Instrument Assignment Based on LATE Results with Defiers

Jiacheng Xie

November 2024

THE UNIVERSITY OF SYDNEY

School of Economics

University of Sydney

Thesis submitted in partial fulfilment of the award course requirements of the

Bachelor of Economics (Honours)

Supervised by Dr. Moyu Liao

STATEMENT OF ORIGINALITY

I hereby declare that this submission is my own work and to the best of my knowledge it contains no material previously published or written by another person. Nor does it contain any material which has been accepted for the award of any other degree or diploma at the University of Sydney or at any other educational institution, except where due acknowledgment is made in this thesis.

Any contributions made to the research by others with whom I have had the benefit of working at the University of Sydney is explicitly acknowledged.

I also declare that the intellectual content of this study is the product of my own work and research, except to the extent that assistance from others in the project's conception and design is acknowledged.

Jiacheng Xie

5 November 2024

Acknowledgement

First, I would like to express my sincere gratitude to my supervisor, Dr. Moyu Liao, who patiently guided me through fundamental concepts and provided invaluable support throughout this journey. His patience and dedication were especially meaningful to me, as I am not a naturally talented guy. I genuinely enjoyed our discussions and meetings over the past year. I am also deeply grateful to my parents for their financial support, which allowed me to focus on my studies without financial concerns and made my university life easier. My appreciation extends to other faculty members, including Dr. Yiran Xie, Dr. Xuetao Shi, Dr. Dak Yung Seong, and other staff members in the Econometrics department, whose teaching provided foundational academic insights and valuable feedback that greatly enriched my research. Finally, a special thanks to my girlfriend, who has been a constant support, especially for taking care of me, even cooking fried rice for me at 3 am during my late-night work sessions.

I acknowledge the use of ChatGPT (https://chat.openai.com/) to polish academic language and accuracy of my work. On 3 November 2024, I submitted my entire thesis with the instruction to "polish my work by improving the accuracy of vocabulary and grammar." Further, I altered the output to better express myself in my own tone.

Abstract

While most previous studies have focused on treatment assignment, this thesis emphasises instrument assignment to estimate Local Average Treatment Effects (LATE). We introduce a refined methodology to address limitations in the traditional assumptions set by Imbens and Angrist (1994), specifically by relaxing the monotonicity assumption to account for defiers. This work extends the contributions of Liao (2022) by integrating continuous control variables, namely KWW(Knowledge of the World Work) and IQ scores, into the LATE framework. We empirically validate our approach using college proximity as an instrumental variable. This study examines the impact of college attendance on wage outcomes, offering new insights into how treatment effects vary among individuals with different cognitive and job-related skills. The results conclude that individuals with lower KWW and IQ scores experience the highest wage gains from college attendance, highlighting the potential of targeted educational interventions. By accommodating defiers, this approach improves the robustness and applicability of LATE estimation across diverse empirical settings.

1 Introduction

Most prior empirical studies focus on direct treatment assignment when assessing policy implementation, aiming to evaluate the treatment effects on groups that are expected to benefit the most. However, direct manipulation of treatment assignment is often impractical. In such cases, Instrumental Variable (IV) methods are introduced to determine the causal effects of interventions in economics. Our study shifts the focus from the traditional emphasis on treatment assignment to examining the assignment of the instrument itself.

For instance, in the real-world context of evaluating the impact of attending a four-year college on income, we cannot randomly assign students to attend college. Instead, we employ an alternative approach by using proximity to a college as an instrument to explore the causal effect on income. This approach allows us to indirectly assess the treatment effect, using college proximity to determine the likelihood of attending college.

We situate ourselves in this context by adopting born near a college as the instrumental variable, log wage in 1976 as the outcome variable, and the individual who attended 4-year college education as the treatment variable. The dataset was obtained from Card's study(1993).

Moreover, the shift from treatment to instrument assignment is significant because, while most studies estimate the effect of a treatment (such as attending college) on an outcome (like wages), they often overlook the role of instruments in isolating the exogenous variation necessary for addressing endogeneity problems.

The Local Average Treatment Effect (LATE) concept developed by Imbens and Angrist (1994) is the most suitable measure of the outcome in the IV framework. It is particularly effective in estimating when the treatment effect is heterogeneous. It captures the effect on individuals whose treatment status is directly influenced by the instrument.

When the treatment effect is the same for everyone (uniform), the LATE estimate simplifies to the classic two-stage least squares (2SLS) estimator, which measures the average treatment effect across the whole population. However, the LATE becomes particularly valuable when treatment effects vary between individuals (heterogeneous treatment effects). This is crucial in our context, where factors such as individual cognitive ability (measured by IQ) and practical job-related knowledge (KWW scores) influence the likelihood of attending college and its subsequent impact on wages. These factors introduce variation in how the treatment (college attendance) impacts different individuals, making a uniform treatment effect assumption unsuitable.

LATE represents a weighted average of the treatment effects, where the weights depend on the likelihood of individuals responding to the treatment assignment (i.e., being Compliers). This makes LATE more informative in understanding the effect of this responsive subgroup in the presence of varied responses across individuals. By focusing on LATE, the analysis provides insights into policy implications, such as potential interventions to encourage college attendance.

This study situates itself within the broader econometric literature by extending and challenging existing models. We build on the foundational work of Imbens and Angrist (1994), who introduced two traditional assumptions on which LATE estimation relies. One of the two assumptions is monotonicity, known as the no-defier assumption. It asserts that the instrument will lead the individuals to respond to the treatment with its intended effects. The other assumption is that the instrument is uncorrelated with unobserved factors influencing the outcome, known as impendence.

Although these assumptions have been widely accepted in empirical research, they remain open to challenge. Previous researchers did not specify solutions if the assumptions were violated, and even these were not tested in most studies. Recently, the validity of these assumptions has been increasingly questioned, particularly the 'no defiers' condition, which may not hold if some individuals act contrary to the instrument's intended effect. Specifically, this is evidenced in the recent advancements in the LATE framework by researchers like Kitagawa (2015) and Mourifié et al. (2017), who have highlighted the limitations of traditional IV assumptions.

Our study adopts a refined LATE framework with two assumptions that have been relaxed by Liao (2022) to address these limitations while allowing for the potential existence of defiers. This refinement to the traditional assumption enhances the robustness and applicability of LATE estimation through accommodating scenarios where traditional assumptions may not fully apply.

Previous research has addressed assumption violations without incorporating control variables. Our study extends this work by developing a methodology that includes a continuous control variable, providing a more comprehensive LATE estimation approach that addresses existing defiers. The control variable we used is the combination of KWW and IQ score. Incorporating control variables like cognitive skills (IQ) and practical knowledge (KWW) allows for more precise identification of individuals who benefit most from the treatment. This approach results in a more accurate assessment of policy impacts, enhancing the reliability of the findings.

Exploring an advanced framework for estimating LATE with a continuous control variable is novel and complex. We begin by considering the more straightforward case where the outcome variable (Y) is continuous and no control variable is included, providing fundamental mathematical intuitions.

We use an empirical approach to estimate the treatment effect, demonstrating its validity and practical application in estimating the LATE. The results from our LATE estimation reveal that as combined KWW and IQ scores increase, the positive effect of being born near a college on wages diminishes. This indicates that individuals with lower combined KWW and IQ scores benefit the most from the intervention.

Specifically, our results indicate that the most efficient allocation of resources would involve targeting individuals with KWW and IQ scores below 1.02, representing 25% of the population. Their estimated LATE values exceed 3.26, highlighting a substantial potential impact. Suppose resources permit subsidies for a more significant portion of the population. In that case, we recommend extending support to individuals with KWW and IQ scores below 1.33 (representing 50% of the population) and 1.47 (75% of the population), where the associated LATE values are 2.03 and 1.23, respectively. These findings suggest that policies should prioritise reducing geographic barriers to education for those with lower KWW and IQ scores to improve their wage-earning potential.

During the density estimation process for compliers, we identified the existence of defiers, as evidenced by instances where the density estimates for compliers fell below zero. This observation necessitated the adoption of a relaxed assumption in our analysis to accommodate the presence of defiers, ensuring the robustness of our LATE estimates.

Our study further contributes to the literature by addressing the violation of the traditional "no defiers" assumption. We adopt a more flexible approach by allowing for the existence of defiers and developing a refined methodology for estimating LATE under these relaxed conditions. This adjustment ensures the robustness of LATE estimates, providing a realistic assessment of causal effects. By incorporating continuous control variables, our study captures finer details in the data, which leads to more precise policy recommendations, such as targeting educational subsidies to those most likely to benefit.

Previous studies have primarily focused on solving the defier issue while conditioning on discrete variables. This is more simplified compared and has the limitation of leading to biased or imprecise estimates of LATE. It overlooks the continuous nature of crucial covariates, such as IQ scores or years of experience, which may vary significantly within populations. Since we recognised this limitation, our research extends the LATE framework by exploring and validating a methodology that accommodates continuous control variables. Doing so provides future researchers with more flexibility in selecting control variables, enhancing a more accurate assessment of treatment effects across diverse subpopulations with varying characteristics.

This research has policy implications for education and wage inequality. We analyse how variations in cognitive ability (IQ) and job-related skills (KWW) influence the returns to education, going beyond estimating the average effect of college attendance on wages. We offer insights into the wage disparities by examining differences in returns across subpopulations with varying ability levels. These findings can offer suggestions to policymakers in designing education policies to reduce wage gaps and improve social mobility. Our approach of estimating LATE as a function of KWW and IQ helps identify groups that benefit the most from college attendance and informs more targeted educational policies.

Our methodology has broader applications beyond education, with potential implications for labour market policies. It can be adapted to analyse the impact of various interventions, such as job training programs or subsidies, on different subgroups defined by their cognitive and job-related skills. By considering individual heterogeneity, our approach further contributes to reducing labour market disparities and improving economic opportunities across diverse populations.

2 Literature review

Card (1993) provides a framework for addressing endogeneity in educational attainment by using proximity to a college as an instrument. He employed an IV model and Two-Stage Least Squares (2SLS) to estimate the returns to education, though his approach was limited in addressing heterogeneity in treatment effects. We adopt the same IV model and instrument but employ a different estimation procedure. Specifically, we use LATE estimation instead of 2SLS, which provides more accurate estimates when treatment effects are heterogeneous, thereby enhancing the precision of the estimated treatment effect.

Our study further extends Card's model by introducing additional complexities. While Card's model primarily focuses on the average effect of college attendance, it does not account for how individual characteristics, such as cognitive skills and practical knowledge, might influence these effects. Our approach incorporates continuous control variables into the LATE estimation, specifically IQ and KWW scores. This refinement allows for a more nuanced understanding of how the returns to education vary across individuals with different ability levels, offering a deeper analysis of the heterogeneity in treatment effects that were not addressed in Card's original study.

