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Identification of Causal Intensive Margin Effects by Difference-in-Difference Methods *

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Abstract

This paper discusses identification of causal intensive margin effects. The causal intensive margin effect is defined as the treatment effect on the outcome of individuals with a positive outcome irrespective of whether they are treated or not (always-takers or participants). A potential selection problem arises when conditioning on positive outcomes, even if treatment is randomly assigned. We propose to use difference-in-difference methods - conditional on positive outcomes - to estimate causal intensive margin effects. We derive sufficient conditions under which the difference-in-difference methods identify the causal intensive margin effect in a setting with random treatment.

JEL Classification: C21, C24, C18

Keywords: Intensive margin effect, difference-in-difference, corner solution models, potential outcomes, policy evaluation.

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1 Introduction

A decomposition of a binary treatment (e.g., policy intervention) into extensive and intensive margin effects is of special interest when studying economic outcomes with a corner solution at zero.[1] Economic outcomes with corner solutions include health expenditures, working hours, and trade volumes. The average effect of a treatment on an outcome with a corner solution at zero can be decomposed into 1) the average change in the outcome of those with a positive outcome irrespective of treatment (always-takers or participants), plus 2) the average outcome of those with a positive outcome in the case of treatment and a zero outcome in the case of no treatment, minus 3) the average outcome of those with a zero outcome in the case of treatment and a positive outcome in the case of no treatment (Lee, 2012, 2017; Staub, 2014). Part 1) represents the weighted causal intensive margin effect. The sum of 2) and 3) captures the weighted causal extensive margin effect.[2]

Even if treatment is randomly assigned, a mean comparison of treatment and control groups with positive outcomes does not identify the causal intensive margin effect without additional assumptions (Angrist, 2001). This can be illustrated by the following example. Consider an experiment in which the outcome of interest is health expenditure. A randomly assigned treatment group receives an insurance contract with a low copayment rate and a control group receives a contract with a high copayment rate. Suppose we are interested in the treatment effect of those having positive health expenditure irrespective of whether they face a high or a low copayment rate (intensive margin effect). The sample of individuals with positive health expenditure consists of two groups: 1) the group of individuals with positive health expenditure irrespective of whether they face a high or a low co-payment rate; and 2) the group of individuals with positive health expenditure only because they face a low copayment rate, who would have zero health expenditure if they faced a high copayment rate. For the causal intensive margin effect, we are only interested in the first group. Group membership, however, is not observed in

[1] Corner solutions at alternative thresholds are possible as well. For simplicity and illustration, we consider the case where the threshold is at zero.

[2] The weights are given by the relative size of the group in the population.

the data, because we observe either the outcome in the case of treatment or the outcome in the case of no treatment. The unobserved characteristics of the two groups are likely to be different. On average, individuals in the second group are likely to be in better health than individuals in the first group because they exhibit zero health expenditure when facing the high copayment rate. Therefore, health expenditure in the first group is likely to be higher than in the second group. As a result, a mean comparison conditional on positive health expenditure does not have a causal interpretation due to a potential selection bias.

The literature on policy evaluation has developed well established methods to deal with selection problems. The list of methods includes difference-in-difference, instrumental variable, regression discontinuity, control function approaches, and matching. For this reason, it appears appropriate to use these methods to overcome the potential selection bias and estimate the causal intensive margin effect.

In this paper, we discuss difference-in-difference methods to estimate the causal intensive margin effect in a setting with random treatment.[3] In contrast to standard difference-in-difference estimators, we condition on individuals with positive outcomes.[4] We derive sufficient conditions under which the causal intensive margin effect is identified. Analogous to the standard difference-in-difference literature, we discuss the treatment-versus-control, the pre-versus-post, and the difference-in-difference estimator. Moreover, we discuss how the identifying assumptions can be motivated.

This paper is related to the literature on models for non-negative outcomes with a mass point at zero. This includes Tobit models (McDonald & Moffitt, 1980; Tobin, 1958), two-part models (Cragg, 1971; Duan, Manning, Morris, & Newhouse, 1983), and selection models (Heckman, 1979). Moreover, the paper is closely related to the literature employing *principal stratification* following Frangakis and Rubin (2002) to study causal extensive and intensive margin treatment effects for variables with nonnegative outcomes (Lee, 2012, 2017; Staub, 2014). This literature decomposes the average treatment ef-

[3]We consider the term difference-in-difference methods to include both difference and difference-in-difference methods.

[4]We refer to the term "standard difference-in-difference" to denote difference-in-difference methods that do not condition on positive outcomes.

fect into a population-weighted sum of treatment effects on participants and switchers.[5] Studying outcomes with a corner solution at zero, Staub (2014) derives nonparametric bounds for the treatment effects on participants and switchers. He further discusses point identification of causal intensive and extensive margin effects in censored regression, selection, and two-part models. Lee (2012, 2017) analyzes total, extensive, and intensive margin effects in general sample selection models, with the corner solution outcome as a special case. Lee (2012) analyzes nonparametric methods to estimate extensive and intensive margin effects, whereas Lee (2017) discusses point identification of intensive and extensive margin effects in semiparametric linear models.

