Introduction to Econometrics II: Recitation 11

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1 Treatment Effects

1.1 Difference-in-Difference Estimation

This involves a specific framework where we can clearly define a 'before and after' - denoted as t_0 and t_1. For now, we assume that there are just these two periods. No one is treated at t_0 but there is a subset of people ($G_i = 1$) that are treated at t_1. Those in $G_i = 0$ are never treated in either time period. If we define a framework similar to our cross-sectional potential outcomes as

$$y_{i,t} = G_i y_i(1,t) + (1 - G_i) y_i(0,t) \;\; (t \in \{t_0, t_1\})$$

then we will be able to observe $(y_{i,t_1}, y_{i,t_0}, G_i, X_i)$ for every unit i. What we do not observe is $y_i(1,t_1)$ for those in $G_i = 0$ and $y_i(0,t_1)$ for those in $G_i = 1$, similar to the fundamental problem of missing data for our cross-sectional potential outcome framework. The analogue to the conditional independence assumption is a **parallel trend assumption**, defined as

$$(y_i(1,t_1) - y_i(1,t_0), y_i(0,t_1) - y_i(0,t_0)) \perp\!\!\!\perp G_i | X_i$$

Using the fact that no one is treated in t_0, we can skip G_i notation for t_0 and write $y_i(G_i, t_0) = y_i(t_0)$ for $G_i \in \{0,1\}$ and write the above condition as

$$(y_i(1,t_1) - y_i(t_0), y_i(0,t_1) - y_i(t_0)) \perp\!\!\!\perp G_i | X_i$$

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Both versions of the parallel trend assumption imply that the changes in treatment outcome for both groups do not depend on G_i once we condition for X_i. This would be violated if uncontrolled factors affect the changes in the outcome or if there are differences in the trends of $y_{i,t}$ across treated and control groups even before the experiment.

To see why they are equal, consider a setting where $D_i = G_i$ and write

$$\begin{aligned} y_i = y_{i,t_1} - y_{i,t_0} &= D_i(\underbrace{y_i(1,t_1) - y_i(1,t_0)}_{y_i(1)}) + (1-D_i)(\underbrace{y_i(0,t_1) - y_i(0,t_0)}_{y_i(0)}) \\ &= D_i y_i(1) + (1-D_i) y_i(0) \end{aligned}$$

Thus, we can write the parallel trend assumption as conditional independence assumption.

$$(y_i(1), y_i(0)) \perp\!\!\!\perp D_i | X_i$$

To apply this in regression, we can write

$$y_{it} = \beta_0(X_i) + \beta_1(X_i) \cdot \mathbb{1}[t = t_1] + \beta_2(X_i) \cdot G_i + \beta_3(X_i) \cdot G_i \cdot \mathbb{1}[t = t_1] + \epsilon_{it}$$

where X_i is a set of covariates, which can include a constant. In this context, the treatment effect for $X_i = x$ would be

$$\begin{aligned} TE(x) &= E[y_i(1) - y_i(0) | X_i = x] \\ &= E[(y_i(1,t_1) - y_i(1,t_0)) - (y_i(0,t_1) - y_i(0,t_0)) | X_i = x] \\ &= x \cdot E[\{(\beta_0 + \beta_1 + \beta_2 + \beta_3) - (\beta_0 + \beta_2)\} - \{(\beta_0 + \beta_1) - (\beta_0)\} | X_i = x] \\ &= x \cdot E[(\beta_1 + \beta_3) - \beta_1 | X_i = x] \\ &= \beta_3 x \end{aligned}$$

So β_3 would be our parameter of interest.

One difficulty arises from testing the parallel trends assumption. Loosely speaking, what we can do is to select a time period $\tilde{t} < t_0$ and find out if the difference $y_i(t_0) - y_i(\tilde{t})$ is independent with G_i. If not independent, we say that there is an pre-trend. This is why when difference-in-difference is presented, many researchers also plot the pre-treatment outcome. This does not perfectly test parallel trends, as plotting the coefficients does not perfectly take into account the influence of unobserved factors that may violate parallel trends. However, if there are noticeable trends pre-treatment, that is a sure sign of nonparallel trends.

