital accumulation during enrollment in JC.

Second, the analysis implies that the results from the study of the ITT effects of JC on wages (e.g., Lee, 2009) are conservative because the noncompliance issue is likely to dilute the effect of JC enrollment on wages. In particular, for the two samples, and regardless of whether or not Assumption 3.6 is employed, the positive region covered by the bounds on the effect of JC enrollment on wages for the cEE stratum is larger than the positive region covered by the bounds on the ITT effect of JC on wages for the EE stratum. This is consistent with the results presented in Subsection 4.3.1, regarding the effect of JC on other labor market outcomes not suffering from sample selection, which show that the $LATE$ estimates of the effects are larger than the ITT estimates. This conclusion is also consistent with the literature on point estimation of the wage effects of JC.[20]

[20] Frumento et al. (2012) find that their point estimate of the effect of JC enrollment on wages at week 208 for the always-employed compliers is larger than the point estimate of the ITT effect in Zhang, Rubin,

Finally, to analyze the sensitivity of the results for missing values of relevant variables, and to compare the results to those in Frumento et al. (2012), I estimate the bounds on Δ that account for this issue. I employ another weight constructed by the NJCS using non-public data that accounts for both sample design and non-response.[21] Thus, the key assumption is that the probability that the information is missing for a given individual is random conditional on the set of variables used to construct the weight. Frumento et al. (2012) employ the same assumption but use variables available in the public version of the NJCS data. To construct the data used in this exercise, all the individuals who responded to the 48-month interview are included and those with missing values for weekly working hours, weekly earnings, or enrollment information are dropped.[22]

The estimation results are presented in Table 4.10. The estimated lower and upper bounds for the effect of JC on ln(wages) for the cEE stratum are below those estimated in Table 4.9, which ignore the non-response issue. Focusing on Non-Hispanics, the positive region covered by the bounds under both sets of assumptions (monotonicity and mean dominance) is slightly less than the corresponding region covered by the bounds in Table 4.9. The estimated upper bound in Table 4.10 equals .153, which is close to the one presented in Table 4.9 (.161). The estimate of the lower bound in Table 4.10 is also lower than, but still relatively close to, that presented in Table 4.9. In general, although adjusting for non-response slightly weakens the previous findings, the results

and Mealli (2009), who estimate the effect of JC on wages for the always-employed individuals without adjusting for non-compliance.

[21]More specifically, the weights address sample design, 48-month interview design, and 48-month interview non-response.

[22]As discussed in Section 4.3.1, the sample used in the previous tables includes only individuals with non-missing values for weekly earnings and weekly hours worked for every week after random assignment. This is done to make the results comparable to those in Lee (2009).

still strongly suggest a positive average effect of JC enrollment on wages four years after random assignment for the always-employed compliers.

As previously mentioned, Frumento et al. (2012) point identify the average effect of JC enrollment on wages, adjusting for sample selection, non-compliance, and missing outcomes, by imposing a different set of assumptions from the two sets discussed here. Employing a different sample from the one here, they estimate this effect to be about 3.8 percent for the always-employed compliers in the population. This point estimate is consistent with the estimated bounds for this effect presented in Table 4.10 for the entire population under Assumptions 3.1 through 3.5 and, although 3.8 percent is below the estimated lower bound under Assumptions 3.1 through 3.6 (4.4 percent), it falls inside the 95 percent confidence interval constructed under the six assumptions. Thus, the point estimate of the effect of JC on wages for the cEE stratum in Frumento et al. (2012) is consistent with the bounds adjusting for non-response.

4.3.4 Simulation Exercises

Simulation exercises aim to examine how the bounds in Propositions 3.1 and 3.2 behave when Assumptions 3.5 and 3.6 fail. By mimicking the characteristics of the sample involving all the individuals with continuously non-missing values of key variables, the simulation results suggest that the bounds are relatively robust even if the two assumptions fail.

In each simulation, 9090 observations are generated, with each randomly assigned to the treatment with the probability of $5491/9090$. Membership of principal strata is drawn from a uniform distribution. By mimicking $p_{ds|z}$ in the sample, the true values

Table 4.10: Bounds for the Effects of JC on ln(wage) for cEE Adjusting for Non-Response

	Entire Sample	Non-Hispanics
Number of observations	10520	8701
π_{aEE}	.017** (.002)	.018** (.002)
π_{nEE}	.162** (.005)	.162** (.005)
π_{cEE}	.391** (.009)	.381** (.010)
π_{cNE}	.038** (.010)	.049** (.011)
π_{aNN}	.026** (.003)	.028** (.003)
π_{nNN}	.108** (.004)	.108** (.004)
π_{cNN}	.258** (.006)	.254** (.007)
α_{cEE}	.877** (.022)	.851** (.024)
$E[Y(1)\|aEE]$	2.010** (.050)	2.001** (.052)
$E[Y(0)\|nEE]$	2.033** (.015)	2.032** (.016)
$E[Y(0)\|cEE]$	1.985** (.013)	1.961** (.015)
$\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$	2.449** (.065)	2.383** (.056)
$\overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$	1.666** (.036)	1.702** (.034)
Bounds with Monotonicity (Proposition 1)		
$[LY_{1,cEE}, UY_{1,cEE}]$	[1.957, 2.102]	[1.940, 2.114]
CLR 95% level confidence interval	(1.928, 2.127)	(1.911, 2.140)
$[L_{cEE}, U_{cEE}]$	[-.028, .117]	[-.021, .153]
CLR 95% level confidence interval	(-.065, .153)	(-.060, .191)
Bounds with Monotonicity and Mean Dominance (Proposition 2)		
$[LY_{1,cEE}, UY_{1,cEE}]$	[2.029, 2.102]	[2.028, 2.114]
CLR 95% level confidence interval	(2.013, 2.128)	(2.011, 2.142)
$[L_{cEE}, U_{cEE}]$	[.044, .117]	[.067, .153]
CLR 95% level confidence interval	(.015, .155)	[.035, .193]

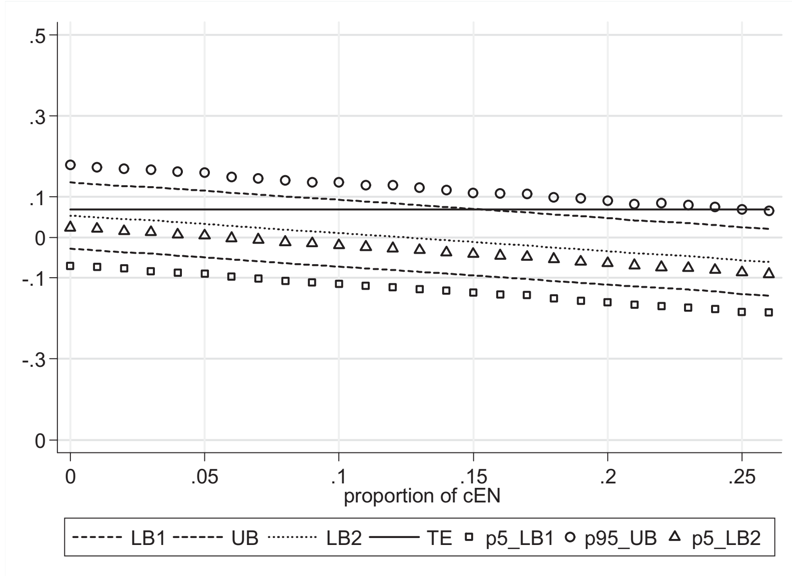
Note: ** denotes estimate is statistically different from 0 at 5% level and numbers in parentheses are standard errors. Computations use weights accounting for sample design, interview design and interview non-response. The standard error is calculated by a 5,000-repetition bootstrap. Numbers in square brackets are half-median unbiased estimators for the bounds, while numbers below them are CLR 95% level confidence intervals.

of proportions of strata are set as follows: $\pi_{aNN} = .030$, $\pi_{aEE} = .015$, $\pi_{nNN} = .105$, $\pi_{nEE} = .155$, $\pi_{cEE} = .395 - \pi_{cEN}$, $\pi_{cNN} = .260 - \pi_{cEN}$, and $\pi_{cNE} = .040 + \pi_{cEN}$. Note that without Assumption 3.5, $\pi_{cEN} \neq 0$. The observed treatment and employment statuses, D and S, are jointly determined by the membership of principal strata and the random assignment indicator. Observed wages of different strata follow lognormal distributions with means: $E[Y(1)|cEE] = 2.04$, $E[Y(0)|cEE] = 1.97$, $E[Y(1)|aEE] = 2.035$, $E[Y(0)|nEE] = 2.035$ and $E[Y(0)|cEN] = 2$. Thus, the true effect Δ is equal to .07. The variances of these average outcomes are equal to .2. These values are chosen such that they are close to the observed $\overline{Y}^{zds}$ in the sample.

Simulation 1 examines the bounds when only Assumption 3.5 fails. Assumption 3.6 holds as $E[Y(1)|cEE] - E[Y(1)|cNE] = .16$. Figure 4.1 shows the bounds when π_{cEN} changes over $[0, .26]$. Note that the range of π_{cEN} is determined by the proportions of other strata. In order to give a sense about the variability of the estimates of the bounds, this subsection also shows the 5th percentile of the estimates of the lower bound from 1000 repeated simulations, and the 95th percentile of the estimates of the upper bound.[23] The bounds slope downward as π_{cEN} increases. This is because in the trimming cell $\{Z = 1, D = 1, S = 1\}$, π_{cEE} decreases and π_{cNE} increases along with the increasing π_{cEN}, and since $E[Y(1)|cEE]$ is larger than $E[Y(1)|cNE]$, the proportion of the observations with larger values of $\log(wage)$ increases while the proportion with smaller values decreases. As a result, with the trimming proportions and range of $\log(wage)$ in the trimming cell fixed, the averages obtained from both the top and the bottom distribution become smaller. The true effect is above the lower bound in

[23] For example, 95 percent of the estimates of the upper bound fall below p95_UB, and 95 percent of the estimates of LB1 lie above p5_LB1.

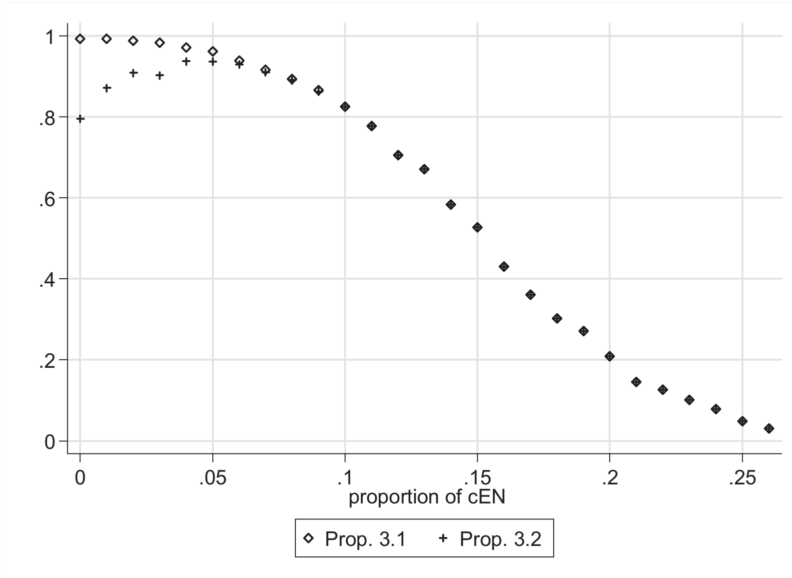
Figure 4.1: Bounds When Monotonicity Fails in Simulation 1



Proposition 3.2 over the entire range of π_{cEN}, and it falls outside the upper bound when $\pi_{cEN} > .16$. Note that the lower bound in Proposition 3.2 identifies a positive treatment effect as long as $\pi_{cEN} \leq .12$. Figure 4.2 shows percentage of times out of 1000 repetition the true effect falls inside the bounds. The percentage of times for Proposition 3.2 starts at a lower value than that for Proposition 3.1 because the true effect is very close to the lower bound in Proposition 3.2 at the origin. The percentage of times for both propositions coincide with each other when $\pi_{cEN} > .06$. Note that both assumptions hold at the origin. The simulated bounds are $[-.027, .136]$ in Proposition 3.1 and $[.054, .136]$ in Proposition 3.2, respectively, similar to the empirical results for the entire sample in Table 4.9.