This paper is connected to the literature on policy evaluation in the potential outcomes framework. See Angrist and Pischke (2009) for a summary. In particular, we apply difference-in-difference methods to identify the causal intensive margin effect. See Lechner (2010) for a survey on difference-in-difference methods from a potential outcomes perspective. The difference-in-difference estimator presented in this paper relies on a common trend assumption similar to the common trend assumption of standard difference-in-difference estimators. In contrast to standard difference-in-difference methods, monotonicity assumptions are additionally required to identify the causal intensive margin effect.

The main contribution of this paper is to extend the literature on identification of intensive margin effects by borrowing well established difference-in-difference methods from the policy evaluation literature. The difference-in-difference estimator on positive outcomes represents an alternative to estimate intensive margin effect when pretreatment information is available. Moreover, this paper discusses sufficient conditions under which a mean comparison conditional on a positive outcome (treatment-versus-control estimator on positive outcomes) identifies the causal intensive margin effect. A mean comparison is often applied in two-part models to estimate the intensive margin effect. Therefore, this paper clarifies in which cases estimates of two-step estimators possess a

[5]Switchers (compliers and defiers) represent individuals with a positive outcome in the case of treatment and a zero outcome in the case of no treatment, as well as individuals with a zero outcome in the case of treatment and a positive outcome in the case of no treatment.

causal interpretation.

A decomposition into extensive and intensive margin effects can provide valuable information for policy design. Take as an example the effect of the introduction of a partial retirement policy. Suppose that in the status quo, individuals must withdraw the full pension at a given age, but are allowed to continue working. Under the partial retirement policy, individuals have the choice between a partial and a full pension, and are allowed to continue working. The total effect on labor supply of such a policy may be zero or negative, suggesting that the policy has been ineffective.[6] The zero result, however, could be explained by a positive extensive margin effect that was offset by a negative intensive margin effect. Older workers who would have retired in the absence of a partial retirement policy now decide to stay in the labor market. Likewise, individuals who would have worked full-time in the absence of a partial retirement policy, decide to work part-time. The welfare effect of such a policy may be positive through retained human capital, although the total effect on labor supply is small.

The remainder of the paper is organized as follows. In section 2, we describe how the intensive margin effect is embedded in the causal decomposition based on potential outcomes. Identification of the causal intensive margin effect is described in section 3. In section 4 we discuss how the identifying assumptions can be verified. The last section concludes the paper.

2 Causal decomposition of a treatment effect

2.1 Notation and setup

We consider the standard potential outcomes framework with a non-negative outcome Y and a binary treatment D (Rubin, 1974). Each individual i is endowed with two potential outcomes. The potential outcome in the case of treatment ($D_i = 1$) is denoted by Y_i^1 and in the case of no treatment ($D_i = 0$) by Y_i^0. We only observe one of the two potential outcomes. We observe individuals in the pretreatment period $t-1$, and in the

[6] See, for example, Börsch-Supan, Bucher-Koenen, Kutlu-Koc, and Goll (2018) for evidence on the effect of partial retirement policies on labor supply in eleven OECD countries.

post-treatment period t; that is $Y_{i,t}$ and $Y_{i,t-1}$. A randomly assigned binary treatment D_i takes place between period $t-1$ and period t. Potential outcomes of individual i are denoted with superscript; that is:

a) $Y^1_{i,t}$: Potential outcome in period t in the case of treatment.

b) $Y^1_{i,t-1}$: Potential outcome in period $t-1$ in the case of treatment.

c) $Y^0_{i,t}$: Potential outcome in period t in the case of no treatment.

d) $Y^0_{i,t-1}$: Potential outcome in period $t-1$ in the case of no treatment.

Observed outcomes are denoted without superscript; that is $Y_{i,t}$ and $Y_{i,t-1}$. As an implication of random treatment assignment, treatment is independent of joint potential outcomes; that is $D_i \perp\!\!\!\perp (Y^1_{i,t}, Y^1_{i,t-1}, Y^0_{i,t}, Y^0_{i,t-1})$.