1.2 Two-way fixed effects

For a setup with just two time periods, running a difference-in-difference is equivalent to

$$y_{it} = a_i + b_t + cD_{it} + e_{it}$$

where $D_{it} = 1$ if unit i is treated at period t and 0 if otherwise. We extend the simple difference-in-difference model by including more time periods and allowing absorbing treatment where individuals enter the treatment at different time and stay treated afterwards. In other words, if $D_{it} = 1$, then for $\tau > t$, $D_{i\tau} = 1$.

While the implementation is simple, there may be problems. There may exist a compositional effect where the overall trend in the data is reversed or attenuated compared to a trend that appears in the groups composing the data. The consequence is that in the case where there is a strong heterogeneity of treatment across groups or time, the coefficient of c, which is are treatment effect variable, may end up with the opposite sign of a true effect.

To see why this can happen, we need to understand that the estimate of c is a weighted average of various $i \times t$ cells[1]. Let G_i be the first period in which i is treated. If i is never treated, let $G_i = \infty$. By applying (two-step) within estimation, we can get

$$\hat{c} = \frac{\sum_{i.t} \tilde{y}_{it} \widetilde{D}_{it}}{\sum_{i.t} \widetilde{D}_{it}^2}$$

where $\tilde{y}_{it} = y_{it} - \bar{y}_{i\cdot} - \bar{y}_{\cdot t} + \bar{y}$ and similarly for $\widetilde{D}_{it}$. Other way to see this is to apply the Frisch-Wald-Lovell theorem: We get the same estimate by regressing y_{it} onto the residual of

$$D_{it} = a_i + b_i + u_{it}$$

In this way, we can write

$$\hat{c} = \frac{\sum_{i,t} y_{it} \hat{u}_{it}}{\sum_{i,t} \hat{u}_{it}^2} = \sum_{i,t} y_{it} \left(\frac{\hat{u}_{it}}{\sum_{i,t} \hat{u}_{it}^2} \right)$$

Thus, you can see that $\hat{c}$ is a weighted sum of the values of the outcome variable across various $i \times t$ cells. Theoretically, each $i \times t$ has a different weight and some observations end up getting *negative* weights. This happens since $\hat{u}_{it}$ is not necessarily binary, and those whose treatment intensity is below the mean gets negative weights. These are usually the

[1] Among many DiD papers, the most straightforward explanation comes from Goodman-Bacon (2021 Journal of Econometrics) and Jakiela (2021, WP)

early adopters in later years (high unit-level treatment mean and time-level treatment mean) that are practically treated as comparison group to the recently treated group.

To deal with this, the minimum requirement is to vary the coefficient c across different time by letting it differ by the implementation period, $c(G_i)$. This is usually not enough, however. The best approach is to compare the average treatment effect for those treated at a earlier period vs. those that are treated later. Specifically, for a specific $g \leq t$, and for any $g' > t$, write

$$E[y_{it}(g) - y_{it}(\infty)|G_i = g] = E[y_{it} - y_{i,g-1}|G_i = g] - E[y_{it} - y_{i,g-1}|G_i = g']$$

In this way, we are not making a wrongful comparison against the early-adopters. Also, it is nicer to have those who are never treated (pure control) since they always serve as a safe comparison group.

1.3 Violation of Conditional Independence Assumptions

There are cases where the assignment, even if we condition on observables, are not random. So in the regressional framework of $y_i(d) = \mu(X_i, d) + \epsilon_i(d)$, the error terms is not independent of $D_i|X_i$. The conditional independence assumption can be broken because:

- Participants self-select based on expected benefit: Think about a job training program for plumbing. Then, maybe those who are more healthy and suffer less in terms of costs are likely to join. If health is not perfectly observed, we risk breaking the conditional independence assumption
- Participants may be selected, consciously or not, to join: Think of the clinical trial where participation is voluntary. In such case, individuals who are more risk-loving are more likely to join. In other words, participants and nonparticipants systematically differ on risk-averseness - something that is not usually observed.
- There may be equilibrium effects: A tuition subsidy program that intends to increase the number of people entering college may have a spillover effect by increasing supply of college graduates at the labor market, leading to a decrease in college premium. This may induce students to enter college less. If TE is the (rate of) college entrance, such equilibrium effect may be influential.