Simulation 2 analyzes how the mean dominance assumption affects the bounds when $\pi_{cEN} = 0$. The horizontal line shows the difference between $E[Y(1)|cEE]$ and $E[Y(1)cNE]$, and the assumption holds in the right region of the axis origin. The down-

Figure 4.2: Percentage of Times True Effect Falls within the Bounds in Simulation 1



ward slope of the bounds in Figure 4.3 follows a different story in Simulation 2: larger values of the outcome of cNE members are gradually replaced with smaller values; with the constant proportions of cEE and cNE in the trimming cell, the bounds decrease along the x-axis. The true effect Δ falls within the bounds in Proposition 3.1 over the entire range $[-.5, .5]$ of the difference. Instead, Δ crosses the lower bound in Proposition 3.2 at the origin. This is because when $\pi_{cNE} = 0$ the direction of the mean dominance assumption determines whether the quantity in Proposition 3.2 is a lower bound or an upper bound for the parameter of interest Δ. The lower bound in Proposition 3.2 identifies a positive treatment effect over the entire range of the difference. Note that the lower bound in Proposition 3.1 also identifies a positive effect when the direction of the mean dominance assumption is reversed at $E[Y(1)|cNE] - E[Y(1)cEE] \geq .15$. As shown in Figure 4.4, Δ falls within the bounds in Proposition 3.1 in almost every single simulation. In contrast, the percentage of times for Proposition 3.2 increases as the mean

Figure 4.3: Bounds When Mean Dominance Fails in Simulation 2

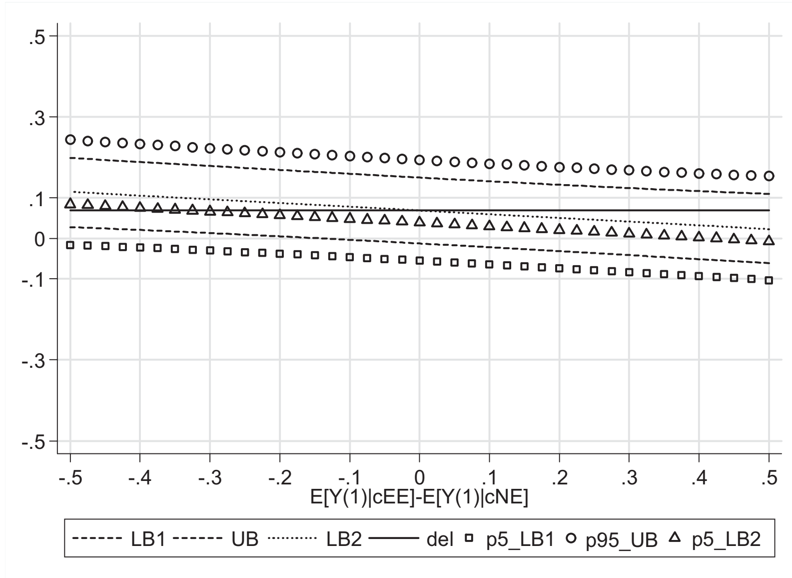
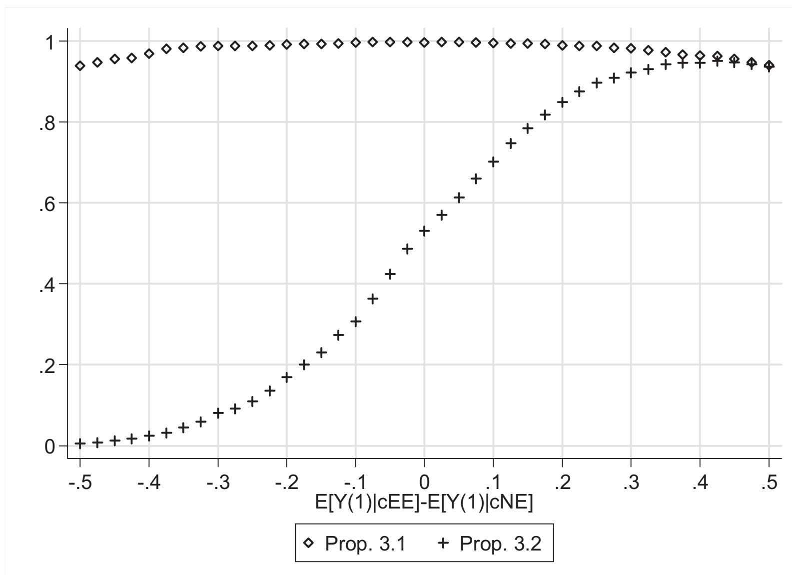


Figure 4.4: Percentage of Times True Effect Falls within the Bounds in Simulation 2



dominance assumption holds to a greater extent. Note that at the origin the percentage of times is around 50%.

Simulations 3 to 5 illustrate how the bounds behave when both assumptions fail, with π_{cEN} set as .05, .10 and .15, respectively. Comparing among Figures 4.3, 4.5, 4.7, and 4.9, Δ falls within the bounds in Proposition 3.1 over the shrinking range of $E[Y(1)|cEE]-E[Y(1)|cNE]$ as π_{cEN} increases. In contrast to Figure 4.3 where $\pi_{cEN}=0$, the intersection of Δ with the lower bound in Proposition 3.2 moves slightly leftward in Figures 4.5, 4.7 and 4.9. Mathematically, when the difference between $E[Y(0)|cEN]$ and $E[Y(0)|cEE]$ stays positive, the intersection moves leftward away from the origin as π_{cEN} increases. The tilt of the bounds over the entire range of the difference increases because the effect of cEN members through their wages on the identified quantity of $E[Y(0)|cEE]$ is strengthened as π_{cEN} increases at each value of the difference. The increasing tilt plays different roles in how these bounds identify the sign of Δ. When $\pi_{cEN} = .05$ and $\pi_{cEN} = .10$, the upper bound stays positive. Accordingly, the extent to which the lower bound in Proposition 3.2 identifies a positive treatment effect is attenuated by the increasing proportion of π_{cEN}, as shown by the narrower range of the difference over which the lower bound stays beyond the zero line. Instead, the range over which the lower bound in Proposition 3.1 identifies a positive effect enlarges slightly, in spite of the negative region of the axis. When $\pi_{cEN} = .15$, the lower bounds in Propositions 3.1 and 3.2 identify a positive effect when the difference is not larger than $-.05$ and .15, respectively. Moreover, the upper bound identifies a negative effect when the difference is not smaller than .35. In contrast with Figure 4.4, Figures 4.6, 4.8, and 4.10 display a bell shape of percentage of times the propositions are verified. The bell covers a shrinking area as π_{cEN} increases. The percentages of times for both propositions coincide with each other when Δ falls outside the bounds in Proposition 3.1. Otherwise, the one for Proposition 3.1 is above that for Proposition 3.2, with its center at the origin. Instead, the center for Proposition 3.2 moves slightly leftward as

Figure 4.5: Bounds When Both Assumptions Fail with $\pi_{cEN} = .05$ in Simulation 3

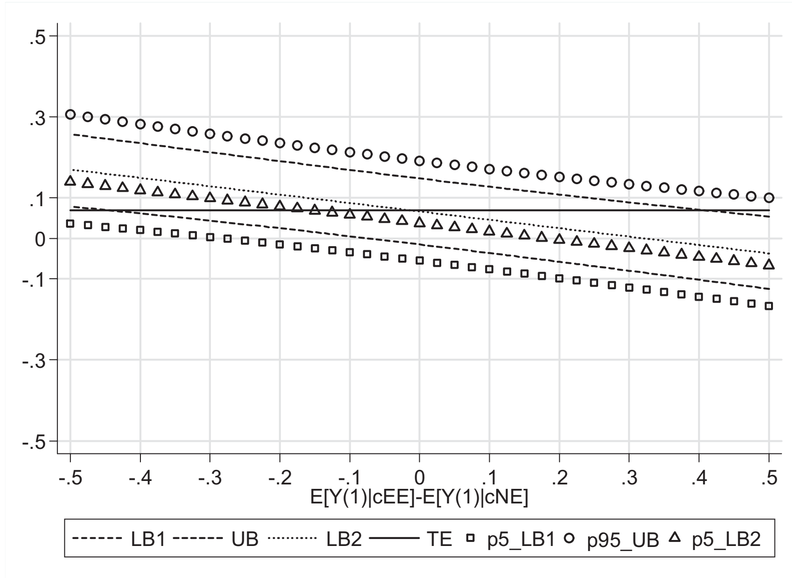
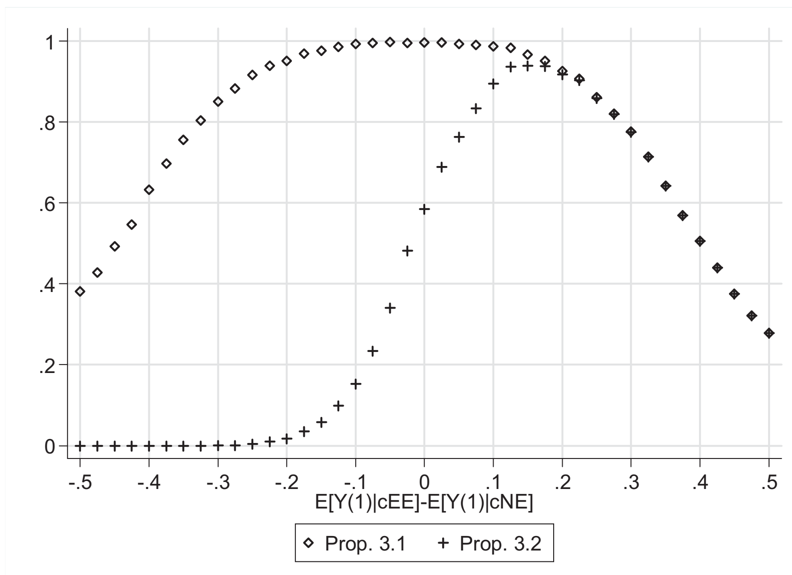


Figure 4.6: Percentage of Times True Effect Falls within the Bounds in Simulation 3



π_{cEN} increases. Note that the percentage of times for Proposition 3.2 increases at the origin as π_{cEN} increases.