Imben and Angrist (1994) proved that even a valid instrument does not guarantee the identification of the Average Treatment Effect (ATE) if the treatment effect varies across individuals. Imben and Angrist (IA) addressed this by establishing monotonicity and independence, which together ensure that the IV estimand identifies the LATE for compliers. The independence assumption requires that the instrument (Z_i) is jointly independent of potential outcomes $(Y_i(1), Y_i(0))$ and treatment assignments $(D_i(1), D_i(0))$. This condition guarantees that Z is a valid instrument in that it does not directly affect the responses $Y_i(1)$ and $Y_i(0)$ and does not affect the probability of participation in the program (i.e., that is correlated with D). The existence of a valid instrument implies that the endogeneity of treatment assignment can be dealt with, but it does not address the issue of treatment effect variation.

As a result, he introduced another monotonicity assumption, also known as the "no defiers" condition. This asserts that the instrument should have a consistent directional effect on the treatment across all individuals. This means that no one should be encouraged to take the opposite action in response to the instrument compared to others. These assumptions are crucial for identifying the LATE. LATE is particularly effective when the treatment effect is not uniform across all individuals, as it provides a local estimate of the causal impact on the subpopulation of compliers. The group of individuals who only take the treatment when encouraged by the instrument is identified as the compliers.

Although the LATE theory from IA has been widely influential in the applied economics literature, there are still concerns about the validity of the key assumptions. Building on IA's framework, Kitagawa (2015) introduced a novel method for empirically evaluating the assumptions using a variance-weighted Kolmogorov-Smirnov test. This test checks whether the observed outcome densities support the assumption that the instrument validly influences treatment assignment. They concluded that the strongest testable implication for instrument validity is the nonnegativity of point-identifiable compliers' outcome densities. The test supports the evaluation of instrument validity by ensuring that compliers' densities remain non-negative, indirectly providing evidence that aligns with the broader assumptions needed for LATE estimation.

Kitagawa's approach extends the application of this test to both discrete and continuous outcomes and further accommodates conditioning covariates, ensuring the robustness of the instrument's validity across different settings. Hence, we would follow their conclusion by maintaining the nonnegativity condition to sustain the instrument's validity throughout our analysis.

Similarly, Mourifié and Wan (2017) proposed a complementary method, formulating LATE assumptions as testable implications through conditional inequalities. Their approach allows for evaluating the presence of defiers by testing whether the conditions for monotonicity and instrument validity hold in a given setting. This complements Kitagawa's variance-weighted Kolmogorov-Smirnov test by offering different power properties and methodological approaches, such as conditional moment inequalities and local linear regression.

In particular, IA's joint assumptions of LATE monotonicity and independence were found to be rejected for certain subgroups using the college proximity instrument (Mourifié & Wan, 2017). Since our approach used the same dataset, these findings raise our concerns, prompting the need to refine the LATE assumptions in our analysis.

Various researchers have recently proposed numerous solutions to address the issue of violated assumptions. Dahl, Huber, and Mellace (2023) offered an alternative by introducing a local monotonicity condition, allowing for compliers and defiers instead of setting a strict "no defiers" assumption. This approach enables accurate treatment effect estimation even when the instrument's influence is not uniform across individuals. They provided empirical evidence for the existence of compliers and defiers, proving that standard LATE estimation is not robust if the defier is ignored. 2SLS was imprecisely estimated because compliers and defiers netting out in the first stage created a weak instrument problem. As a solution, they developed techniques to estimate LATE without relying solely on traditional two-stage least squares (2SLS). Despite this, it could be problematic in the presence of defiers. Their work concluded that accounting for defiers provided more precise estimates than standard LATE approaches.

Liao (2022) proposed a similar solution to address the issue of the violation of the classic assumptions. He proposed a "relaxed" assumption framework incorporating minimal defiers and a conditional type-independent instrument assumption. His approach estimates LATE with the relaxed assumption by adjusting the negative complier density back to zero by adding the minimal value of the defiers, thereby allowing the existence of defiers. Liao's methodology offers a non-refutability criterion, ensuring that the relaxed assumptions cannot be rejected based on observable distributions, providing a stronger foundation for the estimation process.

Further, he proved the validity of this refined LATE estimation methodology with empirical application through conditioning on discrete variables. This led to improvements in the empirical results of Card's (1993) dataset, which concludes with a more reasonable sign and scale of the LATE estimates. We would adopt his methodology as the foundation, extending it with a continuous conditional control variable.

Moreover, Kitagawa and Tetenov (2018) introduced the Empirical Welfare Maximization (EWM) algorithm, focusing on treatment assignment to optimise resource allocation. The EWM method estimates a treatment assignment policy that maximises social welfare based on observed data, making it applicable for policy design under constraints such as limited budgets or eligibility criteria. Their study could offer valuable insights into policy, especially when feasible treatments are constrained. While their EWM approach optimises treatment allocation, our focus on instrument assignment offers a complementary strategy to improve causal effect estimation for specific subpopulations.

3 Data

Our approach used data from the literature: the college proximity instrument from Card (1993), which has been widely utilised in causal inference. Card examined how geographic proximity to colleges influenced educational attainment and earnings. Card's data came from the National Longitudinal Survey (NLS) of Young Men, which began in 1966 and includes men aged 14–24 at the time. The survey continued with follow-up studies through 1981. Based on the respondent's county of residence in 1966, the Card's data provide the presence of a four-year college in the local labour market. In 1976, the interview was conducted to collect the follow-up wage and educational attainment, constructing the data of years of education and wages. The original dataset contained 3,613 observations. After cleaning the data and removing records with missing information, the sample size was reduced to 2,040 observations.

Unlike Card's study, we represent educational attainment as a binary variable and use it as our treatment variable (D). Educational attainment has the range from 8 to 18 years, with 16 or more years categorised as achieving a four-year college degree.

In our application, Z denotes the instrumental variable, where $Z_i = 1$ indicates that the individual was born near a four-year college, and $Z_i = 0$ indicates otherwise. D denotes the treatment variable, where $D_i = 1$ indicates that the individual attended a four-year college, and $D_i = 0$ indicates otherwise. Y is used as the outcome variable, which denotes the individual's log wage in 1976. X is used as the control variable, denoting the combinations of KWW and IQ scores.

The following table provides descriptive statistics for key variables used in the analysis, including their mean, standard deviation, minimum, maximum, and median values.

Table 1: Descriptive Statistics of Key Variables

	Mean	Standard Deviation	Minimum	Maximum	Median
Z	0.708	0.455	0	1	1
D	0.334	0.472	0	1	0
Y	6.335	0.418	4.718	7.785	6.358
X (KWW&IQ)	1.323	0.202	0.615	1.866	1.341
KWW	0.636	0.135	0.179	1	0.643
IQ	0.688	0.104	0.336	1	0.691

This table presents the key statistics of the variables we used. We could draw a few key points by analysing these stats in the context of our study. The mean of $Z = 0.708$ indicates that approximately 70.8% of the sample was exposed to the instrument, which is significant for identifying compliers whose treatment status is influenced by Z. The outcome Y, with a mean of 6.335, provides a baseline for understanding the average effect of the treatment. Additionally, the control variable X, which combines cognitive and job-related skills (KWW and IQ), has a mean of 1.323, offering insight into the overall skill level in the sample, which may moderate the treatment's impact.

We consider being born near a college an instrumental variable based on three criteria: relevance, independence, and exogeneity. Firstly, there is a strong correlation between college attendance (D) and proximity to a college (Z). The financial and logistical barriers to attending college, such as housing and transportation costs, are significantly reduced for individuals living near a college. This increased convenience raises the likelihood that those who live near a college will choose to attend.

Secondly, the instrumental variable Z is independent of unobserved factors, such as motivation or intrinsic intelligence, which could affect the outcome (Y). Being born near a college is considered a random factor and is unlikely to be influenced by an individual's traits or potential. Therefore, it can be assumed that proximity to a college does not correlate with unobservable personal attributes that might affect future income.

Thirdly, Z is a valid instrument because it affects Y mainly through its impact on D rather than through any other direct pathways. This property is crucial for accurately isolating the effect of education, mediated by college attendance, on future income.

We select three continuous control variables: KWW, IQ, and a combination of KWW and IQ. The KWW score measures practical job-related knowledge, while IQ assesses cognitive ability. To normalise the combined KWW and IQ score, we divide it by the maximum score, ensuring a consistent scale across all values. This normalisation helps to facilitate comparisons between individuals without altering the interpretation.

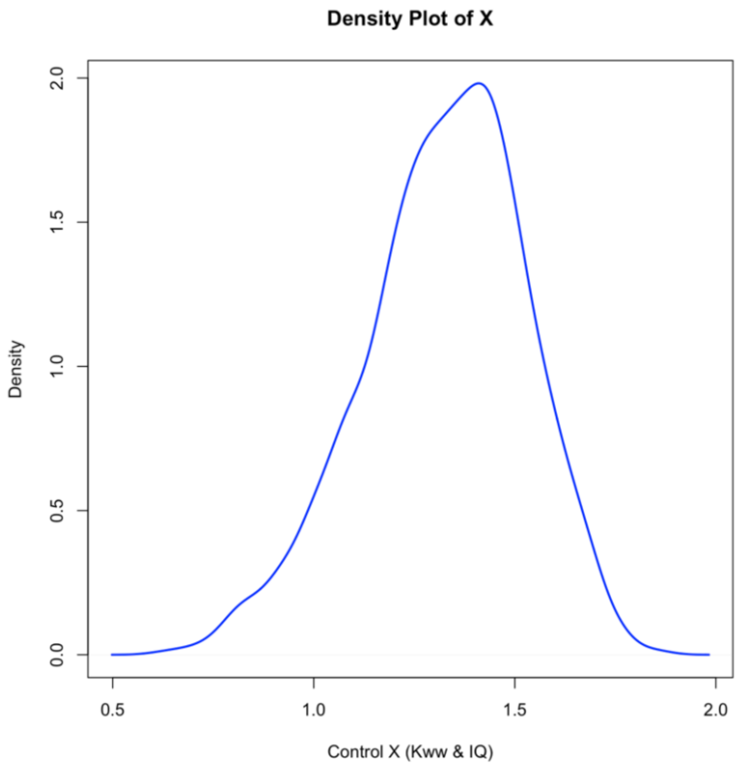
In econometrics and labour economics, KWW is frequently used to account for job-specific knowledge when examining the relationship between education, skills, and wages. We used combined KWW and IQ as control variables for several reasons. Including both allows us to capture a broader spectrum of individual abilities, reducing bias and increasing the accuracy of our IV approach.