Mathematically, what happens is that when we calculate $E[y_i|D_i = 1, x] - E[y_i|D_i = 0, x]$,

we end up with

$$\begin{aligned} E[y_i|D_i = 1, x] - E[y_i|D_i = 0.x] &= \mu(x,1) - \mu(x,0) + E[\epsilon_i(1)|D_i = 1, x] - E[\epsilon_i(0)|D_i = 0, x] \\ &= TE + \underbrace{E[\epsilon_i(1)|D_i = 1, x] - E[\epsilon_i(0)|D_i = 0, x]}_{\text{(Positive/Negative) selection bias}} \end{aligned}$$

The error term no longer can be erased from the equation since CIA assumption is not applicable. The difference between the error term is the **selection bias** that can be both negative and positive (Also appears in Angrist, Pischke 2009). This also means that the error terms $(\epsilon_i(1), \epsilon_i(0))$ and the u_i in $D_i = \mathbb{1}[u_i < p(x_i)]$ can covary. The result is that the treatment effect estimated from here can be inaccurate.

There are two possible solutions. Old method relies on **Heckman correction**. Recent focus is on IV to derive **marginal treatment effects** and **localized average treatment effect**.

1.4 A Brief Discussion of the Heckman Correction

Suppose that we are in the situation where we have a data generating process

$$y_i = \max\{X_i\beta + \sigma\eta_i, 0\}, \eta_i \sim N(0,1)$$

So we only see y_i if $X_i\beta + \sigma\eta_i > 0$. We are able to observe D_i, specified as

$$D_i = \begin{cases} 1 & \text{if } \eta > -\frac{X_i\beta}{\sigma} \\ 0 & \text{otherwise} \end{cases}$$

Then, for the observed sample, we are likely to have an η_i that is positively selected. So the idiosyncratic error is no longer random and we have a biased estimates, shown as (assuming X_i is observable and σ is known)

$$\begin{aligned} E[y_i|y_i > 0] &= X_i\beta + \sigma E[\eta_i|y_i > 0] \\ \implies E[\eta_i|y_i > 0] &= E\left[\eta_i|\eta_i > -\frac{X_i\beta}{\sigma}\right] = \int_{-X_i\beta/\sigma}^{\infty} t\phi(t|\eta_i > -X_i\beta/\sigma)dt \\ &= \int_{-X_i\beta/\sigma}^{\infty} t\frac{\phi(t)}{\Pr(\eta_i > -X_i\beta/\sigma)}dt = \frac{1}{1-\Phi(-X_i\beta/\sigma)}\int_{-X_i\beta/\sigma}^{\infty} t\frac{1}{\sqrt{2\pi}}e^{-t^2/2}dt \\ &= \frac{1}{1-\Phi(-X_i\beta/\sigma)}\left[\frac{1}{\sqrt{2\pi}}e^{-t^2/2}\right]_{-X_i\beta/\sigma}^{\infty} = \frac{\phi(-X_i\beta/\sigma)}{1-\Phi(-X_i\beta/\sigma)} \\ &= \frac{\phi(X_i\beta/\sigma)}{\Phi(X_i\beta/\sigma)} = m(-X_i\beta/\sigma) \neq 0 \end{aligned}$$

So the error has nonzero conditional mean. The ratio of the pdf over cdf $\frac{\phi(\cdot)}{\Phi(\cdot)}$ is the inverse Mill's ratio (IMR). Heckman's correction uses the IMR to correct for the bias. The challenge is to identify β/σ. We do this by

1. Use probit to regress D_i onto X_i. Then we can obtain $\widehat{\beta/\sigma}$.
2. Define $\hat{f}(x) = m(-X_i\widehat{\beta/\sigma})$. We include $\hat{f}(x)$ in the control variable and run

$$y_i = X_i\beta + \gamma\hat{f}(x) + \epsilon_i$$

   on the participant sample.