The following conclusions can be drawn from the simulation exercises above. First, when Assumption 3.6 holds, the bounds in both propositions are relatively robust to the

Figure 4.7: Bounds When Both Assumptions Fail with $\pi_{cEN} = .10$ in Simulation 4

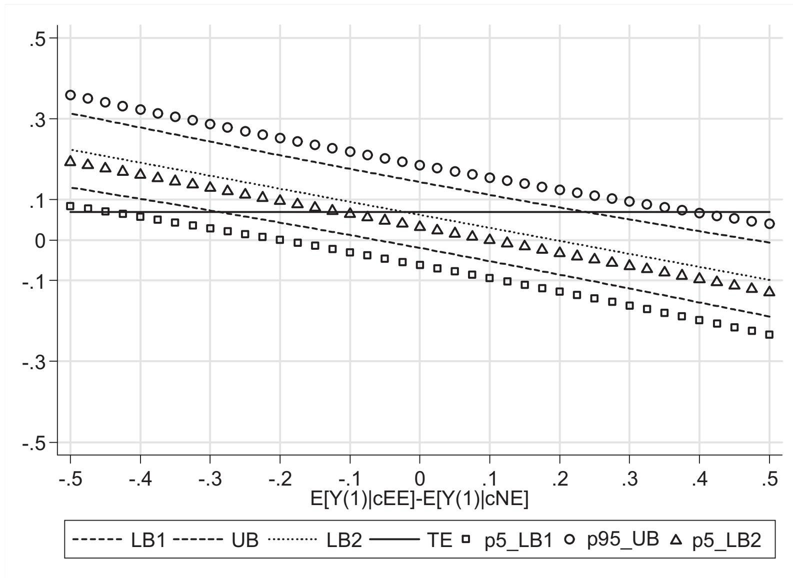


Figure 4.8: Percentage of Times True Effect Falls within the Bounds in Simulation 4

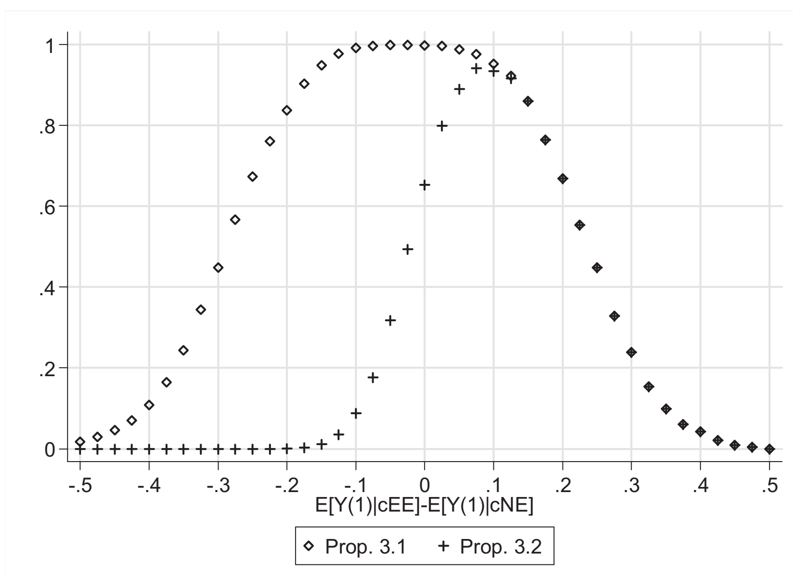


Figure 4.9: Bounds When Both Assumptions Fail with $\pi_{cEN} = .15$ in Simulation 5

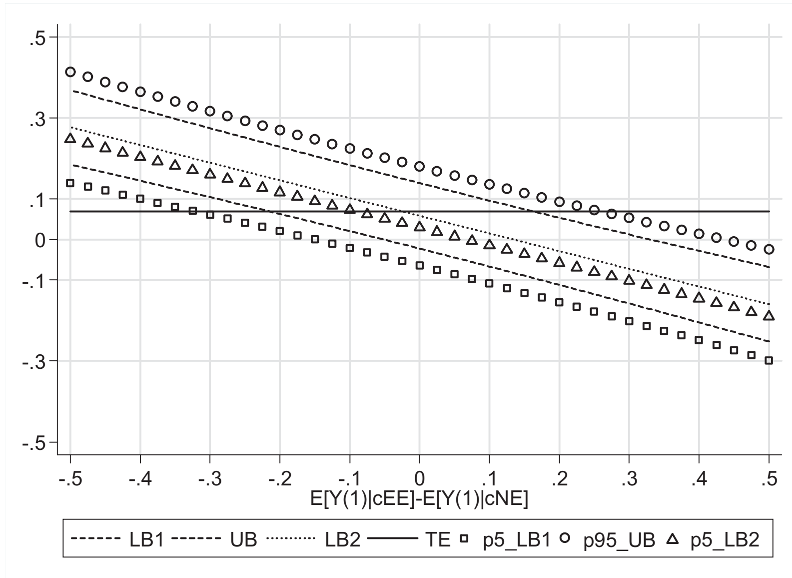
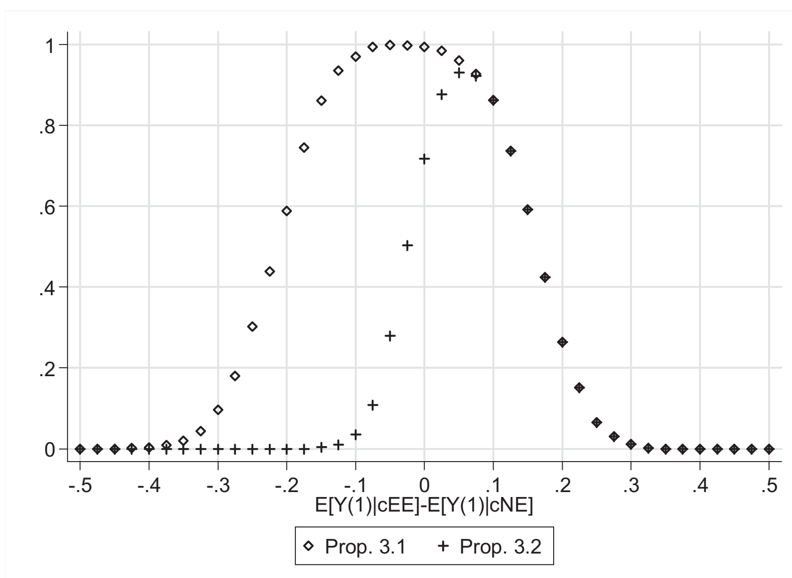


Figure 4.10: Percentage of Times True Effect Falls within the Bounds in Simulation 5



existence of a small fraction of cEN members. As shown in Simulation 1, the allowable limit of π_{cEN} is .16. Second, when both assumptions fail, the bounds in Proposition 3.1 are still valid over a symmetric range of $E[Y(1)|cEE] - E[Y(1)|cNE]$ around the origin, though the range shrinks gradually as π_{cEN} increases. The range for the bounds in Proposition 3.2 also shrinks, but the lower bound becomes less sensitive to the violation of Assumption 3.6 at the origin as π_{cEN} increases. The lower bound in Proposition 3.2 remains to identify a positive treatment effect for cEE as long as small departures occur to the assumptions. In summary, the simulation results show that the bounds in the presence of sample selection and noncompliance are relatively robust to the key identification Assumptions 3.5 and 3.6. This reinforces the robustness of the bounds obtained for the wage effects of JC enrollment in the previous subsection. For the always-employed compliers, the wage effect is bounded between 5.7 percent and 13.9 percent four years after random assignment. This evidence suggests that Job Corps has positive effects not only on the employability of its participants but also on their wages, implying that Job Corps is likely to have positive effects on their human capital.

CHAPTER 5

CONCLUSION

Research on partial identification approaches has recently received increasing attention in program evaluation literature. The main advantage of partial identification over traditional point identification is its dependence on milder assumptions, thus delivering more credible results in empirical applications. This dissertation extends Instrumental Variable (IV) methods in the program evaluation literature by partially identifying treatment effects of interest.

Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996) developed an influential approach within the IV framework. They show that, when allowing for heterogeneous effects, IV estimators point identify the local average treatment effect ($LATE$) for compliers. This dissertation advances the current IV literature in two important ways. First, inspired by a common criticism of their approach that the focus is on the effect for a subpopulation, the dissertation derives sharp nonparametric bounds for the population average treatment effect (ATE) within the $LATE$ framework. It improves the bounds on the ATE proposed in the current IV literature by combining two sets of assumptions. The first is monotonicity in the treatment of the average outcomes of strata without specifying a priori direction. This assumption infers the sign of average treatment effects for other subpopulations from that for compliers. In contrast to the existing literature, monotonicity imposed on the average outcomes within the same subpopulation allows some individuals to experience a treatment effect that has the opposite sign to the $LATE$. The second set of assumptions is mean dominance that compares average potential outcomes across different subpopulations. This assumption infers the unobserved terms from the identified average outcomes across subpopulations. Different from the current literature, some of the bounds do not require a bounded-outcome assumption once combining the monotonicity and mean dominance assumptions. Moreover, indirect evidence regarding mean dominance can be obtained by estimating pretreatment average characteristics of each stratum from an overidentified nonparametric GMM problem.

Second, this dissertation extends the $LATE$ framework to bound treatment effects in the presence of sample selection and noncompliance. The sample selection issue arises when outcomes of interest are only observed for a selected group. The noncompliance problem appears because individuals can choose whether or not to actually take the treatment in most of randomized experiments in economics and other social science fields, especially the experiments with encouragement designs. As a result, it is common that some individuals in the treatment group do not take the treatment while some individuals in the control group do. The dissertation extends the partial identification results in Zhang, Rubin, and Mealli (2008) and Lee (2009), who construct bounds in the presence of sample selection, to also account for noncompliance. Within the framework of principal stratification, it derives nonparametric bounds on the average treatment effect for the always-selected compliers, who would comply with their assigned treatment and whose outcomes are always-selected regardless of treatment assignment. This is the only group of individuals whose outcomes are observable under both treatment *receipt* arms. Additional assumptions are necessary to derive bounds for other subpopulations. More generally, these bounds can be employed in settings where two identification problems are present (e.g., endogeneity and missing outcomes) and there is a valid instrument to address one of them. Thus, the dissertation provides an important extension to the current partial identification literature in program evaluation, as it is common in empirical applications to face more than one identification problem.

The dissertation employs the derived bounds to evaluate the effectiveness of the Job Corps (JC) program, which is the most comprehensive and largest federally-funded job training program for disadvantaged youth in the United States. It uses experimental data from the National Job Corps Study (NJCS). Though individuals were randomly assigned to a treatment group or a control group, noncompliance arises. In the NJCS, 26% of individuals who were assigned to participate in JC did not enroll, while 4% of individuals who should have been embargoed from JC participation did enroll. Accordingly, previous literature on the evaluation of the JC program usually analyzes intention-to-treat (ITT) effects or the $LATE$ for compliers. Using random assignment as an IV to address noncompliance, the dissertation contributes to this empirical literature by providing credible and informative bounds for treatment effects other than $LATE$ and ITT effects.