2.2 Causal decomposition

We define four exhaustive and mutually exclusive subgroups based on the joint distribution of potential outcomes in period t following Lee (2012) and Staub (2014):

	$Y^0_{i,t}=0$	$Y^0_{i,t}>0$
$Y^1_{i,t}=0$	Nonparticipants	Switchers 2
$Y^1_{i,t}>0$	Switchers 1	Participants

Based on this definition, we decompose the average treatment effect (ATE) at time t as follows:

$$
\begin{aligned}
ATE_t &= E(Y^1_{i,t} - Y^0_{i,t}) && (1)\\
&= E(Y^1_{i,t}|Y^1_{i,t}>0, Y^0_{i,t}=0)P(Y^1_{i,t}>0, Y^0_{i,t}=0) && (2)\\
&+ E(-Y^0_{i,t}|Y^1_{i,t}=0, Y^0_{i,t}>0)P(Y^1_{i,t}=0, Y^0_{i,t}>0) && (3)\\
&+ E(Y^1_{i,t} - Y^0_{i,t}|Y^1_{i,t}>0, Y^0_{i,t}>0)P(Y^1_{i,t}>0, Y^0_{i,t}>0) && (4)
\end{aligned}
$$

The terms in lines (2) and (3) represent the weighted causal extensive margin effect. Line (2) describes the effect of treatment on the outcome of individuals with positive

outcome in the case of treatment and zero outcome in the case of no treatment (switchers 1), weighted by the fraction of switchers 1. Line (3) describes the effect of treatment on the outcome of individuals with zero outcome in the case of treatment and positive outcome in the case of no treatment (switchers 2), weighted by the fraction of switchers 2. The contribution of individuals with zero outcome in the cases of treatment and no treatment (nonparticipants) is zero and therefore dropped.

The term in line (4) represents the weighted causal intensive margin effect. It captures the effect of treatment on the outcome of individuals having a positive outcome irrespective of treatment status (participants), weighted by the fraction of participants.

3 Identification

We are interested in the causal intensive margin effect

$$\gamma_t \equiv E(Y_{i,t}^1 - Y_{i,t}^0 | Y_{i,t}^1 > 0, Y_{i,t}^0 > 0). \tag{5}$$

We derive sufficient conditions under which the causal intensive margin effect is identified in difference-in-difference methods on positive outcomes. Analogous to the standard difference-in-difference literature, we discuss the treatment-versus-control, the pre-versus-post, and the difference-in-difference estimator.

3.1 Treatment-versus-control estimator on positive outcomes

The *treatment-versus-control estimator on positive outcomes* is given by the difference in conditional expectation of treated and untreated individuals with positive outcomes

$$\gamma_t^{TC} = E(Y_{i,t} | Y_{i,t} > 0, D_i = 1) - E(Y_{i,t} | Y_{i,t} > 0, D_i = 0). \tag{6}$$

Proposition 1 (Identification treatment-versus-control estimator on positive outcomes)**.** *Sufficient conditions to identify the causal intensive margin effect using the treatment-versus-control estimator on positive outcomes are:*

1. Stable unit treatment value assumption (SUTVA) (assumption 1), and

2. no switchers (assumption 2).

Or

1. SUTVA (assumption 1), and

2. conditional mean independence (assumption 3).

Assumption 1 (SUTVA)**.** *The stable unit treatment value assumption is given by*

$$Y_{i,t} = (1 - D_i)Y_{i,t}^0 + D_i Y_{i,t}^1 \quad \forall i, \quad \textit{and}$$
$$Y_{i,t-1} = (1 - D_i)Y_{i,t-1}^0 + D_i Y_{i,t-1}^1 \quad \forall i,$$

where $D_i \in \{0, 1\}$ *denotes treatment status.*

The *SUTVA* ensures that we actually observe the potential outcomes in the treatment and control groups. The *SUTVA* implies that the observed outcome of individual i only depends on the potential outcomes and the treatment status D_i, but not on the treatment status D_j of any other individual j. Thus, *SUTVA* rules out general equilibrium effects and spill-over effects.

Assumption 2 (No switchers)**.** *The assumption of no switchers is given by*

$$Y_{i,t}^1 > 0 \quad \Leftrightarrow \quad Y_{i,t}^0 > 0 \quad \forall i.$$

The assumption of *no switchers* states that the potential outcome in the case of treatment is positive if and only if the potential outcome in the case of no treatment is positive. It therefore excludes the possibility that individuals have a positive outcome in the case of treatment and a zero outcome in the case of no treatment (switchers 1), or vice versa (switchers 2).

Assumption 3 (Conditional mean independence)**.** *The assumption on conditional mean independence is given by*

$$E(Y_{i,t}^1|Y_{i,t}^1 > 0, Y_{i,t}^0 = 0) = E(Y_{i,t}^1|Y_{i,t}^1 > 0, Y_{i,t}^0 > 0), \quad \textit{and}$$
$$E(Y_{i,t}^0|Y_{i,t}^1 = 0, Y_{i,t}^0 > 0) = E(Y_{i,t}^0|Y_{i,t}^1 > 0, Y_{i,t}^0 > 0).$$

The assumption of *conditional mean independence* states that the expected potential outcome in the case of treatment of switchers 1 is equal to the expected potential outcome in the case of treatment of participants. Furthermore, the expected potential outcome in the case of no treatment of switchers 2 is equal to the expected potential outcome in the case of no treatment of participants.