The problem is that we assume the normality of the error term and the linearity of the DGP, which is not always true. Thus, it is not always an ideal way to deal with selection on unobservables.

1.5 Instrumental variables approach to treatment

In this setup, we assume that there exist variables Z_i that affect the treatment D_i but not the outcomes (at least on its own). We let $X_i = (W_i, Z_i)$, where W_i is an observable control variable. It should satisfy

- **Relevancy**: It effects the propensity score $p(w, z) = \Pr(D_i = 1|W_i = w, Z_i = z)$
- **Exclusion**: Distribution of the counterfactual outcomes and u_i does not depend on $Z_i|X_i$. To put it in mathematical notation,

$$(y_i(1), y_i(0), u_i) \perp\!\!\!\perp Z_i|W_i$$

- **Monotonicity**: For a given $W_i = w$, z changes the treatment in the same direction for everyone. This is also called a no two-way movement condition.

These conditions need some explanation. First, for the exclusion condition, note that u_i is tied together with the potential outcomes. This implies that u_i and the outcome can be correlated, which is allowed in a non-CIA setup. What matters is that the changes in Z_i not affect u_i conditional on W_i. In addition, if the instrument Z_i directly affects $y_i(d)$ on its own, then the analysis of the effect of the treatment assignment D_i on the outcomes will be biased

since there is also part of the outcome affected by Z_i alone (and not D_i). In that regard, we need to rewrite our $y_i(d)$ notation as

$$y_i(d) = \mu(w, d) + \epsilon_i(d)$$

where $\mu(\cdot)$ does not depend on z.

Second, what does the monotonicity condition do? Suppose treatment assignment is binary with $D_i = \mathbb{1}[u_i < p(w, z)]$ for some value $W_i = w$ and $Z_i = z$. If u_i is independent of Z_i given W_i, then by moving from $p(w, z)$ to some $p(w, z')$ that is larger, we can find a group of individuals who do not get treated at $p(w, z)$ but do get treated at $p(x, z')$. Conversely, if changing z from z' decreases the new propensity score, we may find those who were previously treated not getting treated in a new setup. This type of i is called **compliers**. There are other individuals: an **always-taker** who is in treatment no matter the value of z and a **never-taker** who does not receive the treatment regardless of z'. This assumption allows for these three types of i.

There is one group of i that we have never talked about: the **defiers**. This group has $u_i < p(w, z)$ when $Z_i = z$ but $u_i' > p(x, z') > p(w, z)$ when $Z_i = z'$. This happens when u_i changes with z_i - a violation of the exclusion condition. You also see that changes in the treatment is two-way in the sense that while all other groups have non-decreasing assignment to treatment, defiers have decreasing assignment to treatment. Monotonicity condition is designed to rule out the defiers. However, most available data only has i under only one of $Z_i = z$ or $Z_i = z'$. So this is usually not testable, but still can be argued based on intuition or other proxy tests (How did the enrollment to treatment, not necessarily the same population, change after change in value of Z_i?).

With these assumptions, the parameter of interest that is identifiable are twofold. One is the **local average treatment effect (LATE)**, where we can identify the effect of a treatment on the population of compliers. The other is **marginal treatment effect (MTE)** where we allow $z|w$ to change continuously.

1.6 Local average treatment effect

The idea here is to find a 'localized' treatment in the sense that we narrow our interest to the group of compliers. It still averages the treatment, but on the compliers only. This is achieved by shifting the value of our instrument Z_i from one value to the other. We use the following setup

- Fix $W_i = w$
- Z_i will be a binary instrumental variable. Think of this as a variable that affects eligibility but not related to outcome.
- $D_i(w, z)$ can be characterized as $D_i(w, z) = \mathbb{1}[u_i < p(w, z)]$. Note that as z rises, so will $p(w, z)$ due to relevance condition.
- Z_i itself has no bearing, at least directly, on the outcome. So $y_i(d) = \mu(w, d) + \epsilon_i(d)$. So we still have $(\epsilon_i(1), \epsilon_i(0), u_i) \perp\!\!\!\perp Z_i | X_i$.