Focusing on the population average treatment effect on labor market outcomes and welfare dependence, the monotonicity assumption and the mean dominance assumption together provide the narrowest bounds on the ATEs of JC enrollment: [24.61, 201.04] for weekly earnings and [.042, .163] for employment four years after randomization, and $[-142.76, -84.29]$ for the yearly dependence on public welfare benefits. These bounds are significantly narrower than the bounds proposed in the current IV literature. The lower bounds for weekly earnings and employment are 10 percent higher than their respective ITT effects (22.19 and .038), while the upper bound for public benefits is equal to its ITT effect. The $LATE$s for compliers on the three outcomes also fall within these narrowest bounds. Thus, it is safe to conclude that JC enrollment increases weekly earnings by at least \$24.61 and employment by at least 4.3 percentage points, and decreases the fourth yearly dependence on public welfare benefits by at least \$84.29.

The dissertation also evaluates the wage effect of JC, where both the sample selection issue (wages are only observed for employed individuals) and noncompliance are present. The two key assumptions are a monotonicity assumption on the effect of JC on employment, and a mean dominance assumption stating that the average potential wage under treatment of the always-employed compliers is greater than that of compliers who would be employed only if they participated in JC. As discussed in the dissertation, both assumptions are plausible, especially for Non-Hispanics. The simulation exercises also reinforce the identification power of the bounds under these two assumptions. Focusing on the always-employed compliers, who would comply with their assigned treatment and who would be always employed regardless of assignment status, the wage effect of JC enrollment is between 5.7 percent and 13.9 percent four years after random assignment, and between 7.7 and 17.5 percent for Non-Hispanics. This evidence suggests that Job Corps has positive effects not only on the employability of its participants but also on their wages, implying that Job Corps is likely to have positive effects in their human capital. Therefore, it is very important to consider the potential benefits of Job Corps and other training programs on wages when evaluating their effect.

The assumptions and methodologies employed in this dissertation can be used in partial identification of treatment effects in other settings. For example, one important extension is to bound the population ATE when the instrument does not satisfy the exclusion restriction. Argument over an IV usually concentrates on the plausibility of the exclusion restriction. Thus, how to make inference about the ATE when this assumption is violated is of great importance. Another interesting extension is to identify direct and indirect effects (e.g., Rubin, 2004; Sjölander, 2009; VanderWeele, 2011) in the presence of one identification issue (e.g., sample selection, noncompliance). When the causal channel between the treatment and the outcome is intervened by an intermediate variable, the direct effect refers to the causal effect of the treatment on the outcome net of the part that works through the intermediate variable while the indirect effect refers the part that works through the intermediate variable. Current literature on direct and indirect effects usually involves intensive computation and thus deliver numerical solutions. The methodology in this dissertation can be used to derive analytical bounds in the presence of one identification issue, for example, endogeneity. While beyond the scope of this dissertation, these extensions are at the top of the research agenda. Further topics to develop the present methodology include narrowing bounds by exploiting variations among covariates and deriving bounds for treatment effects in alternative designs, for example, an IV with three values, which is common in the fields of public health and epidemiology.

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APPENDIX A

DERIVATION OF THE BOUNDS

A.1 Derivation of the Bounds in Section 3.2

This section only presents the proof of Proposition 2.2, as the proofs for the rest of the propositions are similar.

Proof. Under Assumptions 1.1 through 1.4, AIR (1996) show that $LATE_c = (E[Y|Z = 1] - E[Y|Z = 0])/(p_{1|1} - p_{1|0})$. By Assumption 2.2(ii), and since Z is ordered such that $p_{1|1} > p_{1|0}$, the direction of the monotonicity in Assumption 2.2(i) is identified from the sign of $LATE_c$. Here I consider only the case when $LATE_c > 0$, as the sharp bounds when $LATE_c < 0$ are constructed in the same way. By equation (2.1), $ATE = \pi_{at}(E[Y(1)|at] - E[Y(0)|at]) + \pi_{nt}(E[Y(1)|nt] - E[Y(0)|nt]) + \pi_c LATE_c$. Under Assumptions 1.1 though 1.4, the sampling process identifies each of the quantities to the right of this equation except for $E[Y(1)|nt]$ and $E[Y(0)|at]$, and thus equation (2.2) follows. Since there are no restrictions on these two means other than those imposed by Assumptions 2.1 and 2.2(i), these two assumptions directly imply the bounds $y^u \geq E[Y(1)|nt] \geq E[Y(0)|nt] = \overline{Y}^{10}$ and $\overline{Y}^{01} = E[Y(1)|at] \geq E[Y(0)|at] \geq y^l$. The lower (upper) bound on ATE in Proposition 2.2 is obtained from equation (2) by setting $E[Y(1)|nt]$ at its lower (upper) bound and $E[Y(0)|at]$ at its upper (lower) bound.

For sharpness, first of all, ATE attains its smallest value when $E[Y(0)|at] = \overline{Y}^{01}$ and $E[Y(1)|nt] = \overline{Y}^{10}$. Otherwise, always-takers or never-takers violate Assumption 2.2(i). Similarly, ATE attains its largest value when $E[Y(0)|at] = y^l$ and $E[Y(1)|nt] = y^u$. Otherwise, always-takers or never-takers violate Assumption 2.1. Next, I will show that $\forall\alpha \in [LB, UB]$, there exist distributions consistent with observed data, and $ATE = \alpha$ evaluated under such distributions. $\forall\alpha \in [LB, UB]$, it can be written as $\alpha = \overline{Y}^{11} p_{1|1} - \overline{Y}^{00} p_{0|0} + q_1 p_{0|1} - q_0 p_{1|0}$, where $q_1 \in [\overline{Y}^{10}, y^u]$ and $q_0 \in [y^l, \overline{Y}^{01}]$. Let $F_{Y_1|Z,D}(y_1|1,d)$ denote the distribution of the potential outcome $Y(1)$ conditional on $Z = 1$ and $D = d$. Similarly, $F_{Y_0|Z,D}(y_0|0,d)$ denotes the distribution of the potential outcome $Y(0)$ conditional on $Z = 0$ and $D = d$. Then, define

$$F_{Y_1|Z,D}(y_1|1,d) = \begin{cases} F_{Y|Z,D}(y|1,1), & \text{if } D = 1 \\ 1[y_1 \geq q_1], & \text{if } D = 0 \end{cases}$$

and

$$F_{Y_0|Z,D}(y_0|0,d) = \begin{cases} F_{Y|Z,D}(y|0,0), & \text{if } D = 0 \\ 1[y_0 \geq q_0], & \text{if } D = 1 \end{cases} .$$

$ATE = E[Y(1) - Y(0)]$

$= E[Y(1)|Z = 1] - E[Y(0)|Z = 0]$

$= p_{1|1}E[Y(1)|Z = 1, D = 1] + p_{0|1}E[Y(1)|Z = 1, D = 0] - p_{1|0}E[Y(0)|Z = 0, D = 1] - p_{0|0}E[Y(0)|Z = 0, D = 0]$

$= p_{1|1}E[Y|Z = 1, D = 1] + p_{0|1}E[Y(1)|Z = 1, D = 0] - p_{1|0}E[Y(0)|Z = 0, D = 1] - p_{0|0}E[Y|Z = 0, D = 0]$

$= p_{1|1}\overline{Y}^{11} + p_{0|1}q_1 - p_{1|0}q_0 - p_{0|0}\overline{Y}^{00}$

$= \alpha.$

The second line follows Assumption 1.1, the third line follows Law of Iterated Expectation, and the fourth and fifth lines follow the defined distributions. ■

A.2 Derivation of the Bounds in Section 3.3

A.2.1 Proof of Proposition 3.1

First, I show that under Assumptions 3.1 through 3.5 L_{cEE} and U_{cEE} are the smallest and largest possible values, respectively, for the average treatment effect for the stratum cEE. Next, I prove that for $\forall\Delta \in [L_{cEE}, U_{cEE}]$, there exist distributions for cEE, aEE, and cNE consistent with the observed data of Y in $\{Z=1, D=1, S=1\}$ and the constraint that $E[Y(1)|aEE] = \overline{Y}^{011}$. In other words, the interval $[L_{cEE}, U_{cEE}]$ contains any other bounds that are consistent with Assumptions 3.1 through 3.5. The first-step proof is similar to that in Horowitz and Manski (1995), except that a binding constraint should be satisfied under the lower and upper bounds. Since both L_{cEE} and U_{cEE} depend on the range of $\overline{Y}^{011}$, I need to discuss multiple cases in the proof.

Proof. First by Assumptions 3.1 through 3.5, the proportions of each stratum are uniquely determined by the observed data. Thus, the proof is completed given the proportions of the strata. Second since $E[Y(0)|cEE]$ is point identified by Assumptions 3.1 through 3.5, the proof can be completed with respect to $E[Y(1)|cEE]$ instead of the average treatment effect for cEE. Let $\theta = E[Y(1)|cEE]$, and then $\Delta = \theta - E[Y(0)|cEE]$. Third, since both $LY_{1,cEE}$ and $UY_{1,cEE}$ depend on the range of $\overline{Y}^{011}$, I have to discuss multiple cases.

Let Qy be the observed distribution of Y in the cell $[Z=1, D=1, S=1]$. $\overline{y}_{aEE} = \overline{Y}(y^{111}_{\alpha_{cEE}} \le Y \le y^{111}_{1-\alpha_{cNE}})$, $\widetilde{y}_{aEE} = \overline{Y}(y^{111}_{\alpha_{cNE}} \le Y \le y^{111}_{1-\alpha_{cEE}})$. Denote the expression of $LY_{1,cEE}$ as $\widetilde{LY}_{1,cEE}$ when $\overline{y}_{aEE} > \overline{Y}^{011}$ and the expression of $UY_{1,cEE}$ as $\widetilde{UY}_{1,cEE}$ when

$\widetilde{y}_{aEE} < \overline{Y}^{011}$, The probability density functions for each stratum are fy_{cEE}, fy_{aEE}, and fy_{cNE}, and their corresponding distributions are Fy_{cEE}, Fy_{aEE}, and Fy_{cNE}.

For the first-step proof, I discuss the two cases to show that $LY_{1,cEE}$ is the smallest possible value for $E[Y(1)|cEE]$. The other two cases for the $UY_{1,cEE}$ can be shown in the same way.

1. $LY_{1,cEE} = \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}})$, when $\overline{Y}^{011} \geq \overline{y}_{aEE}$.

Let $Gy = \begin{cases} \frac{Qy}{\alpha_{cEE}}, & \text{if } y \leq y^{111}_{\alpha_{cEE}} \\ 1, & \text{if } y > y^{111}_{\alpha_{cEE}} \end{cases}$.

To show that $\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}})$ is the smallest value, I have $Gy \geq Fy_{cEE}$, for all Fy_{cEE} $\in \{\alpha_{cEE}Fy_{cEE} + \alpha_{aEE}Fy_{aEE} + \alpha_{cNE}Fy_{cNE} = Qy, E[Y(1)|aEE] = \overline{Y}^{011}\}$ and all $y \in \mathscr{R}$.

If $y \leq y^{111}_{\alpha_{cEE}}$, $Gy < Fy_{cEE} \Rightarrow Qy < \alpha_{cEE}Fy_{cEE} \Rightarrow Qy < \alpha_{cEE}Fy_{cEE} + \alpha_{aEE}Fy_{aEE} + \alpha_{cNE}Fy_{cNE}$. This contradicts the feasible set of $Fy_{cEE.}$

If $y > y^{111}_{\alpha_{cEE}}$, $Gy - Fy_{cEE} = 1 - Fy_{cEE} \geq 0$.