KWW captures practical skills directly related to job performance, while IQ measures cognitive skills such as problem-solving, learning, and adaptability. Together, they provide a more comprehensive view of an individual's abilities, which is critical for determining labour market success and wage outcomes.

Since we treat combined KWW and IQ as the control variable, we could account for differences in ability that might otherwise introduce bias, such as when individuals with higher cognitive skills are more likely to attend college. This improves our analysis by minimising the risk of bias in estimating the effects of college attendance on wages. Including these control variables ensures that the IV approach—using proximity to a college as an instrument—more effectively estimates the LATE across different ability levels.

Moreover, the choice of KWW and IQ as control variables aligns with existing literature on wage determination and education returns. Returns to education vary with individual ability, with higher cognitive skills often leading to greater wage gains. (Card, 1999) Including both KWW and IQ in our model helps account for these variations, thereby providing a more precise estimate of the causal impact of college education.

Figure 1: Plot of KWW & IQ distribution



The density plot for the combined KWW and IQ scores illustrates that the density is concentrated between scores of 1.0 and 1.5, indicating that most individuals fall in this range. The more clustered the observations are, the more precise the LATE estimation becomes. More observations are clustered within this middle range, which provides a more substantial basis for reliable estimation. Consequently, the estimation may not fully capture the treatment effects for individuals at the extreme ends of the spectrum, with very low or very high combined scores, as these cases are underrepresented. This could potentially limit the generalizability of the findings, especially when considering populations that deviate significantly from the average in terms of KWW and IQ.

4 Methodology

Our primary objective is to explore and validate the methodology of estimating LATE with the outcome variable Y conditioned on a continuous variable X. However, this is novel and very complicated. Therefore, we would start with a simple example of Y being continuous without conditioning on a variable. This would provide us with essential intuitions for the later derivation.

4.1 Intuition derived from the case without a control variable

We start with a simple application with a binary treatment, D, a binary instrument variable, Z and a continuous outcome variable Y. By adopting the IV Model and potential outcome framework, these variables formulate the estimation equation:

$$Y_i = Y_i(1) * D_i + Y_i(0) * (1 - D_i)$$

$$D_i = Z_i * D_i(1) + (1 - Z_i) * D_i(0)$$

where Y_i, D_i, and Z_i are observed and known. We could extract these values straight from the dataset. $D_i(1), D_i(0), Y_i(1)$ *and* $Y_i(0)$ are potential and unknown. $D_i(1)$ and $D_i(0)$ refer to the potential treatment decisions for individual i under different values of the instrument Z_i. It captures counterfactual scenarios for the treatment decision. Specifically, $D_i(1)$ indicates whether individual i receives the treatment when the instrument Z_i equals 1 while $D_i(0)$ indicates whether individual i receives the treatment when the instrument Z_i equals 0. $Y_i(1)$ represents the potential outcome for individual i if the individual receives the treatment. $Y_i(0)$ represents the potential outcome for individual i if the individual does not receive the treatment.

Angrist, Imbens, and Rubin (1996) define the following four latent groups in the population: compliers, always-takers, never-takers, and defiers. In the potential outcome framework, the population is categorised into the four different latent groups based on their potential decisions.

$$Compliers: D_i\,(1) = 1, D_i\,(0) = 0$$

- $D_i\ (1) = 1$: They take the treatment when it is assigned
- $D_i\ (0) = 0$: They do not take the treatment when it is not assigned.

Compliers only respond to the assignment of the treatment, meaning they participate if assigned but otherwise do not. This behaviour makes them essential for causal inference because they reflect how the treatment affects outcomes. Since they only take the treatment when assigned, they represent a subgroup directly responsive to treatment assignment, helping us isolate the treatment effect.

$$Always - takers: D_i(1) = 1, D_i(0) = 1$$

- $D_i(1) = 1$: They take the treatment when it is assigned.
- $D_i(0) = 1$: They still take the treatment even when it is not assigned.

Always-takers receive the treatment regardless of assignment, which means they do not respond to the treatment assignments. They do not help in understanding the effect of treatment assignments because they always participate, whether they were encouraged to do so.

$$Never - takers: D_i(1) = 0, D_i(0) = 0$$

- $D_i(1) = 0$: They do not take the treatment even when it is assigned.
- $D_i(0) = 0$: They also do not take the treatment when it is not assigned.

Never-takers are those who never participate in the treatment, regardless of assignment. Since they do not respond to the assignment, they do not contribute to estimating the treatment effect based on assignment.

$$Defiers: D_i(1) = 0, D_i(0) = 1$$

- $D_i(1) = 0$:They do not take the treatment when it is assigned.
- $D_i(0) = 1$: They take the treatment when it is not assigned.

Defiers act oppositely to the treatment assignment, meaning they refuse the treatment when it is assigned but take it when it is not. This group is typically assumed to be non-existent or irrelevant in causal studies because their behaviour complicates the estimation of causal effects. If Defiers exist, it suggests the assignment instrument does not work as intended for some individuals.

LATE, as our major measurement of outcome, focuses on estimating the treatment effect on the subpopulation known as “compliers”. Compliers are the group of individuals who will “comply” the treatment if they are given the instrument. Since compliers are the only group that changes their treatment status based on the instrument, they represent the individuals for whom the instrument has an effect. Estimating the treatment effect for this group tells us how effective the intervention is for those who are actually influenced by the assignment.

It also isolates the treatment effect among individuals who receive the treatment solely because of the assignment or encouragement. Subsequently, the result of LATE values could be a significant indicator in determining the instrument assignment. The LATE equation is developed by IA (1994), as:

$$LATE = E[Y_i(1) - Y_i(0) | D_i (1) = 1, D_i (0) = 0]$$

$Where\ the\ conditions\ represent\ the\ subgroups\ of\ compliers.$ $Y_i(1)$ represents the potential outcome for individual i if they receive the treatment. $Y_i(0)$ represents the potential outcome for individual i if they do not receive the treatment. So, $Yi(1) - Yi(0)$ captures the individual causal effect of the treatment for person iii: the difference in outcome between receiving and not receiving the treatment. This equation takes the difference in expected outcomes between receiving and not receiving the treatment, specifically for individuals who comply with the instrument.

We could only identify the density of the subgroup of compliers by deriving from the potential outcome framework. The following table categorises individuals into eight scenarios based on their latent behaviours within the potential outcome framework.

Table 2: Classification of Latent Groups in the Potential Outcomes Framework under IV Approach

Data	Potential Decision		Instrument	Potential Outcomes	Latent groups	Notation
$D_i = 0, Y_i = y$ conditioned on $Z_i = 0$	$D_i(1) = 0$	$D_i(0) = 0$	$Z_i = 0$	$Y_i(0) = y$	Never takers	A
	$D_i(1) = 1$	$D_i(0) = 0$	$Z_i = 0$	$Y_i(0) = y$	Compliers	B
$D_i = 1, Y_i = y$ conditioned on $Z_i = 0$	$D_i(1) = 0$	$D_i(0) = 1$	$Z_i = 0$	$Y_i(1) = y$	Defiers	C
	$D_i(1) = 1$	$D_i(0) = 1$	$Z_i = 0$	$Y_i(1) = y$	Always Takers	D
$D_i = 0, Y_i = y$ conditioned on $Z_i = 1$	$D_i(1) = 0$	$D_i(0) = 0$	$Z_i = 1$	$Y_i(0) = y$	Never takers	E
	$D_i(1) = 0$	$D_i(0) = 1$	$Z_i = 1$	$Y_i(0) = y$	Defiers	F
$D_i = 1, Y_i = y$ conditioned on $Z_i = 1$	$D_i(1) = 1$	$D_i(0) = 0$	$Z_i = 1$	$Y_i(1) = y$	Compliers	G
	$D_i(1) = 1$	$D_i(0) = 1$	$Z_i = 1$	$Y_i(1) = y$	Always Takers	H

This table categorises individuals into distinct latent groups based on their behaviour regarding treatment decisions and potential outcomes in IV analysis. The framework assesses how individuals behave when exposed to an instrument (Z) and whether they take the treatment (D). It also examines the potential outcomes $Y_i(1)$ and $Y_i(0)$ with and without the treatment.

The first column of the table contains data of conditional density functions. In causal inference, particularly with LATE estimation, we are trying to measure how treatment probabilities change when the instrument changes. This requires calculating the instrument's causal effect on treatment. Conditional density functions, like $f_{y,D_i=1 \mid Z_i=1}$, help isolate the instrument's impact on the likelihood of treatment assignment, which is key to interpreting causal relationships.

To identify the compliers, we need to impose the IA's two traditional assumptions: monotonicity and independence. The independence assumption refers to the potential outcomes ($Y_i(1)$ and $Y_i(0)$) and potential treatment decisions ($D_i(1)$ and $D_i(0)$) being jointly independent of Z_i.

Monotonicity indicates that the instrument affects all individuals' treatment choices in the same direction, mathematically represented by:

$$D_i(1) \geq D_i(0)\ for\ all\ i\ \ or\ D_i(1)\ \leq\ D_i(0)\ for\ all\ i$$

Monotonicity is also known as no defier assumption as it affirms the non-existence of defiers by assuming that no individual behaves oppositely to the instrument's intended effect. Specifically, suppose the instrument Z (e.g., living near a college) is designed to encourage participation in the treatment (e.g., attending a four-year college). In that case, monotonicity requires that individuals who will attend when $Z_i = 1$ (instrument is active) cannot refuse to attend when $Z_i = 0$ (instrument is inactive).

Using the table and IA's assumptions, we could identify the density of the compliers from the following derivation:

1. Density for $Y_i = y$, $D_i = 0$ conditioned on $Z_i = 0$:

$$f_{y,D_i=0\ |Z_i=0} = A + B$$

   Here, A represents never-takers and B represents compliers.

2. Density for $Y_i = y$, $D_i = 0$ conditioned on $Z_i = 1$:

$$f_{y,D_i=0,|Z_i=1} = E + F$$

By assumption of monotonicity, the defier group F does not exist, meaning $F = 0$.

Therefore:

$$f_{y,D_i=0|Z_i=1} = E$$

3. Identifying the relationship between E and A:

   By the assumption of independence, $E = A$. This is derived under this independence assumption, implying that the distribution of potential outcomes (or the density) when $Z_i = 0$ (for never-takers) should be the same as when $Z_i = 1$ if we are isolating the effect of the instrument. This step uses the idea that instrument Z does not affect these underlying distributions for individuals who are not compliers (never-takers and always-takers), ensuring that any difference is attributed to the treatment effect for compliers.