The formal way to define local average treatment effect is as follows

$$LATE(w, z, z') = E[y_i(1) - y_i(0) | p(w, z) < u_i < p(w, z'), W_i = w]$$

To derive the local average treatment effect equation whose estimand is comprised of observable data, we take the following approach. Note that $x' = (w, z')$ and $x = (w, z)$ and W_i is always fixed at w

$$\begin{aligned}
E[y_i|X_i = x'] - E[y_i|X_i = x] &= E[\mathbb{1}[u_i < p(x')]y_i(1) + \mathbb{1}[u_i > p(x')]y_i(0)|x'] \\
&\quad - E[\mathbb{1}[u_i < p(x)]y_i(1) + \mathbb{1}[u_i > p(x)]y_i(0)|x] \ (\because \text{Relation beween } u_i \text{ and } D_i) \\
&= E[\mathbb{1}[u_i < p(x')]y_i(1) + \mathbb{1}[u_i > p(x')]y_i(0)] \\
&\quad - E[\mathbb{1}[u_i < p(x)]y_i(1) + \mathbb{1}[u_i > p(x)]y_i(0)] \ (\because \text{Exclusion}) \\
&= E[(\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)])y_i(1) - (\mathbb{1}[u_i > p(x')] - \mathbb{1}[u_i > p(x)])y_i(0)] \\
&= E[(\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)])(y_i(1) - y_i(0))] \\
&= \Pr(\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = 1) \\
&\quad \times E[y_i(1) - y_i(0)|\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = 1]
\end{aligned}$$

Always-takers and never-takers are dropped out as $\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = 0$ for both of them. Since we rule out defiers using the monotonicity assumption, we need not worry about $\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = -1$. Lastly, $\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = 1$ holds iff $p(x) < u_i < p(x')$, or for compliers only. So to continue,

$$\begin{aligned}
&\Pr(\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(x)] = 1)E[y_i(1) - y_i(0)|\mathbb{1}[u_i < p(x')] - \mathbb{1}[u_i < p(z)] = 1] \\
&\quad = \Pr(p(x) < u_i < p(x'))E[y_i(1) - y_i(0)|p(x) < u_i < p(x')]
\end{aligned}$$

(To match with class notes, let $C_i = \mathbb{1}[p(x) < u_i < p(x')]$)

Therefore, we are able to back out the definition of the LATE and can identify them as

$$LATE(w,z,z') = \frac{E[y_i|X_i = x'] - E[y_i|X_i = x]}{\Pr(p(x) < u_i < p(x'))} = \frac{E[y_i|X_i = x'] - E[y_i|X_i = x]}{p(x') - p(x)}$$

Or in terms of the propensity score (and by bringing W_i, Z_i back in)

$$LATE(w, p(w,z), p(w,z')) = \frac{E[y_i|p = p(w,z')] - E[y_i|p = p(w,z)]}{p(w,z') - p(w,z)}$$

We can go further: Estimate propensity scores with

$$p(w,z) = E[D_i|W_i = w, Z_i = z]$$

and get

$$LATE(w,z,z') = \frac{E[y_i|w,z'] - E[y_i|w,z]}{E[D_i|w,z'] - E[D_i|w,z]}$$

To obtain this from regressions, we follow these steps (parametrically or nonparametrically):

1. Regress D on Z and other covariates W to get $\widehat{D} = \hat{p}(w,z)$
2. Regress Y on other covariates W and $\widehat{D}$.

The LATE estimator can then be obtained here is called the Wald estimator.

1.7 Marginal treatment effects

The marginal treatment effect at $p(w,z) = p$ is defined as the treatment effect on individuals whose $u_i = p(w,z)$. We can write

$$MTE(p) = E[y_i(1) - y_i(0)|u_i = p]$$

The conditional expectation above is not directly obtainable from the data. Heckman and Vytlacil show that

$$MTE(p) = \frac{\partial E[y_i|p(w,z) = p]}{\partial p}$$

which is obtainable from the data. This is done by

1. Estimate $p(w,z) = \Pr(D_i = 1|W_i = w, Z_i = z)$

2. Regress y_i on the estimated $p(w,z)$ and W_i in a flexible setting - preferably not just linearly but with some nonlinearities and interaction between W_i and $p(w,z)$. The former is to check whether MTE and p has nonlinear regression and the latter is to incorporate heterogeneities across different W_i
3. Take a derivative with respect to p. (or local linear estimator)
4. For treatment effects, evaluate the $E[y_i|p(w,z),x]$ at $p(w,z) = 1$ and $p(w,z) = 0$ and identify the difference. (You can obtain $E[y_i|\cdot]$ by getting the predicted values).