Next, I show that the distributions exist, when $E[Y(1)|cEE] = \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}})$ and $E[Y(1)|aEE] = \overline{Y}^{011}$.

When $\overline{Y}^{011} \geq \overline{y}_{aEE}$ and $\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$, let ty be the observed density of $Y(Y \geq y^{111}_{1-\alpha_{aEE}})$ and hy the observed density of $Y(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$. Then $\exists \tau \in [0,1]$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}}) + (1-\tau)\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) = \overline{Y}^{011}$. Therefore, $gy = fy_{cEE}$, $\tau ty + (1-\tau)hy = fy_{aEE}$ and $\frac{(1-\tau)\pi_{aEE}}{\pi_{cNE}}ty + (1 - \frac{(1-\tau)\pi_{aEE}}{\pi_{cNE}})hy = fy_{cNE}$.

When $\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}^{011} \leq \overline{y}_{aEE}$, let ty be the observed density of $Y(Y \geq y^{111}_{1-\alpha_{cNE}})$ and hy the observed density of $Y(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$. Then $\exists \tau \in [0,1]$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{1-\alpha_{cNE}}) + (1-\tau)\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) = \overline{Y}^{011}$. Similarly, $gy = fy_{cEE}$, $\tau ty + (1-\tau)hy = fy_{aEE}$ and $(1 - \frac{\tau\pi_{aEE}}{\pi_{cNE}})ty + \frac{\tau\pi_{aEE}}{\pi_{cNE}}hy = fy_{cNE}$.

2. $LY_{1,cEE} = \widetilde{LY}_{1,cEE}$, when $\overline{Y}^{011} \leq \overline{y}_{aEE}$.

In this case, I first prove that $\overline{Y}(Y \leq y^{111}_{p1-_{1,cNE}})$ is the smallest feasible value for the quantity $\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} E[Y(1)|cEE] + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} E[Y(1)|aEE]$, and then show $\widetilde{LY}_{1,cEE}$ is the smallest feasible value for $E[Y(1)|cEE]$.

Let $Gy = \begin{cases} \frac{Qy}{1-\alpha_{cNE}}, & \text{if } y \leq y^{111}_{1-\alpha_{cNE}} \\ 1, & \text{if } y > y^{111}_{1-\alpha_{cNE}} \end{cases}$.

To show that $\overline{Y}(Y \leq y^{111}_{p1-_{1,cNE}})$ is the smallest value for $\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} E[Y(1)|cEE] + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} E[Y(1)|aEE]$, I have $Gy \geq \frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{cEE} + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{aEE}$, for all $Fy_{cEE} \in \{\alpha_{cEE} Fy_{cEE} + \alpha_{aEE} Fy_{aEE} + \alpha_{cNE} Fy_{cNE} = Qy, E[Y(1)|aEE] = \overline{Y}^{011}\}$ and all $y \in \mathscr{R}$.

If $y \leq y^{111}_{1-\alpha_{cNE}}$, $Gy < \frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{cEE} + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{aEE} \Rightarrow Qy < \alpha_{cEE} Fy_{cEE} + \alpha_{aEE} Fy_{aEE} \Rightarrow Qy < \alpha_{cEE} Fy_{cEE} + \alpha_{aEE} Fy_{aEE} + \alpha_{cNE} Fy_{cNE}$. This contradicts the feasible set of $Fy_{cEE.}$

If $y > y^{111}_{1-\alpha_{cNE}}$, $Gy - (\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{cEE} + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{aEE}) = 1 - \frac{\pi_{cEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{cEE} - \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} Fy_{aEE} \geq 0$.

Since $E[Y(1)|aEE] = \overline{Y}^{011}$, $\widetilde{LY}_{1,cEE}$ is the smallest value for $E[Y(1)|cEE]$.

Next, I show that there exist distributions of cEE, aEE and cNE in the trimming cell, such that $E[Y(1)|cEE] = \widetilde{LY}_{1,cEE}$ and $E[Y(1)|aEE] = \overline{Y}^{011}$.

When $\overline{Y}^{011} \leq \overline{y}_{aEE}$ and $\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$, let sy be the observed density of $Y(Y \geq y^{111}_{1-\alpha_{cNE}})$, ty the observed density of $Y(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$ and hy the observed density of $Y(Y \leq y^{111}_{\alpha_{aEE}})$. Then $\exists \tau \in [0,1]$, $s.t.$ $\tau \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{\alpha_{aEE}}) = \overline{Y}^{011}$. Therefore, $sy = fy_{cNE}$, $\tau ty + (1-\tau)hy = fy_{aEE}$ and $(1 - \frac{\tau \pi_{aEE}}{\pi_{cEE}})ty + \frac{\tau \pi_{aEE}}{\pi_{cEE}} hy = fy_{cEE}$.

When $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) \leq \overline{Y}^{011} \leq \overline{y}_{aEE}$, let *sy* be the observed density of $Y(Y \geq y^{111}_{1-\alpha_{cNE}})$, *ty* the observed density of $Y(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$ and *hy* the observed density of $Y(Y \leq y^{111}_{\alpha_{cEE}})$. Then $\exists \tau \in [0,1], s.t.\ \tau\overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}) = \overline{Y}^{011}$. Therefore, $sy = fy_{cNE}$, $\tau ty + (1-\tau)hy = fy_{aEE}$ and $\frac{(1-\tau)\pi_{aEE}}{\pi_{cEE}}ty + (1 - \frac{(1-\tau)\pi_{aEE}}{\pi_{cEE}})hy = fy_{cEE}$.

For the second-step proof, I have four cases to discuss, taking into account the lower and upper bound simultaneously. Since they form different segmentations of *Qy*, in each case I use some cutoff values to discuss $\forall\theta \in [LY_{1,cEE}, cutoff]$ and $\forall\theta \in [cutoff, UY_{1,cEE}]$ separately. In either interval for θ, I have to discuss the range of $\overline{Y}^{011}$ as I have done in the first-step proof. The four cases are listed as follows.

1. $LY_{1,cEE} = \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), UY_{1,cEE} = \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})$

It happens when $\overline{y}_{aEE} \leq \overline{Y}^{011} \leq \widetilde{y}_{aEE}$; in other words, $\pi_{cNE} \geq \pi_{cEE}$. Since $\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}) \leq \overline{Y}^{111} \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cEE}})$ and $\overline{Y}(Y \leq y^{111}_{1-\alpha_{cEE}}) \leq \overline{Y}^{111} \leq \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})$, thus, $cutoff = \overline{Y}^{111}$.

For $\forall\theta \in [\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), cutoff]$, it is necessary to discuss two cases when $\overline{Y}^{011} \geq \overline{y}_{aEE}\ \&\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ and $\overline{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

For $\forall\theta \in [cutoff, \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})]$, it is necessary to discuss two cases when $\overline{Y}^{011} \leq \widetilde{y}_{aEE}\ \&\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$ and $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}}) \leq \overline{Y}^{011} \leq \widetilde{y}_{aEE}$.

2. $LY_{1,cEE} = \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), UY_{1,cEE} = \widetilde{UY}_{1,cEE}$

In this case, I have to discuss the relationship between $\overline{y}_{aEE}$ and $\widetilde{y}_{aEE}$ first. I solve this problem with two different cutoffs:

For $\forall\theta \in [\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), cutoff]$, I have two cases to discuss: $\overline{Y}^{011} \geq \overline{y}_{aEE}\ \&\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ and $\overline{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

For $\forall\theta \in [cutoff, \widetilde{UY}_{1,cEE}]$, I have two cases to discuss: $\overline{Y}^{011} \geq \widetilde{y}_{aEE}$ &$\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ and $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

3. $LY_{1,cEE} = \widetilde{LY}_{1,cEE}, UY_{1,cEE} = \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})$

Like case 2, I have to discuss the relationship between $\overline{y}_{aEE}$ and $\widetilde{y}_{aEE}$ first. I solve this problem with two different cutoffs:

For $\forall\theta \in [\widetilde{LY}_{1,cEE}, cutoff]$, I have two cases to discuss: $\overline{Y}^{011} \leq \overline{y}_{aEE}$ &$\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$ and $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) \leq \overline{Y}^{011} \leq \overline{y}_{aEE}$.

For $\forall\theta \in [cutoff, \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})]$, I have two cases to discuss: $\overline{Y}^{011} \leq \widetilde{y}_{aEE}$ &$\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$ and $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}}) \leq \overline{Y}^{011} \leq \widetilde{y}_{aEE}$.

4. $LY_{1,cEE} = \widetilde{LY}_{1,cEE}, UY_{1,cEE} = \widetilde{UY}_{1,cEE}$

This happens when $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{y}_{aEE}$; in other words, $\pi_{cNE} \leq \pi_{cEE}$. In this case, it is difficult to get a uniform cutoff. I discuss multiple cases conditional on the proportions of the strata.

When $\pi_{cNE} \leq \pi_{cEE}$ and $\pi_{aEE} \leq \pi_{cEE}$, I discuss the intervals $\forall\theta \in [\widetilde{LY}_{1,cEE}, \overline{y}_{aEE}]$ and $\forall\theta \in [\widetilde{y}_{aEE}, \widetilde{UY}_{1,cEE}]$ to complete the proof in the entire range, i.e., $\forall\theta \in [\widetilde{LY}_{1,cEE}, \widetilde{UY}_{1,cEE}]$. For $\forall\theta \in [\widetilde{LY}_{1,cEE}, \overline{y}_{aEE}]$, the two cases are $\overline{Y}^{011} \leq \overline{y}_{aEE}$ &$\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$ and $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}}) \leq \overline{Y}^{011} \leq \overline{y}_{aEE}$. For $\forall\theta \in [\widetilde{y}_{aEE}, \widetilde{UY}_{1,cEE}]$, the two cases are $\overline{Y}^{011} \geq \widetilde{y}_{aEE}$ &$\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ and $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

When $\pi_{cNE} \leq \pi_{cEE} \leq \pi_{aEE}$, I discuss the intervals $\forall\theta \in [\widetilde{LY}_{1,cEE}, \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})]$ and $\forall\theta \in [\overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}), \widetilde{UY}_{1,cEE}]$ to complete the proof in the entire range. For $\forall\theta \in [\widetilde{LY}_{1,cEE}, \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})]$, I have $\overline{Y}^{011} \leq \overline{y}_{aEE}$ &$\overline{Y}^{011} \leq$

$\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$. For $\forall\theta \in [\overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}), \widetilde{UY}_{1,cEE}]$, I have $\overline{Y}^{011} \geq \widetilde{y}_{aEE}$ & $\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

From all the above, I can find that though cutoffs are different in the four cases, the discussion of $\overline{Y}^{011}$ is repeated. Cases 2 and 3 compose a complete discussion of $\overline{Y}^{011}$. In the following, I only write the proof of Case 2. Case 3 can be shown in a similar way.

First, let me discuss the relationship between $\overline{y}_{aEE}$ and $\widetilde{y}_{aEE}$ in Case 2 and derive two different cutoffs.