Proof. Under *SUTVA* and *random treatment*, and by the law of iterated expectations, equation (6) can be rewritten as

$$\begin{aligned}\gamma_t^{TC} = &\left[pE(Y_{i,t}^1|Y_{i,t}^1 > 0, Y_{i,t}^0 > 0) + (1-p)E(Y_{i,t}^1|Y_{i,t}^1 > 0, Y_{i,t}^0 = 0)\right] \\ &- \left[qE(Y_{i,t}^0|Y_{i,t}^1 > 0, Y_{i,t}^0 > 0) + (1-q)E(Y_{i,t}^0|Y_{i,t}^1 = 0, Y_{i,t}^0 > 0)\right],\end{aligned}$$

where $p \equiv Pr(Y_{i,t}^0 > 0|Y_{i,t}^1 > 0)$ and $q \equiv Pr(Y_{i,t}^1 > 0|Y_{i,t}^0 > 0)$. This term is equal to the causal intensive margin of interest in equation (5) if a) $p = q = 1$ (assumption of *no switchers*), or if b) the expected potential outcome in the case of treatment of switchers 1 is equal to the expected potential outcome of participants, and the expected potential outcome in the case of no treatment of switchers 2 is equal to the expected potential outcome of participants (assumption of *conditional mean independence*). □

3.2 Pre-versus-post estimator on positive outcomes

The *pre-versus-post estimator on positive outcomes* is given by the difference in the conditional expectations between pretreatment and posttreatment outcomes for treated individuals with positive outcomes in both periods

$$\gamma_t^{PP} = E(Y_{i,t} - Y_{i,t-1} | Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 1). \tag{7}$$

Proposition 2 (Identification pre-versus-post estimator on positive outcomes)**.** *Sufficient conditions to identify the causal intensive margin effect using the pre-versus-post estimator on positive outcomes are*

1. *SUTVA (assumption 1),*
2. *no anticipation (assumption 4),*
3. *treatment monotonicity at the extensive margin (assumption 5),*
4. *time monotonicity at the extensive margin (assumption 6), and*
5. *no time trend in positive outcomes (assumption 7).*

Assumption 4 (No anticipation)**.** *The no anticipation assumption is given by*

$$E(Y_{i,t-1}^1 - Y_{i,t-1}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) = 0.$$

The *no anticipation* assumption states that in expectation, individuals with a positive outcome in both periods in the case of treatment do not differ in their potential outcomes in period $t-1$. Hence, in expectation, individuals do not change their behavior depending on their treatment status in period $t-1$ in anticipation of their treatment between period $t-1$ and t.

Assumption 5 (Treatment monotonicity at the extensive margin)**.** *The treatment monotonicity at the extensive margin assumption is given by*

$$\begin{aligned} Y_{i,t}^1 > 0 \quad &\Rightarrow \quad Y_{i,t}^0 > 0 \quad \forall i, \quad or \\ Y_{i,t}^0 > 0 \quad &\Rightarrow \quad Y_{i,t}^1 > 0 \quad \forall i. \end{aligned}$$

The assumption of *treatment monotonicity at the extensive margin* states that a positive outcome in the case of treatment implies a positive outcome in the case of no treatment

or vice versa. Therefore, the treatment response is monotone with respect to the extensive margin decision. Note that this assumption only restricts the sign of the extensive margin effect. Thus, given the potential outcome in the case of treatment is positive, the potential outcome in the case of no treatment is allowed to be higher or lower than the potential outcome in the case of treatment. The assumption only requires that the potential outcome in the case of no treatment is positive. The assumption of *treatment monotonicity at the extensive margin* is weaker than the *no switchers* assumption because *treatment monotonicity at the extensive margin* allows for one type of switchers (either switchers 1 or switchers 2).

Assumption 6 (Time monotonicity at the extensive margin)**.** *The time monotonicity at the extensive margin assumption is given by*

$$\begin{aligned} Y_{i,t}^0 > 0 \quad &\Rightarrow \quad Y_{i,t-1}^0 > 0 \quad \forall i, \quad \textit{and} \\ Y_{i,t}^1 > 0 \quad &\Rightarrow \quad Y_{i,t-1}^1 > 0 \quad \forall i. \end{aligned}$$

The assumption of *time monotonicity at the extensive margin* states that a positive outcome in period t implies a positive outcome in period $t-1$, both in the cases of treatment and no treatment. Thus, we assume that there are no individuals participating in period t who are not participating in period $t-1$. This assumption only restricts the sign of the extensive margin effect. Thus, given the potential outcome in period t is positive, the potential outcome in period $t-1$ is allowed to be higher or lower than the potential outcome in period t.