Intuitively, what is going on with MTE is as follows: By changing p slightly by dp, we are able to identify the marginal compliers who move from not being treated to being treated. We are finding out how their outcome changes as they move from non-participation to participation into the treatment.

To see why the above result holds, we define

$$G(p) = E[y_i \cdot \mathbb{1}[p(w,z) = p]]$$

Since $y_i = D_i y_i(1) + (1 - D_i)y_i(0) = \mathbb{1}[u_i < p(w,z)]y_i(1) + \mathbb{1}[u_i > p(w,z)]y_i(0)$, we can rewrite the above as

$$\begin{aligned} G(p) &= E[y_i(1) \cdot \mathbb{1}[u_i < p(w,z)] \cdot \mathbb{1}[p(w,z) = p] + y_i(0) \cdot \mathbb{1}[u_i > p(w,z)] \cdot \mathbb{1}[p(w,z) = p]] \\ &= E[y_i(1) \cdot \mathbb{1}[u_i < p] \cdot \mathbb{1}[p(w,z) = p]] + E[y_i(0) \cdot \mathbb{1}[u_i > p] \cdot \mathbb{1}[p(w,z) = p]] \\ &= G_1(p) + G_0(p) \end{aligned}$$

By the exclusion condition and the fact that $u_i \sim U[0,1]$ (and thus $f(u_i) = 1$), we can write

$$\begin{aligned} E[y_i(1) \cdot \mathbb{1}[u_i < p] \cdot \mathbb{1}[p(w,z) = p]] &= E[y_i(1) \cdot \mathbb{1}[u_i < p]] \Pr(p(w,z) = p) \\ &= \int_0^p E[y_i(1)|u = t]dt \Pr(p(w,z) = p) \end{aligned}$$

And similarly,

$$E[y_i(0) \cdot \mathbb{1}[u_i > p] \cdot \mathbb{1}[p(w,z) = p]] = \int_p^1 E[y_i(0)|u = t]dt \Pr(p(w,z) = p)$$

So

$$E[y_i \cdot \mathbb{1}[p(w,z) = p]] = G(p) = \left(\int_0^p E[y_i(1)|u = t]dt + \int_p^1 E[y_i(0)|u = t]dt \right) \Pr(p(w,z) = p)$$

with the fact that $E[y_i \cdot \mathbb{1}[p(w,z) = p]] = E[y_i | p(w,z) = p] \cdot \Pr(p(w,z) = p)$, this implies that

$$E[y_i | p(w,z) = p] = \frac{G(p)}{\Pr(p(w,z) = p)} = \int_0^p E[y_i(1)|u = t]dt + \int_p^1 E[y_i(0)|u = t]dt$$

Then, by Leibniz's integral rule

$$\frac{\partial E[y_i | p(w,z) = p]}{\partial p} = E[y_i(1)|u = p] - E[y_i(0)|u = p] = MTE(p)$$

Theorem 1.1 (Leibniz Integral rule). *For the integral of the form*

$$\int_{a(x)}^{b(x)} f(x,t)dt, \;\; a(x), b(x) \in (-\infty, \infty)$$

the derivative with respect to x is

$$\frac{d}{dx}\left(\int_{a(x)}^{b(x)} f(x,t)dt\right) = f(x, b(x))\frac{d}{dx}b(x) - f(x, a(x))\frac{d}{dx}a(x) + \int_{a(x)}^{b(x)} \frac{\partial}{\partial x} f(x,t)dt$$

1.8 Caveats for LATE and MTE

For the above methods to work, we need the Z_i instruments to satisfy

- Z belongs in the treatment equation (relevancy): $D_i = 1(u_i < p(W_i, Z_i))$
- Z does not belong in the outcome equation (exclusivity): $y_i(d) = \mu(w,d) + \epsilon_i(d)$
- In other words, we get $(\epsilon_i(1), \epsilon_i(0), u_i) \perp\!\!\!\perp Z_i | W_i$

For the range of $p(w,z)$ available, the above condition allows us to estimate the LATE and MTE. For MTE, you also want $p(w,z)$ to be continuous with z so that derivatives are defined.