When $\overline{y}_{aEE} \leq \widetilde{y}_{aEE} \leq \overline{Y}^{011}$(i.e., $\pi_{cNE} \geq \pi_{cEE}$), I have $LY_{1,cEE} \leq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cEE}})$ and $\widetilde{UY}_{1,cEE} = \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}})\frac{\pi_{cEE}+\pi_{aEE}}{\pi_{cEE}} - \overline{Y}^{011}\frac{\pi_{aEE}}{\pi_{cEE}} \geq \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}})\frac{\pi_{cEE}+\pi_{aEE}}{\pi_{cEE}} - \overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}})\frac{\pi_{aEE}}{\pi_{cEE}} = \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$. The first inequality is derived from $\overline{Y}^{011} \leq \overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}})$, and the last inequality is derived from $\pi_{cNE} \geq \pi_{cEE}$. Thus, $\widetilde{UY}_{1,cEE} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \geq \overline{Y}(Y \leq y^{111}_{\alpha_{cNE}})$. Therefore, the cutoff value $cutoff = \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

When $\widetilde{y}_{aEE} \leq \overline{y}_{aEE} \leq \overline{Y}^{011}(\pi_{cNE} \leq \pi_{cEE})$, $LY_{1,cEE} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}}) \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cEE}})$. The last inequality is derived from $\pi_{cNE} \leq \pi_{cEE}$. Thus, $LY_{1,cEE} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cEE}})$. And $\widetilde{UY}_{1,cEE} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) \geq \overline{Y}(Y \leq y^{111}_{\alpha_{cNE}})$. Its derivation is the same as that in the last paragraph. Therefore, $cutoff = \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$.

Second, $\forall\theta \in [cutoff, \widetilde{UY}_{1,cEE}]$, where $cutoff$ is the corresponding value in either case $\overline{y}_{aEE} \leq \widetilde{y}_{aEE}$ or $\widetilde{y}_{aEE} \leq \overline{y}_{aEE}$, $\exists\lambda \in (0,1], s.t.\ \lambda\widetilde{UY}_{1,cEE} + (1-\lambda)\overline{Y}(Y \leq y^{111}_{\alpha_{cNE}}) = \theta$, since $\overline{Y}(Y \leq y^{111}_{\alpha_{cNE}}) \leq \theta \leq \widetilde{UY}_{1,cEE}$. ($\lambda = \frac{\theta - \overline{Y}(Y \leq y^{111}_{\alpha_{cNE}})}{\widetilde{UY}_{1,cEE} - \overline{Y}(Y \leq y^{111}_{\alpha_{cNE}})}$.)

To construct fy_{aEE}, it is necessary to discuss the value of $\overline{Y}^{011}$. One case is $\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$. This happens when either $\pi_{cEE} \leq \pi_{aEE}$, or $\pi_{cEE} \geq \pi_{aEE}$

but aEE take up the very top quantiles of the observed distribution. The other case is $\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$. This is true only when $\pi_{cEE} \geq \pi_{aEE}$.

(1) $\overline{Y}^{011} \geq \widetilde{y}_{aEE}$ &$\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

Let ty be the observed density of $Y(y \geq y^{111}_{1-\alpha_{aEE}})$, hy the density of $Y(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ and gy the density of $Y(y \leq y^{111}_{\alpha_{cNE}})$. Since $\overline{Y}(Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}^{011} \leq \overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}})$, $\exists \tau \in (0,1]$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{1-\alpha_{aEE}}) = \overline{Y}^{011}$. $\tau ty + (1-\tau)\frac{\pi_{cEE}}{\pi_{cNE}+\pi_{cEE}}hy + (1-\tau)\frac{\pi_{cNE}}{\pi_{cNE}+\pi_{cEE}}gy = fy_{aEE}$. Since $\widetilde{UY}_{1,cEE}$ is obtained by temporarily assuming aEE is above $y^{111}_{\alpha_{cNE}}$, $\exists \phi \in [0,1]$, $s.t.$ $\phi\overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}}) + (1-\phi)\overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}}) = \overline{Y}^{011}$. Thus, $\lambda\{\frac{\pi_{aEE}}{\pi_{cEE}}ty + hy - [\phi ty + (1-\phi)hy]\frac{\pi_{aEE}}{\pi_{cEE}}\} + (1-\lambda)gy = fy_{cEE}$. Since $\alpha_{aEE}ty + \alpha_{cEE}hy + \alpha_{cNE}gy = \alpha_{aEE}fy_{aEE} + \alpha_{cEE}fy_{cEE} + \alpha_{cNE}fy_{cNE}$, the corresponding density for cNE is $\frac{\pi_{aEE}}{\pi_{cNE}}[1-\tau-\lambda(1-\phi)]ty + \frac{\pi_{cEE}}{\pi_{cNE}}[1-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}} - \lambda(1-(1-\phi)\frac{\pi_{aEE}}{\pi_{cEE}})]hy + [1-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}} - (1-\lambda)\frac{\pi_{cEE}}{\pi_{cNE}}]gy = fy_{cNE}$.

(2) $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

Let ty be the observed density of $Y(y \geq y^{111}_{1-\alpha_{cEE}})$, hy the density of $Y(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$ and gy the density of $Y(y \leq y^{111}_{\alpha_{cNE}})$. Since $\overline{Y}(Y \leq y^{111}_{\alpha_{cNE}}) \leq \overline{Y}^{011} \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}})$, $\exists \tau \in (0,1)$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{\alpha_{cNE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{\alpha_{cNE}}) = \overline{Y}^{011}$. $\tau\frac{\pi_{cEE}}{\pi_{aEE}+\pi_{cEE}}ty + \tau\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cEE}}hy + (1-\tau)gy = fy_{aEE}$. As in (1), since $\widetilde{UY}_{1,cEE}$ is obtained by temporarily assuming aEE is above $y^{111}_{\alpha_{cNE}}$, $\exists \phi \in [0,1]$, $s.t.$ $\phi\overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}}) + (1-\phi)\widetilde{y}_{aEE} = \overline{Y}^{011}$. Thus, $\lambda\{ty + \frac{\pi_{aEE}}{\pi_{cEE}}hy - [\phi ty + (1-\phi)hy]\frac{\pi_{aEE}}{\pi_{cEE}}\} + (1-\lambda)gy = fy_{cEE}$. Similarly as in (1), I finally get $\frac{\pi_{cEE}}{\pi_{cNE}}[1-\tau\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} - \lambda(1-\phi\frac{\pi_{aEE}}{\pi_{cEE}})]ty + \frac{\pi_{aEE}}{\pi_{cNE}}[1-\tau\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} - \lambda\phi]hy + [1-(1-\tau)\frac{\pi_{aEE}}{\pi_{cNE}} - (1-\lambda)\frac{\pi_{cEE}}{\pi_{cNE}}]gy = fy_{cNE}$.

Third, $\forall\theta \in [\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}), cutoff]$, $\exists\lambda \in [0,1)$, $s.t.$ $\lambda\overline{Y}(Y \geq y^{111}_{\alpha_{cEE}}) + (1-\lambda)\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}) = \theta$, since $\overline{Y}(Y \leq y^{111}_{\alpha_{cEE}}) \leq \theta \leq \overline{Y}(Y \geq y^{111}_{\alpha_{cEE}})$. ($\lambda = \frac{\theta - \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}})}{\overline{Y}(Y \geq y^{111}_{\alpha_{cEE}}) - \overline{Y}(Y \leq y^{111}_{\alpha_{cEE}})}$.)

To discuss the value of $\overline{Y}^{011}$, one case is $\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$. It happens when either $\pi_{cNE} \leq \pi_{aEE}$, or $\pi_{cNE} \geq \pi_{aEE}$ but aEE take up very top quantiles of the observed distribution. The other case is $\overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$ when $\pi_{cNE} \geq \pi_{aEE}$.

(1) $\overline{Y}^{011} \geq \overline{y}_{aEE}$ &$\overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

Let ty be the observed density of $Y(y \geq y^{111}_{1-\alpha_{aEE}})$, hy the density of $Y(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$, and gy the density of $Y(y \leq y^{111}_{\alpha_{cEE}})$. Since $\overline{Y}(Y \leq y^{111}_{1-\alpha_{aEE}}) \leq \overline{Y}^{011} \leq \overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}})$, $\exists \tau \in (0,1]$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{1-\alpha_{aEE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{1-\alpha_{aEE}}) = \overline{Y}^{011}$. $\tau ty + (1-\tau)\frac{\pi_{cNE}}{\pi_{cNE}+\pi_{cEE}}hy + (1-\tau)\frac{\pi_{cEE}}{\pi_{cNE}+\pi_{cEE}}gy = fy_{aEE}$. For cEE, I have $\lambda\frac{\pi_{aEE}}{\pi_{cNE}+\pi_{aEE}}ty + \lambda\frac{\pi_{cNE}}{\pi_{cNE}+\pi_{aEE}}hy + (1-\lambda)gy = fy_{cEE}$. Finally, $\frac{\pi_{aEE}}{\pi_{cNE}}(1-\tau-\lambda\frac{\pi_{cEE}}{\pi_{aEE}+\pi_{cNE}})ty + [1-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}} - \lambda\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}}]hy + \frac{\pi_{cEE}}{\pi_{cNE}}[\lambda-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}})]gy = fy_{cNE}$.

(2) $\overline{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

Let ty be the observed density of $Y(y \geq y^{111}_{1-\alpha_{cNE}})$, hy the density of $Y(y^{111}_{\alpha_{cEE}} \leq Y \leq y^{111}_{1-\alpha_{cNE}})$, and gy the density of $Y(y \leq y^{111}_{\alpha_{cEE}})$. Since $\overline{Y}(Y \leq y^{111}_{1-\alpha_{cNE}}) \leq \overline{Y}^{011} \leq \overline{Y}(Y \geq y^{111}_{1-\alpha_{cNE}})$, $\exists \tau \in (0,1]$, $s.t.$ $\tau\overline{Y}(Y \geq y^{111}_{1-\alpha_{cNE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{1-\alpha_{cNE}}) = \overline{Y}^{011}$. $\tau ty + (1-\tau)\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cEE}}hy + (1-\tau)\frac{\pi_{cEE}}{\pi_{aEE}+\pi_{cEE}}gy = fy_{aEE}$. For cEE, I have $\lambda\frac{\pi_{cNE}}{\pi_{cNE}+\pi_{aEE}}ty + \lambda\frac{\pi_{aEE}}{\pi_{cNE}+\pi_{aEE}}hy + (1-\lambda)gy = fy_{cEE}$. Finally, $(1-\tau\frac{\pi_{aEE}}{\pi_{cNE}} - \lambda\frac{\pi_{cEE}}{\pi_{aEE}+\pi_{cNE}})ty + \frac{\pi_{aEE}}{\pi_{cNE}}[1-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}} - \lambda\frac{\pi_{cEE}}{\pi_{aEE}+\pi_{cNE}}]hy + \frac{\pi_{cEE}}{\pi_{cNE}}[\lambda-(1-\tau)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{aEE}}]gy = fy_{cNE}$. ■

A.2.2 Proof of Proposition 3.2

The proof of Proposition 3.2 is similar to that of Proposition 3.1, except for two differences: multiple cases reduce to two due to L_{cEE}, and the constructed distributions for cEE and cNE should also satisfy the mean dominance assumption.