   Thus:

$$f_{y,D_i=0|\,Z_i=0} = E + B = f_{y,D_i=0|\,Z_i=1} + B$$

   Here, A represents never-takers and B represents compliers.

4. Complier identification for $D_i = 0$:

$$B = f_{y,D_i=0\,|Z=0} - f_{y,D_i=0|\,Z_i=1}$$

Similarly, we apply the same logic to identify the complier group for $D_i = 1$ as follows:

1. Density for $Y_i = y, D_i = 1$ conditioned on $Z_i = 1$:

$$f_{y,D_i=1\,|\,Z_i=1} = G + H$$

   Where G represents compliers and H represents always-takers.

2. Density for $Y_i = y, D_i = 1$ conditioned on $Z_i = 0$:

$$f_{y,D_i=1|\,Z_i=0} = C + D$$

   With C representing defiers (which are assumed to be zero, $C = 0$) and D representing always takers.

3. Complier identification for $D_i = 1$:

$$G = f_{y,D_i=1|Z_i=1} - f_{y,D_i=1|\,Z_i=0}$$

However, many recent research empirically proved that the IA's two traditional assumptions were violated with the dataset used by Card. Our result of estimating the density of compliers also offers evidence that concludes with the same defier issue. The results showed negative density estimations of the complier amount for both $Y_i(1)$ and $Y_i(0)$ cases. This violates the rule of a probability being greater than 0. Consequently, the assumption of monotonicity is rejected. Further, the complier density for $D_i = 1$ does not equal to the density for $D_i = 0$. Therefore, the independence assumption is no longer sustained.

To address this, we adopt relaxed assumptions (Liao, 2022) for estimating LATE by allowing the existence of defiers. We relaxed the traditional assumptions of monotonicity, the 'No Defier' condition to the minimal defier condition and the independent instrument to a type independent instrument assumption. The type independent is represented mathematically as:

$$Y_i(1), Y_i(0) \perp Z_i \mid D_i(1), D_i(0)$$

The traditional monotonicity assumption fails when the density estimation of compliers is less than zero, specifically the density difference of $f_{y,D_i=1 \mid Z_i=1} - f_{y,D_i=1 \mid Z_i=0}$ for $D_i = 1$ and $f_{y,D_i=0 \mid Z_i=0} - f_{y,D_i=0 \mid Z_i=1}$ for $D_i = 0$. If the difference of estimated density is less than 0 for any y value, it violates the property of the probability of being greater than 0.

In this approach, we reassign any negative densities of the complier group for any value of y to the defier densities. Consequently, the negative complier density for any value of y is adjusted to zero. This process must be applied iteratively across all y values. Originally, we had the total density of the compliers as:

$$B * \Pr(Z_i = 0) + G * \Pr(Z_i = 1)$$

$$= \left(f_{y,D_i=0 \mid Z_i=0} - f_{y,D_i=0 \mid Z_i=1}\right) * Pr(Z_i = 0)$$

$$+ \left(f_{y,D_i=1 \mid Z_i=1} - f_{y,D_i=1 \mid Z_i=0}\right) * Pr(Z_i = 1) \text{ for any value of y.}$$

If the complier density is negative for any y value, its negative density is reassigned as defier density, and the complier density is adjusted back to zero. Here, we allow the existence of defiers; thus, the defier density equals the sum of $B * \Pr(Z_i = 0) + G * \Pr(Z_i = 1)$ for any y value.

This relaxed assumption of monotonicity allows us to proceed to estimate and ensure LATE is well-defined. This procedure, which involves verifying whether the density estimates for compliers align with our relaxed assumptions, must be applied to each individual estimate for all individual values of Y.

As a result, we impose an indicator function into the LATE equation. The indicator function verifies whether the difference between the density estimations meets the criteria of our relaxed monotonicity assumption.

Under the proper conditions, Liao (2022) expresses LATE in the context of an IV approach as:

$$LATE = E[Y_i(1) \mid Z_i = 1] - E[Y_i(0) \mid Z_i = 0]$$

$E[Y_i(1) \mid Z_i = 1]$ represents the expected outcome Y_i for individuals who would take the treatment if assigned the instrument (where $Z_i = 1$). It captures the expected value of the outcome for those who receive the treatment due to the instrument. $E[Y_i(0) \mid Z_i = 0]$ represents the expected outcome Y_i for individuals who would not take the treatment if not assigned the instrument (where $Z_i = 0$). It captures the expected value of the outcome for those who do not receive the treatment due to the instrument.

The equation calculates the difference in outcomes between two groups: those who received the treatment because they were encouraged by the instrument (compliers who take the treatment when $Z_i = 1$) and those who did not receive the treatment because they were not encouraged by the instrument (compliers who do not take the treatment when $Z_i = 0$)

We would adopt this LATE equation as this transformation simplifies the expression for LATE by moving from a condition on treatment compliance to one on the instrumental variable's effect, leveraging the IV assumptions to make this shift valid.

We would calculate $E[Y_i(1) \mid Z_i = 1]$ and $E[Y_i(0) \mid Z_i = 0]$ separately. The equations are given as such:

$$E[Y_i(1) \mid Z_i = 1] = \frac{(\int y \cdot (f_{y, D_i=1 \mid Z_i=1} - f_{y, D_i=1 \mid Z_i=0}) \cdot 1_{(a-b>0)} dy)}{(\int (f_{y,D_i=1 \mid Z_i=1} - f_{y,D_i=1 \mid Z_i=0}) \cdot 1_{(a-b>0)} dy)}$$

$$Where\ a - b = f_{y, D_i=1 \mid Z_i=1} - f_{y, D_i=1 \mid Z_i=0}$$

$$E[Y_i(0) \mid Z_i = 0] = \frac{(\int y \cdot (f_{y, D_i=0 \mid Z_i=0} - f_{y, D_i=0 \mid Z_i=1}) \cdot 1_{(c-d>0)} dy)}{(\int (f_{y,D_i=0 \mid Z_i=0} - f_{y,D_i=0 \mid Z_i=1}) \cdot 1_{(c-d>0)} dy)}$$

$$Where\ c - d = f_{y, D_i=0 \mid Z_i=0} - f_{y, D_i=0 \mid Z_i=1}$$

The equations above represent the conditional expectations $E[Y_i(1) \mid Z_i = 1]$ and $E[Y_i(0) \mid Z_i = 0]$ used to identify the expected outcomes under treatment and control conditions, given different levels of the instrument Z. The first equation calculates $E[Y_i(1) \mid Z_i = 1]$, the expected outcome when the individual is treated due to the instrument. This expectation is determined by integrating the outcome y weighted by the difference in conditional densities $f_{y, D_i=1 \mid Z_i=1} - f_{y, D_i=1 \mid Z_i=0}$, with the indicator function $1(a - b > 0)$ ensuring that only positive density differences contribute to the integral. Here, $a - b = f_{y, D_i=1 \mid Z_i=1} - f_{y, D_i=1 \mid Z_i=0}$ captures the instrument's impact on the outcome distribution for treated individuals.

Similarly, the second equation estimates $E[Y_i(0) \mid Z_i = 0]$, the expected outcome when the individual is not treated, by integrating y weighted by the density difference $f_{y, D_i=0 \mid Z_i=0} - f_{y, D_i=0 \mid Z_i=1}$, and conditioned by the indicator function $1(c - d > 0)$, where $c - d = f_{y, D_i=0 \mid Z_i=0} - f_{y, D_i=0 \mid Z_i=1}$. These equations allow for isolating the treatment effect by conditioning on the instrument's impact on the outcome distribution specific to compliers.

The indicator functions $1(a - b > 0)$ and $1(c - d > 0)$ restricts numerators and denominators for both equations of expected values. This ensures that only cases where the density for $Z_i = 1$ exceeds that for $Z_i = 0$ are considered when estimating $E[Y_i(1) \mid Z_i = 1]$. In the case of estimating $E[Y_i(0) \mid Z_i = 0]$, the indicator function ensures that only cases where the density for $Z_i = 0$ exceeds that for $Z_i = 1$ are considered.

4.2 Transformation of the equation

Under property conditions, the LATE formula could transform to the case of containing a control variable by adding X_i into the given LATE equation:

$$LATE = E[Y_i(1)|Z_i = 1, X_i] - E[Y_i(0)|Z_i = 0, X_i]$$

We need to adapt the identification conditions by introducing the X condition. The relaxed monotonicity assumption is implemented mathematically through the indicator functions $1(a - b > 0)$ and $1(c - d > 0)$ in the equation of $E[Y_i(1) \mid Z_i = 1]$ and $E[Y_i(0) \mid Z_i = 0]$ respectively.

Specifically, we adjust the density difference between a and b by conditioning on X in the indicator function$1(a - b > 0)$, where $a - b = f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}$. For the indicator function of $1(c - d > 0)$, $c - d$ is defined as $f_{y, D_i=0 \mid Z_i=0, X_i} - f_{y, D_i=0 \mid Z_i=1, X_i}$.

The type independence would be adapted by incorporating X into the conditioning, as follows:

$$Y_i(1), Y_i(0) \perp Z_i \mid D_i(1), D_i(0), X_i$$

We separately calculated the $E[Y_i(1)|Z_i = 1, X_i]$ and $E[Y_i(0)|Z_i = 0, X_i]$. We will begin by deriving $E[Y_i(1)|Z_i = 1, X_i]$, building upon the formula for $E[Y_i(1)|Z_i = 1]$ without a control variable, as outlined in Section 4.1, while incorporating the adapted identification conditions.

$$E[Y_i(1) \mid Z_i = 1, X_i] = \frac{\left(\int y \cdot (f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}) \cdot 1_{(a-b>0)} dy\right)}{\left(\int (f_{y, D_i=1 \mid Z_i=1,\, X_i} - f_{y, D_i=1 \mid Z_i=0,\, X_i}) \cdot 1_{(a-b>0)} dy\right)}$$

$$Where\ a - b = f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}$$

We transform the equation of $E[Y_i(1)|Z_i = 1]$ by adding the control variable into the given conditions of all four density functions. The indicator function $1(a - b >$

0) is also conditioned on X, specifically $f_{y,D_i=1|Z_i=1,X_i} - f_{y,D_i=1|Z_i=0,X_i}$. In this case, the indicator functions ensure that only cases where the density for $Z_i = 0$ conditioning on X_i exceeds that for $Z_i = 1$ are considered.