With this framework, we can also test if conditional independence assumption holds. Recall that conditional independence assumption is satisfied when

$$(\epsilon_i(1), \epsilon_i(0)) \perp\!\!\!\perp u_i | X_i$$

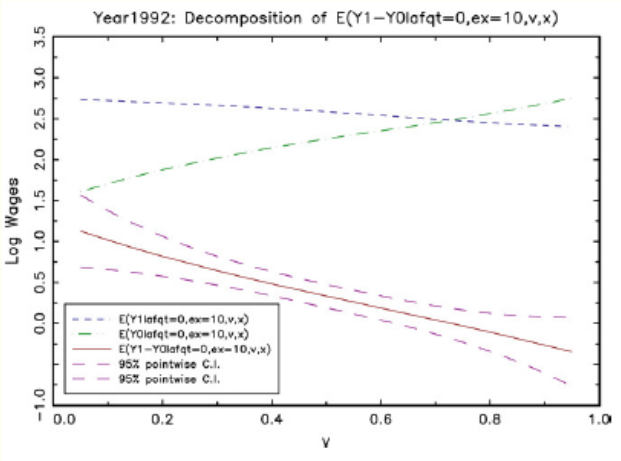
In cases where this is true, then the outcomes are independent of u_i conditional on X_i. Thus,

$$MTE(x,p) = E[Y_i(1) - Y_i(0)|X_i = x, u_i = p] = E[Y_i(1) - Y_i(0)|X_i = x]$$

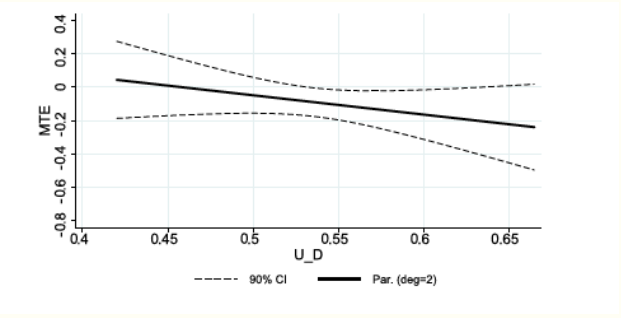
This means that $u_i = p$ does not affect the value of $E[Y_i(1) - Y_i(0)|X_i = x]$. We know that $MTE(x,p) = \frac{\partial E[Y_i|X_i=x,p(w,z)=p]}{\partial p}$, which means that $E[Y_i|X_i = x, p(x,z) = p]$ is linear in p if CIA holds. Thus, it is highly recommended to put polynomial terms of $p^k, k = 1,2,3,...$ when you estimate marginal treatment effects. Then, test to find whether the nonlinear terms have coefficient zero. This is feasible if you have 3 or more points of $Z|W$.

Example 1.1 (Carneiro & Lee, JoE 2009)**.** *The paper estimates the impact of attending college on log wage distributions. The paper finds that individuals more likely to attend college (and have low resistance parameters) are more likely to have higher college wage ($Y_i(1)$) over high school wage ($Y_i(0)$). The opposite holds true for people with high school degree. They have a MTE figure (at figure 3) that maps MTE as a function of resistance parameters.*

Example 1.2 (Johnson & Taylor, QE 2019)**.** *The paper shows that causal impact of migration decreases longevity. This is even with the consideration that migrants are more likely to be educated and have higher baseline earnings compared to non-migrants. They use the MTE to (with railcar traffic at the town of origin as one of their IVs). They document that those who have lower latent ability (high U_d) suffer more from migrating out, reflected in the downward sloping MTE*



(a) Carneiro, Lee (2009)



(b) Johnson, Taylor (2019)