Proof. As in the proof of Proposition 3.1, I first show that $LY_{1,cEE}$ is the smallest feasible value for $E[Y(1)|cEE]$, and then show $\forall\theta \in [\,LY_{1,cEE}, UY_{1,cEE}]$, there exist distributions Fy_{cEE}, Fy_{aEE} and Fy_{cNE} satisfying Assumptions 3.1 through 3.6.

For $\forall\theta \in [LY_{1,cEE}, UY_{1,cEE}]$, by equation (3.6), I have

$E[Y(1)|cEE] - E[Y(1)|cNE]$

$= \theta - (\overline{Y}^{111}\frac{\pi_{cEE}+\pi_{cNE}+\pi_{aEE}}{\pi_{cNE}} - \theta\frac{\pi_{cEE}}{\pi_{cNE}} - \overline{Y}^{011}\frac{\pi_{aEE}}{\pi_{cNE}})$

$\geq (1+\frac{\pi_{cEE}}{\pi_{cNE}})LY_{1,cEE} + \overline{Y}^{011}\frac{\pi_{aEE}}{\pi_{cNE}} - \overline{Y}^{111}\frac{\pi_{cEE}+\pi_{cNE}+\pi_{aEE}}{\pi_{cNE}} = 0.$

If there was another lower bound smaller than $LY_{1,cEE}$, $E[Y(1)|cEE] - E[Y(1)|cNE]$ would be negative when $E[Y(1)|cEE]$ reached that lower bound. This contradicts Assumption 3.6. Thus, $LY_{1,cEE}$ is the smallest value for $E[Y(1)|cEE]$ under Assumptions 3.1 through 3.6.

As in Proposition 3.1, I have to show the distributions exist when $E[Y(1)|cEE] = LY_{1,cEE}$ and $E[Y(1)|aEE] = \overline{Y}^{011}$. According to the range of $\overline{Y}^{011}$, I have four cases to discuss:

(1) $\overline{Y}^{011} \leq \tilde{y}_{aEE} \& \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$

(2) $\overline{Y}(y^{111}_{\alpha_{aEE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}}) \leq \overline{Y}^{011} \leq \tilde{y}_{aEE}$

(3) $\overline{Y}^{011} \geq \tilde{y}_{aEE} \& \overline{Y}^{011} \geq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

(4) $\tilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

These four cases correspond with those in the second-step proof that for $\forall\theta \in [LY_{1,cEE}, UY_{1,cEE}]$, there exist distributions Fy_{cEE}, Fy_{aEE}, and Fy_{cNE} satisfying Assumptions 3.1 through 3.6. Since for $\forall\theta \in [LY_{1,cEE}, UY_{1,cEE}]$, $\exists\lambda \in [0,1]$, $s.t.\ \lambda UY_{1,cEE} + (1-\lambda)LY_{1,cEE} = \theta$, the proof that the distributions exist when $E[Y(1)|cEE] = LY_{1,cEE}$ and $E[Y(1)|aEE] = \overline{Y}^{011}$ is a special case of the second-step proof, i.e. $\lambda = 0$. In the following, I take Case (4) as an example to illustrate the second-step proof of Proposition 3.2.

(4) $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$

Let ty be the observed density of $Y(y \geq y^{111}_{1-\alpha_{cEE}})$, hy the density of $Y(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{cEE}})$, and gy the density of $Y(y \leq y^{111}_{\alpha_{cNE}})$.

$\forall \theta \in [LY_{1,cEE}, \widetilde{UY}_{1,cEE}], \exists \lambda \in [0,1], s.t.\ \lambda \widetilde{UY}_{1,cEE} + (1-\lambda) LY_{1,cEE} = \theta.$

As in the proof of Proposition 3.1, since $\widetilde{y}_{aEE} \leq \overline{Y}^{011} \leq \overline{Y}(y^{111}_{\alpha_{cNE}} \leq Y \leq y^{111}_{1-\alpha_{aEE}})$, $\exists\ \tau \in (0,1]$, $s.t.\ \tau \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}}) + (1-\tau)\overline{Y}(Y \leq y^{111}_{1-\alpha_{cEE}}) = \overline{Y}^{011}$. And $\tau ty + (1-\tau)\frac{\pi_{aEE}}{\pi_{cNE}+\pi_{aEE}} hy + (1-\tau)\frac{\pi_{cNE}}{\pi_{cNE}+\pi_{aEE}} gy = fy_{aEE}$. Since $\widetilde{UY}_{1,cEE}$ is obtained by temporarily assuming aEE is above $y^{111}_{\alpha_{cNE}}$, $\exists \phi \in [0,1]$, $s.t.\ \phi \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}}) + (1-\phi)\widetilde{y}_{aEE} = \overline{Y}^{011}$. Thus, $\lambda\{ty + \frac{\pi_{aEE}}{\pi_{cEE}} hy - [\phi ty + (1-\phi) hy]\frac{\pi_{aEE}}{\pi_{cEE}}\} + (1-\lambda)(\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}} ty + \frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}} hy + \frac{\pi_{cNE}}{\pi_{cEE}+\pi_{cNE}} gy - fy_{aEE}\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}}) = fy_{cEE}$. After some algebra, $[\lambda(1-\phi\frac{\pi_{aEE}}{\pi_{cEE}}) + (1-\lambda)(\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}} - \tau\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}})]ty + \{\lambda\phi\frac{\pi_{aEE}}{\pi_{cEE}} + (1-\lambda)\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}}[1-(1-\tau)\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cNE}}]\}hy$ $+(1-\lambda)\frac{\pi_{cNE}}{\pi_{cEE}+\pi_{cNE}}[1-(1-\tau)\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cNE}}]gy = fy_{cEE}$. Then, the corresponding density for cNE is $\{-\frac{\pi_{aEE}}{\pi_{cNE}}\tau + \frac{\pi_{cEE}}{\pi_{cNE}}[1-\lambda(1-\phi\frac{\pi_{aEE}}{\pi_{cEE}}) - (1-\lambda)(\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}} - \tau\frac{\pi_{aEE}}{\pi_{cEE}+\pi_{cNE}})]\}ty + \frac{\pi_{aEE}}{\pi_{cNE}}\{-\lambda\phi - [1-(1-\lambda)\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}}][1-(1-\tau)\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cNE}}]\}hy + [1-(1-\lambda)\frac{\pi_{cEE}}{\pi_{cEE}+\pi_{cNE}}][1 -(1-\tau)\frac{\pi_{aEE}}{\pi_{aEE}+\pi_{cNE}}]gy = fy_{cNE}$.

The inequality in the second paragraph of this proof shows that Assumption 3.6 holds, as long as $E[Y(1)|cEE] \geq LY_{1,cEE}$. Since $\theta \in [LY_{1,cEE}, UY_{1,cEE}]$ holds by construction, the constructed densities satisfy Assumption 3.6. ■

APPENDIX B

PROCEDURE FOR ESTIMATION AND INFERENCE

In this section, I use the upper bound for $E[Y(1)|cEE]$ in Proposition 3.1 as an illustration to show how to employ the methodology proposed by Chernozhukov, Lee, and Rosen (2011) (hereafter CLR) to calculate the half-median-unbiased estimators and the confidence interval for the true parameter of interest when the bounds involve max or min operator.

The upper bound for $\theta_0 = E[Y(1)|cEE]$ is given by $\theta_0^u = \min_{v\in\mathscr{V}=\{1,2\}} \theta^u(v)$, with $\theta^u(1) = \overline{Y}(Y \geq y^{111}_{1-\alpha_{cEE}})$ and $\theta^u(2) = \overline{Y}(Y \geq y^{111}_{\alpha_{cNE}})\frac{q_{1|0}-p_{01|1}}{p_{01|0}-p_{01|1}} - \overline{Y}^{011}\frac{p_{11|0}}{p_{01|0}-p_{01|1}}$. Then the precision-corrected estimate of θ_0^u is given by $\widehat{\theta}^u(p) = \min_{v\in\{1,2\}}[\widehat{\theta}^u(v) + k(p)s(v)]$, where $\widehat{\theta}^u(v)$ is a consistent estimate of $\theta^u(v)$, $s(v)$ is its standard error and $k(p)$ is a critical value.

The selection of $k(p)$ relies on standardized process $Z_n(v) = \{\theta^u(v) - \widehat{\theta}^u(v)\}/\sigma(v)$, where $\sigma(v)/s(v) \to 1$ uniformly in v. CLR (2011) approximate this process by a standardized Gaussian process $Z_n^*(v)$. Specifically, for any compact set V, CLR (2011) approximate by simulation the p-th quantile of $\sup_{v\in V} Z_n^*(v)$, denoted by $k_{n,V}(p)$, and use it in place of $k(p)$. Since setting $V = \mathscr{V}$ leads to asymptotically valid but conservative inference, CLR (2011) propose a preliminary set estimator $\widehat{V}_n$ of $V_0 = \arg\min_{v\in\mathscr{V}} \theta^u(v)$ for the upper bound (where arg min is replaced by arg max for the lower bound), which they call an adaptive inequality selector. Intuitively, $\widehat{V}_n$ selects the bounding functions that are close enough to binding to affect the asymptotic distribution of the estimators of the upper and lower bounds.

Now let me describe the precise procedure to obtain half-median-unbiased estimators for the upper bounds of Δ and $E[Y(1)|cEE]$ in Proposition 3.1.[24] These upper bounds can be written as $\theta^u_{n0} = \min_{v\in\mathscr{V}=\{1,2\}} \theta^u_n(v)$, where the bounding functions $\theta^u_n(v)$ are given in Proposition 3.1, as illustrated in the example above.[25] Let $\gamma_n = [\theta^u_n(1)\ \theta^u_n(2)]'$ be the vector containing the two bounding functions and let $\widehat{\gamma}_n = [\widehat{\theta}^u_n(1)\ \widehat{\theta}^u_n(2)]'$ denote its sample analog estimator, which can be shown to be consistent and asymptotically normally distributed using standard results (e.g., Newey and McFadden, 1994; Lee, 2009). The specific steps are as follows.

1. Let Ω_n denote the asymptotic variance of $\sqrt{n}(\widehat{\gamma}_n - \gamma_n)$. A consistent estimate of Ω_n, $\widehat{\Omega}_n$, is obtained by a 5000-repetition bootstrap. Let $\widehat{g}_n(v)'$ denote the v^{th} row of $\widehat{\Omega}_n^{1/2}$, $s_n(v) = \|\widehat{g}_n(v)\|/\sqrt{n}$ and, following CLR (2011), set $c_n = 1 - (.1/\log n)$.

2. Simulate $R = 1{,}000{,}000$ draws from $\mathscr{N}(0, I_2)$, denoted $Z_1, ..., Z_R$, where I_2 is a 2×2 identity matrix, and let $Z^*_r(v) = \widehat{g}_n(v)' Z_r / \|\widehat{g}_n(v)\|$ for $r = 1, ..., R$.