By deriving $E[Y_i(1) \mid Z_i = 1, X_i]$ formula further, we break down the two densities inside the formula as such:

$$f_{y,\,D_i=1\,|\,Z_i=1,\,X_i} = f_{y|D_i=1,Z_i=1,X_i} * Pr[D_i = 1\,|\,Z_i = 1,\, X_i]$$
$$f_{y,\,D_i=1\,|\,Z_i=0,\,X_i} = f_{y|D_i=1,Z_i=0,X_i} * Pr[D_i = 1\,|\,Z_i = 0,\, X_i]$$

Estimating the conditional density $f_{y,\,D_i=1\,|\,Z_i=1,\,X_i}$ and $f_{y,\,D_i=1\,|\,Z_i=0,\,X_i}$ directly is complex because it combines the outcome distribution and the treatment assignment in a single function. By breaking it down, we could use separate methods (like conditional density estimation for ($f_{y|D_i=1,Z_i=1,X_i}$ *and* $f_{y|D_i=1,Z_i=0,X_i}$) and conditional probability models for ($Pr[D_i = 1\,|\,Z_i = 1,\, X_i]$ and $Pr[D_i = 1\,|\,Z_i = 0,\, X_i]$) to estimate each part more efficiently and accurately.

As a result, we could divide the calculation of $E[Y_i(1) \mid Z_i = 1, X_i]$ into 3 steps:

1. Estimating complier densities ($f_{y|D_i=1,Z_i=1,X_i}$ and $f_{y|D_i=1,Z_i=0,X_i}$) through CDE (Conditional Density Estimation)
2. Conditional probability estimation ($Pr[D_i = 1\,|\,Z_i = 1,\, X_i]$ and $Pr[D_i = 1\,|\,Z_i = 0,\, X_i]$)
3. Filtering the difference between the estimated conditional densities under our relaxed monotonicity assumption, $1(a - b > 0)$

We apply the exact derivation and calculation procedure to $E[Y_i(0)|Z_i = 0, X_i]$ by replacing the situation where $D_i = 1, Z_i = 1$ *and* $D_i = 1, Z_i = 0$ to $D_i = 0, Z_i = 0$ *and* $D_i = 0, Z_i = 1$ respectively.

$$E[Y_i(0) \mid Z_i = 0, X_i] = \frac{(\int y \cdot (f_{y,\,D_i=0\,|\,Z_i=0,\,X_i} - f_{y,\,D_i=0\,|Z_i=1,\,X_i}) \cdot 1_{(c-d>0)} dy)}{(\int (f_{y,D_i=0\,|\,Z_i=0,\,X_i} - f_{y,D_i=0\,|\,Z_i=1,\,X_i}) \cdot 1_{(c-d>0)} dy)}$$

$$Where\ c - d = f_{y,\,D_i=0\,|\,Z_i=0,\,X_i} - f_{y,\,D_i=0\,|\,Z_i=1,\,X_i}$$

Further,

$$f_{y,\,D_i=0\,|\,Z_i=0,\,X_i} = f_{y|D_i=0,Z_i=0,X_i} * Pr[D_i = 0\,|\,Z_i = 0,\, X_i]$$

$$f_{y,\,D_i=0\,|\,Z_i=1,\,X_i} = f_{y|D_i=0,Z_i=1,X_i} * Pr[D_i = 0\,|\,Z_i = 1,\, X_i]$$

4.3 Performing Conditional Density Estimation (CDE):

When the outcome variable Y is continuous, we use CDE to estimate the densities non-parametrically. CDE is particularly suitable for estimating the density of a variable Y, conditional on specific values of another variable X, using the “np” package in R, which allows for flexible modelling without imposing strict parametric assumptions. This approach is beneficial when the relationships between variables, such as the interaction between IQ/KWW and wages, are complex or nonlinear. The CDE technique facilitates a refined estimation of the LATE across various values of X, thereby revealing how factors such as cognitive ability (IQ) and practical skills (KWW) influence the effect of college education on wages.

The following density functions will be estimated using CDE:

$$f_{y|D_i=1,Z_i=1,X_i},\ f_{y|D_i=1,Z_i=0,X_i},\ f_{y|D_i=0,Z_i=0,X_i}\ and\ f_{y|D_i=0,Z_i=1,X_i}$$

We begin by estimating $f_{y|D_i=1,Z_i=1,X_i}$. The data is grouped by setting the condition $D_i = 1$ and $Z_i = 1$. Next, we extract all values of X and Y from this subset of the data, which will be used for the CDE procedure.

During the CDE process, we define the range of values over which the estimation will be performed. The range for the outcome variable Y spans from 0.35 to 12.03, with 1000 intervals within this range. The control variable X ranges from 0.9 to 1.85, divided into 95 grids. The bandwidth parameters for both X and Y are set to 1.059 to smooth the density estimation. These values ensure a balanced approach to smoothing while capturing meaningful differences across the range of X.

We carefully choose the grid points and range of values for Y and X to focus on regions with sufficient data for accurate density estimation. Specifying the range and number of grid points helps control the estimate’s resolution, affecting the detail and smoothness of the resulting density estimates. Setting an appropriate range for Y

ensures that we cover a wide range of possible wage values. Similarly, we use grid points spaced 0.01 apart within the specified X range to capture meaningful changes in the density estimation.

For bandwidth selection, we use 1.059 as a data-driven choice based on Scott's Rule of Thumb, which balances under-smoothing and over-smoothing. This choice ensures that density estimates remain reliable without excessive noise. The estimation generates 95×1000 density estimates, where Y is analyzed for 95 different X_i values, each corresponding to 1000 different Y_i values. We replicate this process for other combinations of conditions: $D_i = 1, Z_i = 1\ to\ D_i = 1, Z_i = 0,\ D_i = 0, Z_i = 0\ and\ D_i = 0, Z_i = 1$, estimating $f_{y|D_i=1,Z_i=0,X_i}$, $f_{y|D_i=0,Z_i=0,X_i}$ and $f_{y|D_i=0,Z_i=1,X_i}$ respectively.

However, CDE faces limitations, particularly in high-dimensional settings. Data sparsity occurs when few observations fall within specific regions, making it difficult to estimate the density reliably. For example, when applying CDE to subsets of data (e.g., $Z_i = 1$ and $D_i = 1$), the number of observations may decrease significantly after filtering, from 3610 to 595. If smaller bandwidths are used, they may result in many NAs at the extremes of X, necessitating larger bandwidths to maintain sufficient observations. Additionally, spreading data across a high number of bins (95,000 grid points) exacerbates sparsity, potentially leading to overfitting as the model tries to estimate densities in regions with insufficient data.

4.4 Conditional Probability Estimation

We need to estimate four conditional probabilities based on Section 4.2, including: $Pr[D_i = 1 \mid Z_i = 1,\ X_i]$, $Pr[D_i = 1 \mid Z_i = 0,\ X_i]$, $Pr[D_i = 0 \mid Z_i = 0,\ X_i]$ and $Pr[D_i = 0 \mid Z_i = 1,\ X_i]$.

We divide the X into 95 grids for the CDE, resulting in 95 distinct probabilities for each of the four conditional probabilities listed. The bandwidth for these estimations is set at 0.2. Initially, we group the dataset based on $Z_i = 1$. Then, we filter and count the observations that fall within the range of $X_i \pm 0.2$ to get $\Pr[Z_i = 1, Xi]$.

Bandwidth values of 0.01, 0.05, 0.1, 0.15, and 0.2 were tested, with 0.2 being the only one that ensured observations at the extreme value of X. Smaller bandwidths resulted in missing values (NAs) due to no observations within the bandwidth.

We calculate $\Pr[D_i = 1 \mid Z_i = 1, X_i]$ by counting the number of college attendees (where $D_i = 1$) among those born near a college ($Z_i = 1$) with a given value of X_i and dividing this by the total number of individuals in the $Z_i = 1$ *and* X_i group:

$$\Pr[D_i = 1 \mid Z_i = 1, X_i] = \frac{\Pr[D_i = 1, Z_i = 1, X_i]}{\Pr[Z_i = 1, Xi]}$$

This calculation is performed for each of the 95 distinct values of X_i. The exact procedure applies to the other probabilities: $\Pr[D_i = 1 \mid Z_i = 0, X_i]$, $\Pr[D_i = 0 \mid Z_i = 0, X_i]$ and $\Pr[D_i = 0 \mid Z_i = 0, X_i]$.

4.5 Filtering applicable estimations under relaxed assumptions:

Not all estimated conditional complier densities can be directly applicable. If an estimated density is less than 0, based on our relaxed assumption, it is adjusted back to 0 by adding the minimal value of the defiers. For $E[Y_i(1)|Z_i = 1, Xi]$, we verify the estimated densities using the indicator function:

$$1(a - b) > 0,\ where\ a - b = f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}$$

Similarly, for $E[Y_i(0)|Z_i = 0, Xi]$, the indicator function is

$$1(c - d) > 0,\ where\ c - d = f_{y,\, D_i=0 \mid Z_i=0,\, X_i} - f_{y,\, D_i=0 \mid Z_i=1,\, X_i}$$

Each X_i corresponds to 1000 different Y_i values. We calculate 1,000 individual differences between the estimated densities associated with 1,000 different y values for each individual X_i value. We iterate this for 95 different X_i values, expecting both positive and negative results. We systematically check whether each individual difference is less than 0. If it is, we adjust it back to 0 in accordance with our relaxed assumption. The same procedure applies for the $1(c - d > 0)$ case.