Assumption 7 (No time trend in positive outcomes)**.** *The assumption of no time trend in positive outcomes is given by*

$$E(Y_{i,t}^0 - Y_{i,t-1}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) = 0.$$

The assumption of *no time trend in positive outcomes* states that there is no time trend in the expected potential outcome in the case of no treatment of those with positive outcome in both periods in the case of treatment.

Proof. Under *SUTVA* and *random treatment* assignment, equation (7) can be rewritten as $E(Y^1_{i,t} - Y^1_{i,t-1}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0)$. By the *no anticipation* assumption, this term can be rewritten to $E(Y^1_{i,t} - Y^0_{i,t-1}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0)$. By the *no time trend in positive outcomes* assumption, this term can be rewritten to $E(Y^1_{i,t} - Y^0_{i,t}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0)$. Using *time* and *treatment monotonicity at the extensive margin*, the conditioning set can be rewritten to $E(Y^1_{i,t} - Y^0_{i,t}|Y^1_{i,t} > 0, Y^0_{i,t} > 0)$.[7] □

3.3 Difference-in-difference estimator on positive outcomes

The *difference-in-difference estimator on positive outcomes* combines the aforementioned estimators and is given by the difference of differences

$$\gamma_t^{DiD} = E(Y_{i,t} - Y_{i,t-1}|Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 1) - E(Y_{i,t} - Y_{i,t-1}|Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 0). \tag{8}$$

Proposition 3 (Identification difference-in-difference estimator on positive outcomes)**.** *Sufficient conditions to identify the causal intensive margin effect using the difference-in-difference estimator on positive outcomes are*

1. *SUTVA (assumption 1),*
2. *no anticipation (assumption 4),*
3. *treatment monotonicity at the extensive margin (assumption 5),*
4. *time monotonicity at the extensive margin (assumption 6), and*
5. *common trend in positive outcomes (assumption 8).*

Assumption 8 (Common trend in positive outcomes)**.** *The common trend in positive outcomes assumption is given by*

$$E(Y^0_{i,t} - Y^0_{i,t-1}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0) = E(Y^0_{i,t} - Y^0_{i,t-1}|Y^0_{i,t} > 0, Y^0_{i,t-1} > 0).$$

[7] By the *time monotonicity at the extensive margin*, the conditioning set can be reduced to $E(Y^1_{i,t} - Y^0_{i,t}|Y^1_{i,t} > 0)$. Using the *treatment monotonicity at the extensive margin*, this term can be expanded to $E(Y^1_{i,t} - Y^0_{i,t}|Y^1_{i,t} > 0, Y^0_{i,t} > 0)$.

The *common trend in positive outcomes* assumption represents the key assumption for identification. The *common trend in positive outcomes* assumption is closely related to the standard common trend assumption,[8] except that we require the common trend to hold between two specific subgroups: the subgroup with a positive outcome in both periods in the case of treatment, and the subgroup with a positive outcome in both periods in the case of no treatment.

Proof. Assuming *SUTVA* and *random treatment*, equation (8) can be rewritten to

$$\gamma_t^{DiD} = E(Y_{i,t}^1 - Y_{i,t-1}^1 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) - E(Y_{i,t}^0 - Y_{i,t-1}^0 | Y_{i,t}^0 > 0, Y_{i,t-1}^0 > 0) \tag{9}$$

Adding and subtracting $E(Y_{i,t-1}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0)$ and $E(Y_{i,t}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0)$ to equation (9) and rearranging yields

$$\begin{aligned}
\gamma_t^{DiD} = {} & E(Y_{i,t}^1 - Y_{i,t}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) && (10)\\
& + E(Y_{i,t-1}^0 - Y_{i,t-1}^1 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) && (11)\\
& + E(Y_{i,t}^0 - Y_{i,t-1}^0 | Y_{i,t}^1 > 0, Y_{i,t-1}^1 > 0) && (12)\\
& + E(Y_{i,t-1}^0 - Y_{i,t}^0 | Y_{i,t}^0 > 0, Y_{i,t-1}^0 > 0). && (13)
\end{aligned}$$

Assuming *common trend in positive outcomes*, the sum of the two terms in line (12) and (13) equals 0. Moreover, under the *no anticipation assumption*, the sum of the term in line (11) is equal to zero. Assuming *time* and *treatment monotonicity at the extensive margin*, line (10) can be rewritten to $E(Y_{i,t}^1 - Y_{i,t}^0 | Y_{i,t}^1 > 0, Y_{i,t}^0 > 0)$.[9] □

Graphical derivation

Figure 1 illustrates the derivation. It is critical to note that even though treatment is randomly assigned, the expected outcomes of treatment and control groups with positive outcomes in periods t and $t-1$ are possibly different in period $t-1$. This is due to conditioning on positive outcomes in both periods t and $t-1$. For illustration purposes,

[8] In the standard difference-in-difference, the common trend assumption is given by $E(Y_{i,t}^0 - Y_{i,t-1}^0 | D_i = 1) = E(Y_{i,t}^0 - Y_{i,t-1}^0 | D_i = 0)$.