3. Let $Q_p(X)$ denote the p-th quantile of a random variable X. Compute $k_{n,\mathscr{V}}(c_n) = Q_{c_n}(\max_{v\in\mathscr{V}} Z^*_r(v), r = 1, ..., R)$; that is, for each replication r calculate the maximum of $Z^*_r(1)$ and $Z^*_r(2)$, and take the c-th quantile of those R values. Then, use the critical value to compute the set estimator $\widehat{V}_n = \{v \in \mathscr{V} : \widehat{\theta}^u_n(v) \leq \min_{\widetilde{v}\in\mathscr{V}}\{[\widehat{\theta}^u_n(\widetilde{v}) + k_{n,\mathscr{V}}(c_n) s_n(\widetilde{v})] + 2k_{n,\mathscr{V}}(c_n) s_n(v)\}$.

4. Compute $k_{n,\widehat{V}_n}(p) = Q_p(\max_{v\in\widehat{V}_n} Z^*_r(v), r = 1, ..., R)$, so that the critical value is based on $\widehat{V}_n$ instead of $\mathscr{V}$.

[24] For further details on the procedure see CLR (2011), especially in Appendix A and Section 4.1

[25] The subscript "n" indicates local parameters.

5. To get the half-median-unbiased estimator of θ^u_{n0}, $\widehat{\theta}^u_n(1/2)$, Set $p = 1/2$ and compute $\widehat{\theta}^u_n(1/2) = \min_{v\in\mathscr{V}}[\widehat{\theta}^u_n(v) + k_{n,\widehat{V}_n}(1/2)s_n(v)]$.

To obtain half-median-unbiased estimators for the lower bounds in Proposition 3.1, which have the form $\theta^l_{n0} = \max_{v\in\mathscr{V}=\{1,2\}}\theta^l_n(v)$, $\widehat{V}_n$ in step 3 above is replaced by $\widehat{V}_n = \{v \in \mathscr{V} : \widehat{\theta}^l_n(v) \geq \max_{\widetilde{v}\in\mathscr{V}}[\widehat{\theta}^l_n(\widetilde{v}) - k_{n,\mathscr{V}}(c_n)s_n(\widetilde{v})] - 2k_{n,\mathscr{V}}(c_n)s_n(v)\}$, and in step 5 I set $\widehat{\theta}^l_n(1/2) = \max_{v\in\mathscr{V}}[\widehat{\theta}^l_n(v) - k_{n,\widehat{V}_n}(1/2)s_n(v)]$.[26]

To describe the construction of confidence intervals for the true parameter θ_0, let $\widehat{\theta}^u_n(p) = \min_{v\in\mathscr{V}}[\widehat{\theta}^u_n(v) + k_{n,\widehat{V}_n}(p)s_n(v)]$ and $\widehat{\theta}^l_n(p) = \max_{v\in\mathscr{V}}[\widehat{\theta}^l_n(v) - k_{n,\widehat{V}_n}(p)s_n(v)]$, where the critical values are obtained as described above. Following CLR (2011), let $\widehat{\Gamma}_n = \widehat{\theta}^u_n(1/2) - \widehat{\theta}^l_n(1/2)$, $\widehat{\Gamma}^+_n = \max(0, \widehat{\Gamma}_n)$, $\rho_n = \max\{\widehat{\theta}^u_n(3/4) - \widehat{\theta}^u_n(1/4), \widehat{\theta}^l_n(1/4) - \widehat{\theta}^l_n(3/4)\}$, $\tau_n = 1/(\rho_n \log n)$ and $\widehat{p}_n = 1 - \Phi(\tau_n\widehat{\Gamma}^+_n)\alpha$, where $\Phi(\cdot)$ is the standard normal CDF. Note that $\widehat{p}_n \in [1-\alpha, 1-\alpha/2]$, with $\widehat{p}_n$ approaching $1-\alpha/2$ when the length of the identified set tends to zero. Then, an asymptotically valid $1-\alpha$ confidence interval for θ_0 is given by $[\widehat{\theta}^l_n(\widehat{p}_n), \widehat{\theta}^u_n(\widehat{p}_n)]$, i.e., $\inf_{\theta_0\in[\theta^l_{n0},\theta^u_{n0}]} P(\theta_0 \in [\widehat{\theta}^l_n(\widehat{p}_n), \widehat{\theta}^u_n(\widehat{p}_n)]) \geq 1 - \alpha + o(1)$.

[26]Note that, because of the symmetry of the normal distribution, no changes are needed when computing the quantiles in steps 3 and 4.

APPENDIX C

GMM MOMENT FUNCTIONS

C.1 GMM Moment Functions for Assumption 2.3

The moment functions for average baseline characteristics of all the strata is based on the conditional expectation defined by $\{Z,D\}$. Let $\bar{x}_k$ denote the expectation of a scalar baseline variable for a certain stratum k. The moment function for this variable is defined as:

$$g(\{\bar{x}_k\}) = \begin{bmatrix} (x-\bar{x}_{at})(1-Z)D \\ (x-\bar{x}_{nt})Z(1-D) \\ (x-\bar{x}_c\frac{\pi_c}{p_{1|1}}-\bar{x}_a\frac{\pi_{at}}{p_{1|1}})ZD \\ (x-\bar{x}_c\frac{\pi_c}{p_{0|0}}-\bar{x}_n\frac{\pi_{nt}}{p_{0|0}})(1-Z)(1-D) \\ x-\sum_k \pi_k\bar{x}_k \end{bmatrix} \tag{C.1}$$

where $\{\bar{x}_k\} = \{\bar{x}_{at}, \bar{x}_{nt}, \bar{x}_c\}$. By Law of Iterated Expectation, $E[g(\{\bar{x}_k\})] = 0$ when evaluated at the true value of $\{\bar{x}_k\}$.

C.2 GMM Moment Functions for Assumption 3.6

Similarly, the moment functions for average baseline characteristics of all the strata in this setting is based on the conditional expectation in each cell defined by $\{Z,D,S\}$. Let $\bar{x}_k$ denote the expectation of a scalar baseline variable for a certain stratum k. The moment function for this variable is defined as:

$$g(\{\bar{x}_k\}) = \begin{bmatrix} (x-\bar{x}_{aNN})(1-Z)D(1-S) \\ (x-\bar{x}_{aEE})(1-Z)DS \\ (x-\bar{x}_{nNN})Z(1-D)(1-S) \\ (x-\bar{x}_{nEE})Z(1-D)S \\ (x-\bar{x}_{cEE}\frac{\pi_{cEE}}{p_{01|0}}-\bar{x}_{nEE}\frac{\pi_{nEE}}{p_{01|0}})(1-Z)(1-D)S \\ (x-\bar{x}_{cNN}\frac{\pi_{cNN}}{p_{10|1}}-\bar{x}_{aNN}\frac{\pi_{aNN}}{p_{10|1}})ZD(1-S) \\ (x-\bar{x}_{cNE}\frac{\pi_{cNE}}{p_{00|0}}-\bar{x}_{cNN}\frac{\pi_{cNN}}{p_{00|0}}-\bar{x}_{nNN}\frac{\pi_{nNN}}{p_{00|0}})(1-Z)(1-D)(1-S) \\ (x-\bar{x}_{cNE}\frac{\pi_{cNE}}{p_{11|1}}-\bar{x}_{cEE}\frac{\pi_{cEE}}{p_{11|1}}-\bar{x}_{aEE}\frac{\pi_{aEE}}{p_{11|1}})ZDS \\ x-\sum_k \pi_k \bar{x}_k \end{bmatrix} \quad \text{(C.2)}$$

where $\{\bar{x}_k\} = \{\bar{x}_{aNN}, \bar{x}_{aEE}, \bar{x}_{nNN}, \bar{x}_{nEE}, \bar{x}_{cNN}, \bar{x}_{cEE}, \bar{x}_{cNE}\}$. By Law of Iterated Expectation, $E[g(\{\bar{x}_k\})] = 0$ when evaluated at the true value of $\{\bar{x}_k\}$.

Alternatively, it is possible to write the moment function for the proportions of all the strata and then to estimate the model together with the average baseline characteristics simultaneously by GMM. However, such GMM estimators do not behave well in the JC data employed in the dissertation. Thus, in the applications, I first identify the proportions of all the strata, and then estimate all the average baseline characteristics given the identified proportions. In the case of addressing sample selection and noncompliance, as seen in $g(\{\bar{x}_k\})$, for each variable, nine equations (eight derived from the conditional expectations defined by $\{Z, D, S\}$ plus one from the expectation for the entire sample) are used to identify seven means, i.e., $\{\bar{x}_k\}$. Since the standard errors obtained from this GMM model do not take into account the fact that the proportions of the strata are also estimated, I use a 500-repetition bootstrap to calculate the standard errors of the estimated average baseline characteristics.

Table C.1: Average Baseline Characteristics for the cEE and cNE Strata

	Entire Sample			Non-Hispanics		
	cEE	cNE	Diff.	cEE	cNE	Diff.
Female	.396** (.015)	.630** (.165)	-.234 (.174)	.390** (.016)	.544** (.134)	-.154 (.145)
Age at Baseline	18.44** (.056)	19.19** (.699)	-.749 (.735)	18.39** (.068)	19.18** (.592)	-.786 (.632)
White, Non-hispanic	.299** (.012)	.260** (.126)	.039 (.133)	.369** (.015)	.259** (.126)	.110 (.135)
Black, Non-Hispanic	.445** (.013)	.622** (.158)	-.177 (.166)	.550** (.015)	.624** (.136)	-.074 (.147)
Has Child	.161** (.011)	.229** (.112)	-.068 (.119)	.151** (.012)	.210* (.111)	-.059 (.119)
Number of children	.215** (.018)	.356* (.187)	-.141 (.200)	.209** (.019)	.280 (.179)	-.071 (.192)
Personal Education	10.22** (.040)	10.34** (.504)	-.123 (.529)	10.24** (.048)	10.27** (.402)	-.036 (.434)
Ever Arrested	.230** (.012)	.223* (.128)	.007 (.136)	.228** (.013)	.292** (.112)	-.064 (.121)
At Baseline						
Have job	.241** (.011)	.174 (.108)	.068 (.115)	.244** (.012)	.159 (.102)	.084 (.110)
Weekly hrs. worked	24.07** (.583)	25.27** (6.365)	-1.196 (6.766)	24.05** (.613)	25.23** (5.768)	-1.187 (6.160)
Weekly earnings	113.86** (3.987)	120.08** (39.90)	-6.219 (40.90)	115.48** (3.500)	142.57** (34.31)	-27.09 (36.51)
Had job, Prev. Yr.	.714** (.013)	.585** (.141)	.129 (.151)	.718** (.014)	.588** (.126)	.130 (.136)
Mths. employed,Prev.Yr.	4.346** (.122)	3.286** (1.201)	1.060 (1.280)	4.435** (.137)	2.935** (1.105)	1.500 (1.201)
Earnings, Prev.Yr.	3396.2** (128.63)	3136.2** (1185.0)	260.02 (1250.6)	3377.6** (128.55)	2879.7** (1009.5)	497.88 (1095.9)

Note: Numbers in parentheses are standard errors. ** and * denote that estimate is statistically different from 0 at 5% and 10% level, respectively. Computations use design weights. Missing values for each of the baseline variables were imputed with the mean of the variable. The standard error is calculated by a 500-repetition bootstrap.

C.3 Baseline Characteristic Estimates

To assess Assumption 3.6, Table C.1 shows the average baseline characteristics for the cEE and cNE strata and their difference in Section 4.3. It is obtained by estimating the above GMM function (C.2).