After completing all three steps of obtaining all numerical elements, we calculate $E[Y_i(1) \mid Z_i = 1, X_i]$ and $E[Y_i(0) \mid Z_i = 0, X_i]$ according to these 2 equations from section 4.2:

$$E[Y_i(1) \mid Z_i = 1, X_i] = \frac{(\int y \cdot (f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}) \cdot 1_{(a-b>0)} dy)}{(\int (f_{y, D_i=1 \mid Z_i=1,\, X_i} - f_{y, D_i=1 \mid Z_i=0,\, X_i}) \cdot 1_{(a-b>0)} dy)}$$

$$Where\ a - b = f_{y,\, D_i=1 \mid Z_i=1,\, X_i} - f_{y,\, D_i=1 \mid Z_i=0,\, X_i}$$

$$E[Y_i(0) \mid Z_i = 0, X_i] = \frac{(\int y \cdot (f_{y,\, D_i=0 \mid Z_i=0,\, X_i} - f_{y,\, D_i=0 \mid Z_i=1,\, X_i}) \cdot 1_{(c-d>0)} dy)}{(\int (f_{y, D_i=0 \mid Z_i=0,\, X_i} - f_{y, D_i=0 \mid Z_i=1,\, X_i}) \cdot 1_{(c-d>0)} dy)}$$

$$Where\ c - d = f_{y,\, D_i=0 \mid Z_i=0,\, X_i} - f_{y,\, D_i=0 \mid Z_i=1,\, X_i}$$

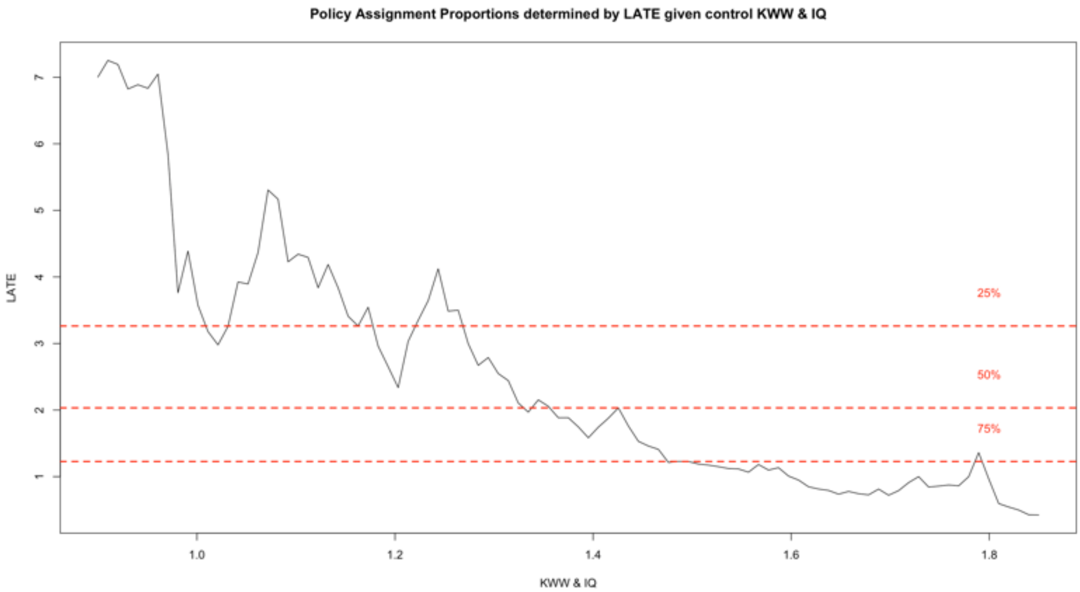
The final LATE result simply equals $E[Y_i(1) \mid Z_i = 1, X_i]$ - $E[Y_i(0) \mid Z_i = 0, X_i]$. There will be 95 different LATE values associated with 95 different X_i values. The results could be positive or negative. We would expect positive results that refers the treatment has a positive effect on the outcome for compliers, implying a higher outcome for those who received the treatment than those who did not.

5 Results

5.1 LATE results

The graph plots the LATE values across different combined KWW and IQ score levels. Analysing the curve's shape could reveal some key insights.

Figure 2: Variation of Local Average Treatment Effect (LATE) Across Levels of Combined KWW and IQ Scores with Policy Assignment Thresholds



In this graph, the x-axis represents our selected continuous control variable, which combines KWW and IQ scores. The y-axis shows the LATE values resulting from the college proximity intervention. The black line illustrates the LATE values across various levels of combined KWW and IQ scores.

As the combined KWW and IQ scores increase, the effect of the policy intervention, likely related to education, on the outcome diminishes. This suggests that individuals with higher cognitive abilities or knowledge benefit less from the intervention than those with lower scores.

While there are slight fluctuations, the overall trend is a decline in LATE as KWW and IQ increase. The curve shows that LATE is significantly high (above 6) when KWW and IQ scores are lower, around 1.0. Most values are clustered between 1 and 3, with few exceeding 3. We suggest that policymakers focus on reducing geographic barriers to education for individuals with lower KWW and IQ scores as they enhance their opportunities for higher wages.

Since resources in our economies are scarce, they should be allocated to those who benefit the most, which are the individuals with lower KWW and IQ scores. Most of the time, the government's resources can only provide subsidies to a limited percentage of the population. Hence, we add three red lines in the graph that show the distribution of LATE values across the population, highlighting the top 25%, 50%, and 75% of the population with the highest LATE values.

The 25% line (LATE ≈ 3.26) indicates that the top 25% of the population, in terms of LATE values, has values above 3. If the government subsidy can only be allocated to 25% of the population, the most efficient allocation would be to individuals with KWW&IQ combined scores below 1.02, as they achieve the highest LATE. Focusing on these individuals would maximize the impact of the intervention, but due to the fluctuations, some parts may be allocated to higher values.

50% line (LATE ≈ 2.03) represents the median LATE value, meaning that 50% of the population has LATE values above 2. If the subsidy can be distributed to 50% of the population, the next target group would be individuals with KWW&IQ combined scores below 1.33. These individuals still benefit significantly.

75% line (LATE ≈ 1.23) suggests that 75% of the population has LATE values above 1.2. If considering subsidizing 75% of the population, individuals with KWW&IQ combined scores below 1.47 should be included. However, a policy targeting would be wide-reaching, though the marginal benefit would be smaller.

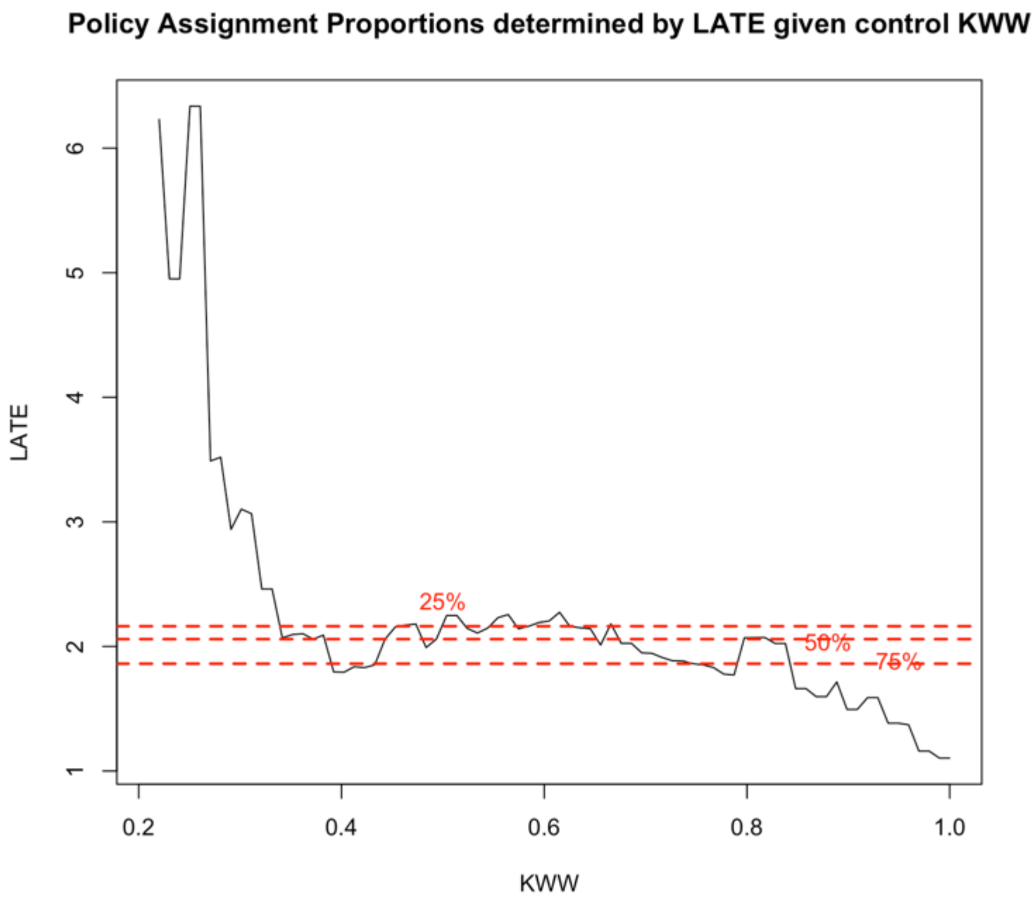
The distribution of KWW & IQ shown in Section 2 aligns closely with the LATE graph, revealing how the clustering of individuals with moderate KWW and IQ scores (between 1.0 and 1.5) influences the LATE outcomes. The density plot highlights that most of the population is concentrated within this score range. Consequently, where combined KWW and IQ scores fall between 1.0 and 1.5, the LATE estimates are the most reliable and precise, corresponding to values between approximately 3.25 and 1.

This alignment underscores that the concentration of observations in this range enhances the accuracy of LATE estimates, making policy recommendations more relevant for individuals in this middle score range.

5.2 Robustness

The following two graphs depict the LATE values across different levels of KWW and IQ scores. These plots give us insight into how the treatment effect varies across the population based on their KWW and IQ levels.

Figure 3: Variation of Local Average Treatment Effect (LATE) Across Levels of KWW Scores with Policy Assignment Thresholds



Generally, the graph using the KWW score as the control variable shows a similar trend and implications as when the control variable is the combined KWW and IQ score. The black line represents the LATE values as KWW increases. Initially, the LATE is relatively high, starting above 6 for individuals with very low KWW values around 0.2. As KWW increases, the LATE rapidly declines. Around a KWW value of 0.4, the LATE dips below 3 and fluctuates slightly, but the overall trend is downward. As KWW reaches the range of 0.5 to 1.0, LATE values generally stabilize around 2 or lower. This suggests that the intervention of proximity to college has a more significant impact on individuals with lower KWW scores, and the impact diminishes as KWW increases.