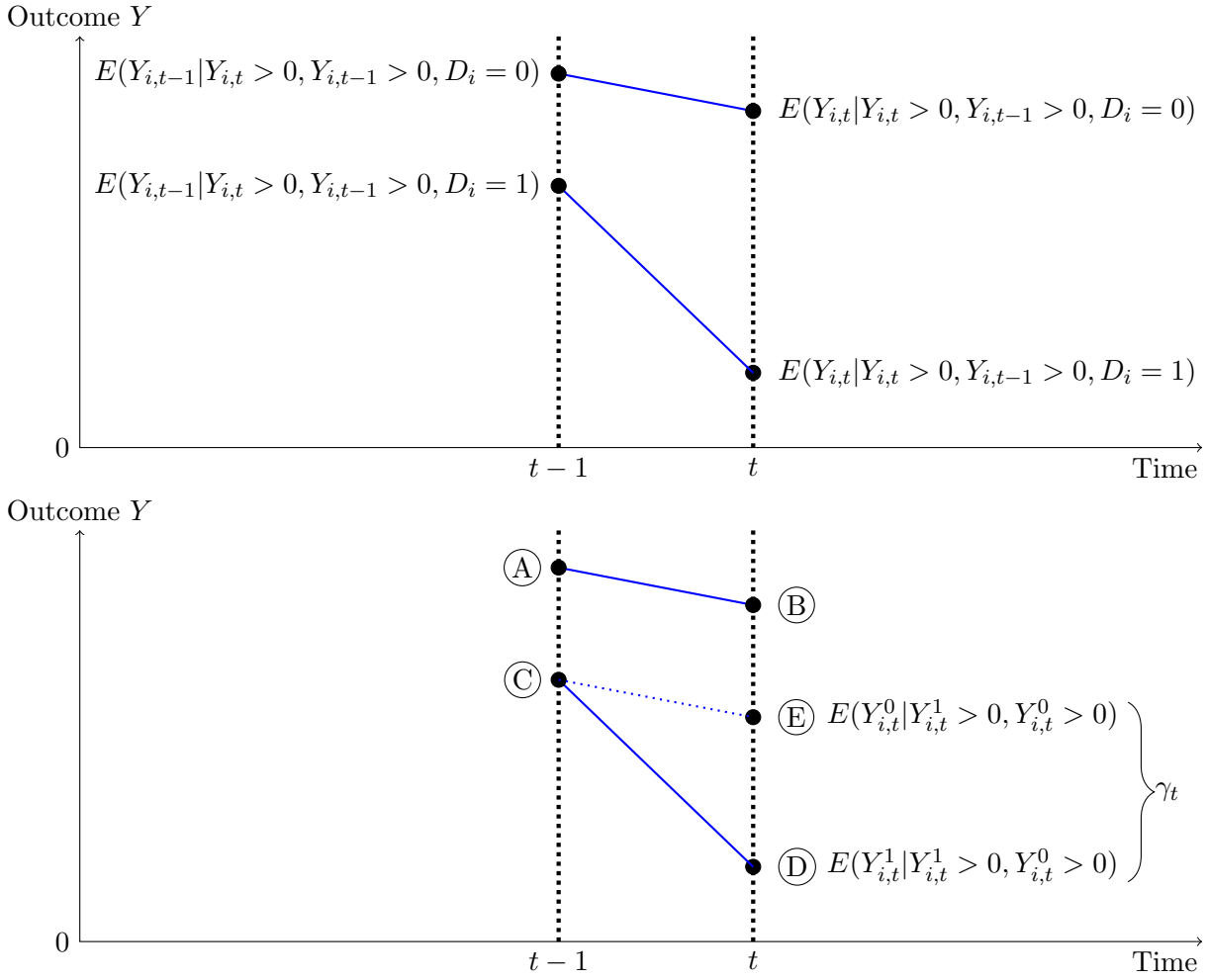
[9] See footnote 7.

we specify the expected outcome of the control group in both t and $t-1$ to be above the expected outcome of the treatment group.

As an example, consider the partial retirement policy from the introduction. Individuals reach the retirement age between period $t-1$ and t. Suppose that in the control group, individuals must claim the full pension at a given age (retirement age) but are allowed to continue working. Continued work does not increase future pension entitlements. Individuals in the treatment group have the choice between a partial and a full pension. A partial pension can be claimed if they reduce their working hours. If they claim a partial pension, continued work increases their future pensions. Therefore, individuals in the control group have strong incentives to leave the labor market at the retirement age, whereas individuals in the treatment group have incentives to work part-time after reaching the retirement age. In period t, individuals in the treatment group have lower working hours compared with individuals in the control group. Individuals in the treatment group face partial retirement incentives and therefore continue working, but with reduced working hours. In period $t-1$, no individual is treated. Individuals in the treatment group, however, are likely to have lower working hours in period $t-1$ compared with individuals in the control group. Control group individuals with a positive outcome in period t participate in the labor market, although they have strong incentives to leave the labor market. Therefore, control group individuals working in both periods are likely to have a higher attitude toward work and tend to work more than treatment group individuals working in both periods.

The four bold dots in the upper graph of Figure 1 depict the observed quantities. In (A), *SUTVA* and *random treatment* implies that the observed quantity is equal to $E(Y^0_{i,t-1}|Y^0_{i,t} > 0, Y^0_{i,t-1} > 0)$. Similar in (B), *SUTVA* and *random treatment* implies that the observed quantity is equal to $E(Y^0_{i,t}|Y^0_{i,t} > 0, Y^0_{i,t-1} > 0)$. In (C), *SUTVA*, *random treatment*, and the *no anticipation* assumption imply that the observed quantity is equal to $E(Y^0_{i,t-1}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0)$. The *common trend in positive outcomes* assumption implies that the rewritten quantities in (A), (B), and (C) identify $E(Y^0_{i,t}|Y^1_{i,t} > 0, Y^1_{i,t-1} > 0)$. Using *time* and *treatment monotonicity*, this term can be rewritten as (E) $= E(Y^0_{i,t}|Y^1_{i,t} >$

Figure 1: Graphical derivation of the *difference-in-difference estimator on positive outcomes*



Note: Observed quantities are shown in the upper graph. Causal intensive margin effect is shown in the lower graph.

$0, Y^0_{i,t} > 0)$. In (D), *SUTVA*, *random treatment* as well as *time* and *treatment monotonicity* imply that the observed quantity is equal to $E(Y^1_{i,t}|Y^1_{i,t} > 0, Y^0_{i,t} > 0)$. The difference between the quantities in (D) and (E) equals the causal effect of interest in equation (5).

4 Verification of identifying assumptions

With the exception of the *time monotonicity at the extensive margin* assumption, we cannot directly test the identifying assumptions. Instead, we propose alternative tests that can be used to motivate the identifying assumptions.[10]

Table 1 provides an overview of the assumptions for identification of the causal intensive margin effect by the estimators presented in section 3. Moreover, Table 1 summarizes the alternative tests that aim to motivate the identifying assumptions.

1) SUTVA: This assumption cannot be tested. One has to evaluate whether general equilibrium effects and spill-over effects between individuals are sufficiently small.

2) No switchers: The *no switchers* assumption cannot be directly tested. Alternatively, one can perform a test that the difference in the fraction with positive outcomes between the treatment and control groups; that is, $P(Y_{i,t} > 0|D_i = 1) - P(Y_{i,t} > 0|D_i = 0)$ is sufficiently close to zero. Under *SUTVA* and *random treatment* assignment, this term can be rewritten in terms of potential outcomes: $P(Y_t^1 > 0) - P(Y_t^0 > 0)$. This can be expanded to $P(Y_{i,t}^1 > 0, Y_{i,t}^0 = 0) - P(Y_{i,t}^1 = 0, Y_{i,t}^0 > 0)$. Note that even if $P(Y_{i,t}^1 > 0) - P(Y_{i,t}^0 > 0)$ is sufficiently close to zero, this does not rule out the case in which the proportion of switchers 1 and switchers 2 are positive and equal; that is $P(Y_{i,t}^1 > 0, Y_{i,t}^0 = 0) = P(Y_{i,t}^1 = 0, Y_{i,t}^0 > 0)$. In this case, we could mistakenly conclude that no switchers are present.

3) Conditional mean independence: This assumption cannot be tested using observed outcomes. Alternatively, one can compare pretreatment outcomes of the treatment and control groups with positive outcomes in period t and $t-1$; that is, $E(Y_{i,t-1}|Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 1) - E(Y_{i,t-1}|Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 0)$. Thereby, we test whether (A) and (C) in Figure 1 are congruent. As described in the example with partial retirement of the graphical derivation in section 3.3, congruent outcomes in period $t-1$ indicate that unobserved characteristics of treatment group and control group with positive outcome

[10] We cannot rule out the possibility that a given identifying assumption is violated even though the alternative test is not able to reject the null hypothesis that the assumption is fulfilled. The opposite is also possible. A given identifying assumption could be fulfilled, even if the alternative test rejects the null that the assumption is fulfilled.