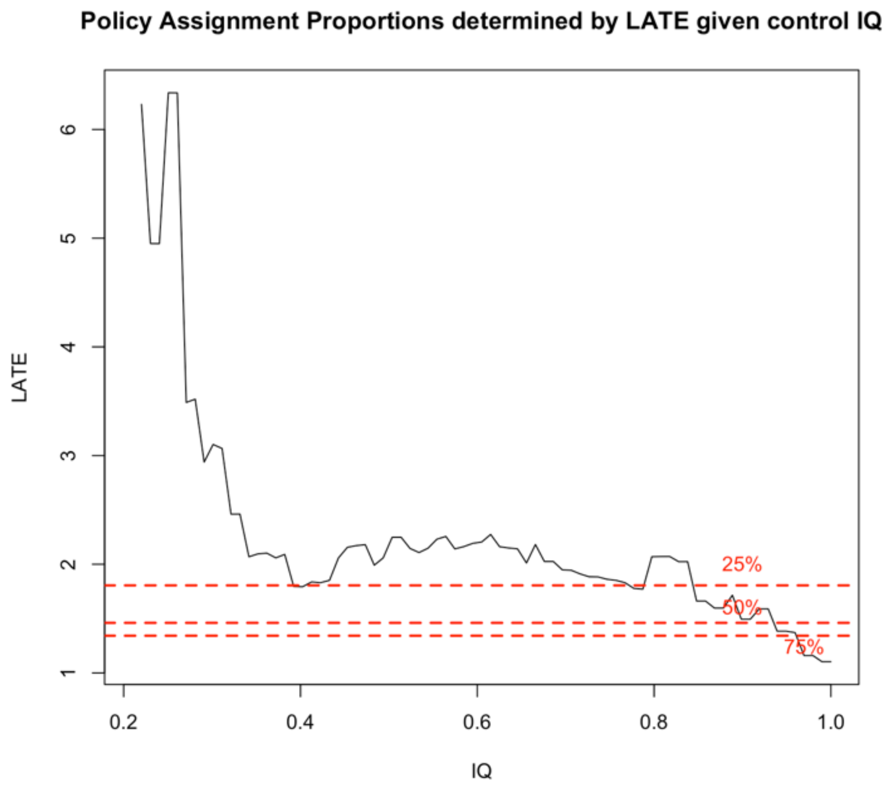
The 25% line shows that the top 25% of the population, in terms of LATE values, have values above 2.01, suggesting that targeting individuals with KWW scores below 0.36 would maximize the intervention's impact. The 50% line indicates a median LATE of approximately 1.63, so expanding to cover individuals with KWW scores up to around 0.66 could still yield substantial benefits. At the 75% line, with a LATE threshold of 1.45, covering up to KWW scores near 0.83 would allow a broader reach, though with a reduced marginal impact, offering a balance between inclusivity and benefit per capita.

When performing CDE using KWW as the variable X, an error specifically affected the LATE calculation for $E[Y_i(0) \mid Z_i = 0, X_i]$. Although the results for $E[Y_i(1) \mid Z_i = 1, X_i]$ were computed successfully, $E[Y_i(0) \mid Z_i = 0, X_i]$ returned an NA (Not Available) value, even with the same X values. This issue is known as the boundary or edge issue.

Boundary effects occur when density estimation approaches the extremes of the dataset. At these boundaries—particularly at the lower or upper ends of X (KWW) or Y (wages)—the CDE estimator encounters fewer data points to base its calculations on. The lack of nearby data points can lead to unreliable or even missing values, as seen in the $E[Y_i(0) \mid Z_i = 0, X_i]$ calculations. In our case, the LATE results for KWW values between 0.08 and 0.2 yielded NA values. At these lower boundary points, the estimator lacks sufficient neighbouring data, resulting in inaccurate density estimates for $E[Y_i(0) \mid Z_i = 0, X_i]$.

We discarded data points associated with X values between 0.08 and 0.2 from our analysis to address this issue. This adjustment prevents our LATE results from being skewed by unreliable boundary estimates, thus enhancing the robustness of our findings. Removing these problematic data points eliminates instances where the CDE process returned NA due to edge effects, allowing us to proceed with a more reliable and accurate LATE evaluation.

Figure 4: Variation of Local Average Treatment Effect (LATE) Across Levels of IQ Scores with Policy Assignment Thresholds



The second graph illustrates the LATE values across different levels of IQ. Like the KWW graph, the LATE value starts high, above 6, for individuals with low IQ scores (around 0.2). As IQ increases, the LATE rapidly decreases. Around an IQ value of 0.4, LATE falls below 3 and gradually declines.

After IQ reaches 0.6 to 1.0, the LATE values stabilize around 2 or lower, suggesting that individuals with higher IQ scores experience a much smaller treatment effect. However, the drop in LATE is slightly less volatile than the KWW graph.

As IQ increases, the LATE values show a consistent decrease, meaning that the treatment significantly impacts on those with lower IQ scores. In this graph, the 25% line indicates that the top 25% of the population, ranked by LATE values, have LATE values exceeding 2.05, with most falling within IQ scores below 0.65. This suggests that focusing policy interventions on individuals with IQ scores below 0.65 would maximize impact.

At the 50% line, the median LATE is approximately 1.72, meaning that half the population has LATE values above this threshold; expanding the intervention to include individuals with IQ scores up to about 0.75 would effectively cover this group.

Finally, the 75% line at a LATE threshold of 1.54 suggests that extending the intervention to cover individuals with IQ scores below 0.85 would increase reach but may reduce per-capita benefits, offering a trade-off between breadth and effectiveness.

Overall, LATE follows a similar decreasing trend as the control variables increase in all three graphs (combined KWW & IQ, KWW, and IQ). LATE is consistently high for low values of the control variables, whether IQ, KWW, or their combination, and stabilizes at a low level for higher values.

Although there are many differences among the three graphs, the range of control variables differs across them. The combined KWW and IQ graph has a broader range (from around 1.0 to 1.8) compared to KWW and IQ, which range from 0.2 to 1.0. This broader range in the combined graph would explain why the peak LATE values are slightly lower.

Additionally, the behaviour of the three curves differs; the combined KWW and IQ graph shows more volatility in the middle range (around 1.2–1.4), while the IQ and KWW graphs exhibit a smoother decline. The steepness of the decline also varies, with the KWW and IQ graphs showing sharper drops in LATE values compared to the more gradual decline in the combined graph.

The fluctuations in the combined KWW and IQ graph suggest that combining these variables introduces more variability in treatment response, likely due to the interaction between KWW and IQ. This added complexity results in greater heterogeneity in how individuals respond to the treatment, explaining the volatility in the middle range.

If the government can only assess one of these two variables as the indicator for policy intervention, evaluating which variable, IQ or KWW, yields the highest returns would be critical. When determining the more effective variable, we should consider the magnitude of LATE at lower values, the rate of decline, and the overall range and spread of LATE across the population. Both IQ and KWW capture high treatment effects for individuals with low scores (0.2–0.4). However, the decline in LATE for IQ is more gradual than for KWW, which stabilizes at lower values more quickly. This means that IQ provides a broader range of moderately high LATE values across a more significant portion of the population. At the same time, KWW is effective mainly for individuals with very low scores.

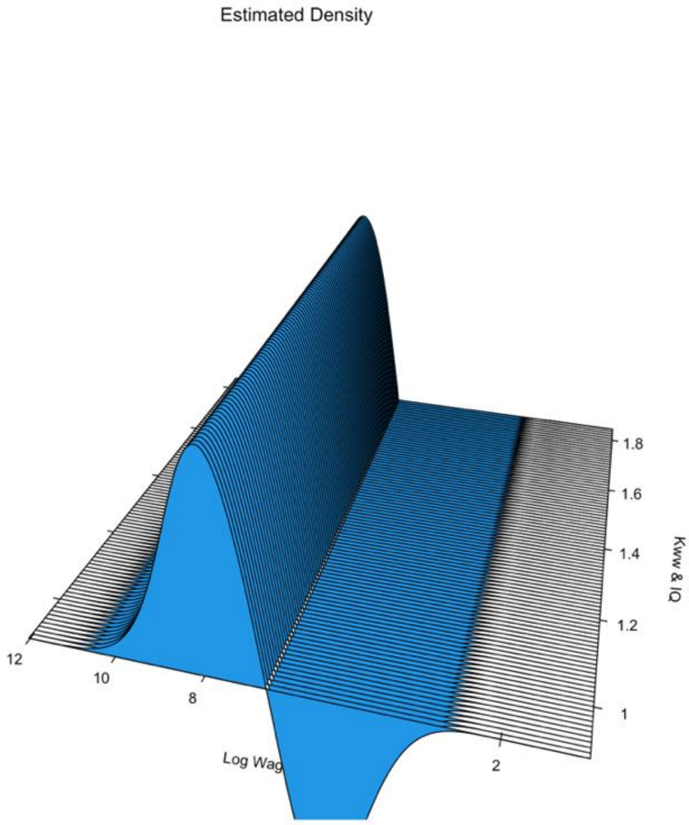
5.3 CDE results

The following graphs display the CDE results, illustrating differences between conditional densities ($a - b$ and $c - d$). On each graph, the x-axis represents the log of wages, while the y-axis shows KWW and IQ scores. The surface height visually indicates the estimated density of individuals possessing these characteristics.

In estimating LATE, these visualizations help differentiate individuals' estimated densities, determining if they meet the criteria set by the indicator function. The indicator function specifies that if the estimated density of compliers falls below zero, it must be adjusted to zero by adding the minimal value of the defier group. This adjustment ensures that the complier density remains nonnegative and symmetrically applies consistently across each individual estimation.

The surface plot highlights where density estimates are concentrated and where they dip or fall below zero. In this case, the negative regions indicate areas of the estimated densities where certain combinations of control variables (KWW & IQ) and log wages are less than zero. These estimates are adjusted to zero in these regions, concentrating our LATE analysis on the positive density values.

Figure 5: Comparison of Estimated Conditional Density of Log Wages Conditioned on KWW & IQ Scores Between Groups A and B

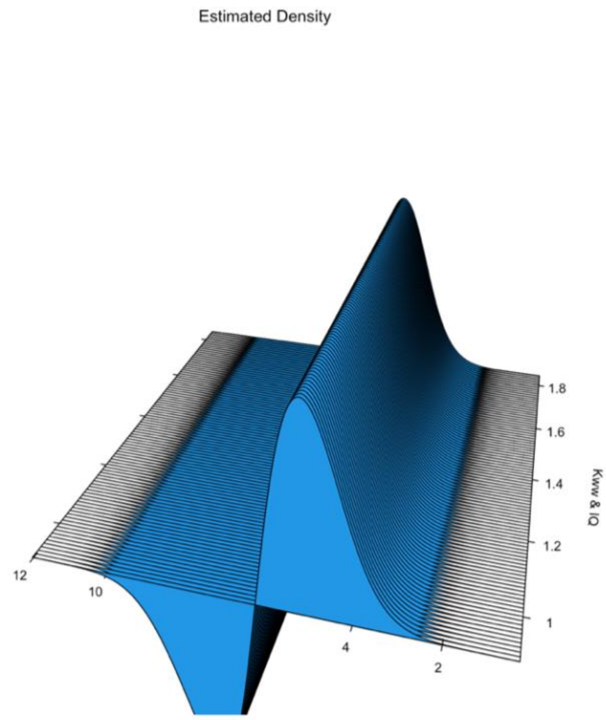


The first graph depicts the indicator function's results for estimating $E[Y_i(1)|Z_i = 1, X_i]$, focusing on differences in conditional densities between individuals who attended college based on their assignment to college proximity. Specifically, it compares those who attended college when assigned college proximity (a) with those who attended college without such an assignment (b), using KWW &

IQ as a control variable. The overall trend shows that the density begins in the negative region and decreases as wage outcomes increase. It then shifts into the positive region at a wage level of around 7, rising steadily and peaking near a wage outcome of 8, before rapidly declining back to zero.