Table 1: Verification of identifying assumptions for causal intensive margin effects

	Estimators			Testing	
Identifying assumption	TC	PP	DiD	Testable	Alternative
1) SUTVA	x	x	x	No	
2) No switchers	x*			No	$P(Y_{i,t} > 0 \mid D_i = 1) - P(Y_{i,t} > 0 \mid D_i = 0)$ sufficiently close to zero.
3) Conditional mean independence	x*			No	$E(Y_{i,t-1} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 1) - E(Y_{i,t-1} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 0)$ sufficiently close to zero.
4) No anticipation		x	x	No	$E(Y_{i,t-1} \mid D_i = 1) - E(Y_{i,t-1} \mid D_i = 0)$ sufficiently close to zero.
5) Treatment monotonicity at ext. margin		x	x	No	Must be motivated by economic theory.
6) Time monotonicity at ext. margin		x	x	Yes	$P(Y_{i,t} > 0 \mid Y_{i,t-1} = 0, D_i = 1)$ and $P(Y_{i,t} > 0 \mid Y_{i,t-1} = 0, D_i = 0)$ sufficiently close to zero.
7) No time trend in positive outcomes		x		No	1) $E(Y_{i,t} - Y_{i,t-1} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 0)$ sufficiently close to zero. 2) $E(Y_{i,t-1} - Y_{i,t-2} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 1)$ sufficiently close to zero.
8) Common trend in positive outcomes			x	No	$[E(Y_{i,t-1} - Y_{i,t-2} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 1)$ $-E(Y_{i,t-1} - Y_{i,t-2} \mid Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 0)]$ sufficiently close to zero.

Note: *Treatment-versus-control estimator on positive outcomes* (TC), *pre-versus-post estimator on positive outcomes* (PP), *difference-in-difference estimator on positive outcomes* (DiD). *Either *no switchers* or *conditional mean independence* must hold.

in period $t-1$ and t are similar.

4) No anticipation: The *no anticipation* assumption is not directly testable using observed outcomes. One can, however, inspect the difference in unconditional expectation in the pretreatment period; that is, $E(Y_{i,t-1}|D_i = 1) - E(Y_{i,t-1}|D_i = 0)$.

5) Treatment monotonicity at the extensive margin: The assumption of *treatment monotonicity at the extensive margin* cannot be tested using observed outcomes. Economic theory must be used to argue whether the assumption is fulfilled.

6) Time monotonicity at the extensive margin: This assumption can be directly tested by verifying whether individuals with a zero outcome in period $t-1$ have a positive outcome in period t. Therefore, one can test whether $P(Y_{i,t} > 0|Y_{i,t-1} = 0, D_i = 1)$ and $P(Y_{i,t} > 0|Y_{i,t-1} = 0, D_i = 0)$ are sufficiently close to zero.

7) No time trend in positive outcomes: This assumption cannot be directly tested. We propose two ways to motivate the assumption. One possibility is to test whether the difference in outcomes between $t-1$ and t of the control group with positive outcomes in period t and $t-1$, that is $E(Y_{i,t} - Y_{i,t-1}|Y_{i,t} > 0, Y_{i,t-1} > 0, D_i = 0)$, is sufficiently close to zero. Alternatively, one can test whether the pretreatment differences of the treated with positive outcomes are sufficiently close to zero; that is, $E(Y_{i,t-1} - Y_{i,t-2}|Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 1)$.

8) Common trend in positive outcomes: This assumption cannot be directly tested. Analogue to the assumption of *no time trend in positive outcomes*, one can inspect pretreatment outcomes. One can test whether the pretreatment differences between the treatment and control groups are sufficiently close to zero; that is, $E(Y_{i,t-1} - Y_{i,t-2}|Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 1)$ - $E(Y_{i,t-1} - Y_{i,t-2}|Y_{i,t} > 0, Y_{i,t-1} > 0, Y_{i,t-2} > 0, D_i = 0)$.

5 Conclusion

This paper extends the literature on the identification of causal intensive margin effects. We borrow difference-in-difference methods from the policy evaluation literature to identify the causal intensive margin effect in a setting with random treatment assignment.

We derive sufficient conditions under which the treatment-versus-control estimator on positive outcomes, the pre-versus-post estimator on positive outcomes, and the difference-in-difference estimator on positive outcomes identify the causal intensive margin effect. Furthermore, we discuss how the identifying assumptions can be motivated.

We show that the treatment-versus-control estimator on positive outcomes, often applied in two-part models, provides a causal estimate if there are *no switchers* or if *conditional mean independence* holds. We show that the difference-in-difference estimator on positive outcomes, compared to the standard difference-in-difference estimator, additionally requires *time* and *treatment monotonicity at the extensive margin*. Although we focus on the setting with random treatment, the methods discussed in this paper could be extended to a setting in which treatment is as good as randomly assigned conditional on observables.

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