A notable feature is the occurrence of negative regions for log wages below approximately 7, which covers a considerable segment of the lower wage range. The monotonicity assumption suggests that individuals born near a college are more likely to attend college and earn higher wages. However, negative density values at low wages suggest a breakdown in this relationship for some individuals. This might indicate groups who, despite attending college, end up with lower wages or defiers who do not align with the expected trend. Consequently, while LATE estimates for $E[Y_i(1) \mid Z_i = 1, X_i]$ largely reflect higher wage-driven effects, these insights underscore the heterogeneity in the treatment effects across different wage ranges.

Figure 6: Comparison of Estimated Conditional Density of Log Wages Conditioned on KWW & IQ Scores Between Groups C and D



The second graph illustrates the indicator function results for estimating $E[Y_i(0) \mid Z_i = 0, X_i]$, focusing on the differences in conditional densities $(c - d)$ for individuals based on their assignment to college proximity. Specifically, it compares the density variations between individuals who did not attend college and were not assigned to college proximity (c) and those who did not attend college but were assigned to college proximity (d). The general trend is that as wage outcomes increase, the density increases, peaking at wage outcomes of been 5, then rapidly decreases down to 0 when the wage outcomes around 7, further dropping into the negative region.

The negative region appears to occur when log wages are above 7. This region covers a significant portion of the higher range of log wages. The lower wage outcomes will mainly drive the LATE estimation for $E[Y_i(0) \mid Z_i = 0, X_i]$. Similarly, with the application to the negative estimated density, the negative estimates need to be adjusted back to 0, following our relaxed monotonicity assumption.

As wage outcomes increase, the density also rises, peaking around log wages of 5, and then quickly decreases to zero when wages reach approximately 7, eventually dropping into negative values regardless of KWW and IQ scores. The negative region primarily appears at log wages above 7, encompassing a substantial part of the higher wage spectrum. This implies that, for $E[Y_i(0) \mid Z_i = 0, X_i]$, lower wage outcomes will predominantly influence LATE estimation.

The occurrence of negative densities around these higher wage values suggests a violation of the traditional monotonicity assumption, which typically presumes that the treatment effect remains positive across the entire population. The relaxation of the traditional monotonicity assumption in our approach accommodates these negative estimates, thereby capturing the complexities in the treatment effects that might be overlooked. We apply the exact procedure for $a - b$ to address this issue. We adjust these negative complier densities back to zero to ensure that the LATE estimation aligns with our relaxed monotonicity framework, allowing for more accurate and consistent evaluation across wage levels.

Our approach to LATE combines $E[Y_i(1) \mid Z_i = 1, X_i]$ and $E[Y_i(0) \mid Z_i = 0, X_i]$ estimations by calculating the difference between the two. Notably, the ending point of the positive region in the $c - d$ graph perfectly aligns with the starting point in the $a - b$ graph, connecting the positive portions of the two density estimations to form a complete complier density estimation with all positive density estimations. This alignment allows us to evaluate the treatment effect consistently across all wage values without gaps, ensuring a comprehensive analysis.

In summary, the negative regions in both graphs indicate where the assumption of traditional monotonicity is violated. Negative estimates contradict the assumption that the treatment effect should be uniformly non-negative across the population. It would lead to contradictory treatment effects, such as negative treatment effects in certain subpopulations. This would undermine the overall analysis if we did not replace it with our relaxed monotonicity assumption. Adjusting the negative estimates back to 0 allows us to focus on regions where the treatment effect (LATE) is well-defined and follows the expected direction (non-negative). This step is crucial for providing a reliable measure of the treatment effect that can be interpreted consistently across the population and ensuring that policy conclusions drawn from the LATE estimates are accurate.

Traditional monotonicity is violated, possibly due to the nonlinear relationship between attending college and wages or having more complex dynamics that are not captured by the monotonicity assumption. For instance, attending college might increase wages to a certain point, after which the returns diminish or even reverse. This could be the case for highly skilled individuals, where further education adds little value to their already established earning potential. Such nonlinearities could violate the assumption that attending college always increases wages for everyone, leading to negative estimates in certain regions.

The intuition behind this violation is that the empirical relationship between college attendance, proximity to college, and wages may be more complex than initially assumed. It suggests heterogeneity in the population—some individuals might follow the expected pattern, while others do not. This leads to negative density estimates in certain wage regions as the model struggles to fit the data where these exceptions to the rule occur. Adjusting these negative density estimates helps to ensure that the analysis focuses only on the regions where the empirical model behaves as expected. However, it also highlights limitations in the traditional monotonicity assumption about the population's behaviour.

5.4 Policy advises:

Our results conclude that individuals with low combined KWW and IQ scores, who would benefit most from government assistance in reducing geographic barriers to education, have higher opportunities for increased wages.

The government could implement relocation assistance programs in to address the limited access to college for individuals with lower KWW and IQ scores. For instance, providing housing subsidies or moving grants would be applicable. These would help students live closer to colleges, reducing the logistical and financial barriers associated with relocation. Further, these subsidies would increase the likelihood of college attendance for students who would benefit the most. The government could prioritize programs for students with lower KWW and IQ scores by making these programs available only to those whose scores fall below a specified threshold.

Additionally, expanding college access in underserved or remote areas is crucial to improving proximity for populations traditionally marginalized from higher education. Establishing satellite campuses or partnerships with community colleges could bridge the geographic divide. This would make college education accessible for those who would otherwise be isolated by distance. This expanded access would provide low KWW and IQ individuals with more opportunities, thus supporting their educational attainment and aligning to reduce educational inequality.

5.5 Further research:

In examining the limitations of our study, one notable shortcoming is the potential correlation between the instrumental variable and parental income. The intuition behind this is that higher parental income might significantly influence residential choices, potentially affecting proximity to colleges. This correlation raises questions about the independence of the instrument, as family income could indirectly influence educational opportunities through location choice, thereby challenging the assumption of instrument independence in this context.

Our research also highlights the importance of considering heterogeneous treatment effects, particularly regarding cognitive ability and knowledge. However, future research could broaden this focus to include additional demographic factors, such as family background, gender, or geographic factors, which might also influence returns to education. By incorporating more continuous variables, future studies could achieve a more comprehensive understanding of how educational subsidies impact different groups and help identify those who benefit most from such interventions.

Another observation pertains to diminishing returns in treatment effects. For individuals with higher IQ or KWW scores, the impact of the intervention appears smaller, suggesting that as cognitive ability increases, the marginal benefit from additional education decreases. This finding implies that alternative interventions could be explored for high-ability individuals, as the effectiveness of proximity to college as an instrumental variable may not yield substantial gains for this subgroup.

While our study primarily focuses on educational policy, our methodology has broader applications in other policy areas. For instance, the same instrumental variable approach could be adapted to assess the impact of healthcare access or social welfare support, where continuous control variables, such as health status or income, are relevant. Such an extension could enable policymakers to evaluate effectiveness in various contexts, using similar IV models tailored to different public policy challenges.

6 Conclusion

The thesis develops a novel methodology for estimating LATE by including KWW and IQ scores as continuous control variables in the model while accounting for the defier. I used college proximity as an instrumental variable. I applied this approach to examine the wage impact of college attendance, finding that individuals with lower KWW and IQ scores gain the most in terms of wages. The findings suggest that policies like educational subsidies could be more effective if directed toward students with lower cognitive and job-related skills. Additionally, allowing for defiers enhances the robustness and applicability of LATE estimates across diverse empirical settings. Future research could expand this framework to include other demographic factors, such as family background and gender, which may also affect returns to education.

References

Angrist, J. D., & Keueger, A. B. (1991). Does Compulsory School Attendance Affect Schooling and Earnings? *The Quarterly Journal of Economics*, *106*(4), 979–1014. https://doi.org/10.2307/2937954

Angrist, J.D., Imbens, G.W. and Rubin, D.B. (1996). Identification of Causal Effects Using Instrumental Variables. *Journal of the American Statistical Association*, 91(434), pp.444–455. doi:https://doi.org/10.1080/01621459.1996.10476902.

Card, D. (1993, October 1). Using Geographic Variation in College Proximity to Estimate the Return to Schooling. Retrieved from www.nber.org website: https://www.nber.org/papers/w4483

Card, D. (1999). The Causal Effect of Education on Earnings. *Handbook of Labor Economics*, *3*, 1801–1863. https://doi.org/10.1016/s1573-4463(99)03011-4

Dahl, C. M., Huber, M., & Mellace, G. (2023). It is never too LATE: a new look at local average treatment effects with or without defiers. *Econometrics Journal/the Econometrics Journal Online*, *26*(3), 378–404. https://doi.org/10.1093/ectj/utad013

Imbens, G. W., & Angrist, J. D. (1994). Identification and Estimation of Local Average Treatment Effects. *Econometrica*, *62*(2), 467–475. https://doi.org/10.2307/2951620

Kitagawa, T. (2015). A Test for Instrument Validity. *Econometrica*, *83*(5), 2043–2063. https://doi.org/10.3982/ecta11974

Kitagawa, T., & Tetenov, A. (2018). Who Should Be Treated? Empirical Welfare Maximization Methods for Treatment Choice. *Econometrica*, *86*(2), 591–616. https://doi.org/10.3982/ecta13288

Liao, M. (2022, March 10). Estimating Economic Models with Testable Assumptions: Theory and Applications. https://doi.org/10.48550/arXiv.2002.10415

Mourifié, I., & Wan, Y. (2017). Testing Local Average Treatment Effect Assumptions. *The Review of Economics and Statistics*, *99*(2), 305–313. https://doi.org/10.1162/rest_a_00622

Wasserman, L. (2006). All of Nonparametric Statistics. In *Google Books*. Springer Science & Business Media. Retrieved from https://books.google.com.au/books?hl=en&lr=&id=MRFlzQfRg7UC&oi=fnd&pg=PA2&dq=asserman

ChatGPT. (2024). Chatgpt.com. https://chatgpt.com/c/672433d3-ed60-8013-ac97-4cbd10b714ea